the week another meeting took place, at which a very large number of the practitioners of Birmingham and the neighbourhood were present, when it was determined to wait until the annual meeting of the members of the Institute was held, so that those members who disapprove of the admission of homœopaths may see whether the Committee are resolved to pursue their present course to admit homœopaths even at the cost of the enforced retirement of no inconsiderable number of the most sincere well-wishers and friends of the Institute.

Correspondence.

"Audi alteram partem."

EXAMINATIONS IN PHYSIOLOGY IN THE UNIVERSITY OF GLASGOW.

To the Editor of THE LANCET.

SIR,—Having just finished my winter's labour in the University, I take the earliest opportunity of referring to a report of the General Medical Council put by them on permanent record against me; and I have to request you to place my reply on the equally imperishable record of your pages.

The report to which I refer is made by Professor Humphry and Dr. Quain, members of the General Medical Council, along with Dr. Barclay and Mr. Power, visitors appointed by the Council. It relates to the examinations in Physiology at the University of Glasgow, and I reply to their report by an unqualified contradiction in so far as any blame connected with it is ascribable to me. *Quoad ultra* I offer no opinion.

I place Professor Humphry's name first because, being, as I understand, himself a physiologist, he cannot but be aware of the conditions under which alone practical physiology can be successfully taught, and thereafter fairly made a subject of examination. Had he condescended to communicate with me before writing his report, he would either not have written the report at all, or, supposing him to possess ordinary candour, he would have converted into eulogy his disparaging observations. I ask Professor Humphry if it be possible to teach practical physiology without instruments, without a cabinet in which to place the few that could be procured, without a laboratory, without funds, and without an assistant. All this I did for five-and-twenty years. So long as my son lived he and I did the whole work from the mere love of it, and without costing the University of Glasgow one farthing. On his death—a loss irreparable to me—I placed my necessities before the University authorities, requesting of them money to buy instruments and a laboratory, and funds to pay an assistant, and defray my expenses. I received a sum to buy instruments, but having not a single room or cabinet in which to place them I could only buy such as I could carry backward and forward from my own house. At length, after ten years, a laboratory is in process of construction, but there is no provision to keep it up, or for paying an assistant to keep it in order and take charge of the valuable instruments—the property of the University or my property—which must be deposited in it. Ten years seems to be the appointed time for all great struggles, from the siege of Troy downward to the Free Church conflict, and the laboratory fight in the University of Glasgow. How I wish that all my exploits could be recorded. But I am more luckless still than those heroes of antiquity whose sad fate the poet has so sweetly bewailed: "*Carent quia vate sacro*"; for not only have I had no bard to sing my praises, but I have had four discreet visitors from the General Medical Council, who have proclaimed me everywhere throughout the three kingdoms, with the name in full and in capital letters, as deficient in that very branch of physiology which it is my chief credit to have been one of the foremost to introduce into this country, and for which I have at length obtained a tardy recognition from the University of Glasgow.

The visitors further object to my questions as not calculated to test the knowledge of the candidates, and to my

estimate of the answers as much above their merit. May I be allowed to ask for credentials of competency on the part of those gentlemen to pronounce summary judgment on my physiological questions? and, knowing of none, may I be allowed to suggest the following test of their capabilities? I subjoin a copy of the questions put by me on the 10th inst. to the candidates for M.B. of the present session. I have to request each of those gentlemen to send answers to THE LANCET with the usual attestation *sine auxilio*. I shall thereafter give through the same medium my estimate of their answers. The medical public will thus be gratified by hearing the opinion of learned men on some very interesting and much-contested physiological problems, and they will be enabled to judge both of the competency of the visitors to perform their high duties and whether it be true that I assign extravagantly high marks to answers of inferior merit.

I am, Sir, yours most sincerely,

A. BUCHANAN.

April 26th.

[Copy.]

PHYSIOLOGY.—WRITTEN EXAMINATION.

1. Why does the blood move alternately faster and slower in the proximal part of its circuit, or that nearest the left ventricle of the heart; and why does it move with an equable velocity in the distal part of the circuit, or that nearest the right auricle?

2. At what part of the circuit is the unequable movement exchanged for the equable, and is it always at the same part?

3. Why is a pulsatile movement sometimes seen in the capillaries of the web of the frog's foot, and how can that movement be exhibited at will?

4. What is the error involved in the conclusion drawn from Lavoisier's experiments, that the excretion from the skin is greater than that from the lungs?

