—such, for instance, as the medulla oblongata and the contiguous nerve-centres at the base of the brain. Hence epilepsy or apoplexy may be about to supervene; the *petit mal* of the former disease being the signal note of the labyrinth, betokening the condition of its circulation, and suggesting the search for sources of suspended nerve inhibition.

In the disturbance of some one or more of the multitudinous sympathies, thus imperfectly sketched, will be found an explanation of the occurrence of those *simple attacks* of labyrinthine vertigo, which come on *without any previously existing ear-disease*, the desire to understand which furnished the key-note of this paper.

AN EXPERIMENT ON THE DISINFECTION OF ENTERIC EXCRETA.

By JOHN DOUGALL, M.D., F.F.P.S.G.,

Lecturer on Materia Medica in the Glasgow Royal Infirmary School of Medicine.

IN the sixth Public Health Report of the Medical Officer of the Privy Council for 1875, is an extremely able account by Dr. Baxter of an experimental study of certain disinfectants. Mr. Simon, in his preface, states that the investigation was carefully planned by Dr. Baxter and Dr. Burdon Sanderson. It may be mentioned that, in the experiments, three infective poisons, viz., that of glanders, that of vaccine, and that of intensified or infective inflammation, were submitted to the action of four alleged disinfectants; viz., permanganate of potash, sulphurous acid, chlorine, and carbolic acid. At the end of the report, the principal inferences which seemed to flow from the results of the investigation are embodied in a series of eleven propositions. In the fourth proposition, it is stated that, "when permanganate of potash is used to disinfect a virulent liquid containing much organic matter, or any compounds capable of decomposing the permanganate, there is no security for the effectual fulfilment of disinfection, short of the presence of undecomposed permanganate in the liquid after all chemical action has had time to subside".

As this conclusion, which is founded on a great number of very interesting experiments, seemed a safe basis for testing further, in an easy and very practical manner, the disinfecting power of the permanganate, I resolved to try its strength in the form of Condry's fluid with enteric excreta, this fluid being the state in which the permanganate is almost exclusively used by the public. The experiment was conducted as follows. To a newly passed, characteristic, ochry-looking enteric stool, quantity unknown, a portion of a known quantity of Condry's fluid was added. The mixture was then stirred and set aside for a short time, and, when the pink colour had changed to a brown, more of the fluid was added, and the process repeated till the pink colour was found permanent after the lapse of twelve hours; in other words, chemical action in the mixture had subsided, as indicated by the presence of the undecomposed pinkish permanganate liquid. The mixture was now measured, and the quantity of Condry's fluid added being known, this was subtracted from the whole, thus showing the amount of faecal matter acted on. To find the exact relation between the quantity of Condry's fluid deoxidised and the amount of enteric faeces disinfected, was now a simple matter of calculation. Avoiding fractions, it amounted to this (and I confess the result astonished me): one ounce of enteric faeces had deoxidised not less than ten ounces of Condry's fluid; in other words, there is no security that enteric faecal matter is effectually disinfected by Condry's fluid, unless the bulk of the fluid used be ten times as great as the bulk of the enteric faeces to be disinfected. In the same manner, I experimented with a fluid ounce of enteric urine, and here the result was, that one ounce of the urine deoxidised at least two ounces of Condry's fluid.

Now, supposing a typhoid patient passing twelve ounces of faecal matter and twenty ounces of urine during each twenty-four hours, say for a week, which, it will be conceded, are not excessive quantities, and supposing the Condry's fluid sold to the public in eight-ounce bottles at one shilling each is used, it follows that two hundred and eighty ounces of Condry's fluid are required to oxidise or disinfect the week's urine, which, at one shilling per eight ounces, amounts to £1 15s.; and that eight hundred and forty ounces are required to oxidise or disinfect the week's faeces, which, at one shilling per eight ounces, amounts to £5 5s.; in all, £7 per week, or at the rate of £364 *per annum*. Supposing a hospital with thirty enteric patients, on an average, constantly under treatment, on these data, it would take £10,920 worth of Condry's fluid to disinfect their yearly excretions.

CLINICAL MEMORANDA.

