the bacillus under an ordinary quarter-inch object glass, and with very ordinary London daylight as my only illuminator.

It is not very surprising to find that Dr. Gibbes has so poor an opinion of Ehrlich's process when one reads his description of it on page 736 of the *British Medical Journal* for Oct. 14th. It runs thus: "In Ehrlich's process the stain for the bacillus is too faint, and the vesuvin used to stain the ground substance too opaque; consequently the bacillus appears a faint pink colour on a dense yellowish-brown ground, and is not easily made out without high power or special illumination." If with Dr. Ehrlich's process the staining of the bacillus is found to be too faint, the fault will lie with the experimenter and not with the process. In August last Dr. Gibbes evidently did not know what was meant by Ehrlich's process, else he would surely never have said that at that time, or for months before that time, Dr. Ehrlich used vesuvin to give a "dense yellowish-brown ground," to a bacillus of a "faint pink colour." Since April last Dr. Ehrlich has used methylene blue as a contrast colour for red stained bacilli.—I am, Sir, yours faithfully,

London, Dec. 5th, 1882. G. A. HERON.

"PICRIC ACID AND SUGAR TESTING."

To the Editor of THE LANCET.

SIR,—In reply to Dr. Pavy's imputation on my memory or my pen, allow me to say that I have neither denied nor doubted the existence of unoxidised sulphur compounds in normal urine, but in answer to his statement that the presence of such compounds "means, in other words, the capacity for producing a sulphide on boiling with potash," I ask him for some proof of this other than a reference to well-known text-books. He may, perhaps, consider that in describing the reaction which occurs on boiling normal urine with liquor potassæ mixed with lead, he has given sufficient proof; but Dr. Pavy will doubtless admit that practically there is a very wide difference between the blackening of albuminous urine when boiled with lead-contaminated potash, and the slight and even doubtful colouration which results from treating normal urine in the same way. And now I ask him to give careful consideration to my son's letter in your last number, where he has shown that prolonged boiling of normal urine with caustic potash, while it destroys the sugar, does not form an alkaline sulphide, I also request his particular attention to the facts and arguments contained in this week's letter from my son. He there demonstrates and explains the fact that while a lead sulphide is formed by boiling albuminous urine with liquor potassæ *mixed with lead*, no alkaline sulphide is formed when the same urine is boiled with *pure liquor potassæ*. His observation explains the fact mentioned in my last letter to you, that albuminous urine containing the normal proportion of sugar—i.e., about ·5 of a grain per ounce,—gives exactly the same depth of colour when tested with picric acid and potash as the same specimen after the removal of the albumen by coagulation and filtration.

I beg now to thank Dr. Pavy for his friendly criticism, the result of which has been to elicit a more speedy and complete demonstration than without it would probably have been given—that there is no substance, either in normal or in albuminous urine, to interfere with, or to render in any way fallacious, the picrate test for sugar.

I hope in the course of a few days to send you a detailed description of a method of quantitative analysis of saccharine urine by which I obtain results practically identical with those arrived at by my son, who analyses the same specimens by means of Dr. Pavy's ammonio-cupric method. The advantage which I claim for my method is that it requires only a very simple and inexpensive apparatus, that it is easily learnt and practised, and that by its means an exact analysis of a sugar solution may be made in about five minutes.—I am, Sir, yours faithfully,

Savile-row, Dec. 18th, 1882. GEORGE JOHNSON.

To the Editor of THE LANCET.

SIR,—The argument employed by Dr. Pavy in his communication to you of the 12th instant is that, as an alkaline sulphide is produced by boiling albumen with caustic alkali solution, so an alkaline sulphide will likewise be formed by the action of boiling potash upon those unoxidised sulphur compounds which have been proved to exist in healthy urine, and which are presumably nearly allied to albumen in their chemical properties and constitution.

Now it is stated and accepted as a fact in the chemical text-books, and is to be found in so many words in "Watts' Dictionary," that albumen gives rise to an alkaline sulphide when boiled with solution of caustic alkali. It was, therefore, very surprising to me to find that dried ovalbumen, a specimen of which happened to be handy, produced no trace of sulphide even after boiling for half an hour with a strong solution of potash; the solution thus obtained gave no dark colour or precipitate with a solution of acetate of lead, and no reaction with nitro-prusside of sodium, even after diluting the liquid till the excess of caustic alkali was no longer sufficient to interfere with the production of the characteristic colour on the addition of a single drop of H_2S water.

At first I was inclined to doubt whether the specimen of albumen employed had not been subjected to some process which had deprived it of its sulphur, but this idea was at once negatived by the fact that if the lead solution was added to the albumen and potash *before boiling*, and heat afterwards applied, an abundance of lead sulphide was produced. And here is the clue to the mystery. The text-books direct to add the lead *before* boiling the albumen with potash. The fact is that a sulphide can be produced only by boiling an alkaline solution of albumen (made in the cold) with a metallic solution, such as one of lead or copper. Clearly the sulphur of the albumen is converted into some sulphur salt of potash (not sulphide) by boiling with caustic potash, for the solution thus obtained gives no sulphur reaction with the picrate test, lead test, or nitro-prusside test.

Taking into consideration the importance of this observation, and especially the fact that it contradicts accepted statements concerning the action of alkalies upon albumen, it was thought necessary to establish the fact by yet further, and if possible, more convincing evidence. With this end in view I performed the following experiment:—Some of the alkaline albumen solution, obtained by prolonged ebullition of white of egg with caustic potash, was placed in a two-necked Woulffe's bottle and a stream of pure carbolic anhydride gas passed through it till all air was expelled. This having been accomplished, the liquid was rapidly acidulated with pure acetic acid, and the issuing gas caused to bubble through a slightly acidulated solution of lead acetate. Not the faintest turbidity was observed after prolonged passage of the gas through the lead solution, and no darkening of colour, though the issuing gas possessed a peculiar fœtid odour. This test is, of course, one of extreme delicacy, the CO_2 gas tending to carry on with it any gas that might be in solution in the acid liquid, and at the same time preventing the possibility of any H_2S being oxidised by the air. Anyone may convince himself that these observations upon the albumen of the egg apply equally to the seralbumen found in albuminous urine. Place some albuminous urine in two test-tubes, add to one test tube some caustic potash and a few drops of acetate of lead solution, whilst to the other an equal portion of caustic potash is added, but no lead. Then boil the contents of both tubes for three or four minutes, and add to the liquid containing potash and urine only, a quantity of lead equal to that added to the other tube. It will be found that the liquid to which the lead was added after boiling remains clear (the colouring matter is somewhat reddened, but no sulphide of lead is formed), whilst the other solution will be blackened by lead sulphide in proportion to the quantity of albumen in the specimen under examination.

It seems, then, that *no sulphide* is produced by boiling even albumen with caustic alkali, unless a metallic solution be added *previously* to boiling, and as no metallic solution except the potash is employed in the picrate test, it follows that the presence of albumen in urine can in no way interfere with the application of this test to the detection or quantitative estimation of grape sugar therein. And in this respect the picrate test possesses a manifest superiority over that of Fehling. I am, Sir, yours truly,

G. STILLINGFLEET JOHNSON.

King's College, Dec. 18th, 1882.

"DEATHS AFTER ABDOMINAL OPERATIONS FROM HEART-CLOT."

To the Editor of THE LANCET.

SIR,—Mr. Lawson Tait declines to enter into the question raised by my reading of his cases of death from heart-clot, and shelters himself behind the easy device of the denial of