attributed to heredity." Now in order to be admissible, a theory must harmonise with all the phenomena. It is an admitted axiom in science that even one "negative" case is all but fatal to a theory. Dreams occasionally "come true," and persons who have been impressed by such coincidences base on them a belief in ghosts. But the great majority of dreams do *not* come true, and therefore science does not recognise the existence of ghosts. Careful investigation may bring out many explanations of a few cases of deaf "mutes" apparently speaking with the accent of their native districts, without attributing the phenomena to heredity. But if dialectal accent is hereditary, how are we to explain the 400 cases cited by Prof. Bell (to name one competent observer alone) in which no such accent was observable?

Mr. Axon quotes (apparently with approval) M. Hémet's declaration of inability to conceive "how in losing the use of speech, deaf mutes should retain the unconscious memory of accent." I wish to suggest that such phenomena may be due to automatic activity of the cerebral tissue. In his last "Causerie," in the *Rappel*, M. Victor Meunier mentions the case of a young man, an inmate of a French asylum, who six years ago became deaf with the right ear through the effects of typhoid fever. He is occasionally conscious of sounds on the right, or deaf side, of which the left ear gives no indication whatever. He hears entire sentences, distinctly articulated, and as these are sometimes of an offensive character, they have involved him in many quarrels, as he has attributed them to perfectly innocent persons who have chanced to be near his right side at the time. Notwithstanding this hallucination, his judgment has remained sound, and having discovered that he sometimes hears (with his right ear) absent or stopped clocks strike loudly, he has learned to disregard any sounds but those which his left ear communicates to him. M. Luys, of the hospital of La Salpêtrière, gives many illustrations of such automatic activity.

Remembering that memory has a physiological basis, and believing in the psychological basis of language, I find it far less difficult to conceive that after the loss of speech deaf mutes should retain the "unconscious memory" of accent than that accent should be hereditary. Indeed the loss of speech might even be favourable to such retention; for the particular cells concerned might keep the original impression unimpaired by subsequent impressions, to be accurately given forth again when the requisite conditions came into operation.

This does not explain the case of Daniel Fraser, said to have been mute from his birth; but, on the other hand, "one swallow does not make a summer."

F. J. FARADAY

Manchester, March 4

Vignettes from Nature

WITH all due deference to Dr. Carpenter, for whose supreme authority on all matters of biological fact I have, of course, the profoundest respect, I must plead that he evidently has not looked into my little book, "Vignettes from Nature," but has taken his statements at second hand from the necessarily condensed account given in Mr. Wallace's kind review. Had he consulted the book itself, he would have found most of my remarks intentionally so guarded as to escape his strictures.

First, as to the sharks. Dr. Carpenter says, "None of these, in the judgment of Mr. Grant Allen and Mr. Wallace, surpassed the forty-foot sharks of the present time"; and he goes on to speak of a *Carcharodon* tooth from the Crag, 4 inches long by 3 broad. Now, in "Vignettes," p. 76, I wrote, "The teeth of what seems to have been the biggest known fish, a prodigious shark, are dredged up in the modern ooze of the Pacific. . . . They have become extinct at a very late date." I took my facts from Dr. Günther's "Study of Fishes," p. 321, where we read as follows:—" *Carcharodon* teeth are of very common occurrence in various tertiary strata. . . . Some individuals attained to an immense size, as we may judge from teeth found in the Crag, which are 4 inches wide at the base, and 5 inches long. . . . The naturalists of the *Challenger* expedition have made the highly interesting discovery that teeth of similar size are of common occurrence in the ooze of the Pacific, between Polynesia and the west coast of America. . . . The gigantic species to which these teeth belonged must have become extinct within a comparatively recent period." In short, the very shark which Dr. Carpenter claims as tertiary, I had previously claimed as also nearly modern.