5. Explain and exemplify what is meant by voluntary movements; what by movements wholly, or in part, involuntary; what by automatic movements; and what by reflex movements.

N.B.—Three answers required, including either No. 4 or No. 5.

A NEW OPHTHALMIC OPERATION.

To the Editor of THE LANCET.

SIR,—Every ophthalmic surgeon must have read with amazement the description in THE LANCET of April 24th of "a new ophthalmic operation" recently performed by Mr. Brudenell Carter. According to your report, it would appear that the chief advantage claimed for this ingenious method of complicating one of the easiest operations in ophthalmic surgery—viz., "an iridectomy for optical purposes," is that a V-shaped pupil, having its broader extremity opposite the more central part of the cornea, is obtained. It is difficult to imagine that any experienced ophthalmic surgeon should not be familiar with the fact that a simple excision of the pupillary margin of the iris is followed by the same result, or that, with a knowledge of this fact, he should abandon a safe and easy procedure for one which is fraught with danger at every step; because, firstly, unless the patient is so profoundly anæsthetised as to be absolutely motionless, it is obviously less safe to introduce a knife behind the sclero-corneal junction than in front of it. Secondly, if when the scissors are passed beyond the pupillary margin with their "points" resting against the lens capsule, they are made to excise a fold of iris, the chances are that even in the hands of a dexterous operator traumatic cataract will not unfrequently ensue. Thirdly, supposing the operation to have been thus far successfully accomplished, the scissors are withdrawn, and there remains within the anterior chamber a detached fragment of iris, for the removal of which it is necessary to introduce a pair of iridectomy forceps; and although the excised piece of iris seems (in Mr. Carter's cases) to have evinced an obliging disposition to rush within their grasp, the evidence of three cases is not sufficient to warrant the assumption that so fortunate an event will always happen. It is, moreover, a well-known fact that the removal of loose bodies from the anterior chamber is often attended with very considerable difficulty and not less danger.

Finally, it is stated that Mr. Carter "proposes to use Calabar bean in his next case, with a view to steady the iris, as in one of the three cases upon which he has effected this operation he derived assistance from the presence of an anterior synechia." Happy thought! but great will be his disappointment when he finds that in Calabar-myosis, as in atropine-mydriasis, the escape of aqueous humour is followed by an instantaneous return of the pupil to its natural condition.

I remain, Sir, yours truly,

Royal London Ophthalmic Hospital, May 3rd, 1875. F. BULLER.

CROUP AND DIPHTHERIA.

To the Editor of THE LANCET.

SIR,—In your report of the observations I made at the Clinical Society on the subject of the identity of croup and diphtheria I am made to say that I have "recently been converted to the view that croup and diphtheria are identical." I should like to say that this does not quite express my position. The observations I made were to the effect that although I had long believed in the distinctness of the two diseases, this opinion of mine had recently been "rudely shaken"; and I instanced a case I had lately had under my care, which would probably have been registered as croup if a post-mortem examination had not shown it to be diphtheria.

But while I am entirely convinced that many cases of diphtheria have been registered as croup, I am far from believing that sufficient evidence has been produced to settle the question in favour of their identity, and I am altogether opposed to the tone of dogmatic finality which has been assumed by some on the identity side, as it is essentially hostile to that liberal and patient spirit of inquiry which in medical, as in all scientific investigations, can alone lead to real advancement in knowledge.

It has also occurred to me, in connexion with this discussion, that I have found a far larger experience and familiarity with cases of so-called croup, and with epidemics of diphtheria, amongst medical men in large general practice, especially in the provinces, than amongst the physicians of general hospitals in London. So far as I have been able to observe, cases of croup and diphtheria are comparatively rare in the general wards of London hospitals. As these cases are exceedingly acute, and often rapidly fatal, I am inclined to believe that by far the greater number of them fall, exclusively, under the observation of our brethren in large general practice, and it is to them therefore that we should look for evidence which may decide this dispute.

I am, Sir, your obedient servant,

[Hertford-street, Mayfair, May 1st, 1875.

I. BURNEY YEO.

To the Editor of THE LANCET.

SIR,—I think that the limit of my remarks at the last meeting of the Clinical Society was missed by your reporter, whose accuracy on former occasions this session I am glad to have the opportunity of acknowledging.

I wished to point out that, whereas in an epidemic of diphtheria cases in which the inflammation falls mainly upon the larynx are exceptional, cases of membranous inflammation of the larynx are constantly being admitted into the London hospitals without one's seeing anything of the cases of ordinary diphtheria affecting the palate and pharynx, which ought to occur in still larger numbers if Sir W. Jenner's theory were correct.

