A further examination will show, in a most distinct manner, the origin of these worms, and that they have not proceeded from the intestinal cavity, which indeed never contains any. The peritoneal surface bears no marks of attack from these animals, but on several parts of the membrane, or rather beneath it, and principally between and about the layers forming the mesentery and meso-rectum, may be seen corpuscles of an opaque, fuscous colour, of various sizes, mostly about that of a pin's head; these consist externally of a sac or cyst, the smallest containing only a white speck surrounded with mucus, but in the larger and more forward, a small worm may be distinguished in a coiled-up position. These are evidently the ova, from which the erratic worms in the abdomen are produced; nor can any instance of spontaneous generation be more apparent. The question then arises, whether intestinal worms in general have not a similar origin; the supposition that their ova are taken in with the food being very unsatisfactory.

Rudolphi, in his great work on the Entozoa, mentions having observed in dissecting a dog a small body adhering to the villous coat of the intestines, in which he traced a resemblance to the head of a variety of *tænia*, two or three of which were in the alimentary canal. This is the only evidence we have of the kind, but I believe the intestines have not often been examined with this view.

With respect to the worms found in the mackerel, I may merely mention that their organisation is exceedingly simple; they are white and semitransparent, having an opaque spot near the head; their length varies from one to three inches; they do not seem furnished with digestive, vascular, or generative organs, and, consequently, exhibit no sexual distinctions; they are very tenacious of life, continuing in active motion in water for a day or two after removal from the fish.

I will not trespass further than to observe that though, as I mentioned above, the prevalence of this disease in mackerel is very general, and in some very extensive, I have not witnessed any decided ill effects having arisen from the use of this fish as an article of food, but have reason to think some caution in this respect to be necessary. I have once, only, observed trout to be similarly affected. I remain your obliged,

H. PEACOCK, Surg.

18, Salisbury-sq., June 9, 1831.

CAUSES AND TREATMENT OF DEAFNESS.

To the Editor of THE LANCET.

SIR,—I have waited patiently until Dr. Fosbroke's observations on the above subject were concluded, which I presume they are, and I now claim from your impartiality the favour of occupying a portion of your valuable columns in reply. This indeed I should not have done, but have suffered his papers to gravitate down to their proper level, had he not assumed, so exclusively, all knowledge, honour, integrity, disinterestedness, veracity, &c. &c. &c., that not an atom can be left for any other person. A portion of those qualifications having been assigned to me by the most respectable members of the profession, as well as many of my patients, indeed I may say by most of them, I shall adduce some few facts and reasoning in opposition to Dr. Fosbroke's remarks, after which let the profession and the public judge for themselves. As to the motives which have actuated Dr. Fosbroke in making this sweeping attack, they are sufficiently obvious; however, I shall give him credit for those he *publicly* avows, and show how far he has proved himself capable of appreciating the abilities of others, or has developed superior attainments in himself.

The subdivision of the profession into various departments has been of many years standing, and I contend that that arrangement has taken place very properly in regard to diseases of the auditory organs, for they are so little understood by the general practitioner, that patients have had trifling cases of deafness rendered much worse, or incurable, and there are instances where the imperfect knowledge possessed by these gentlemen, and their self-sufficiency on the subject, have absolutely endangered the life of the patient; if, however, any argument beyond the opinions of our first scientific men be required to prove the propriety of the subdivision, Dr. Fosbroke, in his display of knowledge on this subject, must convince every one who has read his papers, that such subdivision is *highly necessary*.

It cannot be supposed, by any who know me, that I am about to become the apologist of *some aurists*; I hold in such contempt their charlatanic puffs, their advertisements, their *pretended* cures of *incurable* cases, and their treatises made up by *hirelings*, that I think them below even St. John Long.

