able fact is alluded to by Liebig, in his "Letters on Chemistry," p. 403-4; and he attempts to account for it by stating that the intestines perform, in those animals, the functions of the kidneys, the phosphates being cast off among the solid fæces. Liebig further draws an analogy between this supposed function of the intestines of the herbivora and the diseased action in diarrhoea in the human subject.

I wish to call the attention of chemists and physiologists to these statements, as it is evident they have a most important bearing upon the whole subject of excretion, in its most extended sense, as elucidated by the modern investigations of chemistry and the microscope.

If the analyses of the celebrated men above mentioned are correct, we have presented to us the fact, that the phosphoric acid of the brain, bones, and protein compounds of the herbivora, which are thrown off by the constant waste going on in the system of these animals, is excreted, not by the depurating action of the kidney, but by the intestinal tube; and this not by an abnormal exosmosis, as in diarrhoea, cholera, mælæna, &c., but, by a clear act of organic selection, which I believe I may state has no parallel in physiological science. *Qui facit per alium, facit per se.* If the intestines depurate the blood in the ruminantia and the equidæ, why not in the pachydermata (as the pig), in the rodentia (as the hare), or even in the omnivora (as man)? I need not point out to the comparative anatomist that the equidæ are placed between the pachyderms and rodents; and yet there are phosphates found abundantly in the urine of both the pig and the hare. We must not look for an explanation of this anomaly, then, in anatomical relation, for the ruminant is much further removed from the equid than the latter is from the pachyderm or the rodent; in fact, equidæ are placed by Cuvier among the pachydermata. Neither can we find an explanation in the nature of the food; for the question is not how the phosphoric acid gets into the system, (and get in we well know it must, and that in the ordinary process of chyliification and absorption,) but the difficulty is, how it is removed from the blood so as to get into the fæces, and not appear at all in the urine. If it is simply by exosmosis, then the process may take place as well in the human subject; so that when we find no phosphates in the urine, abundance may have passed off from the blood by the intestines.

Thus all our recent notions of the chemical pathology of the urine would be upset. If, again, the intestinal tube in the herbivora acts as an organ of excretion to the blood, in the same sense as the kidney does, then must our notions of the physiology of secretion undergo great modification.

In the hope that some of our physiologists may throw light upon this interesting point I submit it to the notice of our Association.

STRONGYLUS SUIS IN THE BRONCHIAL TUBES OF A PIG.

By C. R. BREE, Esq, STOWMARKET.

A few weeks ago, when examining the lungs of a recently-killed pig, I found the bronchial tubes infested with vast numbers of entozoa, which Professor Owen, to whom I sent the specimen, informs me are the rare *strongylus suis* of Rudolphi (Synopsis Entozonum, p. 36, No. xxxviii.)

This parasite was about three quarters of an inch to one inch in length, and was attached to the angles of division of the bronchia through the whole lung. Having placed one of them in a watch-glass, I was fortunate enough to witness, through the microscope, the exclusion of vast numbers of ova from the female. I could not satisfactorily make out whether I had both male and female specimens coiled up and attached to each other, or only one female. At a few lines distant, however, from the extremity of one, there was a process formed in the shape of an $\angle$, from which I observed the slow and gradual emission of a diaphanous tube, filled with minute circular flattened discs, of a dark colour with a pellucid centre. As this tube emerged from the vulva, I observed that it increased in thickness, and contained more ova in a line. These ova appeared to have a slight motion. Several of these tubes, measuring about half the length of the parent parasite were emitted.

I made these observations about eleven A.M. When I again placed the glass under the microscope, in the evening at six o'clock, I was astonished to find a vast mass of young fry, struggling and wriggling about with great activity. Some had escaped from the hyaline tubes, but the greater number were struggling to get out. I could form no idea of the numbers—they were countless. They continued active until I went to bed; and failing to leave sufficient water in the watch-glass, they were all dried up in the morning.

Stowmarket, 1851.

CASE OF POISONING BY THE BI-CHROMATE OF POTASH.

By JOHN WICKENS WEST, Esq., POOLE.

