pearances which have been described would not have been observed.

4. It would be interesting to make a series of observations on rabbits and other animals, with a view of determining the circumstances which favour or oppose the erosion of the stomach by the gastric juice. The observations made by Dr W. Philip, in the third edition of his singularly admirable work on the Vital Functions, pp. 131, 132, appear to be too general on this point.

5. We might possibly employ the gastric juice in the minute dissection of the nerves, since this texture appears to resist the action of this agent, whilst that of the other parts is destroyed by it. The fact itself is mentioned by M. Cruveilhier, in his *Medecine Pratique*, Cahier i. p. 143.

ART. VI.—*Description of Apparatus and Experiments for determining the composition of the Blood in health and disease.* By W. REID CLANNY, M. D. Sunderland.

IN my lecture * upon typhus fever, which was published about sixteen months ago, I necessarily confined myself to the mere outlines, in order that I might not detain the audience more than an hour; and in printing this lecture, I neither added nor subtracted from what was delivered.

I now proceed, for the satisfaction of the profession, to give a brief sketch of the plan which I adopted in these investigations.

I ordered a twenty ounce graduated air-tight flask to be made, to which a stop-cock was screwed, and having attached the flask to the aperture in the plate of the air-pump, by means of a brass tube, I exhausted the atmospheric air from the flask by the air-pump. Having removed the exhausted flask from the air-pump, I attached to it above the stop-cock a tube about the diameter of a swan's quill, bent at a very obtuse angle, and having a ball blown upon the angle.

When the blood flowed from the vein, I held the glass tube as near as I could to the bleeding vein, but not touching it, and kept the flask in a suitable position till the glass bulb was filled. I now opened the stop-cock, and the blood rushed into the empty flask from the glass bulb. In this way I so managed the stop-cock, that, as the blood continued to flow into the glass bulb, it was permitted to rush into the exhausted flask, till I had taken a suitable quantity of blood *in vacuo*.

After some practice, I was enabled to manage the stop-cock,

* For a notice of the contents of Dr Clanny's Lecture, see the Medical Intelligence of this Journal, xxx. 221.

so that I could take whatever quantity of blood I required with the greatest facility.

The next step was to shut the stop-cock, and remove the glass bulb from the flask, and attach to it a well constructed apparatus for drawing whatever gas might be contained in the blood through a graduated flask of lime-water, placed in the exhausted receiver of the air-pump. In this manner the carbonic acid of the blood coming into union with the lime of the lime-water, and forming carbonate of lime, the quantity of carbonic acid in the blood was very readily and very accurately ascertained. I was particularly cautious, by a proper arrangement of valves, that no lime-water found its way into the flask containing the blood, which otherwise will always be the case when the atmospheric air is permitted, (after this part of the experiment is finished,) to pass into the receiver of the air-pump.

It is necessary to mention that the flask, previous to its being used, was weighed; and after the carbonic acid was removed from the blood by the above-mentioned apparatus, the flask, with its contents, was again weighed.

The next process was to set the flask containing the blood upon its side, and after it had stood in that position for two or three hours, I poured off the whole of the serum in the most careful manner. The serum was coagulated at a well regulated temperature, and being cut into small pieces, was placed upon a perforated Wedgewood funnel, and the serosity drained off; besides which, the coagulated albumen was washed most carefully with warm water, and the washings added to the serosity.

The coagulated albumen was weighed. The serosity and the washings of the coagulated albumen were placed in a Wedgewood capsule and evaporated, and the salts which were left were collected and weighed. The crassamentum from which the serum was poured was weighed; and the fibrin separated from the colouring matter was collected in a fine linen bag, through which a current of distilled water was passed. The fibrin was pressed for some time in a press of my own construction; and when all the water was pressed out, and the moisture removed from its surface, it was weighed.

The solution of the colouring matter which passed through the linen bag was evaporated, and the colouring matter weighed.