I also remarked that such cases are, for the most part (if not always), non-infectious, whereas what I have seen of true diphtheria would lead me to believe that it is highly infectious.

These facts (to which may, of course, be added the arguments adduced by Dr. Moxon and others) appear to me to make it highly improbable that the cases of so-called croup, attended with the formation of false membranes in the larynx, which one sees so frequently in hospital practice in London, are all of them cases of diphtheria; and my belief is that scarcely any of them are so. But I am quite prepared to admit that in each individual case one may be unable to negative the possibility that the disease may belong to the laryngeal form of diphtheria.

I am, Sir, yours, &c.,

C. HILTON FAGGE.

St. Thomas's-street, Southwark, May 2nd, 1875.

To the Editor of THE LANCET.

SIR,—The interesting letter of Sir John Rose Cormack, published in THE LANCET of April 24th, induces me to offer what little assistance I can in throwing some light upon diseases which do not appear to be generally recognised as affecting animals. With regard to the transmissibility of diphtheria from the human to other species, I know of no instances except the one recorded by Professor Bossi and alluded to by your correspondent, and which may have been a case of transmitted disease. I may mention, however, that I have the notes of a case in which whooping-cough appeared to be transmitted to a dog from a child, who during the attacks of coughing frequently vomited, and the animal when present always devoured the expelled matters. I may also note that several cases of diphtheria have occurred in our barracks at Chatham quite recently, and though dogs are numerous and frequent the rooms occupied by those who have been affected, yet none have shown any symptoms of the malady.

Of the existence of croup and diphtheria in animals, there can be no doubt whatever. Simple membranous croup is not at all infrequent; young creatures are more disposed to be attacked by it than those which are old, and it affects all the domesticated animals, though the dog is more exempt than cattle or pigs; highly bred animals of the latter species are very liable, and in them the disease most closely resembles the human *angine couenneuse*, or *pseudo-membraneuse* of the French medical authorities. It has been witnessed in an epizootic form in cats, and it frequently prevails as an enzooty among poultry. The false membranes may be limited to the larynx, or they may extend to the pharynx, mouth, and nasal fossæ in one direction, and the trachea and bronchi in the other.

Diphtheria is certainly rare in animals, and has not yet been alluded to by veterinary writers in this country, I think. It nevertheless exists, and sometimes in a very marked form, differing from croupal angina by the great prostration which accompanies the formation of the diphtheritic exudations, and the tendency to a fatal termination. In addition, we observe a tendency to the production of plastic deposits wherever an artificial inflammation has been attempted to be set up. Blisters, for instance, instead of being followed by vesication or suppuration, are frequently succeeded by a more or less thick layer of fibro-albuminous matter. Setons are also often sheathed in a pseudo-membranous cylinder; the blood is very plastic at the commencement of the disease, and its clot is thick and very firm.

Diphtheritic exudations in the larynx and bronchi are somewhat common in cattle-plague, contagious pleuro-pneumonia, and some other bovine maladies.

We have no strong proof that croup or diphtheria is contagious in animals, except the former in fowls.

Two years ago I had a most interesting case of spasmodic croup in a troop mare, in which tracheotomy had to be resorted to.

I am, Sir, yours very obediently,

GEORGE FLEMING,

Veterinary Surgeon, Royal Engineers.

Brompton Barracks, Chatham, April 27, 1875.

To the Editor of THE LANCET.

SIR,—Allow me to say briefly that the nomenclature suggested by Sir John Rose Cormack appears to me to be likely to increase rather than to lessen the confusion which has resulted from the use of the unfortunate term "croup." He makes three species of the genus croup:—1st. Diphtheritic croup; 2nd. Simple membranous croup; 3rd. Spasmodic croup. The term diphtheritic croup is unobjectionable. "Simple membranous croup" is a substitute for what Trousseau calls "simple membranous sore-throat." Under this term Trousseau describes various forms of sore-throat, especially herpes of the pharynx; but the one great distinction between these cases and membranous croup is that they have no tendency to spread to the air-passages. I am not aware that Trousseau or any French writer of eminence uses a term which could be translated "simple membranous croup." "Spasmodic croup" Sir John Cormack describes as "the strangulatory affection in which there is no formation of false membrane;" and he adds that "this is the *faux* croup of French writers." But surely the *faux* croup of the French writers is the simple or non-specific inflammatory or catarrhal