Dr. Carpenter further says, "Is it clear that *Tridacna* is the largest known Mollusk? I should have thought it exceeded by

the gigantic *Ammonitida*, &c." But if he will turn to "Vignettes," p. 75, he will see that I wrote, "No fossil *bivalve* molluscs to my knowledge are as big as . . . *tridacna*." The word I have italicised makes all the difference. On p. 77 Dr. Carpenter will see that allusion is made to the big Cephalopods, though perhaps none of these were very much larger than the largest modern gigantic squids.

As to the other points, they are really matters of language, and I will not take up your space by answering them in detail. When I spoke of "our whales," I certainly did not mean to exclude extinct whales: I merely meant to contrast them with the great secondary Saurians. Nor did I say that horses, elephants, &c., had been steadily increasing in size from "the earliest epoch of their appearance to the present day"; I said, "to the recent period," which is quite another thing. As I was writing for popular readers, not for biological critics like Dr. Carpenter, I felt bound to use the vague but comprehensible language of ordinary life; and so I described the mammoth as "recent," quite justifiably, I think, for my existing purpose. No technical words were used in the volume, and it was impossible always to find popular ones quite free from objection. But if Dr. Carpenter will kindly read the short chapter in question, I venture to think he will be willing to withdraw his present strictures. The object was merely to combat the vulgar notion that all the animals of all geological ages were positively gigantic; and in doing so, almost every animal mentioned by Dr. Carpenter was expressly adduced as an example.

In answer to ФП, I should like to add that I used the word "accidental" in a strictly Pickwickian sense.

GRANT ALLEN

Miss Cobbe and Vivisection

WILL you allow me as one not only ardently interested in the pursuit of vivisection as a means of extending our knowledge, but also as a sincere hater of unnecessary cruelty to animals, to state the following facts which I know to be true:—

Some little time ago Miss Frances Power Cobbe, who has so identified herself with the cause of anti-vivisection, called on a distinguished man of science to endeavour, by persuasive speech and *vivd-voce* argument, to gain him over to her cause. Three points were observable in Miss Cobbe's outward presentment, viz.: she had an ostrich feather in her bonnet; a bird of paradise in, on, or near her muff; and she carried an ivory-handled umbrella; consequently the distinguished man of science replied as follows:—

"Madam, charity begins at home; when you have given up wearing *ostrich feathers*, which are plucked from the *living bird*, causing the most exquisite pain, and birds of paradise, which, in order to enhance their beauty and lustre, are *skinned alive*; when you have abjured the use of *ivory*, because you know that the tusks are *cut out* of the dying elephant's jaw, then, and then only, come and upbraid me with the cruelty of my operations. The difference between us is, Madam, that I inflict pain in the pursuit of knowledge, and for the ultimate benefit of my fellow-creatures; *you* cause cruelty to be inflicted merely for your personal adornment. . . ."

H. H. JOHNSTON

Zoological Gardens, Tue-day

The Electrical Resistance of Carbon under Pressure

FROM the abstract of the proceedings of the Physical Society, given in *NATURE*, vol. xxv. p. 426, I learn that Prof. S. Thompson has been making some experiments which tend to show that the observed diminution of the resistance of carbon under pressure in such instruments as the carbon relay, rheostat, and microphone-transmitter is really due to the contact between the electrodes and the carbon. No doubt the greater portion of the observed diminution of resistance is due to this cause, and I have already pointed out in my paper on "The Influence of Stress and Strain on the Action of Physical Forces," Part ii., Electrical Conductivity, an abstract of which (*NATURE*, vol. xxv. p. 401) was read before the Royal Society on January 26, that the effect of a given amount of longitudinal traction or compression per unit area on the electrical resistance of some carbon rods was not greater than is the case with the metals tin and lead, for whereas a stress of 1 gramme per sq. cm. produced a variation of conductivity of from 7684×10^{-12} to 11420×10^{-12} per unit in the case of five carbon rods, the corresponding numbers were, with tin and lead, 10540×10^{-12} and 17310×10^{-12} respectively. The carbon rods were of the sort used for the purposes of electric