I adopted the plan of receiving blood *in vacuo*, in order that the oxygen of the atmosphere might have no chance of uniting with the carbon of the blood in its transit from the vein into such vessels as are usually had recourse to for receiving blood; and by way of putting this plan to the proof, I constructed a

graduated jar sufficiently large to hold a suitable portion of warm distilled water, into which the hand of a man with an opened vein could be most conveniently held till fifteen or twenty ounces of blood were taken. To this jar a well-ground plate and stop-cock were attached after the blood was taken, the space which was left by removing the hand being supplied by hollow metal balls. The blood always keeps to the bottom of the water, and not one particle comes up to the surface.

This jar containing the blood and warm water was attached to the apparatus above-mentioned, and placed in the exhausted receiver of the air-pump. I found the same results in both cases.

When I expected to meet a difficulty in procuring blood, I requested the surgeon to secure the arm or wrist in the usual manner, and place himself by the shoulder of the patient; and instead of carrying the lancet into the vein, so as to form an acute angle with the current of the blood, he was desired to penetrate the vein with the point of the lancet towards the hand, as by this plan the blood flows at an obtuse, not an acute angle, to its current towards the heart. This plan suggested itself to me in cases when the veins were peculiarly small, deep-seated, or imbedded in fat, and I have reason to be perfectly satisfied from ample experience, that this is the best plan, even in ordinary cases.

I have always found the greatest difficulty in heating fibrin of the blood, so as to obtain uniform results; for fibrin may be gradually heated till it lose one-half of its weight, though its general appearance and qualities remain the same. On this account I constructed my fibrin press, which I consider indispensable for comparative trials in cases of sound or unsound blood. I may remark in passing, that I could perceive no difference in the time required for coagulation of blood, whether it was taken in the usual manner, in vacuo, or in water. At the same time, I acknowledge that my attention was only slightly directed to this phenomenon; nor did I ever make any direct experiments, so as to be able to form a correct opinion upon this question.

In this manner I have followed up my inquiries, and feel perfectly satisfied, from the great attention bestowed, and the innumerable experiments performed, that I could not have adopted a better method of investigation, for the purpose of ascertaining those important changes which, of a surety, the blood undergoes in many diseases. I confidently recommend this plan to the attention of my professional brethren. All my experiments have been as open as the nature of the subject would permit, and all the apparatus which I have employed in my investigations has been placed in our infirmary for the last two

years, not only for convenience, but also, as intimated in the preface to my printed lecture, for the inspection of the faculty and other men of science.

In conclusion, I feel confident, from the additional experience of sixteen months, that my view of the proximate cause of typhus fever is perfectly correct, and I request that the subject may be entered upon by persons qualified for the task.

ART. VII.—*Case of Spina Bifida.* By WILLIAM LAW, Esq. V
Fellow of the Royal College of Surgeons, Edinburgh.

[Read before the Edinburgh Medico-Chirurgical Society.]

I HAD occasion to treat a case of this disease some years ago, along with another surgeon, but with such unpromising aspects almost from the first, that any detail of its complete failure could hardly be read with interest. When the capsule at the part was emptied each time, there was an evident draining of the fluid previously prominent at the fontanel, flaccidity taking place at the distended sutures of the cranium.

The case, however, I propose now to describe was so near a successful termination, and was reversed so very unexpectedly, undergoing its changes in an unusually short period, that I consider it well worth attention. The child was a male, rather more than two months old when I first saw it; and the disease, of a hemispherical form, two inches in diameter, was in the usual situation on the lower part of the lumbar region. This was about the 20th of September; and a few days after when the mother agreed to have it punctured, I proceeded to do so with a lancet. The instrument was pushed into the most prominent part near the middle, and on this occasion seemed to enter a membranous capsule, distinct from the outer skin; the swelling was more than half emptied of a thin watery fluid mixed with blood, which appeared to flow from the divided edges. A little patch of adhesive plaster was put on the opening, and a fold of linen and flannel with a piece of pasteboard, quilted with tow in a circular form, bound over the part by a roller. The child did not seem much affected by what was done. Three days after, the pasteboard being removed, the swelling was found of its original size, when the water was again drawn off at a new opening, and the pasteboard applied as before. In three or four days it was again done, but I changed the pasteboard for a piece of circular cork, which having a slight projecting rim on the edge of the surface applied to the tumour, and a strap of adhesive plaster across, remained more steadily on the part. After the first occasion,