

a very great degree. The left eye is quite cured, and my opinion is, that it would have assumed a very serious change of structure, had I not taken vigorous measures at once to cut short the incipient disease. With the right eye she can distinctly recognise my countenance, which she was wholly unable to do previously. There is now a prospect of a successful termination of the slight remains of the projecting cornea, by the occasional use of the argent. nit. in its pure form to the surface.

I am of opinion that the repeated application of the argent. nit. to the cornea should be steadily persevered in for some time, as it acts as a direct stimulant to the absorbents, increasing their power of action, to take up the thickened structure of the anterior laminae of the cornea. It is well known how the application of this caustic to opacities of the cornea of long standing will operate in removing the organized deposit; and if it prove so successful in obstinate cases, not to be reached by the use of mercury, the inference is, that it may prove a most valuable agent in controlling and subduing this local disease. It was of most decided service in the first case, where I tried it very steadily for some time.

Tiverton, August 6, 1844.

THE DISCOVERY OF URIC OXIDE IN GUANO.— METHOD OF OBTAINING HIPPURIC ACID FROM HUMAN URINE.

By L. M.

As Unger's discovery of uric oxide in guano will probably lead to a repetition of his experiments by many of the readers of *THE LANCET*, it may not be deemed out of place to give a brief summary of what is already known on the subject, as a guide for future experiments.

There is an error in the assertion, that there are only two recorded cases of uric oxide. In addition to the two quoted cases, there is one recorded by Langier, and another, a few months ago, by Professor Dulk, of Königsberg, who has reported the case, and stated the various steps of his analysis in one of the leading German periodicals.

The usual rule for obtaining the substance is to dissolve the calculus in a solution of potash, and then to precipitate it from this solution, after filtration, by means of a continuous stream of carbonic acid. The same method may be adopted with the guano.

When freshly precipitated, it constitutes a white, uncrystalline powder, which, after drying, becomes of a pale-yellow colour, hard, and assumes a waxy appearance on being rubbed. It is only very slightly soluble in water, and not at all in alcohol or ether; it is perfectly neutral, and when heated it decomposes without previously fusing.

Its composition is expressed by the formula $C_{10}H_4N_4O_6$.

Now as uric acid is represented by the formula $C_{10}H_4N_4O_8$, it is clear that uric oxide differs from uric acid simply in containing two atoms less of oxygen. They may consequently be regarded as compounds of the same radical, in different stages of oxidation.

Uric oxide exhibits no fixed attachment to bases; the compounds of this nature that it does form are readily decomposed, and in this respect it is very different from uric acid. It dissolves in ammonia more readily than uric acid: the evaporation of this solution yields a yellow foliated mass, which contains only a very small quantity of ammonia: hence it is that the addition of hydrochlorate of ammonia to a solution of uric oxide in potash, causes little or no precipitate till the ammonia has evaporated. One important point of distinction between uric oxide and uric acid is founded on the difference of the precipitates which are thrown down from their potash-solutions by carbonic acid. The uric oxide is thrown down as a powder, and does not contain a trace of potash: the uric acid affords a gelatinous precipitate composed of neutral urate of potash.

Liebig and Wöhler assert that uric oxide is soluble in hot nitric acid without any development of gas. Dulk, in his recent paper, grants the solubility, but states that there was a decided evolution of gas, not only in his own case, but also in pure uric oxide. (He refers probably to a specimen in his possession from Stromyer's case.) Uric oxide is said to dissolve more slowly than uric acid, in nitric acid: on carefully evaporating this solution, we obtain an orange-coloured residue, which is not reddened by the vapour of ammonia. Dulk found that by evaporating this yellow residue, adding more nitric acid, again evaporating, and so on, he could change the colour to a much deeper tint. His object in this experiment was to oxidize it, and convert it into uric acid. After this thick yellow residue had stood for some days in a watch-glass, small, hard, transparent crystals appeared, and then the portion which remained fluid gave beautiful blood-red colour

when heated on a platina spatula, over a spirit-lamp. The same change from yellow to red was found to occur spontaneously without the aid of heat in the course of some days, free access of air being permitted. Hence it would seem that by the prolonged influence of nitric acid on uric oxide, uric acid was finally obtained. The minute crystals to which we have adverted are considered by Dulk to consist of *alloxantia*. They communicated a reddish tinge to cold water, and became of a yellow colour. They dissolved perfectly in boiling water; and on the addition of ammonia to the hot concentrated solution, a red colour was evolved, which disappeared on cooling. On mixing a little of the solution in a very concentrated state with nitric acid, and then adding ammonia, a green salt separated itself. Whatever these crystals are, they are well worthy of being again sought for, and submitted to ultimate analysis. Uric oxide dissolves in sulphuric acid with a yellow colour, and is not precipitated on the addition of water. Uric acid, on the contrary, is precipitated under these conditions.

Unger seems to regard its solubility in hydrochloric acid as a discovery. In page 54 of Hoskins' "Translation of Scharling's Essay on Calculi," we find that "it is likewise soluble in hydrochloric and oxalic acids." Most chemists, however, are compelled to adopt Liebig and Wöhler's statements regarding this substance, since they have not the means of repeating their experiments. They assert, and their opinion is apparently adopted without hesitation by Berzelius, Lehmann, and others, that uric oxide is *very slightly* soluble in these acids.

