Dr. MICHAEL FOSTER, examined by Mr. Besley, gave an account of Dr. Ferrier's statements at the International Congress with regard to experiments on certain monkeys carried out with antiseptic precautions. He said that in former years he had removed certain parts from the surface of the brain of monkeys; that in consequence of the removal certain definite conditions of the organs took place; and that those experiments had been corroborated by other experiments by Professor Yeo according to the antiseptic system. There was no public announcement at the Congress that the animals would be exhibited, but witness and others present considered it desirable, and special arrangements were made for the purpose. Dr. Carpenter, Professor Burdon Sanderson, Professor Rutherford, and other eminent gentlemen were present. It was typically a physiological meeting. The first monkey brought in seemed to be perfectly natural, but it showed no sign of hearing when a pistol was fired behind its head. The other monkey was evidently lame. The animal was pinched, to show that its sensations were unaffected. One or two gentlemen spoke on the subject, and then the meeting terminated. Afterwards it was arranged that the dog and the paralysed monkey should be killed, and the brains examined by an independent committee, to ascertain what was the exact nature of the injury.

Sir J. INGHAM: What is the specific experiment calculated to produce pain which you impute to Professor Ferrier? It is proved now that the two monkeys had been operated upon by Professor Yeo.

Mr. WADDY said it was a continued experiment, and Dr. Ferrier took a leading part in it.

Sir J. INGHAM: I confess I think there must be some participation in the original act in order to make it an act of cruelty within the meaning of the Act of Parliament; and there is not only no proof that Professor Ferrier had anything to do with the original act, but the proof, so far as it goes, is to the contrary. If you have nothing more to adduce I must dismiss this summons. You may, if you think proper, take a case upon the point, because it is one of very great interest, no doubt. It is very important, I am sure, to magistrates that some clear definition of the word "experiment" should be laid down by superior authority.

After some discussion,

Dr. WAKLEY (Editor of THE LANCET) was called, and at the request of Mr. Waddy produced the manuscript of the report that appeared in that journal.

Mr. WADDY: Who wrote that report?

Dr. WAKLEY: I have the permission of the gentleman to give his name—Professor Arthur Gamgee, of Owens College, Manchester.

Mr. WADDY asked for an adjournment in order that the evidence of Professor Gamgee might be taken, but finally withdrew the application, and the summons was dismissed.

Mr. GULLY: As Professor Yeo has been mentioned, and as it has been stated that this was an operation conducted by him, I wish to state that he conducted it in strict accordance with the law, using anæsthetics, having a licence for the operation, and having a certificate for keeping the animals alive during the period they were kept alive.

Correspondence.

"Audi alteram partem."

BOUILLAUD AND APHASIA.

To the Editor of THE LANCET.

SIR,—In your notice of Bouillaud, the distinguished French physician, I see no mention of his work on the Brain, and especially his assertion that speech was connected with the anterior lobes. I have always regarded Bouillaud as the first who enunciated this doctrine. His work was written in 1825, and in this he says: "From the observations which I have collected, and from a great number of those which I have read in authors, I believe myself able to declare that it is in the anterior lobes of the brain that the ruling principle of speech resides (*que réside le principe législateur de parole*). In looking through their works I see with extreme satisfaction that their observations accord perfectly with my own, and I am only astonished that they did

not long ago make the easy discovery which is now occupying our attention. I have read attentively the observations of those authors, sometimes beginning with the symptoms and sometimes with the alterations in the brain found after death. Whenever amongst the symptoms there was mention of loss of speech I concluded there would be an alteration in the anterior lobes, and when, on the contrary, by a kind of neuter proof I found in the description of the pathological changes a disorganisation of these same lobes I said on consulting the symptoms there would be loss of speech. I assure you that I have never yet been deceived in this kind of diagnosis." Again, in speaking of "*la perte de la mémoire des noms*," he says, "Now, since on the one hand the loss of speech and of the memory of words is the inevitable consequence of disorganisation of the anterior part of the brain, and since on the other hand this symptom does not accompany alterations of other cerebral convolutions, have we not a right to conclude that the anterior part of the brain is the seat of the organ of articulate language?"

It is remarkable that although in his illustrative cases it is mentioned that one side only of the portal lobes was damaged, Bouillaud makes no reference to it. The localisation to the left side was made out by Dax and Broca, and confirmed by Hughlings-Jackson. It may be remembered that Gall and the phrenologists had before this time asserted that the seat of language lay in the anterior lobes, and supported their doctrine by cases of accident and disease.

I am, Sir, yours obediently,

Grosvenor-st., Nov. 12th, 1881.

SAMUEL WILKS.

