

THURSDAY, MARCH 20, 1873

PERCEPTION AND INSTINCT IN THE LOWER ANIMALS

THE correspondence in these columns, called forth by the letters of Mr. Darwin and Dr. Huggins (*NATURE*, Feb. 13), may be counted among the many indications of the growing interest in psychology; while at the same time it furnishes evidence of how far our knowledge of mind is behind most of the other sciences. Of the important points in the valuable letters of Mr. Darwin and Dr. Huggins we shall speak presently. But let us remark first on the minor and distinct question raised by Mr. Wallace. He says: "The power many animals possess to find their way back over a road they have travelled blindfolded (shut up in a basket inside a coach, for example), has generally been considered to be an undoubted case of true instinct. But it seems to me that an animal so circumstanced will have its attention necessarily active, owing to its desire to get out of its confinement, and that by means of its most acute, and only available sense, it will take note of the successive odours of the way, which will leave on its mind a series of images as distinct and prominent as those we should receive by the sense of sight. The recurrence of these odours in their proper inverse order—every house, ditch, field, and village having its own well-marked individuality—would make it an easy matter for the animal in question to follow the identical route back, however many turnings and cross-roads it may have followed." The objections to this hypothesis, to which Prof. Robertson has given his adhesion, are very serious. Let the scent of the dog be ever so acute, it is in many ways ill suited for supplying the kind of guidance required. A hound on the track of a hare has to follow a stream of the same scent. The association here is between the hare and the smell of the hare. Are not the associations of smell all of this kind? Is there any evidence that either in man or beast one smell ever coheres to another so as to render possible a memory of odours apart from the objects that give them forth? We are not very certain about the facts which the theory is put forward to explain; they are, however, better authenticated than is the fundamental assumption involved in the explanation. But, for the sake of argument, let us grant that a dog shut up in a basket can, as the result of a simple experience, link together several thousand smells in an unbroken series; say, the stink of a dung-hill is associated with the odour of sweet hay, this with the scent of a flock of sheep passed on the road, this again with the smell of a railway station to the right, and so on during a journey of sixty or seventy miles. If it be solely by the aid of this memory of smells that the dog is to return to the place whence it was taken, it must needs make haste back. It will be too late if the sheep have changed their position on the road. Especially is it necessary that it should get home while the wind still continues to blow in the same direction, otherwise its landmarks will be all in confusion. One other difficulty: suppose the dog to have got into the fragrance of the hay-field, which is perhaps forty acres in extent, how is it to find the dung-hill at the north-west corner? particularly if the wind be blowing the wrong way. Is it to scour round the ill-defined outskirts of the perfume until it

comes on the ill smell of the dung-hill? If we try to conceive in terms of vision (we can make nothing of it from our experiences of smell) such a memory of smells as the dog is supposed capable of acquiring, we must represent to ourselves the sensations of being carried through a series of differently coloured mists, which, while they prevent us from seeing objects, blend and shade into one another. In such a case, though we might remember that the red came after the yellow, how, having got into the red, should we know in what direction the yellow might be found? These are among the difficulties that have not, it appears to us, been sufficiently considered by Mr. Wallace and Prof. Robertson.

But what are the facts to be explained? Such home-journeys of dogs as might, by a stretch of imagination, or perhaps more correctly, want of imagination, seem to be accounted for by the smell-hypothesis, rest only on a rather loose kind of evidence, which can be adduced quite as abundantly in support of performances to which this explanation can be in no way applicable. In returning home do dogs "follow the identical route" by which they were taken away? There is no evidence even of the second-hand, loose, hearsay description, that this ever happened in a single instance.* The general impression, on the contrary, is that they despise the windings of rivers, turnpikes, and railways, and make for their destination by the most direct route. For example, and to add one more to the thousands of stories, we may mention that since we sat down to write we have received a letter from a gentleman telling us that about fifty years ago his paternal grandfather, living at Quorn, near Derby, sent two hounds by coach to his maternal grandfather living at Liverpool. Two or three days after their arrival they absconded together; inquiries were set on foot, and it is said they were seen swimming the Mersey at a point a little above Liverpool, where the river is of great width. They could be traced no farther, but after some time they made their appearance at Quorn, "foot-sore and in bad condition." Again, sheep, pigeons, and other animals that have not the miraculous scent of the dog, are believed on as good authority to find their way home through strange regions and from equally long distances.

Alluding to this class of alleged facts, Mr. Spalding, in the February number of *Macmillan's Magazine*, ventured to favour the view that through all the turnings and windings of a long journey the creatures somehow retain a perception of the direction of the place from which they were taken, and he further ventured to think that a hint of a similar faculty is to be found in some men. In this connection the facts with regard to savages would be most valuable. What Mr. Darwin calls the "trifling fact," communicated in his letter of last week, namely that his horse, which had been sent from Kent, *via* Yarmouth, to Freshwater Bay, in the Isle of Wight, on the first day that Mr. Darwin rode him eastward, was very unwilling to return towards his stable, that every time Mr. Darwin slackened the reins "he turned sharply round and began to trot to the eastward by a little north, which was nearly in the direction of his home in Kent;" this observation, together with the circumstance that with the fact before his eyes, Mr. Darwin's "impression was that he somehow knew the direction whence he had been brought," appears

* See letter of "J. T." p. 384. We have other letters to follow.

to us very important indeed. In the present state of our knowledge of the subject a few such "trifling facts" are worth more than many volumes of ingenious speculation.