MOSQUITOES AND FILARIAE.

I REQUEST permission to make a correction of a rather serious error that has, not altogether unnaturally, occurred in the annotation that has appeared in the JOURNAL on this subject.

The statement that "the mosquito is the means of spreading the *filaria sanguinolenta* amongst human beings" is absolutely incorrect. It was through the courtesy of the Pathological Society that I was permitted, on the occasion referred to, to combat the error in question, and to explain that the human parasite taken up by the mosquito had no genetic relation whatsoever with *filaria sanguinolenta*.

At the Linnæan Society, on the 7th instant, I communicated the details of Dr. Manson's discovery, originally announced on January 12th. In the discussion that followed, a general acceptance of the correctness of Manson's views was accorded by Dr. Allen Thomson, Sir Joseph Fayrer, Mr. Curling, Mr. Ray Lankester, Dr. Collingwood, and also by Mr. MacLachlan, the well-known entomologist. The President of the British Association acknowledged, for his part, that he thought that the authors of the two papers before the Society had fairly proved their case; and Sir Joseph Fayrer characterised the subject as one of unparalleled interest and importance in relation to public health in the tropics.

If there were one point more than another upon which I insisted at the Pathological Society, it was on the necessity of not mixing up the *filaria sanguinolenta*—an entozoon of the dog, known to Redi nearly two centuries ago—with the *filaria sanguinis hominis* of Lewis, the *filaria immitis* of Leidy, and the *hamatozoon subulatum* of Leisegang. Unfortunately, as if the nomenclature of these helminths were not already sufficiently complicated, we have Dr. Beale giving the weight of his great authority as a microscopist to an error which I felt it my duty to expose on the night in question, and which, I regret to see, has now even gained the sanction of the valued columns of the JOURNAL. In the last page of the fourth edition of the *Microscope in Medicine*, the author writes as follows: "Dr. Cobbold seems not to be aware that, in his work on *The Pathological Significance of Nematode Hamatozoa*, published in 1874, Dr. Lewis gives an account of the mature *filaria sanguinolenta*, with drawings of the male as well as of the female worm." If anyone will read the context in Dr. Beale's work (p. 505), they will at once perceive that Dr. Beale's object is to defend Lewis's position on the question of priority in reference to the discovery of the sexually mature worm. Dr. Beale's imagination has led him to fancy that the *filaria sanguinolenta* is the same worm as Lewis's *filaria sanguinis hominis* and my *filaria Bancrofti*; and then he has sought to make it appear that I am ignorant of Lewis's memoir—a little work a copy of which was sent to me by the distinguished author himself, and which, I may add, is scarcely ever out of my sight.

The members of the Pathological Society will testify to the earnestness of the appeal I made in reference to the necessity of precision and accuracy in dealing with the numerous forms of hæmatozoa of man and animals. The number of errors that have been published on this head is very considerable. I do not allude to mere questions of microscopic size and so forth, concerning which all of us are liable to err; but when it comes to the confusing of species which have absolutely no connection with one another, and not even any relation as regards their bearers, then I feel it is impossible to remain silent.

T. SPENCER COBBOLD, M.D., F.R.S.

THE ACTION OF OPIUM AND ATROPIA.

THE publication in the JOURNAL of the case of opium-poisoning successfully treated by atropine, and Dr. John Harley's letter of warning, induce me to send a brief account of a case in which the physiological action of these drugs received a *vice versâ* illustration.

Early one morning in November last, shortly after midnight, I was summoned out to a lady who had been seized with angina pectoris, of which complaint she had had several attacks during the past few months. On my arrival, I found her sitting up in bed, with her head and body bent forward between her knees, in agonies of pain; the extremities cold, the pulse feeble, and the pupils dilated. She had also frequent retching, and was incessantly pushing forward her tongue and asking for "water, water". I was told she had vomited several times; but what she had thrown up last had alone been kept, and I found this to consist merely of bile and mucus without any peculiar smell. Having brought with me my hypodermic syringe and a solution of morphia, I gave her at once an injection of about a quarter of a grain of morphia. Almost immediately the pain and retching ceased, the