A RARE TERMINATION OF CANCER OF THE ŒSOPHAGUS.

To the Editor of THE LANCET.

SIR,—As illustrating a rare mode of termination of œsophageal cancer, and as bearing on some of the points raised at the last meeting of the Clinical Society with regard to surgical interference in such cases, I may be permitted to give the following brief details of a post-mortem examination I was fortunate enough to see at the Pathological Institute at Berlin in September last. The case was that of a man about fifty years of age, who was suffering from a stricture of the œsophagus of some duration. He was, moreover, phthisical, and had already given indications of commencing pulmonary gangrene, when he was suddenly attacked with profuse hæmoptysis which carried him off.

The examination was made by Dr. Grawitz on Sept. 19th. The bronchi of the right lung were found to be filled with blood. In the anterior part of the lower lobe, near the root of this lung, was a gangrenous cavity the size of a walnut, surrounded by a zone of hepatisation, and some scattered greyish broncho-pneumonic foci. At the apex of the left lung there was an old smoothly-lined vomica, and dispersed throughout the organ were greyish, pigmented, and caseous granulations, as well as some irregular caseous masses. Both lungs were highly engorged. The œsophagus was adherent to the aorta and trachea opposite to the bifurcation of the latter; and it was here the seat of a large irregularly excavated cancerous (probably epitheliomatous) ulcer. The cavity of the ulcer was filled with blood-clot, as were also the segment of the gullet below and the stomach. On turning out the clot the ulceration was found to have perforated the right side of the trachea at its bifurcation, and the descending aorta about the same level. Above the cancer the gullet was much dilated, whilst below it some secondary nodules occurred in the mucous membrane of the œsophagus, and also in the mucous membrane of the stomach.

The course of events was probably as follows: The cancer of the œsophagus in its middle portion had first ulcerated into the trachea at the point of departure of the right bronchus; hence the supervention of pulmonary gangrene, a by no means infrequent sequence in œsophageal cancer at this region. The perforation into the aorta resulted in the attack of hæmoptysis and hæmorrhage into the stomach, the blood finding its way into the right lung through the previously established fistula. It is this ulceration of the aorta that lends importance to the case, and emphasises the recognised need for the caution with which the introduction of instruments should be used. The supervention of cancer

in a phthisical subject, and the occurrence of secondary growths by direct infection of the mucous membrane beyond the cancer, are also points of pathological interest.

I am, Sir, yours truly,
Weymouth-street, Nov. 14th, 1881. SIDNEY COUPLAND.

PROFESSOR VIRCHOW'S TESTIMONIAL.

To the Editor of THE LANCET.
SIR,—Permit me to acknowledge the receipt of the following additional subscriptions in respect of the above testimonial, and to say that, though I have already transmitted to Professor Küster, of Berlin, the sum of £88 16s., in order that it may reach him before the 19th, I shall still be happy to receive subscriptions.

Sir Thos. Watson, Bart.	...	...	£3	3	0
T. B. Curling, Esq.	...	...	1	1	0
Dr. Pye-Smith	...	...	1	1	0
Dr. Wilson Fox	...	...	2	2	0
Dr. Swanzy	...	...	1	1	0
Dr. Vacher	...	...	2	2	0
Dr. T. Edmondston Charles	...	...	5	5	0
John Simon, Esq....	...	...	1	1	0
Dr. F. Chance	...	...	2	2	0
Spencer Wells, Esq.	...	...	2	2	0
Dr. C. T. Williams	...	...	1	1	0
Dr. W. H. Allchin	...	...	1	1	0
Dr. Grigg	...	...	1	1	0
Dr. E. B. Baxter	...	...	1	1	0
Joseph Lister, Esq.	...	...	2	2	0
Dr. M. Watson	...	...	2	2	0
Dr. A. R. Simpson	...	...	5	0	0
From Manchester (per Dr. Dreschfeld)	17	17	0		
Dr. A. B. Shepherd	...	...	1	1	0
Dr. Cheadle	...	...	1	1	0
Dr. Broadbent	...	...	1	1	0
Dr. Hermann Weber	...	...	3	3	0

I am, Sir, your obedient servant,
Old Burlington-street, Nov. 17th, 1881. J. S. BRISTOWE.

HYPERPYREXIA.

To the Editor of THE LANCET.

