which lately came into my possession). It is those redeeming merits which homœopathy certainly possesses. I remain, Sir, your very obedient servant,

CHARLES SCHMIDT, M.D.
8, Dover-street, Piccadilly,
March 1, 1836.

DOUBLE VISION.

To the Editor of THE LANCET.

SIR: On reading the account of the case of double vision in your Number of the 27th ult., I have thought it might interest some of your readers, to detail what occurred to myself of a similar nature.

In August 1831, during intensely hot weather, I was travelling alone in the Malle Poste from Bordeaux to Paris (a journey which is not performed in less than sixty hours, including the long stoppages on the road), when, on the second day, I was rather surprised at perceiving the distant rows of trees in double lines, one *above* the other, which at first I thought was attributable to the badness of the glass of the windows, which were closed, but upon opening them I found to my surprise, that the cause existed in my own vision, and upon trying the effect of opening and closing each eye, I found clearly that both eyes were similarly affected, but the left much more so than the right. It, however, only seemed to attach to distant objects, for persons or cattle near the carriage I saw perfectly distinct as usual. This state continued until I reached Paris, when, thinking it might be occasioned by want of sleep (not being able to repose in a vehicle), I concluded that a day or two's rest would carry it off; but after that period, finding myself unable to look at any thing steadily, and that the left eye had in its white part a yellowish spot as big as a pea, whilst my bowels were much confined, and my head at the crown felt uneasy, and with a kind of strained feeling, I sent for a medical gentleman, whose name I forget, but who immediately wished me to be cupped. To this, having an unconquerable aversion to lose blood, I objected, but permitted him to physic me *ad libitum*, which he did, and by so doing, unloaded the bowels, and a little relieved the sight; but on my arrival in London a few days afterwards, I found all the symptoms return, and my sight so much affected, that I could not recognise a friend at a few yards distance. Fearing, however, that this evidenced a tendency to apoplexy, I now consented to be cupped, which gave me but temporary relief, and it was not until two months had elapsed, that the affection wore off, which it did gradually, and I have never had a return of it. My eyes are a bluish grey in colour, and my sight has always been remarkably good.

I was inclined to attribute the phenomenon to a redundancy of blood, occasioned by the luxuriant diet of the south of France, and to my taking very little exercise, whereas I have always at home been accustomed to much exercise, both equestrian and pedestrian, and nothing deranges me more than confinement in a carriage for any length of time. My living is always temperate. I am, Sir, your obedient servant,

A CONSTANT BUT NON-MEDICAL
READER.
Stoke Newington, March 3, 1836.

SYPHILIS.

To the Editor of THE LANCET.

SIR: My attention having been directed to some observations on syphilis, by Mr. Eagle, in *THE LANCET*, No. 651, I take the liberty of proposing the following questions to him, and to those who advocate the same opinions.

Mr. Eagle assumes, "that many effects imputed to venereal poison are produced by mercury." How is it, then, that of the tens of thousands who are daily taking mercury for other diseases, numbers are not similarly affected?

If the venereal poison be not absorbed, how can the system be contaminated?

What evidence has Mr. Eagle to prove that chancre will produce gonorrhœa, and gonorrhœa chancre? I am aware that the common answer is, that "Two individuals may have connection with the same woman, and the one have chancre and the other clap." But gonorrhœal and chancrous matter may be left in the vagina from previous intercourse. Indeed, I have known a healthy female communicate the disease, which could only have arisen from this cause.

With regard to mercurial rheumatism, I am not yet a convert to the doctrine of Hahnemann, viz., that a disease is produced and cured by the same medicine. Lastly, I have met with nothing to shake my faith in the mercurial treatment of syphilis, and, I may add, that the opinions of Messrs. Guthrie and Rose are in opposition to those of the majority of British practitioners. I am, Sir, yours respectfully,

INVESTIGATOR.

March 7, 1836.

PROPOSAL OF LIGATURES TO BLEEDING VESSELS.

