tion was gone, though the sensation had not entirely disappeared; that the sensation on the right side and extremities was partially affected: thus my statement of the dangerous effects of intercepting this source of the circulation through the brain will be fully confirmed. That the vertebral arteries, and the opposite internal carotid, are not sufficient for the purposes of the animal economy in the brain, will be further elucidated by the operation performed by Mr. Key in January, 1824, which reflects great credit on his dexterity as a surgeon. Though it was performed in the best possible manner, yet a numbness took place in the side, and continued till his death, which happened in about a week. Now if the vertebral arteries could have carried on the circulation effectually, the operation having been performed in both these cases by the most skilful and scientific method, death would not have ensued. I am inclined to think Mr. Key views this subject in the same light as I do, as lately another patient with an aneurism in the carotid artery applied to him, and he wisely declined so hazardous an undertaking. This operation has likewise been performed by Mr. Coates of Salisbury, and Dr. Duffin, in both which cases it was unsuccessful, and death was the result. Under a due sense of all these considerations, my judgment strongly militates against it. Where we have reason to fear the tumour will soon break, it may be deemed advisable, for the chance of the prolongation of the life of the patient, to perform it. Wretched as the alternative is, I fear it will be useless, and after all we can do,

χειρουργὸς ἰατρὰι δάνατος.

I offer these observations to the notice and consideration of the profession, in order to impress on their minds, before they consent to operate, to take sufficiently into account the peril to which the patient is exposed, as weighed against the chance of benefit.

Howland Street, Aug. 10, 1829.

WEISS'S URETHRAL FORCEPS.*

To the Editor of THE LANCET.

SIR,—It was with some degree of surprise that I found, on reading your LANCET of the 1st instant, that you should suffer so much umbug to intrude in the pages of your

* This instrument is an admirable invention, and might have been used with the most perfect success in many cases which we could name, where the patients were unnecessarily submitted to the excruciating operation by the knife.—ED. L.

valuable journal. I allude to the following, page :—

“In case the stone, or a fragment of it, should escape from the claws of the ‘*pince*,’ the fruitful imagination of the Baron has supplied a remedy; the ‘*mandrin*’ is withdrawn, and a very delicate instrument, consisting of a canula, a steel rod, and three very fine elastic tenaculæ are introduced, the substance is seized and replaced within the jaws of the larger instrument, and the process of its destruction is resumed.”

You have omitted to mention the little bird, or “*oiseau*,” of twenty-five pieces, which whispers in the Baron’s ear, that a fragment had fallen out of the large instrument’s jaws, and it is as readily picked up as if the stone lay upon a dessert plate, which every body in the room could see. It appears that they have quite forgotten the possibility (which the success of Sir A. Cooper and Mr. Brodie has fully shown) of extracting calculi larger than a hazel nut by my instrument, the urethral forceps, and with which Sir A. Cooper extracted eighty-four calculi from one gentleman, which, I think, it would puzzle the Baron to do with an instrument even of eighty-four pieces.

I am, Sir,

Your obedient servant,

JOHN WEISS.

Strand, Aug. 8, 1829.

FORMATION OF THE FLAT BONES.

To the Editor of THE LANCET.

SIR,—Allow me to call your attention to a subject which may be interesting to some scientific men, particularly to physiologists; and which, I presume, goes to disprove the apparent correctness of our present ideas on the subject of osteogeny, which have led us for ages past to suppose that the osseous deposition in the flat bones, particularly the cranial, begins from a central point, proceeding thence in radii.

I seem to have been the first who has been able to demonstrate the process in the stage *previous* to that in which the fibres take on the straight line. In the preparation which I have, it may be seen decidedly and beautifully reticulated.

If you, Sir, can refer me to any English author, who has described or mentioned this peculiarity, previous to the last six or seven years, I shall feel much obliged by the communication.

Any professional gentleman wishing to satisfy himself by ocular proof, may see the bones at my residence.

Your humble servant,

JOHN M. DRAPER.

1, Little Hermitage Street, near the London Docks, Aug. 3, 1829.