THE INFLUENCE OF CERTAIN INGESTA IN CAUSING ABNORMAL PRODUCTS IN THE URINE.

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THE subject of the observations I propose to submit on this occasion is the influence of certain articles of food and medicine in causing abnormal products in urine, especially those usually considered diagnostic of disease.

Notwithstanding the rapid progress which has been made of late in the elucidation of diseases, especially those of the kidneys, by reason of the regard which is now paid to the varying states of the urine, the accidental influences to which this secretion is liable do not appear to have received all the attention they deserve.

Of the importance of this inquiry, I need say nothing. It is obvious that in no case can any given condition of the urine be regarded as pathognomonic of disease, unless we are assured that that condition springs from a strictly pathological, and not from an accidental source.

It has long been known that vegetable food greatly exasperates, if it do not directly induce, one well known disease, *diabetes*; but it is now believed that the *fons et origo* of this malady is due to the malassimilation of starch. In normal digestion, starch is converted first into glucose, and subsequently into lactic and acetic acids; but in diabetes this metamorphosis stops with the formation of glucose, which, being a powerfully diuretic substance, determines the phenomena of the disease.

In like manner it has been supposed that oxaluria is caused by the malassimilation of sugar. In normal digestion, sugar, by combining with oxygen, becomes carbonic acid and water; but if imperfectly oxidised (that is, with a fourth less oxygen), water and oxalic acid are formed.

It is true, that another theory of the formation of oxalic acid has been given by Liebig; namely, that it is derived from the imperfect oxidation of uric acid. And it has been urged that experimental proof is wanting of oxalic acid having been formed from sugar within the body; but, be this as it may, that this acid often arises from vegetable food there are strong grounds for believing.

Dr. Watson, in his valuable lectures on the *Practice of Physic*, mentions two articles of diet which should be avoided by those in whom the oxalic diathesis obtains—these are the rhubarb plant, of which so much is consumed in tarts in this country; and sorrel, of which the French partake freely in salads.

Some years since, it occurred to me to ascertain by direct experiment how far this interdict with regard to the rhubarb plant was justified—whether, in short, it was capable of producing oxalate of lime in urine previously free from this salt. Four individuals, whose urine was first ascertained to be normal, ate freely of rhubarb-tart at dinner for ten consecutive days. In the course of a day or two, oxalate of lime was detected in the urine of three of them. But as these experiments formed the subject of a communication in the *Provincial Medical and Surgical Journal* for 1846, I shall only, in referring to them now, add that subsequent observation confirms the conclusion to which those experiments pointed; namely, that the use of the rhubarb plant as food, and the presence of oxalate of lime in urine, are to each other in the relation of cause and effect.

On any other supposition, I am unable to explain one circumstance which I have observed, and which has been confirmed by the independent observation of a friend, which is, that oxalate of lime is present in urine during the months of May and June, when the rhubarb plant is generally in use more frequently than at any other period of the year. The obvious practical deduction from this is, that the presence of oxalate of lime cannot be regarded at such times as of any pathological importance. But in truth, I am inclined, with others, to doubt that the formation of oxalic acid within the economy, under any circumstances, is productive of all the morbid phenomena which have been attributed to it; for I have frequently found

oxalate of lime in the urine of individuals whose health has been in no way affected, and in whom none of the symptoms supposed to be characteristic of oxaluria have been present. I would submit that the oxalic acid should be regarded more as an effect than as a cause—that the symptoms referred to are occasioned by some inscrutable condition of the gastric mucous surface, which obtains in certain forms of indigestion, and that they do not arise from oxalic acid—which, so far as we know, has never been detected in the fluids or tissues of the body uncombined with lime, the base by which its toxic properties are most effectually neutralised.

It was the opinion of Lehmann, that the excretion of oxalate of lime by the kidneys was vicarious of that of carbonic acid from the lungs; that when from disease, as in bronchitis and pulmonary emphysema, the quantity of carbon exhaled from the lungs was diminished, the kidneys compensated by eliminating it in the form of oxalic acid.