Dr. Fosbroke quotes Saunders in terms of commendation. Saunders adopted an erroneous practice which he afterwards abandoned; he professed to have discovered, in precisely the same *chance* way, the mercurial treatment which Fouseca adopted *acci-*

dentally 300 years before him! I could say more on this subject, but Saunders has long ceased to live. If Mr. Henry Earle be the person intended by Dr. Fosbroke, I know of no new light which he has thrown on the treatment of diseases of the ear; he certainly published a paper in the *Medico-Chirurgical Transactions*, relative to the thickening of the lining of the auditory passage, and the membrana tympani, which he considered had escaped the notice of all authors on deafness, and advised a *strong solution of lunar caustic*. Mr. Earle was incorrect in every particular, for the same hypothesis and treatment had been adopted very profitably to himself, but *injuriously* to his patients, by another person, until he applied it to the Duke of Wellington, the result of which is well known, and even now felt by that illustrious individual.

I was not aware that any author of the name of Parry had written on the subject; if it be Dr. Parry, of Bath, it shows that one practical truth is worth a host of theories, for I have been consulted by the mother and sister of that gentleman, and if his opinions or modes of treatment had been useful, probably there would have been no necessity for their visit to me. As to Bell, Sym, and Abercrombie, I know nothing against, or in favour of, their practice; in fact, the two latter I have never consulted; my methods of treatment are the result of my own researches, and I consider that every man, after sufficient experience and education, should be independent of the theories or opinions of others, and have strength of mind to act and think for himself. As to the idea which Dr. Fosbroke endeavours to inculcate, that a month's time is sufficient to teach the general practitioner all that can be known of the causes and modes of treating deafness, it is too ridiculously absurd to merit a reply.

Dr. Fosbroke, in his second paper, says, "The left side is almost always the least deaf, although the membrane on that side may have been ruptured." I know not what he wishes to prove by this, but common sense is against it; and on looking over many hundred cases in my books, I find a refutation of the first part of his remark, whilst every person capable of reasoning on the subject, will see that the latter part must be erroneous. I know one noble family, all of whom hear *least* on the *left* side, and another, every member of which has, more or less, a malformation of both ears. Dr. Fosbroke is equally incorrect in saying, "If both ears be in a state of inflammation and discharge, that the discharge is generally most from the right."

Polypus in the nostrils is not so very common in this country, as to allow M. Hard with reason to make the observation

which Dr. Fosbroke states, viz., that the deafness of the majority of the English who apply to him is caused by polypi in their nostrils. There are but few English who apply to that gentleman, of whom I have seen several, that found no relief from his treatment, and not one of them had any such disease: but more of this subsequently.

I know but of one family bearing the name of Baghot, and Dr. Fosbroke says, Lady Baghot was deaf after every cause of mental depression. I attended two of Lady Baghot's daughters occasionally from 1817 till 1825, she always accompanying them. I saw her ladyship frequently in the latter year, and when her health was materially reduced from an accident, which caused corresponding depression of mind. She had great confidence in me, and had she experienced any diminution of hearing, she would have consulted me; but few persons had more acute hearing than Lady Baghot, and up to the period of her decease, I have been assured by a gentleman who was intimately acquainted with her, that she never had the least deafness.

I have not time to follow Dr. Fosbroke through his reasoning as to exciting causes, because if I did, I must repeat what I have already published; I shall be content with remarking, that Dr. Fosbroke has fallen into the same error of which he accuses others, for he diverges into minute distinctions of seats and causes, which are only imaginary; for instance, his supposition that the water used by a patient of his was impregnated with lead! Why did not Dr. Fosbroke test it? He might have ascertained the fact. There are many of these suppositions constantly floating in the minds of people which have no foundation whatever when examined by persons competent to do so. River water does not contain any property which can oxidise lead, and spring water must be of such a quality as to be unfit for domestic use, if it can produce such a result.

I shall content myself with citing another example:—Dr. Fosbroke says, he "became hard of hearing in one ear, with scantiness, and induration of wax." Now here are *two incompatible causes* stated to be *co-existent* to produce one effect! I am afraid, if Dr. Fosbroke's "case-books full of facts" be not more carefully compiled, the world will not be much benefitted if they be published.

I shall pay more attention to the investigation of Dr. Fosbroke's advice, as to the treatment of deafness, because it is of importance to the profession that they should not be misled on points of practice which may involve their character, by submitting their patients to experimental theories.

I am, Sir, your obedient servant,
W. WRIGHT, Surg. Aurist.
London, June 17th, 1831.