VERY lately I was requested to visit a professional friend residing in Poole, who had inadvertently tasted a solution of the bi-chromate of potash. The quantity he swallowed did not exceed a teaspoonful, if it were as much. He discovered his mistake shortly afterwards, but did not experience any ill effects until about half an hour, when he felt sick, his countenance became pale, and he became quite prostrate. He being at a midwifery case at the time, was compelled to leave, and requested me to attend on his behalf. Before the

delivery of the woman I was sent for to him. I found him in bed; he had been profusely sick, and had thrown up a serous-looking fluid, resembling the evacuations of rice fluid in cholera; diarrhoea constant, and the evacuations passing away involuntary; the bed was perfectly swamped; cramp in his legs, thighs, and in the region of the heart; great prostration; pulse small and exceedingly depressed, so much so, that I felt a little alarmed at his state. On his arrival at home, and previously to my seeing him, he had taken some warm tea, which increased the vomiting. As the diarrhoea was profuse, I administered the following draught:—R. Tinct. Catechu., dr. ij.; Tinct. Opii, m. xl.; Tinct. Cinnam. Co., dr. j.; Aquæ, oz. iss. Sit haust.

I also gave him strong brandy and water at short intervals. In about an hour the sickness had subsided, and the diarrhoea was not so urgent; it had produced great debility. Flatus was then oppressing his stomach exceedingly, and I gave him the citrate of potash, which relieved him much; he had also a warm bath. Before the close of the day, all his symptoms were relieved. He took some arrowroot, and afterwards procured a little sleep. When calling on him in the morning, I found he had passed a good night, and was sufficiently recovered to resume his professional duties. I put this case on record, as poisoning by crome or its preparations is of rare occurrence. Professor Taylor, in his excellent work on Medical Jurisprudence, speaks of only one case having been related; I have not been able to discover any instance of it myself.

It will appear from the foregoing case, that the bichromate of potash produces symptoms exactly similar to cholera: such as vomiting, purging, cramp, and great prostration of strength; and as soon as the poison is evacuated from the stomach, must be treated in the same way.

WHAT OUR ASSOCIATION MIGHT DO.

BY C. RADCLYFFE HALL, M.D.,
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THE frequency of difference of opinion amongst medical men is proverbial, and is deemed by every non-professional person to be a sufficient excuse for deciding for himself any point on which "doctors disagree." Our differences of opinion, referring as they do principally to general and superficial questions, are placed more prominently before the public, and are more open to the apprehension and judgment of the non-medical world, than the innumerable truths of medicine on which we all agree.

On what do these differences of opinion depend; and, being evil in their consequences, how far are they unavoidable?

Individual differences of opinion on matters which

require minute investigation depend on the difference in perceptive power, attentiveness and clearness of thought, of different men, and must always exist. In the possession of these qualifications mainly consists the superiority of one medical man over another. This, however, at least in theory, is as plain to the public as to ourselves; and from this the profession does not suffer in general estimation. The differences of opinion from which it does suffer concern subjects which ought to be matters of fact, but which are at present still matters of opinion. We cease to differ upon any point the moment that fact is substituted for opinion. No medical man gives it as his opinion that small-pox is not contagious; because the question has been decided by facts, and is no longer a matter of opinion. But one man considers cholera to be non-contagious, and is supported in such a view by the combined intellect, &c., of Government; whilst another, unable to gainsay his own experience, asserts that cholera is contagious. So soon as a sufficient accumulation of facts has settled the question there will be as little difference of opinion upon this as there is upon the contagiousness of small-pox. All we need in order to render our views unanimous upon any point is simply to learn the exact truth regarding it. In proportion to the extent and exactness of our knowledge of facts will be the unanimity of our sentiments. Upon the same fact two persons can differ in their opinion only when the fact in all its bearings—i. e., the whole of the fact, is not known to both. The same fact being perfectly known to the one, and but partially known to the other, is virtually two different facts. Each person reasons from his own premise correctly; but only one of the two has a true premise, and consequently he alone can obtain the true inference. Or, again, from imperfect knowledge of the fact, any number of false premises may be taken, and any number of correct inferences, but erroneous conclusions, deduced from them. We are brought back to the same point—namely, that the only mode of obviating differences of opinion is to ascertain the facts.

Every question demanding for its solution minute investigation of particular facts must ever remain the work of an individual or of a few acting in unison. But there are many questions of a general character requiring multitude rather than minuteness of observations, which the experience of one or of a few individuals can never be sufficient to decide. It is precisely on these general questions that we differ most, and most conspicuously to the world. Every man prefers a resting-place for his mind, and rather than possess no opinion he makes one from his own limited experience. The grounds being inadequate in each instance, we may find as many opinions as thinkers. Nor is this all. In the ignorance of the actual truth we do not content ourselves with merely drawing inferences from our own experience, but we arrange for Nature what she ought to do, and retain tenaciously just those observations which support our programme.

Has it ever once happened in the history of science that the fact when clearly ascertained proved to be