neck of the impregnated and gravid uterus, and on the body of the uterus, continuous with the sympathetic and spinal nerves. These structures have been examined by many of the most distinguished anatomists throughout Europe, and have been declared to be true nervous structures, and some of the best microscopical observers have arrived at the same conclusion.

There are, however, still a few anatomists who maintain that these structures are not nervous; that the uterus has no ganglia, and only a few small filaments of nerves, which undergo no change during pregnancy. They affirm, in fact, as Rehsends did, respecting the heart, that the uterus is an insensible organ; and, unlike all other muscular organs, that it is almost wholly destitute of nerves.

The only individual who has formed such an opinion, after dissecting the nerves of the uterus, is Mr. Snow Beck; and the Royal Medal in Physiology for 1848 having been awarded to him, on the strength of a report to the Physiological Committee, by Drs. Sharpey and Todd, which is now proved to have been a false report, a certain adventitious importance has been given to these two dissections, which they little merit, and could never have acquired by any other means. It is impossible to deny that these dissections are altogether worthless, and that no physiological inference whatever can be drawn from them. The bloodvessels and all their accompanying nerves have been cut away, and the ganglia at the cervix uteri torn into shreds. In the plates, the gravid uterus is actually represented with a smaller supply of nerves than in the unimpregnated state. What would be the value of a representation of the nerves of the heart, if the coronary arteries, and all their accompanying nerves, were cut away in the dissection, and not displayed? What would be the value of a map of India, without the Ganges and the Indus; of Egypt, without the Nile; of Germany, without the Danube and the Rhine? Such is the estimation in which the dissections of Mr. Beck should be held by all physiologists, pathologists, and practical accoucheurs—as not only worthless, but mischievous.—I am, Sir, your obedient servant,

London, July, 1847.

SENEX.

ON DEFORMITY OF THE CHEST IN CHILDREN.

To the Editor of THE LANCET.

SIR,—I feel it my duty to reply to a very grave, though totally unfounded, charge made against me by Dr. G. A. Rees in the last number of *THE LANCET*. He says that a paper read by me to the Westminster Medical Society in 1841 contained exact quotations from a paper of his in the *Medical Gazette*, but without any mention of the source whence the quotations were derived. The very reverse of all this is true. I referred to his paper in the most handsome manner, stating his name, and the date and place of it, but made no quotations from it, as any one may ascertain. My paper was published in the *Medical Gazette* of April 9th, 1841. The title of it was, "On Distortions of the Chest and Spine in Children, from Enlargement of the Abdomen." Dr. Rees' communication was entitled, "On Deformity of the Chest in Young Children, from Disease of the Lungs." How any extract from his paper could have served my purpose it is not easy to conceive. In his former complaint to which he alludes, he did not accuse me of making exact quotations, but chiefly confined himself to endeavouring to show that my cases were of the same nature as his own, and that they did not depend on enlargement of the abdomen. He did, indeed, make an insinuation, for which he ought to have apologized after my reply, which appeared in the following number of *THE LANCET*—that of April 10th, 1841, at page 112. As Dr. Rees has withdrawn his opinion, that the change which he met with in the lungs was the result of inflammation, and as he now admits that there is usually abdominal enlargement, I am inclined to believe that his cases are of the same nature as those which I described. In the case detailed in my paper, the greater part of the lungs was collapsed, and void of air. I offered no explanation then of the cause of this, but I now consider that the deformity in question, after it attains a certain degree, may cause compression of the lungs, and have the same effect on them as a pleuritic effusion would have, and thus terminate life. In my opinion, then, the condition of the lungs, which Dr. Rees considers to be the cause of the deformity, is an occasional consequence of it. When deformity is the result of disease of the lungs, it does not affect both sides of the chest alike.

On the late occasion I alluded again, in the Society, to Dr. Rees' paper, and stated his opinions, previous to mentioning my own paper, although if I had not done so, but had included

him amongst authors in this country in the abbreviated language of the report, and had so passed from Dupuytren to my paper in the Society, on which I was speaking, he would have had no ground for complaint. As to my stating who really did first point out these cases, Mr. Hird had already done that in mentioning Dupuytren, whose cases, now that Dr. Rees has just alluded to them for the first time, he tries to show differ from his own, though to me that is not apparent.

The letter I am now answering commences by a statement, that Mr. Hird's paper was a transcript, to some extent, of one by Dr. Rees. I feel assured that Mr. Hird knew nothing of that paper till I alluded to it. Dr. Rees, indeed, says, a transcript, "most probably without the author being aware of it." Now, it so happens that Mr. Hird wrote nothing, and read nothing, but only related a case or two, and made some remarks on them. But supposing that he had really written and read a paper, as he said that he had intended, and that, in some peculiar state of unconsciousness, he had made a "transcript to some extent" of Dr. Rees' paper, how could that gentleman be aware of such transcription, as the supposed paper has not been published, but only, as he says himself, "mentioned"?

I shall be much obliged by the early insertion of this reply; and I remain, Sir, your obedient servant,

Frith-street, Soho-square, July, 1847.

JOHN SNOW.

To the Editor of THE LANCET.

SIR,—In the last week's number of your journal, I observe, as a sort of preface to some remarks upon a peculiar deformity of the chest occurring in children, by Dr. G. A. Rees, a statement, charging me with reading a paper at the Westminster Medical Society, in which I made a transcript to some extent, probably without my knowledge, of a paper published by Dr. Rees in the *Medical Gazette* in 1839.

Allow me to state, that until this day, when my attention was drawn to the subject, I have never read any contribution to medicine by Dr. G. A. Rees.

I never read a paper before the Westminster Medical Society upon the subject alluded to by Dr. Rees, and consequently could not make a transcript from any paper the production of that gentleman.

My communication to the Society was a verbal relation of some cases that had occurred both in my practice at the Royal Infirmary for Children, and in my private practice, illustrative of the advantages to be derived from the treatment recommended by Dupuytren, and published in his "Leçons Orales," in 1828, eleven years prior to the time Dr. Rees states his paper to have been written.

I trust you will do me the favour to insert this explanation.—I have the honour to be, Sir, your obedient servant,

Cleveland-row, St. James's, July, 1847.

FRANCIS HIRD.

A LEARNED CHIROPODIST.

To the Editor of THE LANCET.

SIR,—the enclosed appeared in your Paper *THE LANCET* a fortnight ago which is intended to do me harm and injure me professionally without any just cause on your part—I beg to draw your attention to some of your remarks. the first you Term me a quack—without having the slightest knowledge of me in any way whatever—you then say he Advertises certificates real or false—from several medical men some of their names you have misspelt which will tend to hurt me as many will say there is no such medical men in Leicester.

You then rank me with such men as a Mr. Woolf—I assure you sir I unfortunately have had a great deal to put up with from the public through such fellows—whose conduct I am not accountable for—I beg to inform you that my practise is strictly honourable and I visit the same Towns two or three times over—and advertise my Fees in the public newspapers—which I think none of the other chiropodists do, and know my profession so that the medical men are always glad to encourage the true practioner from the humbug—in fact there is no other chiropadist holds such Testimonials of cures as myself—though I have nothing to do with any of them, I do all in my power to oblige all persons on whom I have the honour to wait, and think it very hard that I should have been delt with so unfairly I think there must have been some mistake I have spoken to several of my medical friends and they all think the same oppinion—I should suppose it is not you desire to Injure me, in any way as I have given not the slightest cause—I beg to submit my