contents, and the efforts used to force them out, will in all probability cause inflammation and suppuration of the sac. None of the patients on whom I have operated ever complained of pain from the introduction and movements of the needle; one gentleman indeed even spoke of the latter as causing rather a pleasing sensation.

It has occurred to me, that a similar mode of operating might be applied to hydrocele; and that a cure of that disease might be accomplished by opening a communication, by means of the cataract needle, between the cavity of the tunica vaginalis and the cellular tissue of the scrotum. No suitable opportunity has presented itself of putting this idea to the test of experiment; but the trial is one which, in the hands of a cautious surgeon, would in all probability effect at least a temporary cure, and which could not be productive of any injurious or unpleasant consequences.

Glasgow, 9, *Gordon Street*,
June 8th, 1825.

XIX.

Experiment to determine whether the Blood in the Veins is moved by a Suction power in the Heart. By Thomas Paterson, Member of the Royal College of Surgeons, London.

The femoral artery, vein, and nerve of a rabbit, were separated, for about two inches from their connexions, a little below the groin; the limb was then cut through, leaving these vessels attaching the leg to the body. The vein was punctured, and the blood flowed freely. A ligature being passed above the puncture, and slightly drawn, the flow of blood increased; and when pulled tight, to prevent communication with the heart, it sprung out in a full jet. When the finger was applied to the orifice for a little, and then withdrawn, the blood spouted out with great force; this was frequently repeated, till the animal became exhausted.

I made this experiment, to ascertain if the circulation in the veins is solicited by a suction power of the heart. It does not prove the contrary; but it certainly demonstrates, that the blood can flow along the veins, independent of any suction, and with a force apparently sufficiently strong. Indeed the anatomical

structure of the right auricle of the heart denies to it any suction power. Some, I am aware, maintain, that the suction is in the ventricle, and that, at its diastole, it produces a kind of motion in the blood of the *cavas*, which is retained, or rather restrained, during the contraction of the auricle, and fills it when emptied. The known laws of fluids refute this opinion. But if the blood circulates in the veins, with such a force as we have seen, independent of any suction in the chest; why look for more causes than one? Is the animal economy not already sufficiently mysterious? Such theories show ingenuity, while they betray ignorance.

I may here notice a statement made by Dr Carson in your Journal for April 1824, in attempting to obviate objections to the suction of the heart, advanced by Dr Philip. "If water (says Dr Carson) is to be raised through the same medium, that is through water, then an yielding vessel is sufficient; for the pressure on the internal surface of the tube is equal to that upon its external surface. The pumping of water through the tube, could not in this case cause a collapse in the sides of the vessels, if these vessels were once dilated by the fluid in which they were immersed. But as the animal fabric may, as far as regards the point in discussion, be considered a vessel filled with blood, the veins are immersed in a fluid of the same specific gravity with that which passes through them, and therefore cannot have their sides drawn together by suction originating in the chest." Now, had Dr Carson tied a piece of the inferior *cava*, to the mouth of a syringe, and, after filling it with water and immersing it in the same fluid, pulled up the piston, it would have demonstrated to him, what the laws of hydraulics render evident to any other person, viz. the error of the above statement, when applied to the *suction* or drawing of fluids through yielding tubes. Wo to man if his circulation depended on the suction of the heart, or resilience of the lungs!

I have said nothing as to the cause of the venous circulation; but it may form the subject for some future experiments.

Glasgow, 4th March, 1825.