far from being either courteous or fair; and however satisfactorily your correspondent might have authenticated his communication, I am not at all surprised that he did not dare give his signature to so many mis-statements.

As regards any other case you may allude to, Sir, I am very happy to have an opportunity of informing you and your numerous readers, that I have in store a numerous collection yet unpublished, some of them replete with interest and instruction; these I will lay before the medical public, with such remarks and conclusions as may be profitable to science and humanity. To terminate, I will observe that I have nothing to divulge that is not,—or, at all events, may not,—be known, as I act invariably in the presence of, or conjointly with, professional gentlemen. As regards the full and scientific description of the details of my practice, I wish to embody with them much that may throw very necessary light upon a most important and deeply-interesting subject.

I consider it incumbent on me to reply to what has been said, but had preferred saying nothing, in pursuance of my practice and determination, and as I intend doing for the future, when there are so few claims to attention as in the present instance, where there is too much that is personal, to be considered scientific. I have the honour to be, Sir, your obedient servant,

BARON HEURTELoup.

4, Queen Ann-st., Cavendish-sq.,
June 13, 1836.

DR. INGLIS’S CASE OF DOUBLE FRACTURE, CURED WITHOUT SPLINTS.

To the Editor of The Lancet.

Sir: My attention, as you anticipated, has been attracted by a letter which you inserted in The Lancet of May 28th,—though, had it not been for the note which you kindly adjoined, I should not have noticed the communication, inasmuch as it is anonymous. I now beg to say, in reply to your remark, that the language in my case-book (from which I extracted the report) is as follows: “I found that the left femur was fractured in two places, one near its lower third, and the other just below the greater trochanter, at which place several splinters of bone could be distinctly felt.” This was on the 1st of March 1835. Then, on the 9th of March the report runs thus: “Still improving; little or no pain. The lower fracture appears to have united; there is a distinct thickening over the site of it. On examining the upper fracture, the splintered pieces of bone are felt rubbing against each other with the usual grating noise.” The interpretation of which is, according to the anonymous critic, rendered thus: “No restorative action had commenced!” “Yet,” he adds, “on the twelfth day we are informed that both fractures were united.” Now, is such an induction fair? Does my language warrant it? But besides, who would suppose that nine days could transpire, everything going on so well, without restorative action commencing, and that in the next two days this action would be completed? Certainly no one but the writer of that letter himself.

The critic concludes his short stricture by saying, “This strikes me as a little unusual; at least it is so in Dublin.” And such cures must be “unusual” in Dublin, and in every place else, so long as it is usual to treat fractures by the old mode of splinting and tight bandaging. Cures I have no doubt will continue to be protracted for the space of three or four months which might be effected in three or four weeks by the adoption of that line of practice which I shall ever feel it my duty to advocate. In one respect (but in one only) the anonymous M.R.C.S.I. reminds me of an illustrious surgeon, who in one of his discourses decries most vehemently those “narratives and drawings which stagger all belief, and are too wonderful to have any relation to practice, whilst he himself has helped to swell the catalogue of such a repository. I shall be glad, however, to hear of an Irish revolution in this department of surgery.

I am happy to add that my patient continues to move and run about without even a limp. In fact he feels as little inconvenience from the recent fracture, as if it had never occurred. I remain, Sir, your obedient servant,

JAMES INGLIS, M.D.

Castle Douglas, June 6, 1836.

OVARIAN DROPSY.

To the Editor of The Lancet.

Sir: I send you the annexed brief statement of an interesting case, which I shall feel obliged if you will insert in the next number of The Lancet.

I tapped a married lady yesterday, etat. 49, for ovarian dropsy, which has been accumulating for six years, and drew off twenty quarts of thick chocolate-coloured fluid, weighing altogether exactly fifty pounds avoirdupois weight, the whole having been contained in one single cyst. The subject, previously to the appearance of the disease, was tall and thin, and of spare habit, and had had one abortion of six weeks date. The ribs are extended in a remarkable degree, and there is slight lateral spinal
HYDRIODATE OF POTASH IN ACUTE RHEUMATISM.

