

not entirely absent, as in my case. I can confirm this observation so far that on the previous day I used a 4 per cent. solution in operating on the larynx of a delicate woman under my care at St. Bartholomew's Hospital without producing any marked anaesthesia. Yet the application was made twice, and an interval of at least five minutes was allowed between the applications and between the second application and the operation. It is not improbable that a solution of less strength than 20 per cent. will suffice in operations in the interior of the nose, and this will easily be determined during the next few weeks or even days. Indeed, the sooner it is determined the better, for the muriate of cocaine is at present extremely expensive, costing about one shilling a grain. When the 20 per cent. solution is employed, and the nose or larynx is brushed over twice thoroughly, it will be found that from two to four grains of the cocaine have been used. I am informed that the cost will probably increase during the next few months if the drug is as largely employed as it bids fair to be, for the supply is very limited, and there is not even a good prospect of obtaining a sufficient supply of the leaves from which the alkaloid is manufactured.

Nov. 24th (two days after the operation).—Yesterday I suffered from a slight attack of migraine, not so severe as I have often experienced, and not to be compared with that which followed the first cauterisation. To-day it has completely passed off, for it only lasted a few hours. How much of the difference is to be attributed to the use of the cocaine I am not able to say, but I believe a part of it at least. I mention it to make the case as complete as possible, and to show that there is certainly no reason to apprehend that the after-effect of the alkaloid will exaggerate the suffering of the patient. I remain, Sir, yours truly,

Queen Anne-street, W., Nov. 24th, 1884. HENRY T. BUTLIN.

P.S.—Dr. Semon has just looked through this letter, and thinks I ought to have offered some explanation of the reasons which induced him to perform and me to submit to the cauterisation of the turbinated bones for migraine; but I think it will be better to defer that and the discussion of Hack's theory to a later date, when we have more material within our reach. The object of the present communication is to direct attention to another use of cocaine.

## FLUKES IN MAN.

To the Editor of THE LANCET.

SIR,—Having received from Dr. Manson a copy of Dr. Wallace Taylor's paper entitled “Distomata Hominis,” I request permission to offer a few criticisms in no unfriendly spirit. Dr. Taylor says that “the whole subject of distomata infesting man has probably been worked up better in Japan than elsewhere.” Nevertheless his memoir, which from the clinical standpoint does him much credit, omits all reference to the labours of Lewis, McConnell, Sonsino, Thomas, and others. My papers on *Distoma crassum*, *Bilharzia*, &c., naturally share the same fate.

Dr. Taylor gives a description of *Distoma Ringeri*, but, following Prof. Baelz, he calls the parasite *D. pulmonale*. My description of the original fluke sent by Dr. Manson was published in the *Quekett Journal* in 1880, two years before the publication of Baelz's memoir in the *Berliner Klinische Wochenschrift*, which he quotes (1883). Baelz, it seems, had mistaken the eggs of this fluke for gregarines.<sup>1</sup> But apart altogether from the question of priority, the title (*D. pulmonale*) is badly chosen, because the lung flukes of mammals are so very like one another. The *D. compactum* found by me in the lungs of the Indian ichneumon is apparently a mere variety of, if it be not identical with, *D. Ringeri* of man, and as much may be said of Dr. Kerbert's *D. Westermanni* infesting the lungs of the tiger.<sup>2</sup> In all three corresponding forms the uterine organs have precisely the same character and disposition. Certainly Baelz's and Kerbert's figures leave little to be desired.

