has reported favourably on the general state of the workhouse, which he found to be in good order, and that he did not impugn the conduct of the medical officer or the master or any other officer. The Board do not doubt that the services of the medical officer have been such as fully to justify the confidence which the guardians repose in him; but no length or efficiency of service on his part can relieve him from the necessity of receiving with proper deference any suggestions in relation to his duties which may be made to him by the guardians or this Board. I am, Sir, your obedient servant,

(Signed) ENFIELD, Secretary.

H. Brearey, Esq., Clerk to the Guardians of the York Union, York.

Correspondence.

"Audi alteram partem."

THE CASE OF THE LATE DR. WHEWELL.

To the Editor of THE LANCET.

SIR,—In that very interesting, exact, and graphic description of Dr. Whewell's accident and illness, and of the examination after death, which Dr. Humphry records in THE LANCET of the 17th inst., it is especially noted that the accident was followed (in addition to other symptoms) by motor paralysis of the left arm and hand, and of the lower part of the left side of the face; whilst it is also stated that "there was no loss of power in the lower limb or the upper part of the face." On post-mortem investigation, the cerebral ventricles were found to contain bloody fluid, the blood having escaped from a rent "in the fore part of the right corpus striatum and the adjacent region of the brain, but not presenting at any part of the exterior." The brain was not at all softened.

Dr. Humphry draws attention to the remarkable circumstance "that the paralysis was so slight and partial, although the lesion of the brain was so extensive and in so important a region."

Many of your readers conversant with the literature of cerebral physiology and pathology will remember that formerly it was maintained, on the authority of Foville, Serres, Bouillaud, and others, that disease of the corpus striatum and neighbouring parts of the anterior lobe of the brain produced paralysis of motion in the lower limb, whilst disease of the optic thalamus produced paralysis of the upper limb, of the opposite side. And from these supposed facts it was inferred that the corpora striata and optic thalami presided, as it were, over the lower and upper limbs respectively. I would in these few lines venture to point out that the case of Dr. Whewell, in which the cerebral injury was as limited and as uncomplicated as could possibly be desired, is very instructive in relation to this question; and that it shows well (and, perhaps, in a more conclusive and crucial manner than either any other pathological change or any experiment or artificial mutilation of animals could do) that the above view is utterly untenable. And I would suggest that the case may be looked upon as a useful addition to other facts and cases now fully accepted as entirely supporting the results arrived at by Andral, Lallemand, Longet, and others, in opposition to the authorities above quoted.

I am, Sir, yours obediently,

JOHN W. OGLE, M.D.

Upper Brook-street, Grosvenor-square, March 17th, 1866.

THE NEW INSTRUMENT FOR FLEXIONS OF THE UTERUS.

To the Editor of THE LANCET.

SIR,—In your last number there appeared a very carefully-worded letter from Dr. Robert Greenhalgh, referring to an instrument delineated and described in my paper, "On Extreme Flexions of the Uterus" (THE LANCET, Feb. 24th). This letter was calculated to convey to your readers the impression—1st. That my instrument "closely resembled in principle and construction" a contrivance which Messrs. Weiss made at Dr. Greenhalgh's suggestion two years ago. 2nd. That the instruments were intended for cases of identically the same kind. 3rd. That I had deliberately and knowingly appropriated and described as new what Dr. Robert Greenhalgh had previously invented and introduced.

Dr. Greenhalgh is certainly accurate in one statement. When I showed him my instrument a few days prior to your publication of my paper, he remarked that he considered he had devised something of the same kind, and referred me to the recent work of Dr. Marion Sims for its description.

By the courtesy of Dr. Sims I am enabled to submit to your readers that identical engraving to which Dr. Greenhalgh refers as evidence of my piracy.

It happens that Dr. Greenhalgh, in his note to you, omitted to mention that his instrument had been described and exhibited as having for its sole object the dilatation of a cervix uteri previously incised with the hysterotome, and that it has nothing whatever to do with flexions of the uterus, which formed the subject of my communication. This may appear somewhat disingenuous; but I have no doubt Dr. Greenhalgh had good reason for the omission.

If, as Dr. Marion Sims explains, "this instrument can only be used after an incision of the cervix, or dilatation of it by a sponge or sea-tangle tent," it is evident that no prudent practitioner would think of employing it in an ordinary case of uterine flexion. I believe that the hysterotome is now-a-days employed very indiscriminately; but it is hardly conceivable that anyone would be so foolhardy as deliberately to slit up the neck of a flexed uterus and brave the attendant risks for the mere purpose of introducing an instrument of unproved efficacy. The "numerous medical men in public and private practice in this country and on the Continent" to whom Dr. Greenhalgh refers, have, I presume, merely adopted his little device as a means of keeping patulous the cervix uteri after division. This is the only purpose described by any of the authorities he quotes, there being no mention of its ever having been employed in a case of flexion of the uterus. The instrument of Dr. Aveling is adapted for the same cases as that of Dr. Greenhalgh. There is really nothing new in the use of a spring. I believe it was Kiwisch who first employed it for dilatation of the cervix.

I never saw Dr. Greenhalgh's instrument before the publication of his letter in THE LANCET; being satisfied, by Dr. Marion Sims's description, that it differed in construction, in principle, and in purpose from my intra-uterine pessary for flexions of the uterus. An examination of the instrument has confirmed this opinion.

The spring pessary described by me was actually devised for the relief of a chronic retroflexion, where the suffering had been of long continuance, and every known means had been previously resorted to. I carefully searched through all the records, but found nothing to help me. I personally investigated the most recent instruments in the stores of three eminent makers—Maw, Coxeter, and Savigny. Failing to find what I required, I devised the instrument described in my paper in accordance with the considerations therein detailed. The lady wore the instrument for two several periods, the first time for six, the second for nine days, without experiencing any inconvenience. She is now three months advanced in pregnancy, and may therefore be considered as cured. Her previous suffering from retroflexion and its consequences had continued for four years and a half.

I then availed myself of an opportunity to submit the instrument to the first living authority on such matters, the present learned President of the Obstetrical Society. Dr. Barnes failed to recognise any resemblance between my instrument and those previously devised for relief of uterine flexions, and advised my bringing it under the notice of the profession. "Having mentioned these few facts" (I quote Dr. Greenhalgh's own words), "I leave your readers to draw their own inference."

Yours obediently,

Somerset-street, Portman-square,

March, 1866.

HENRY G. WRIGHT.

To the Editor of THE LANCET.

SIR,—It is certain that the "expanding stem" of Dr. Greenhalgh, the "spring pessary" of Dr. Wright, and my "spring