

opinion "that the child had died from other causes; that the operation of lancing the gum had been necessary, and had been done with due care and caution." The child was then buried. The certificate of the cause of death sent by Mr. Jones, for the registrar of births and deaths, states that the child died from "inflammation, and its consequences!"

These, Sir, are additional facts, separate and apart from the case as a professional report. I again abstain from comment; but, Sir, unless the practitioners who gave an opinion so much at variance with the facts of the case should favour the profession with an explanation of the means by which they arrived at their conclusion, it will become my painful duty to trouble you on the subject.—I remain, Sir, yours respectfully,

Oakham, Rutland, July, 1846.

WILLIAM ANDERSON.

THE REFLEX FUNCTION.

To the Editor of THE LANCET.

SIR,—Your observations in your late articles relative to the Royal Society, and the researches of Dr. Marshall Hall on the nervous system, have caused many in the profession to wish to know more about them. I have referred to the *British and Foreign Medical Review*, and to the *Edinburgh Medical and Surgical Journal*, for this information. In the former there are, at least, a dozen articles on Dr. M. Hall and his labours; but they are all plainly written in ill-nature, and are not calculated to lead a stranger to procure the original volumes. In the latter there is, as far as I can discover, not one such notice in any number of this *Review*. There is certainly something rotten in the state of medicine when such things are.

I am sure it would serve the profession if you would give a full and fair account of this investigation in an early number, stating what part of Dr. Hall's works is most essential to the mastery of the subject.—I am, Sir, your obedient servant,

Liverpool, July, 1846.

TYRO.

* * The facts stated by our correspondent are singular. We shall certainly comply with his request, for we believe there are few persons who really know what advances have been made in the knowledge of the nervous system.—ED. L.

TREATMENT OF UTERINE HÆMORRHAGE.

To the Editor of THE LANCET.

ALTHOUGH far from agreeing with many of the observations of Dr. Craig, as given in the last number of THE LANCET, on the subject of hæmorrhage and protracted labour, we think that he is perfectly justified in attaching very great importance to the judicious administration of opium in the treatment of such cases. For many years past, we have invariably, in our practice, had recourse to this most potent medicine—generally combined with the tinctures of hyoscyamus and digitalis, in camphor mixture or infusion of roses—in all uterine hæmorrhages, whether from the impregnated or unimpregnated uterus. As a matter of course, it may be necessary to employ other remedies at the same time, which may be either depletory or stimulant, according to the circumstances of the case. In one instance it may be proper to bleed; in another, we must administer powerful cordials. In both, however, opium will very generally do good. A vast number of cases of menorrhagia are best treated with the following remedies:—Nitrate of potass, half a drachm; infusion of roses, five ounces and a half; syrup, three drachms; tincture of opium, thirty minims; tincture of henbane, a drachm; tincture of foxglove, a drachm. Mix. A sixth part to be taken every six hours. Perfect quietude to be enjoined, and all drinks to be taken cool.

In the course of one, two, or three days, the hæmorrhage will almost certainly have ceased, (provided no organic mischief be present;) then recourse must be had to quinine, elixir of vitriol, &c., to restore the strength. Very nearly the same treatment will suit ninety out of every hundred cases of early miscarriage. With respect to the management of protracted labours, and the criticisms of Dr. Craig on the two cases of Dr. Lever, reported (from the *Guy's Hospital Reports*, we believe) in the *Medico-Chirurgical Review* for January of the present year, we must leave Dr. L. to reply. We are quite as doubtful as Dr. Craig seems to be, with respect to the propriety of incision of the os uteri, in cases of lingering labour, from rigidity of this part.

July, 1846.

DELTA.

MEDICAL FEES AT ASSURANCE OFFICES.

To the Editor of THE LANCET.

SIR,—A medical friend having this moment called my attention to the letter which Dr. A. P. Stewart has published, in the last number of THE LANCET, as addressed to me, may I request the favour of the insertion of a few hasty lines in reply to the same, in your next number.

Dr. Stewart, of course, intended his letter to apply to assurance offices in general, and my observations are equally general. It would require much of your valuable space to present the entire views of this question taken by the directors of life-assurance institutions. I will therefore simply venture to remind Dr. Stewart, that a very high authority in his own profession—viz., Dr. Forbes, of Old Burlington-street—has distinctly stated, that he considers the exaction of a fee by medical men, for simply giving their opinion whether a particular patient is or is not of sound constitution, to be a somewhat illiberal assertion of their professional privileges and dues. The case would be widely different if the proposer for assurance, or the assurance office, requested the medical referee to institute a new and special examination. It has appeared to me that the arguments of medical gentlemen have always tended to this assumption; and to the view that directors are consulting them professionally on every case. But, as far as I am aware, the medical referee is only requested to state what are the impressions which his past attendance on the proposer have left, with reference to the proposer's constitution and prospects of life. Surely it is not too much to expect from the courtesy of the profession a few brief answers, often merely monosyllabic, to questions of this nature.