The second parasite described by Dr. Taylor is still more unfortunately named. He calls it *D. hepaticum*. Here, again, he follows Baelz, who had previously called it *D. hepatis perniciosum*. It seems incredible that anyone writing on flukes should be unfamiliar with the natural history and characters of the common *D. (Fasciola) hepa-*

ticum so beautifully worked out by Professor Thomas and already known to have been detected some twenty times in the human subject; yet Dr. Taylor naïvely remarks that “whether *D. hepaticum hominis* is of a different species from the *Fasciola hepatica* (Linn.), well known in Europe as the cause of the sheep ‘rot,’ I am unable to decide.” Dr. Taylor's parasite (*D. hepaticum hominis*) is utterly unlike the common liver fluke. If it be a good species, it will be less confusing to call Baelz's fluke (*D. Baelzi*). My impression is that it is only a contracted or broad example of the species which I have called *D. sinense*. Dr. Taylor, however, says of his *D. hepaticum* that, “omitting minor differences, the general description given of the viscera of the *D. pulmonale* will answer for this.” This statement surprises me, for his own, or rather Professor Baelz's, really good figures show that (as regards the position and extent of the vitellaria and other reproductive organs) no two fluke species of the same genus could well be more divergent in the arrangement of their essential organs.

The next species described by Dr. Taylor is Prof. Baelz's so-called *Distoma hepatis innocuum*. This fancied new parasite is (as Manson himself points out in a pencilled side-note) nothing more than the now well-known *D. sinense*, of which I have specimens sent by the original discoverer, Prof. McConnell. Unaware of my prior description, Leuckart named the same parasite *D. spatulatum*. I have part of a liver well infested by this fluke, which is doubtless “perniciosum” in its effects. Baelz's zoological nomenclature is therefore altogether inadmissible.

Dr. Taylor's memoir closes with figures of two other fluke parasites. Here again he is equally unfortunate in his determinations. The large fluke “infesting the stomach of cattle,” which he calls a distoma, is the well-known *Ambistoma conicum*, whilst the smaller fluke, taken from the liver of a cat, is probably Creplin's *Distoma conus*. The size, to be sure, is rather larger than Dujardin states it to be in his description.

The errors which exist in regard to the synonymy of parasitic species are already sufficiently burdensome, and it was in the hope of aiding identification that I published a “Manual of Reference” to all the known human parasites, bringing the literature up to date (1882). Honest and good workers, who are not at the same time systematists, can have little idea of the great trouble and confusion which the introduction of new names for old or familiarly known parasites creates in medicine. Solely in the hope of checking errors of fluke-nomenclature in relation to important diseases I have made these few remarks, which I trust will be accepted in the spirit in which they are offered.

I am, Sir, your obedient servant,

Portsmouth-road, W., Nov. 22nd. T. SPENCER COBBOLD.

## “CHARCOT'S JOINT DISEASE.”

To the Editor of THE LANCET.

SIR,—The interest excited by the discussion at the Clinical Society on “Charcot's Disease” induces me to state that you published in THE LANCET of Jan. 28th, 1871, in a report by me of cases under the care of Dr. Charcot at La Salpêtrière, Paris, the first notice of this disease in England. This report included a very short *résumé* of the clinical facts, and by what, in considering the diagnosis, rheumatism was excluded. In 1876 I begged of Dr. Charcot that he would kindly give some specimens of bones illustrating the disease in question. He informed me that the specimens he had shortly before had at his disposal were sent to the museum of St. Thomas's Hospital. May I here have the pleasure to acknowledge that Dr. Charcot did not send me empty-handed away, but gave me sections of the spinal cord in another form of disease—namely, progressive muscular atrophy, which I exhibited at the Pathological Society May, 1877, as having a relationship with the joint affection.

I append an extract from your journal of the date above named. I am, Sir, your obedient servant

Stratford-place, W., Nov. 1884. T. W. NUNN.

“Dr. Charcot pointed out some cases of joint affection which he believed showed that consecutive to lesion of the nervous system arose mischief in the joints. In the *Archives de Physiologie*, No. 1, 1868, he has published papers: “Sur quelques Arthropathies qui paraissent dépendre d'une Lésion du Cerveau ou de la Moëlle Epinière.” The purpose

<sup>1</sup> See THE LANCET, 1880, vol. ii., pp. 548.

<sup>2</sup> Archiv für Mikroskop. Anat., Bd. 19, S. 529.