

was long ago directed to its habits in this respect by Mr. Darwin in his delightful "Naturalists' Voyage" (p. 33). He there mentions that when watching a male and female of this species in flight, he "distinctly heard a clicking noise, similar to that produced by a toothed wheel passing under a spring catch."

This curious observation I had numerous opportunities of verifying in the course of three visits to Rio Janeiro in 1866, 1867, and 1869.

ROBERT O. CUNNINGHAM

Queen's College, Belfast, January 19

I HAVE noticed that, when moles are burrowing, the worms near make their way to the surface. I have also observed that starlings gather round and under cows in pasture-fields. Their doing so I have been in the habit of ascribing to the tread and grazing-work of the cows producing tremors in the ground, which worms may mistake for mole-work, and therefore crowd to the surface; and I have offered the same explanation for the method of hunting pursued by blackbirds and thrushes. They have practically found out that (earth-tremors induced by) small hopping-runs make "the poor inhabitants below" seek safety above, and that thus the hunters most readily secure a breakfast. I am not acquainted with the habits of those hunters.

Cambuslang

HENRY MUIRHEAD

### Galton's Whistles

WITHIN the last few days I have had the opportunity of making observations with Galton's whistle upon a large number of people and upon some cats, and I have come to some conclusions which are curious and suggestive, even though they may not be absolutely exact. Thus, on the whistle a line is marked which is the usual limit of human hearing, and which represents, I should say, a number of vibrations somewhere between 41,000 and 42,000 per second. Out of many hundreds of persons examined I have only met with one instance, a young man, in which I was satisfied that a note higher than this was heard. As a rule the compass of the ear of women is markedly higher than it is in men, and age seems to lower it sooner in men than in women. Is this a result of the female animal always having the more intimate protection of the young as her work, the young having notes of higher pitch than the adult? The fact is at least suggestive.

Very few of the persons experimented upon seemed to have the compass of one ear exactly the same as that of the other, the right ear usually hearing a higher note than the left, and this is more marked in men than in women.

The sense of direction of the sound in the human ear seems to be lost at a very much lower point than appreciation of the note, but this is not the case with cats; for until the instrument ceases to produce a note altogether, or at least one within their compass, they turn their faces to the source of it the moment it is produced. These facts are also suggestive. The cat still depends to a large extent for its food supply on the appreciation of high notes, and quite as much on the appreciation of the direction from which they come. The power of hearing a note of a pitch beyond the limits of our sense of direction is suggestive that that sense has been blunted by disuse; and it would be extremely interesting to know if the compass of direction is higher in savage than in civilised peoples. From facts known concerning their other senses, I should say it is likely to be higher.

This difference in the two compasses is further indicative that the appreciation of direction is the work of a separate organ, and Dr. Crum Brown's experiments suggest the semicircular canals, or the utricle or succule in association with them, as the seat of this sense. If, as Dr. Brown seems to have shown, the semicircular canals are the organs of the general sense of position and direction, it would not be a far-fetched idea, that the utricle has to do with the sense of the direction of sound and that the canals are additions to it. An analogous relation of the cochlea to the sacculle is suggested by the mere facts of anatomy. If it be, as Helmholtz believes, that the cochlea is the organ for the appreciation of *pitch*, the relations of the three divisions of the organ of hearing are to be easily understood, and these relations offer, at first sight, a singularly strong evolutionary argument. There is, first, the organ for the perception of sound vibrations, having a comparatively limited compass. To this is added an organ for the appreciation of the direction of the sounds, and another for the appreciation

of highly-pitched notes; and a part of the first of these becomes so modified as to be capable of interpreting position and direction generally, independently of sound. The facts of the development of the ear support such a view, and we may conclude that the sense of direction is more important than the appreciation of high notes; for the semi-circular canals appear, or at least one exists, in the Myxine, whilst a very rudimentary cochlea does not appear till we get high up in the fishes.

Birmingham

LAWSON TAIT

### Atmospheric Currents

MR. CLEMENT LEY (in vol. xv. p. 157 of NATURE) asks me for the absolute proof which I suppose to exist (1) that the upper current return trades "flowing from the equator descend again to the surface of the ocean on the polar sides of the calms of Cancer and of Capricorn," and (2) that "these equatorial currents subsequent to their descent on the polar sides of Cancer and of Capricorn are known as the westerly winds of the temperate zones"; (3) he further asks "what proof exists that the upper currents from the polar depressions and those from the equatorial depressions cross one another in the calms of Cancer and of Capricorn so as subsequently to become the trades and anti-trades respectively," and suggests that it is more reasonable to suppose that their currents intermingle, and that their mixed volume is then drawn off north and south, as required, to restore the equilibrium of the atmosphere, as suggested by myself with reference to the equatorial calms. Mr. Clement Ley's three questions may, I think, be fairly answered as one, all depending upon the same proof.