We have already shewn that Dulk conceives that he has obtained uric acid from uric oxide. The attempt has also been made to obtain uric oxide from uric acid by a process of de-oxidation, but hitherto without success.

In concluding these unconnected remarks, I would observe, that whenever uric oxide and uric acid exist together—and it is by no means improbable that they are simultaneously present in occasional forms of urinary sediments,—the best method of separating them consists in the execution of the plan which has been already given for obtaining uric oxide. The sediment must be dissolved in a solution of potash and carbonic acid passed through it: the process is founded on our knowledge of the fact that uric acid is soluble in a dilute solution of carbonate of potash, but uric oxide is insoluble in it.

L. M.

P. S.—I may take this opportunity of mentioning a plan for obtaining hippuric acid from human urine, which in many respects is preferable to those of Liebig and Dr. Golding Bird. Evaporate the urine till the salts spontaneously deposit themselves. Add strong alcohol, and place the mixture in a stoppered bottle. With the aid of gentle heat (for instance, by placing the bottle in warm water) we ensure the solution of urea, the lactates, and hippurates in the alcohol, while uric acid, which is the constituent most likely to interfere with our proceedings, remains with the insoluble constituents. When the supernatant fluid is perfectly clear, it must be decanted, evaporated very nearly to dryness, and re-dissolved in hot water. If a stream of chlorine be passed through the aqueous solution, the urea is destroyed, and by gradual concentration, and the addition of a little free mineral acid, we obtain crystals of hippuric acid.

MIDWIVES' MIDWIFERY.

NO. III.

By T. LITCHFIELD, Esq., Surgeon, Twickenham.

ABOUT four years ago I was sent for to a patient who had been under the charge of a midwife during the night. I was not well at the time, and requested my pupil, then just leaving me, to see what the case was. He came back hurriedly, and said he thought the woman was dying, and that he had made no examination, seeing the very alarming condition she was in, and hearing there was no hæmorrhage. I need hardly say I went immediately, and found the poor creature sinking—in fact, she gasped once only as I entered the room, and then died! I turned aside the bedclothes, and soon discovered the cause of death. The midwife had found difficulty in extracting the placenta, and the funis being thick and strong, she pulled lustily at it, and turned the uterus inside out, as the women in waiting said. The placenta was attached pretty firmly, and the deceased woman being subject to prolapsus of the organ, was unable to stand the lugging and force used. This is case No. 1 in my practice. Now for case No. 2.

A deformed woman, with the pelvis opening much contracted, requested my attendance during labour. I consented to visit her then, and told her mother, *privately*, that it would be necessary to deliver her at the seventh month. This was done safely, and a second time my attendance was desired, and with the same result. Both the children, however, were dead, and I was compelled to adopt craniotomy, for space was fearfully small. A third preg-

nancy occurred, and some "goodnatured friend or another" (School for Scandal) advised her to have a midwife, who promised her a living child at the *full* period! This was something, and naturally filled the poor ignorant woman and her friends with a degree of confidence. I heard what was to be done, and guessed pretty correctly the result, which was terrible! For fifty hours the poor wretched creature was in strong labour, and I heard that the uterine contractions were fearful. I was sent for hurriedly, and found the woman dead; the womb had yielded, and the full-sized foetus I found was cast amidst the intestines. Nothing else could have been expected, and the midwife never after practised here; but where she went, I know not. Heaven knows that the best informed practitioner, and whose experience has been extensive, is frequently perplexed to know what to do in difficult cases of parturition. His anatomical knowledge, although it gives him confidence, yet makes him cautious in dealing with his patients. The ignorant pretender, however, rushes headlong onward, and the "*finis coronat opus*" usually is—death! More anon.

October 7, 1844.

ON ALBUMINURIA; ITS EXISTENCE DURING PUERPERAL CONVULSIONS; ITS PATHOLOGY.

By G. Ross, Esq., Surgeon, Kennington.

I PERCEIVE by a recent Number of *THE LANCET*, that Dr. Lever has lately published some interesting observations on the co-existence of albuminuria and puerperal convulsions. I beg to inquire of that gentleman, through your columns—First, whether, and for how long a period, albumen was discoverable in the urine prior to the convulsions?—Secondly, whether in all, or in how many, of these cases, œdema existed; and what was the particular state of the skin? Thirdly, whether disease of the heart was detected in any of these cases?

I am sensible of putting questions that may not have occurred to the mind of the observer at the time of treating his cases; but until these questions are put and answered, it appears to me that the connexion of albuminuria with puerperal convulsions must remain obscured.