Having observed oxalate of lime to be temporarily present in the urine of individuals, in whose system the carbonaceous and nitrogenous elements might be supposed to have accumulated, I am led to believe that the function of the kidneys is sometimes ancillary to that of the lungs, excreting carbon in aid, not in place, of them. For example; an individual, after several days' eating and drinking to excess, complains of general *malaise*, which I need not describe, but of which constipation shall be an essential feature. On examination, his urine will be found of high specific gravity and colour, and will deposit, on cooling, uric acid, or urate of ammonia, or both; but should this not be the case, should there be no visible deposit, then the probability is that oxalate of lime will be present. Here we may suppose that carbon being in excess in the system, and the lungs being inadequate to the removal of that excess, the kidneys come to their aid by excreting some portion of it in the form of oxalic acid. And a consideration of the effects of remedies under these circumstances is in support of this view; for, after a brisk purge or two, restricted diet, and especially after increased exercise, crystals of oxalate of lime are no longer to be found.

I proceed now to remark on the influence which some medicines have on the urinary secretion.

Whilst in attendance on a case of acute pneumonia, in a plethoric man, aged 50 years, my attention was attracted at one of my visits to a sudden change in the character of his urine. At each preceding visit, it had been loaded with the well-known lateritious sediment; but I was now surprised to find that this had given place to a copious white deposit, with the iridescent pellicle on the surface. On examination, these were found to consist chiefly of the triple phosphate. Recollecting having read in authors that a sudden change of this description was an evil omen, I prepared to see my patient grow worse; but, contrary to this expectation, he being then convalescent, continued to improve, and ere twenty-four hours had elapsed, his urine was again loaded with the lateritious deposit.

Some months afterwards, in the case of a lady suffering from congestion of the liver, a similar circumstance occurred. The urine, previously loaded with the pink urate of ammonia, suddenly presenting a white deposit of the phosphates. It now occurred to me that the cause of this change might possibly be calcined magnesia, which had been given as medicine to both patients:—in the first instance, in combination with the sulphate of magnesia; in the other, with orange-juice. To determine the point, I swallowed, going to bed, half a drachm of Henry's magnesia, with a scruple of rhubarb. The urine of the following morning was alkaline, and yielded, on cooling, a copious white crystalline deposit, which, under the microscope, was seen to be composed entirely of the beautiful crystals of the triple phosphate.

That magnesia should produce the triple phosphate in urine is not difficult to suppose, when its affinity for phosphorus is considered. If it be added to urine out of the body, not a trace of phosphorus can subsequently be detected. And although the influence of the vital powers in

restraining and modifying chemical action is well known, that chemical affinities are more or less exerted within the economy does not admit of doubt. But does not magnesia modify the vital powers? If we consider from whence the phosphorus of phosphatic urine is doubtless derived, that it comes in great part from the brain and its dependencies, of which it forms so important a constituent, as also from the muscles, it is not improbable that whatever may tend to disturb the physical integrity of these important organs, may, *pari passu*, tend to derange their functional manifestations. Can we in this way explain the alleged fact, that magnesia cures warts? It is well known that these excrescences possess a low vitality. We may suppose that magnesia, freely given, by acting on and removing some portion of the phosphorus of those nerves on which this vitality depends, would tend to reduce it still lower, and so induce atrophy of the warts. The fact that they will often disappear from individuals troubled with them, who fall into a state of debility and enervation, as I have observed on several occasions, would appear to support this view. What is singular is, that when once they have begun to disappear, returning health or increasing strength does not arrest their decline. The continued falling off of the hair during convalescence after many exhausting diseases would appear to be an analogous phenomenon.

It would then appear that magnesia ought to be classed among agencies of a depressing or debilitating character. With every wish to guard against an undue bias from preconceived ideas, I cannot avoid the conviction, in confirmation of this supposition, that I have witnessed many instances of debilitated health in individuals accustomed to the frequent use of magnesia as medicine. I would especially mention nursing mothers, suffering from "stomatitis materna", with ill doing infants. In almost all such instances that I have seen, I have ascertained that magnesia had been one of the principal remedies employed before seeking advice.

With a few remarks on the fixed alkalis, I shall conclude.

It is known that the preparations of soda and potass increase the amount of the sulphates and phosphates in urine. Dr. Bence Jones has shown that this is the case, though the urine may still be acid, and without any visible deposit; but large doses, or the long continued use of alkalis, neutralising this acid reaction, the earthy phosphates are then precipitated.