The correctness of my assertions with reference to the atmospheric currents flowing from the equator can be referred to the one crucial test, viz., Are the atmospheric currents which descend to the surface of the ocean on the equatorial and on the polar sides of the two zones of high pressure, similar in their constituents (*i.e.*, when they first become established as winds on the surface of the ocean) is their degree of electricity the same? is their degree of saturation the same? If these questions could be answered in the affirmative it would show that Mr. Ley's supposition with reference to the mixed volume of the upper currents was possible, but if, on the other hand, they are answered in the negative, Mr. Ley can hardly hold, I think, that I have put my statements forward too strongly.

Though I believe that the north-east and south-east trades meeting at the belt of equatorial calms are thrown upwards from the surface of the ocean, and in ascending do mix their volumes, the conditions of atmospheric currents meeting many thousand feet above the sea-level are entirely different, as they have not the ocean as a *point d'appui*, and there is no more difficulty in accounting for their currents passing one another and the heavier under-running the lighter, than there is for the Labrador, augmented by the East Greenland current, meeting and under-running the Gulf Stream.

At Teneriffe, and other mountainous regions, in the latitudes of the trades, observations have been made with reference to the height of the trade winds, and of the neutral strata intervening between them and the upper current, as also of the height of the lower portion of the equatorial return current, which flows at heights varying from 12,000 feet upwards above the sea-level.

Prof. C. P. Smyth, H.M. Astronomer for Scotland, in his very interesting work, "Teneriffe," gives us some very important data with reference to these currents, showing—

1. The extreme dryness of the north-east wind.
2. Its very moderate electricity.
3. The greater saturation of the south-west wind.
4. The descent of the south-west upper current.
5. The chemical difference between the two currents.

Though there is much that I might quote with advantage, I shall content myself with the following four paragraphs:—

Page 110. "If we must live in a wind by all means let it be the south-west, and not the north-east, that effete unwholesome and used-up polar stream. As to the chemical and sanitary qualities of the two winds there could be no comparison between them."

Page 170. "And so indeed we found before we had finished with our expedition, when the south-west wind descended to the very surface of the sea."

Page 184. "In short, whatever the north-east wind did, its electricity was always moderate."

Page 206. "The trade wind is undoubtedly a poor one for

bringing water, but its position in Teneriffe during summer is favourable for making it deposit any which may be present."

I think from these extracts, which are supported by other passages in Prof. Smyli's work, I am quite justified in arguing that the trades and counter-trades are not similar in their constituents, that their degrees of electricity and of saturation are not the same, and that therefore it is not reasonable to suppose that their upper currents intermingle at the belts of tropical calms, and that their mixed volume descends and is then drawn off north and south as required, to restore the equilibrium of the atmosphere.

As these opposite currents flowing in the northern hemisphere from the north-east and the south-west (approximately) do not intermingle, and their mixed volume does not descend in the calms of Cancer it must necessarily follow that the south-west or return equatorial current, does descend to the surface of the ocean on the polar side of the calms of Cancer, and equally that the north-east upper current does descend on the equatorial side.

I have by no means exhausted what I have to say on this subject, but Mr. Ley will doubtless understand that I am unable to treat it at greater length in your columns. The same line of argument would have enabled me to answer Mr. Ley's questions separately had space permitted.

DIGBY MURRAY

#### Mind and Matter

PERMIT me to correct a mistake on the part of Mr. Tupper (*NATURE*, vol. xv. p. 217), who, though starting with a correct notion that my letter (*NATURE*, vol. xv. p. 78) was intended to solve a problem, immediately fell into the error of regarding it as intended to prove an alleged fact.

The fact alleged, that consciousness depends on nervous organisation I assumed to be a fact, and undertook to indicate *how* the dependence might be conceived, or regarded, to exist.

First, I alleged that the hypothesis of matter being as susceptible of consciousness as spirit, was quite *conceivable*, as a *hypothesis*, whether or not it should be proved afterwards to be a wrong hypothesis.

Second, the connection of two so dissimilar entities as matter and subjectivity had not the objection of being anomalous or unique; for energy and matter were equally dissimilar and yet invariably united. The parity of mystery was not intended to establish "parity of probability as to the facts," but merely *parity of conceivability*. For it is surely some help to our entertaining a new conception if we can point to an existing similar conception.

Third, if such a mysterious entity as energy could be divided and combined (using the words in a loose sense) why should there be a difficulty in conceiving of the division and combination of subjectivity. By this I meant that as division of matter involved division of energy, as to *amount*, so division of matter might be conceived involving division of subjectivity, as to *amount*: so with combination.

Thus far, however, I had only cleared away difficulties "real or apparent" in the way of our *conceiving* the relation of consciousness to matter from the "materialistic" stand-point.