To the Editor of The Lancet.

Sir: I have just perused the report of cases occurring in the Westminster Hospital inserted at page 381 of the last Lancet, and must confess that I feel a little astonished at the treatment adopted in them, and consider the rapid recovery under such treatment little less than miraculous. One or two of the cases, at least, appear to me to have been cases of acute rheumatism, arising from exposure to cold, seeming to have yielded to the magic influence of the hydriodate of potassa. I certainly have seen this medicine given in this disease; but it was in the chronic form, attended with thickening of the joints, unaccompanied by any acute inflammatory symptoms, which, it must be well known to the medical profession, sometimes supervene on the chronic form, from any exciting cause. To me it appears that these were fit cases for the use of the lancet, with other rigid antiphlogistic treatment. The first case narrated is that of Ann Wheeler, admitted May 24th. The report says, "She has great pain in all the joints; tongue loaded; pulse 120, hard, and full; skin hot and dry; face flushed; pain in the right side, with difficulty of breathing;" loss of appetite and excessive thirst. In this case, although symptoms of strong inflammatory action were present, requiring the antiphlogistic treatment I have above mentioned (as, I should suppose, the most proper), the hydriodate of potassa is given, and in the short space of ten days the patient perfectly recovers. I must therefore beg the favour that Dr. Roe, or the reporter of the cases, will mention the modus operandi of the medicine in these instances, as I have always been taught to consider iodine and its compounds as belonging to that class of medicines which are called "stimulants." The other ingredients of the mixture are only of secondary importance. My name in confidence I enclose. I am, Sir, your obedient servant,

A. S.

Kennington, June 13, 1836.

EXPERIMENTS ON LIVING ANIMALS.

To the Editor of The Lancet.

Sir: Having been, from a regular perusal of your Journal, led to form a high idea of your character for general humanity, I venture, with a full degree of confidence, to call your attention, and that of your very numerous readers, to the cruel, and as I firmly believe it to be, the indefensible practice of performing experiments on living animals. I am quite aware that I shall be met by the argument of possible benefit to the nobler animal man, and perhaps the adage once applied to criminals may be adduced, "fiat experimentum in corpore vile." But I wish to deny that any link in the chain of creation is "vile," or that we are at liberty to inflict agonizing torments on the inferior tribes, for some hypothetical benefit to ourselves; and to assert roundly that, if we can only be cured by attempts at discovery through the torments of animals, we have no more right, a priori, to the adoption of such means, than we have to seek health by torturing our fellow men.

The particular instance which has called forth this appeal,—the poisoning of a cat by phosphorus, as mentioned in your number of Saturday, May 27,—does not include one of the more revolting circumstances attending such cases. But it revives the recollection of many such. And to you, as the advocate of the nobler qualities of the medical art, and to your conscientious readers, I submit the plain question, whether it be not a high offence to the great Creator, and a flagrant outrage against nature, to seek any kind of knowledge through such crooked paths. Nature, and common sense and feeling, dictate to us with tolerable certainty what are the legitimate uses of the animal creation. Certainly they lift up their voices against such maltreatment; and not the least part of the evil is, I think, to be found in the inevitable hardening of the heart of the experimentalist in such cases. For there can hardly be the redeeming influence which exists in performing operations on the human frame, where painful necessity offers the alleviation of duty, as I think no one could ever have performed the former experiments without at least a doubt that he was doing a very unjustifiable act.

These brief considerations will, I hope, be followed up by the conscientious feelings of your readers. I may simply remark, in conclusion, that, even if the certainty of available discovery could justify oppressive cruelty, which I again beg to deny in the strongest terms, there was no such plea in the present instance. It bears the impress of wanton, and, therefore, decidedly cruel curiosity. There was nothing tempting in