SIR,—In the case published in your number of November 5th (as read before the Clinical Society by Dr. Mahomed), it appears to me that the conclusions drawn are far too positive, and no account is taken by the author of the arguments on the other side. As medical ward-clerk I had the case directly under my observation for three months, from December 1st, 1879, to February 28th, 1880, and although I was extremely sceptical with regard to these temperatures at first, I was compelled to come to the conclusion that they were genuine before my appointment terminated. I watched the case most carefully, and although she was an extremely hyperæsthetic girl, I do not think she was capable of producing fraudulent temperatures. During these three months the temperature averaged about 101° in the morning, and 102° to 103° in the evening, being regularly taken every day; many times it rose to 104°, 105°, and 106°, and these were generally in the evening. On two occasions I took evening temperatures of 109° and 111° (each time it was just after she had had a short sleep) in the left axilla, at which spot I had always had my highest records. I was sitting on the bed holding the thermometer, with my fingers in the axilla and with my other hand holding the patient's arm, so that I know there was not the slightest amount of friction against the thermometer. I may here mention that the left side was the one on which the most mischief was found at the autopsy.

It is very interesting, as far as my experience goes, that her highest temperatures were always recorded in the left axilla, and also (which is a most important fact, and one which Dr. Mahomed denies), I noticed that her skin at that spot was burning hot, so as to be quite unpleasant to touch, and different to the rest of her body. Now, is it not surely more scientific to admit that these temperatures did not exist, than to ridicule them because they cannot be explained?

There is another fact, too, which Dr. Mahomed quite overlooks, and that is, the patient herself would be most

miserable and depressed if she knew her temperature was high, because she feared some further experiments might be made on her; for she disliked most intensely being placed in a private room, and having her temperature taken innumerable times during the day. I may add that my experience is quite confirmed by a previous medical ward-clerk and also by the house-physician who was on duty when her temperature was first noticed to be so abnormally high. Apologising for troubling you,

I am, Sir, your obedient servant,
Guy's Hospital, Nov. 14th, 1881. W. H. C. NEWNHAM,
B.A. (Cantab.), M.R.C.S.

A NEW FRAME FOR HOLDING SPHERICAL AND CYLINDRICAL LENSES DURING THE DETERMINATION AND CORRECTION OF ERRORS OF REFRACTION.

To the Editor of THE LANCET.

SIR,—Having in common with other ophthalmic surgeons experienced great inconvenience from the imperfections of all contrivances hitherto brought out for the above purpose, I have watched the development of Mr. Makins' instrument with much interest, and having now used it for some time I feel justified in saying that it meets all requirements, and I venture to address to you this letter describing it in the hope that others will avail themselves of its use. Up to this time every new apparatus for this purpose has surpassed its predecessors either in complexity or in weight, and without fulfilling in any way the chief indications—viz., the possibility of altering the lateral distances of the centres, and likewise compensating for the vertical variations due to the form of the patient's nose, easy rotation of cylinders either by patient or surgeon, space for the interposition of prisms, lightness, and perfect fixation on the head of the patient. The instrument is entirely the invention of Mr. Makins of Messrs. R. and J. Beck's, 68, Cornhill, and the only point which originated with me was the introduction of certain vulcanite "ring stops" to prevent shaking when lenses of low refractive power are used. Both the vertical and horizontal movements are provided for by the simplest contrivance, but requiring much care in making, and the most recent part of the instrument consists in having the limb that goes behind the ear bent so as to hook firmly around it, and the length of the limb adjustable to the exact degree of tension required by means of a screw. This secures the apparatus so firmly that the patient can keep the glasses on easily for any length of time, and the glasses can be changed as quickly as can be desired, for they simply drop into their places. For correcting errors of refraction by the ophthalmoscope where for any reason the ordinary direct method is undesirable it is simply invaluable. Scarcely a day passes that I do not use this contrivance in determining the errors of young children.

I am, Sir, yours, &c.,
Finsbury-circus, E.C. JAMES E. ADAMS.

THE COMMISSIONERS IN LUNACY AND POOR-LAW MEDICAL OFFICERS.

To the Editor of THE LANCET.

SIR,—Would you kindly insert the enclosed correspondence in this week's LANCET, and oblige,

Yours truly,
3, Bolt-court, Fleet-street, Nov. 15th, 1881. J. WICKHAM BARNES.

3, Bolt-court, Fleet-street, Nov. 4th, 1881.

MY LORD,—We are directed by the Council of the Poor-law Medical Officers' Association to write and complain of certain statements which appear in page 121 of the late report of the Commissioners in Lunacy, which seriously reflect on the capacity of Poor-law medical officers as certifiers of lunatics. The Council venture to remind your lordship that the great bulk of the certification of lunatics throughout the country must of necessity pass through the hands of Poor-law medical officers; most of whom, though holding badly paid offices, are highly educated and honourable men. Being fully cognisant of the very large services you have at all times rendered to the cause of the sick poor, and the kindly manner in which you have at various times