The essential part of my solution which indicated roughly the *modus* of the connection between matter and consciousness and which dealt with the great difficulty of the question—How to account for the *two* aspects of matter, the unconscious and conscious?—has not been touched by Mr. Tupper. This portion he excused himself from examining because he regarded it as based on the assumption that "the probability of subjectivity being a property of matter equals the fact of energy being related to matter," whereas it is based on the fact, or alleged fact, or assumption, that "the dependence of consciousness on nervous organisation seemed by the science of nerve-physiology to be fairly established." To mistake allegations of the conceivability of a notion for assumptions or intended proofs that the notion is true, as has been done by Mr. Tupper, is surely not equivalent to pointing out fallacies in the solution of a problem.

Will he admit that, if a "pointer" could "tell us" he scented a fox and immediately thereafter follow the scent of a hare, such would be an admirable analogy of how to practise "sound logic by the old *à priori* method?"

Stafford, January 17

W. S. DUNCAN

#### Pre-Glacial Man in America

DR. ABBOTT, in his interesting letter on the traces of pre-glacial man in America, supposes that it may be correct that the

American aborigines migrated from the Old World. This may be the case with the Red Indians, but we know that they drove out an earlier people—the mound-builders. However, both mound-builders and Red Indians were certainly post-glacial in their occupation of the northern parts of America, and the oldest traces of their existence may not date back to an earlier time than a late stage of the Neolithic period in Europe.

Palæolithic man in America holds the same relative position to these later peoples as he does in the Old World, and we have so far obtained no evidence to show whether he occupied Europe or America first. The position of his remains in the auriferous drift of California is the same as in that of Siberia; in the loess of the Mississippi as in that of the Danube and the Rhine; in the caves of Brazil along with extinct mammalia as in those of Europe; and in the lowland gravels of Virginia as in those of France and England.

The question of the post or pre-glacial age of palæolithic man depends in America as it does in Europe on that of the age of the deposits in which they are found, and this is at present a matter of inquiry and discussion which might be set at rest, as I have pointed out in the *Quarterly Journal of Science* for July of last year, by a thorough examination of the brick clays at Hoxne where palæolithic implements were first found in England.

Cornwall House, Ealing, January 27

THOMAS BELT

#### Holly Berries

REPORTS of the scarcity and abundance of holly berries have appeared in *NATURE* from the south-east of England and west of Scotland respectively. It may be interesting to note the condition of the holly crop at a point somewhere about midway between these two places. In North Staffordshire and Derbyshire the holly berries are by no means scarce. They are not so plentiful as they were last year, but there is a fair average crop.

I have seldom seen such crops of them as I saw in several places in South Wales about a month ago. It may be also worth adding that the most teeming bush I saw was at a place in Cardiganshire, which was as far as I could learn—and I made diligent inquiries—between four and five miles from the nearest hive of bees. I questioned closely several children on the spot who were intelligent enough to give me a minute description of most of the common birds and insects; not one of them had ever seen a bee.

D. EDWARDES

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#### The Meteor of January 7

AMONG the "Notes" in *NATURE*, vol. xv., p. 244, there is a description of a large meteor, of which I was fortunate enough to secure a good observation; but on comparing the apparent path, as observed by myself, with that recorded in the paragraph, I find the latter somewhat imperfect; the apparent path, as seen from near London, seems to have been curtailed both at beginning and at end of flight; probably the observer in question could further amplify his remarks, or some other correspondent send an observation. The following is an abstract from my note-book:—

"Birmingham, January 7, 10:31 P.M. G.M.T.—Meteor pear-shape, deep yellow merging into ruby-red towards the tail; commenced as a luminous point near  $\eta$  Hydreæ, gradually increased in size, motion very slow and unsteady, appeared to force its way with difficulty, and slight undulation. Near  $\alpha$  Leonis it attained the apparent size of Venus, the forward hemisphere now showing signs of internal commotion by the projection of ebullition prominences, which were swept back towards the tail, then 8° long, and vaporous. The latter portion of its flight was intercepted by houses, but on emergence it burst with a flash below  $\beta$  Leonis at A.R. 182°, D.N. 16°. Length of path, 52°; time of flight, five to six seconds; radiant point (in Fluvius Eridanus), No. 96 Tupman, or No. 164 of the B. A. Catalogue.

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#### Spectrum of New Star

THE spectrum of the new star in Cygnus is changeable, and is now very unlike Cornu's representation of it in a recent number of *NATURE* (vol. xv., p. 158). Your readers may not be aware that it is easy to see several of the bright lines without a powerful instrument, though not to measure them accurately. As observed with a Browning's "miniature spectroscope" attached to a 4½-inch refractor, the brightest line is now about at