To the Editor.—Sir: Knowing the interest which you take in all matters relating to our profession, I beg leave to communicate to you the following passage (occurring in a very curious and ancient work

completely takes away, I think, from Ambrose Paré the honour which is assigned to him by all surgical lecturers and authors, of having been the first who applied ligatures to bleeding vessels. The work is entitled "Gabelhover's Boocke of Physicke," and was first translated into English in the year 1559, being twenty years before Ambrose Paré's account was published. The following is the extract, taken from page 303, being the commencement of the fourth part:

"If then ther be anye vayne discided or heyyed a sunder, you must then knowve that both the endes of the vayne vvchich is discided doe dravve themselues vpvvards in the flesh, vvhereof you must indeavour, with little *instrumētes* fit for such a purpose, to take houlde of that ende of the vayne vvchich is shrunk up into the greater parte of the bodye, dravve it forth, and vvith a crooked needle, to sticke it, and soe to binde it," &c.

I trust you will not think this communication of too little importance for insertion in your valuable periodical, as the explosion of any error, even in the history of our profession, cannot be void of interest. I remain, Sir, your obedient servant,

JOHN MOORE NELIGAN.

5, South College-street, Edinburgh;
March 5, 1836.

CHEMICAL AND PHYSIOLOGICAL RESEARCHES

UPON THE

BLOOD OF THE VENA PORTÆ.

PROFESSOR SCHULTZ has communicated, in Rust's *Magazin für die Gesamte Heilkunde*, the results of some interesting chemical researches upon the blood of the vena portæ. In the first part of his memoir he publishes the results of his experiments upon the portal blood of horses, of which the following is a succinct account. He states that

1. The blood of the vena portæ is in general more dark than venal blood, though this difference is not always very distinguishable. It is not reddened by solutions of neutral salts, nor by the contact of atmospheric air, nor by the action of oxygen gas.

2. The portal blood does not coagulate, or if it do so at any time, it forms cakes, of which the texture is less firm than that of the blood of other veins. When the portal blood forms these cakes it liquifies, either wholly or partially, in about twenty-four hours, and gives, in a similar manner to the portal blood, which does not coagulate, a

black sediment, over which we find a volume of clear serum.

3. Generally speaking, portal blood contains 5.23 per cent in a fresh state, and 0.74 per cent in a dry state, more of fibrin than other venal blood, or arterial blood.

4. Liquid portal blood in general contains less solid particles than other venal blood or arterial blood, i.e. 0.18 to 0.3 per cent.

5. The serum of portal blood generally contains 1.58 less of solid particles, than that of arterial blood, and 0.80 less than that of other venal blood. The first, in a dry state, has an ash-gray colour, the second is yellow, and the third a yellow-green.

6. Portal blood contains proportionally more crassamentum and less albumen. The contrary obtains in arterial blood. The dry crassamentum of the vena portæ is gray and brown in colour. That of other veins is deep-red, and that of the arteries is bright-red.

7. Portal blood contains nearly double the quantity of oily matter that is contained in arterial blood, or blood of other veins, viz.,

In portal blood ..... 1.66.

In arterial blood ..... 0.92.

In blood of other veins ..... 0.83.

8. Dried serum of the vena portæ contains only 0.27 per cent more of oily matter than the dried serum of the arteries and other veins.

9. We find in albuminous crassamentum more albumen by 1.11 per cent than in arterial blood, and 1.21 per cent more than that of the other veins.

10. It is with respect to the quantity of fibrin that the greatest difference is found. Dried fibrin of the vena portæ 10.70 per cent of oily matter; that of the arteries 2.34 per cent, and thus upon the whole the difference is 8.36 per cent.

11. The oily matter of portal blood is brown, black, and unctuous; that of the arterial and of other venal blood is white, or yellow-white, and crystalline; that of chyle is white, two-thirds liquid, and one-third crystalline.

On perusing the paper which contains these statements, the absolute necessity for the adoption of some standard with respect to the fibrin is rendered very obvious. We are aware that after fibrin is dried in the usual manner, the process may be continued until it has become one half less in weight, although the external character shall not be changed. Were the fibrin pressed and dried with a towel, its comparative weight in different diseases would be accurately ascertained, and all we should have to do would be to establish a standard as to the quantity of the fibrin when collected from the blood in the usual manner, and the press and weight of lead employed should also be de-