but the attempt *has* been attended with signal success. With respect to the second part of the statement, I am surprised to find such an assertion coming from one who has paid the attention Mr. Syme has to diseases and operations on the various joints. I think if Mr. Syme were to call to mind the cases of disease of the hip which I should suppose he has examined after death, and if he were to take the trouble to examine the morbid specimens of this disease which are preserved in the museums, he would find that his assertion, that "caries of this joint *never* exists without the bones of the pelvis being equally involved," is quite erroneous. It is at variance with what has been observed by pathologists and practical surgeons, and its incorrectness is amply proved by the results of the very operation Mr. Syme so much condemns.

In the first case which was performed in this country, by Mr. Anthony White, in 1818, the head of the femur was alone diseased, as was proved by the complete recovery of the patient, and as is also beautifully illustrated by the specimen of the parts now in the Hunterian Museum, where, it is seen, a strong and effectual new joint had been formed, which could not have been the case had the pelvic side of the joint been carious, and the diseased portions not removed.

In Mr. Fergusson's first case, also, the head of the bone was alone carious: this was clearly ascertained by that gentleman at the operation, and was, moreover, proved by the "great fact" that the patient entirely recovered without any carious portion of bone coming away through the wound, as I can myself testify to, from having had the dressing of the case in my own hands for several weeks; and here, as in Mr. White's case, neither gouge nor bone forceps were applied to the acetabulum. Moreover, if it could be established that in every case some disease of the pelvis exists, this fact alone would not be a valid objection to the operation; for if the disease is limited to certain portions, it may be got at and removed by proper instruments, in the same manner as carious portions of the scapula may be removed in the operation of excision of the head of the humerus; and I apprehend Mr. Syme would not object to perform this operation because the glenoid cavity should be affected; nor would he say, that in every case of disease of the shoulder both sides of the joint are equally involved.

I admit, Sir, that the circumstance of disease of the pelvis existing in many cases of morbus coxarius constitutes a grave difficulty in the operation; but no surgeon would think of performing it *where he knew* such to exist; and the mere difficulty of ascertaining it should not deter him from operating, if he has good reason to believe, through certain symptoms and the history of the case, that the head of the femur is *alone* chiefly affected.

The circumstance of disease of the kidneys accompanying stone in the bladder, and being difficult of detection, often proves a serious objection to lithotomy; but would Mr. Syme refuse to perform the operation altogether because such frequently exists, and not unfrequently proves a source of death after the operation? Again: would Mr. Syme object to tie the subclavian or femoral artery for aneurism, because patients who have been operated on have died, sooner or later, from the bursting of an aneurism in the abdomen or chest?

Further on Mr. Syme says: "If the disease admits of recovery, excision of the head of the thigh bone is superfluous and useless." True; but it is not in such cases that a good surgeon would either recommend or perform the operation; it is in cases where there is no hope of recovery by natural or remedial measures, and when the surgeon has good reason to suppose that he may by the proceeding prolong or save life, that he is justified in resorting to it.

Again: Mr. Syme remarks, "If the patient recovers after the head of the bone has been cut out, it is a distinct evidence of the uselessness of having excised it." I must confess Sir, I cannot understand this reasoning: it is most extraordinary and unfair. Had he witnessed the deplorable condition of the two patients in King's College Hospital prior to the operation, and then seen them some months afterwards, he would, I am sure, not have used such an argument, which might be employed by any one wishing to decry some particular line of practice.

Mr. Syme ends his philippic in the following manner: "Common sense and their unsuccessful results will no doubt ultimately show the impropriety of such operations." Here, again, this gentleman is unfair. He speaks of the "unsuccessful results" as though the operation had never been attended with success; but he surely must know that it has been successful in several instances, both in this country and abroad,—if not, I would refer him to a paper which he will

find in *THE LANCET* for March and April of the past year. I have, in the compilation of that paper, been at great pains in collecting accounts of all the cases of the operation, but as I have not met with one performed by Mr. Syme, I infer that he has not attempted this operation, and am therefore less surprised at the manner in which he so strongly protests against it.

I am sure, Sir, that all of us who are devoted to our profession must acknowledge the great benefits which the practice and researches of Mr. Syme have conferred upon the science of surgery, and I hope that he will still go on adding to it; but this good work will not be promoted by the hasty rejection of the experience of others equally gifted with himself, or by raising unreasonable objections against any particular line of practice they may put in force.—I am, Sir, yours obediently,

HENRY SMITH.

Caroline-st., Bedford-square.

USE OF COLLODION IN RESTRAINING HÆMORRHAGE FROM LEECH BITES.

To the Editor of *THE LANCET*.

SIR,—I wrote to you yesterday, recommending collodion as an application useful in restraining hæmorrhage from leech bites; but I had not then seen the letter of Mr. Tucker, on the same subject, in *THE LANCET* of December 9th, as that number, through some irregularity, did not reach me till last night. I mention this, because, if you insert my note of yesterday, Mr. Tucker may think it odd that I took no notice of his communication. It appears that the idea of using collodion for the purpose referred to occurred to Mr. Tucker and myself at about the same time.

On the 1st of December, in the morning, I ordered leeches to a gentleman's leg, and as five of the bites continued to bleed during the whole day, I determined on trying the collodion. Not having any lint with me, I dipped some of the fur of a beaver hat in the fluid, and after applying it, and touching it two or three times at different points, where there was a little oozing, by means of a camel's-hair brush moistened with the collodion, the hæmorrhage entirely ceased.

In the hope that you will excuse my thus trespassing on your space, I am, Sir, your obedient servant,

Seething-lane, Dec. 28, 1848.

ROBERT TRACY WYLDE.

PHYSIOLOGY OF THE NERVOUS SYSTEM— "WALKER VERSUS BELL."

To the Editor of *THE LANCET*.

SIR,—The labours and speculations of Sir Charles Bell, that so remarkably yielded him success, and honours which he seemed to bear "unblushingly," are now threatened with a scrutiny more searching than may be agreeable to many of the worshippers of the "mighty dead." Still, however, the halo of the tomb ought not to be allowed to darken the lamp of virtue on the brow of a living though less fortunate and exalted brother. The profession, as a body, should endeavour that not only Mr. A. Walker, but all true philosophers, may have their labours and merits timely appreciated and rewarded. It is not only painful, but dispiriting in the extreme, to contemplate the fate of Mr. A. Walker in regard to this matter, and it is the bounden duty of the profession to grant a full inquiry and hearing to the subject, that it may without delay be set at rest, and "honour be given to whom honour is due," ere it be too late—although this must be but an inadequate recompence for prolonged neglect and injury. I believe that the profession is at present in a mood more favourable for cool and impartial inquiry on this particular matter than it had ever been previously, while dazzled by the brilliancy and success of Sir Charles Bell's career, and when, perhaps, dunned by his dogged and indefatigable perseverance in keeping his views before the public; so that his more retiring rival had but little chance of being heard.

It has always struck me, in perusing Bell's writings on the nervous system, and particularly those in reference to the relative functions of the two roots of the spinal nerves, that his theories depended much more upon his special reasoning than his experiments; many of which experiments, and all those of Magendie, Müller, Kronenberg, &c., went rather to the support of Walker's views, in referring sensation to the anterior, and motion to the posterior roots, respectively, though indeed both roots were found capable of eliciting phenomena, both sensory and motory. Moreover, now-a-days, we are led to refer to comparative anatomy as an infallible guide in such doubtful points; and no doubt here, if we seek for division of