A single teaspoonful of carbonate of soda yields a deposit of phosphate of lime in the urine of some individuals. Solution of potash, or the acetate, will cause urine to opalesce by heat; again becoming clear on the addition of acid. Purgative doses of tartrate of potash cause urine to be alkaline; and a similar result is produced by strawberries, if eaten freely; by the whey from cheese; and by certain wines, champagne, for example, of which Dr. Owen Rees states a glassful, at an interval of every four hours for twenty-four hours, is sufficient.

It is supposed in these cases that the increased sulphates and phosphates are chiefly derived from the oxidation of the sulphur of albuminous tissues, and of the phosphorus of the fatty matter of the brain and nerves. In this way, also, are explained the reputed remedial effects of alkalis in disintegrating and removing morbid exudations, and in dispersing adventitious growths. But from the known effects of alkalis, if long continued, in causing debility and emaciation, it is highly probable that they do not act on morbid growths, without at the same time acting in a corresponding manner upon the healthy tissues generally; so that their tendency may also be, like that of magnesia, to deteriorate the vital forces by robbing the cerebro-spinal organs of some portion of their normal sulphur and phosphorus.

A case related by Dr. Kennedy, before the College of Physicians in Ireland, and recorded in the *Dublin Quarterly Journal* for August of last year, may here be cited as a striking example of the wasting effects of alkalis, when freely taken. A gentleman, the victim of diabetes, took half an ounce of liquor potassæ three times a day. The

effect was a loss of weight at the rate of half a pound per day. That the potass was the cause was proved, by his ceasing to lose weight on discontinuing it.

I would also cite two cases from my own practice in further illustration of this subject. They were cases of ovarian disease.

The first commenced about six weeks after delivery. The abdomen was perceived gradually to enlarge, until, in the course of a few weeks, it had attained a magnitude not less than before parturition. Mercury and iodine having been tried without benefit, the acetate of potash was prescribed, with tight compression by a bandage. In the course of a fortnight, the tumour had perceptibly diminished. At the end of two months, it had entirely disappeared. The patient, however, was greatly reduced in flesh and strength.

In the second case, the tumour was of slower growth. Other remedies having been employed without benefit, the lady was kept constantly, for the space of six months, under the influence of alkaline remedies. At length, she was seized with acute pain in the epigastrium, which, coming on in paroxysms, led her friends to conjecture that her case had been mistaken, that she was pregnant, and that labour was commencing. The pains, however, shortly subsiding, she was noticed to have sunk very much in her person. On now examining the abdomen, it was found greatly reduced—the parietes being flaccid, as after parturition; and on admeasurement, she was ascertained to be only twenty-seven inches in circumference, having been previously thirty-eight inches. The presence of fluid within the abdomen, I had always been able to detect by palpation; but now I could only do so across the lower regions of the abdomen whilst she was in the erect position. There could be no doubt that the tumour had burst, its fluid contents having escaped into the abdominal cavity. It was, however, evident that the tumour did not consist wholly of fluid; for a firm resisting mass, about the size of a clenched fist, could still be felt in the left iliac region. This patient, also, who at the commencement of the treatment was stout and fleshy, was at this period greatly worn and emaciated. The bursting of the tumour is an interesting fact, and would also appear to confirm the views held with regard to the action of alkaline remedies. We may suppose the parietes of the tumour, wasted by this action, in common with other tissues of the body, at length to have given way.*

I would, in conclusion, advert to one practical consideration suggested by the foregoing remarks; which is—the propriety of alkaline remedies being sparingly prescribed in diseases of an asthenic character, where anæmia or other indications of debility prevail. It is obvious that they cannot be given continuously, even for a short period, in such cases, without their reducing effects proving injurious.

On the other hand, in diseases of an opposite tendency, especially those accompanied by exaltation of the sensorial functions, we might augur advantage from their use.

Acting on this idea, I have frequently prescribed magnesia, in combination with aperients when these have been required, in delirium tremens; and I have reason to believe that the duration of the disease in those cases in which it has been so employed has been materially curtailed. It is a fact that the urine in delirium tremens is often alkaline, and loaded with phosphates. With our present knowledge of its pathology, it is no irrational conjecture, that the excretion of phosphates in this disease may possibly be one of the many salutary efforts of nature so frequently to be observed by the physician; and to promote which, is the constant aim of medical art.

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* It may be interesting to add, that this lady is at the present time—six months after the supposed bursting of the tumour—in the enjoyment of excellent health. The tumour, however, is still perceptible, but no longer occasions any inconvenience.