I will only further allude to the fact, that the proposer himself is obviously bound to furnish the office with satisfactory proofs of his eligibility; and that if any charges ought to be incurred in obtaining these proofs, they rest, in all equity, upon the proposer himself.

One word only as to the confidential character of the replies to offices, as noticed in the letter of Mr. Camden, in the same number. Beyond all doubt they ought to be, and I believe generally are, considered strictly confidential, with reference to all other parties than the directors and officials.

I have the honour to be, Sir, your most humble servant,

THE SECRETARY OF THE SCOTTISH PROVIDENT INSTITUTION.

London, July, 1846.

* * It is scarcely necessary to remark, that we entirely dissent from the views of our correspondent.—ED. L.

THE ROYAL SOCIETY.—ANATOMY OF THE DUCK-BILLED PLATYPUS.—(PLATYPUS ANATINUS.)

To the Editor of THE LANCET.

SIR,—Having lately observed in THE LANCET certain complaints against the Royal Society, relative to the papers laid before it, I am induced to send for your perusal a statement of what occurred to myself some years since, which, if you think it at all bears upon the subject, is quite at your service for insertion.

In the year 1832, I received, as honorary secretary to the Norfolk and Norwich Museum, two specimens of the duck-billed animal (*platypus anatinus*) from Van Diemen's Land, preserved in spirits. On examination they were found unfit for stuffing; I therefore determined to make skeletons of them for the institution, previously to which I examined their internal structure, as far as their semi-putrid state would permit. I read the results of this investigation at a committee meeting of the museum, illustrating the paper by a display of the different parts of the animal injected and preserved in spirits, and which are still in the collection of that institution.

Mr. Dawson Turner, F.B. & L.S. &c., of Yarmouth, who, if I am not mistaken, was then President of the Norwich Museum, and was present at the meeting, offered to read the paper before the Royal Society. To this I acceded with much pleasure; and I had coloured drawings made of all the parts, feeling sure that my discovery of the mammary glands would be inserted in the *Philosophical Transactions*, and that I should thus be the means of clearing up some of the difficulties that had surrounded the anatomical structure of this curious animal, in consequence of the paper published in the *Transactions* of the Society, some years previously, by Sir Everard Home. The paper was read, and some time after I received the following letter:—

"SIR,—The Royal Society return you their thanks for your paper, entitled, 'Some Remarks on the Internal Structure of the Platypus Anatinus,' which the committee of papers, although they do not think proper to publish it at present, have directed to be deposited in the archives of the Society.—I am, Sir, &c. &c.,

"June 13th, 1832."

"JOHN GEO. CHILDREN, Sec. R. S.

When, some months after, a paper on the platypus and the kangaroo, by Professor Owen, was read at a meeting of the Society, and published, as it deserved to be, I could not help feeling that my communication, though far inferior to his, was entitled to some notice, the discovery of the mammary glands having been first made and communicated to the Society by myself. I do not assert that the circumstance of Professor Owen being engaged in investigating the same subject was the cause of my paper having been laid aside, though I own it did suggest itself to me as the most probable explanation; for his investigations were on too extensive a scale not to have been known to some members of the Society.

I am, Sir, yours &c.,

RICHARD GRIFFIN, M.R.C.S.

P.S.—I regret that I have not a duplicate of the paper, and can therefore only lay before you a copy of the rough notes, taken at the time, but the original and drawings are in the possession of the Royal Society, and the specimens are in the Norfolk and Norwich Museum, which are sufficient to authenticate this communication.

Weymouth, June 20th, 1846.

ANATOMY OF THE PLATYPUS ANATINUS.

Having been requested by the Committee of the Norfolk and Norwich Museum to make skeletons of the male and female of the platypus anatinus, I thus enjoyed a rare opportunity of examining their internal structure, and such of my observations as disagree with the scientific description given by Sir Everard Home, in the ninety-second volume of the *Philosophical Transactions*, I am desirous of communicating to the public.