I have already published my own conviction, derived from observation, that albuminuria is, in nearly all cases, dependent on disorder of the functions of the skin; and I apprehend that the conditions of albuminuria during puerperal convulsions will tend to illustrate and establish this position. For example: in Mr. Crisp's case of puerperal convulsions attended with albuminuria, described before the Medical Society, and reported in last week's *LANCET*, "an œdematous condition of the face and upper extremities" is expressly noticed. Was there not also, *a fortiori*, disease of heart? In another case, recorded by Dr. Bree, (*LANCET*, No. 9, vol. i. 1844,) the patient is described as being "generally œdematous." (Disease of heart?*)

These remarks are intended to be merely suggestive, and to direct inquiries to the general principle that may lie at the bottom of these various manifestations of albuminuria. It does not appear that albuminuria is an essential symptom of puerperal convulsions, for Dr. Lever himself, on the authority of Drs. Waller and Ramsbotham, has stated two instances of this dissociation. Albuminuria, therefore, although a frequent symptom, is simply a coincidence.

Now, as albuminuria is commonly an effect of disease of the heart, and as such disease may exist without inducing albuminuria as a necessary consequence, it behoves us to inquire what share disease of the heart may have in the causation of puerperal convulsions. Supposing the heart to be diseased, no one would be surprised that the laborious efforts of parturition should induce general convulsions, or even that the pressure of the gravid uterus upon the great vessels should give rise to the same effects.

If any practitioner can give me information on the points herein contained, I shall be obliged.

Note.—At the last meeting of the London Medical Society, Dr. Williams referred to my views of albuminuria, and stated positively that they were not correct. Although no dialectician myself, yet I have seen enough of the adroitness of practised debating to be aware that nothing is so easy as to seize upon a few of the salient points of any complicated argument, and by a little sharpening of the wit, convert them into powerful weapons of offence. Dr. Williams seems to have given my views only a partial consideration, or he could not so hastily and dogmatically have pronounced his dissent.

Dr. Williams said I had affirmed "that the presence of

albumen in the urine was attended by a dry state of the skin:" and again, the doctor says, "that albumen was often found in the urine when the skin was perfectly moist"—a statement in which Dr. T. Thompson and Dr. Rees coincided.

My statement runs thus:—"When the skin was dry, and the anasarca mounted to the hips, coagulable urine would be certainly found," &c.; and this position I will maintain at any time and place. I have never said that mere moisture of the skin was a preventive of albuminuria. I, as well as other gentlemen, have seen a certain amount of albumen in the urine when the *upper part of the body* has been bathed in perspiration; but when the effusion under the skin has covered a greater extent, and the functions of this organ have been, consequently, more interfered with, I have found the amount of albumen proportionably to increase. I have not yet been so unphilosophic as to make an unconditional statement respecting a disease so full of contrarieties. I take it for granted that when the cellular tissue is loaded with serum, the functions of the skin will be arrested to that extent, and if any gentleman can shew me a case to the contrary, I will, with pleasure, journey ten miles to see it.

Let the objectors to this doctrine adduce satisfactory cases against my positions, and I will abandon them without a regret. My aim is, the development of facts and the establishment of truth.

Meanwhile, as this is a most important doctrine, and it might be prejudiced by dogmatic remarks at an influential debating society, I will at any time state my views personally to the members of the society, and be ready to meet any objections that may be offered. This demand is made in the spirit of fair play. Will they refuse it?

October, 1844.

THE LANCET.

LONDON: SATURDAY, OCT. 26, 1844.

THE Government has no right to require that the subject of MEDICAL REFORM shall be argued, or considered, as an *abstract question*.

Whilst introducing THE BILL to the notice of the House of Commons, the HOME SECRETARY appeared studiously to refrain from noticing the protection which the law affords to the members of other professions in this country; and it did not seem to be convenient to him to refer to any of the advantages which had sprung out of the penal provisions of the Statute of 1815—the APOTHECARIES' ACT. In pursuing this discussion, we shall not adopt the course which the Government has marked out. (In referring to the "Government" and the "Home Secretary," of course we allude to them as *identical* authorities.) But whatever aids the cause of medical polity *can* derive from collateral and incidental illustrations, we are determined that it *shall* receive,—so far, that is, as it may be in our capacity to supply them.

We have ascertained, with feelings of great satisfaction, that the clauses which we quoted in the last *LANCET* from the ATTORNIES AND SOLICITORS' ACT, (passed into a law in the Session of 1843,) have been read by medical practitioners with sensations of *astonishment*. When the question is asked, "Ought the medical profession to receive the protection of the law?" the public may possibly infer that to *no other* profession has such a protection been given. But if it be indisputably demonstrated, by the specified sections of an Act of Parliament, that such a protection *has* been awarded to *another* profession, and does actually exist, the question derives immense support when such an illustration is produced in the argument on behalf of a similar protection to medicine. As we have stated, however, our medical brethren have been stricken with astonishment, on perusing the sections of the ATTORNIES AND SOLICITORS' ACT which we printed last week, (page 106.) When the Government proposition with regard to *Medicine* is considered, the provisions which are contained in those clauses against unqualified practitioners in the law, are, indeed, amply sufficient to excite in the minds of the medical practitioners of this country the strongest feelings of indignation. They may, indeed, well feel *indignant* at being

* The case detailed by Mr. Rose, of Swaffham, (*LANCET*, No. 21, vol. i. 1843-1844,) was also "universally anasarcaous." These are the only cases that, at present, I have the opportunity of referring to, but they remarkably illustrate my views of albuminuria.