We come now to the more weighty question which formed the subject of Mr. Darwin's first letter. Is it probable that instincts have any other origin than transmission by inheritance of acquisitions resulting from what we call individual experience? We are here at the very outside edge of human knowledge, in a region where no prudent person would venture to speak with confidence. Indeed the mode of origin recognised in the question still appears a "wild theory" to such respectable representatives of educated opinion as the *Spectator*. Had it been our good fortune to know as matter of certain history that the well-marked instinctive antipathy towards butchers of the dog King and his descendants was originally produced by ill-treatment, we should have had evidence of the most positive and direct kind, that sometimes at least instincts do originate in this way. There seems no hope of getting such evidence in this particular case; and indeed it may well be that the instinct in question is much more ancient than Mr. Darwin seems inclined to suppose. It is however to be hoped that before long some lover of animals will try his hand at actually producing a new instinct. But while Mr. Darwin regards it as probable that most instincts are examples of inherited experience, he thinks it "almost certain that many of the most wonderful instincts have been acquired independently of habit, through the preservation of useful variations of pre-existing instincts. Other instincts may have arisen suddenly in an individual, and then been transmitted to its offspring, independently both of selection and serviceable experience, though subsequently strengthened by habit. The tumbler-pigeon is a case in point, for no one would have thought of teaching a pigeon to turn head over heels in the air; and until some bird exhibited a tendency in this direction, there could have been no selection." The authority of Mr. Spencer may be adduced in support of Mr. Darwin's position. He speaks of "the natural selection of incidental variations," and of feelings that cannot be referred to "the inherited effects of experiences." Nevertheless, let us look closely at this matter. Will Mr. Darwin's view bear to be stated in such a way as to express more than the fact that in a great many instances we cannot conceive how the instincts originated? Will it bear to be put in this form: that it is almost certain that many of the most wonderful instincts had their origin in useful variations or sudden conjunctions of psychological states of such a character as could not by possibility have any relation to the experiences either of the individual itself or of its ancestry? Anything short of this will, it seems to us, scarcely amount to the contention that instincts have a mode of origin distinct from experience and heredity. That some other factor of unknown power may work along with experience and heredity in producing instincts, we are not in a position to deny. But still less are we in a position to say that there is such a factor, or what that factor is, or to admit that it ever operates independently of experience and heredity. We do not know how the tumbling of the tumbler pigeon began. But suppose we were certain that we had witnessed the very first performance of this kind, and saw that it arose suddenly and

without any assignable cause: What then? How did the tumbling begin? To call it an incidental variation is but a way—and, because to some minds it looks like an explanation, a bad way—of stating our ignorance. But could we say so much as that it was in no way connected with experience and heredity? We think not. This tumbling is a fancy instinct, an outlet for the overflowing activity of a creature whose wants are all provided for without any exertion on its part. And if we had before us the evolutionary history of the pigeon we might be able to point to some long obsolete instinct or useful action and say, behold, when on the wing, the superabundant energy of the creature has burst along the old long disused but not obliterated tracks, and see the strange result.

This is the direction in which we think it would not be unscientific to look for an explanation, should we ever have any such facts to explain. A similar line of remark might be followed with respect to what Mr. Darwin calls useful variations of pre-existing instincts. The question is, whence these variations? Further, just in proportion as these variations are slight, must it be difficult to say that they are not connected with experience—with the experience of the individual. In pursuing this inquiry we should doubtless come on the question, What is meant by experience? Everybody, it may be said, surely knows that. Perhaps. It is, we think, probable that the discovery might be made that we have not very clear and well-defined ideas as to the exact nature, extent, and limits of what we call individual experience. Of course we cannot now enter on such an inquiry.

SEPULCHRAL MONUMENTS OF CORNWALL* II.

Nenia Cornubiæ. A descriptive essay, illustrative of the Sepulchres and Funereal Customs of the early Inhabitants of Cornwall. By W. Copeland Borlase, B.A., F.S.A. (London: Longmans; Truro: Netherton, 1872.)

MR. BORLASE, assisted by a party of friends, early in 1872, opened two barrows on the summit of one of the most commanding elevations in the district, about a quarter of a mile east of Trevelgue or Trevelga Cliff Castle, near St. Columb Minor. The most westerly is 250 ft. in circumference, 11 ft. high at the centre, and its greatest axis, having an east and west direction, is 100 ft. At a depth of 2 or 3 ft. from the surface, the entire substratum, to the amount of several hundred cart-loads, was burnt earth, as red and almost as fine as brickdust. Beneath it and towards the eastern side was a cairn of stones about 12 ft. in diameter, and 4 ft. high. Many of them had been brought from the neighbouring beach, and were blackened by fire. Under this lay a large spar stone, such as does not occur in the district, singularly flat for a stone of the kind, measuring 10.5 × 5.4 × 1.75 ft., on a level with the surrounding country, and covering a chamber 6.16 ft. long from N.W. to S.E., 2.5 ft. broad, and 2.75 ft. deep. Its sides were formed of four slate stones, 7 or 8 in. thick, and set on edge, on each of which the covering stone rested. The floor seemed to have been paved with slates, but they had been displaced, and portions of an unusually thick human skull were found below them.

The eastern or more conspicuous barrow was 80 ft. in diameter, 13 ft. high, and had a depression of 1.5 ft. at

* Continued from p. 337