On removing the hair from the abdomen of the female, I discovered two circular portions of skin, separated an inch and a half from each other, and four inches and a half from the verge of the anus, one being on each side of the abdomen. They were perforated by numerous ducts, which, upon removing the skin, I traced to two large mammary glands, covering nearly the whole under surface of the animal. These glands, upon slight examination, might be deemed as portions of fat; indeed, they were considered so by Sir Everard, whose words are—"The size of the body (in the female) is rendered proportionally larger than that of the male, by a quantity of fat lying everywhere under the skin." But upon dissecting one of them, I readily perceived it to be glandular. Sir Everard says, in the vol. for 1819, that "the young provide for themselves, the mother not giving suck." He was not aware of the existence of these glands, or he certainly would not have made this assertion: in fact, he says, when speaking of the ornithoryncus hystrix, "when more of this extraordinary tribe of animals, which, although quadrupeds, are not mammalia, shall have been discovered," &c. &c. How then, we may ask, do they give suck, the mother having no nipples? to which I would venture to reply, that either from the peculiar formation of their beaks, which have lips attached to them, no nipples may be required, or that, in consequence of impregnation, a degree of excitement takes place in those parts which causes a development of the nipples; this, however, can only be decided by future opportunities of examining the animal in an impregnated state. The vagina terminates with the rectum in a common passage; at its upper part is the meatus urinarius, or passage from the urinary bladder; on each side of this are two openings, scarcely admitting a hog's bristle, but easily capable of expansion; these are the orifices of the Fallopian tubes: they are convoluted, small at their vaginal extremities, but in the rest of their course much dilated, having a long oval slit in them at their further ends. Immediately posterior to the vaginal orifices there are two processes, projecting the sixteenth part of an inch into the vagina; in the centre of each is the opening of a duct, which runs about two inches, but was in this animal, unfortunately, injured, so that I cannot say where it terminated, or what was its use. Might it not be for the purpose of aerating the foetus or ovum in the Fallopian tube, which latter may probably act in place of the uterus, which this animal has not. We find, in the account of Sir Everard, similar tubes in the kangaroo, leading to the uterus, for the purpose of aerating the

foetus. At the verge of the anus I found a large opening; its course I was unable to trace, the parts having been removed from the body. From the state of the animal the ovaria could not be detected. The penis of the male, enclosed in the anus, is protruded from it in the act of copulation. When injected it measured about two inches; it has two corpora cavernosa, which do not communicate, as in the human subject; at the end of each are three pointed processes, and not four in one and five in the other, as described in the animal examined by Sir Everard; through these, it is stated, the semen passes, but not the urine. The corpora cavernosa are separated before their termination, causing the extremity of the penis to be forked; it is studded with rough papillæ. The male is provided with spurs on the hind feet; not so the female: hence originated the following remark of Sir Everard: "It is probable, by means of these spurs or hooks, that the female is kept from withdrawing herself in the act of copulation, since they are very conveniently placed for laying hold of her body on that particular occasion. The female has no spur of this kind." This I cannot conceive possible: the fact is, they are intended either as weapons of offence or defence; a canal runs from the base of each spur to near the point, through which, when the animal pierces anything, a fluid escapes, one spur being placed on the top of a small sac, which is situated between the two bones of the leg, so that the least pressure in the spur communicates a pressure to the sac, and consequently the fluid contained in it escapes through this, its only outlet; the sac is only large enough to contain a common pea. That the fluid is poisonous, or at least highly irritating, is proved by the following extract from the Minute-Book of the Linnæan Society, read March, 1817:—Sir John Jameson says—"I wounded one with small shot in New South Wales; and on my overseer taking it out of the water, it stuck its spurs into the palm and back of his right hand with such force, and retained them in with such strength, that they could not be withdrawn until it was killed. The hand instantly swelled to a prodigious size; and the inflammation having rapidly extended to his shoulder, he was in a few minutes threatened with lock-jaw, and exhibited all the symptoms of a person bitten by a venomous snake. The pain from the first was insupportable, and cold sweats and sickness of stomach took place, so alarmingly, that I found it necessary, besides the external application of oil and vinegar, to administer large quantities of the volatile alkali, with opium, which I really think preserved his life. He was obliged to keep his bed for several days, and did not recover the perfect use of his hand for nine weeks." In the cheek-pouches were small shells, but none of the concrete substance mentioned by Sir Everard; the peritonæum of the male was studded with chalky accretions.

THE UNIVERSITY OF LONDON.

THE EXAMINATIONS FOR THE DEGREE OF M.D.

To the Editor of THE LANCET.

SIR,—I venture to direct attention, through the medium of THE LANCET, to an arrangement, in the proceedings of the University of London, which I believe to be as improvident and impolitic as it is injurious and unjust, and which, although not necessarily the result either of the spirit or of the terms of its by-laws, is unhappily permitted to form part of a system at present enforced, and seems to militate against the prosperity of the institution, by inflicting a serious injury upon all, and no small injustice upon many, of the unsuccessful candidates for its degrees in medicine. I am induced to address this communication to you, partly in consequence of the earnestness you have always displayed in protecting the interests of the student, and partly for the reason that letters suggestive of improvements, or memorials burdened with complaints, forwarded directly to the senate, and yet proceeding from personages of authority, or emanating from aggrieved individuals, have in times past been deemed intrusive, and their claims for consideration treated accordingly. It will, however, be sufficiently obvious, that in alluding publicly to the wrong of which I complain, I am actuated by a desire to see a fair and reasonable justice awarded to those who fail in their first attempts to overcome the severe and probing examinations of the University; and thus sincerely anxious to aid in promoting its welfare, by urging upon the senate a less equivocal recognition, and a more equitable adjustment, of the rights of the rejected. I have no authority to boast, no personal grievance on which to declaim; I am disinterested, save in the proceedings and the character of the University, and ambitious only in ministering to the rights of