

It is worthy of remark that in *every one* of the eighty analyses of normal urine Becquerel's table gives an error of deficiency, and the error seems to increase with the increasing specific gravity. Moreover, the utter absence of any approach to regularity in the column of differences clearly indicates the impossibility of deducing a formula, or making a table, which would give sufficiently accurate results for chemical purposes. If, then, in a state of health, such an attempt is hopeless, the utter impossibility of forming a table which would give accurate results, when many additional disturbing forces are called into play, and where various abnormal ingredients occur, must at once be obvious.

It has been suggested by Simon that formula of this nature might be deduced for particular diseases, or classes of disease, which might be sufficiently accurate to be of service to the pathologist, although not to be trusted by the chemist. Thus he proposes that one table should be exclusively devoted to *albuminuria*, another to those diseases in which there is a *deficiency of fibrine in the blood*, another to *inflammatory diseases*, and a fourth to *diabetes*.

Although the scheme is far more rational than Becquerel's, yet it is to be feared that if we cannot form a tolerably accurate estimate of the solid residue from the specific gravity of healthy urine, we shall not be more successful when that secretion is deranged.

We shall attempt to carry out Simon's idea for two diseases, viz., *albuminuria* and *diabetes* :—

No.	Specific gravity.	Residue in 1000 parts, from actual analysis.	Residue in 1000 parts, by Becquerel's table.	Difference.
1	1005.0	11.7	8.25	3.45
2	1006.0	12.4	9.90	2.50
3	1006.9	15.0	11.38	3.62
4	1009.0	20.5	14.85	5.65
5	1009.5	23.9	15.67	8.23
6	1013.4	29.8	22.11	7.69
7	1014.0	33.9	23.10	10.80
8	1015.0	35.0	24.75	10.25
9	1016.0	35.08	26.40	8.68
10	1016.3	38.6	26.89	11.71
11	1016.3	39.3	26.89	14.41
12	1016.4	36.9	27.06	9.84
13	1017.0	51.88	28.05	23.83
14	1019.1	46.5	31.51	14.99
15	1020	53.18	33.00	20.18
16	1022	66.50	36.30	30.20
17	1027	64.20	44.55	19.65
		614.34	420.66	

It is easily shown from these data that Becquerel's table gives a *mean* error in deficiency of nearly 11.4 in 1000 parts, the greatest error being 30.2, and the least 2.5.

If we reject cases 13 and 16, in which the solid residue is unaccountably high, the most accurate empirical formula we can obtain is—

Solid residue in 1000 parts of urine = $\Delta \times 2.33$, which is, as we shall presently see, precisely the same number as Christison has deduced for diabetic urine.

Diabetes.—There is no disease in which we are so likely to find a tolerably constant relation between the density and the solid residue as in this disease, in consequence of the peculiar attention that is usually paid to the *quantity of the ingesta* in this class of patients. It also happens that there is no disease in which it is so requisite to obtain, with considerable accuracy, a knowledge of the quantity of solid matter excreted by the kidneys. With a view to the determination of this point, Dr. Henry performed a series of experiments, upon the results of which he subsequently founded a table, which has been copied into almost every work on urinary pathology in our language. This table, or, at least, the formula upon which it is based, has recently received an important modification from the hands of Dr. Christison. "The late Dr. Henry determined the fact that the quantity of solid matter in 1000 parts of diabetic urine is equal to the

excess of its density over that of water multiplied by the number 2.58. According to the writer, however, the correct number, deduced from many experiments, is 2.33."*

We shall now compare the results of experiment with those of the formulæ of Becquerel, Henry, and Christison. From this table it may be shown, by a very simple arithmetical process, that the average error which would occur from the use of Christison's formula is only .47 in 1000 parts, while the mean errors of Becquerel's and Henry's formulæ are 21.92 and 7.71 respectively.

No.	Specific gravity.	Solid residue per 1000.	Solid residue by Becquerel's table.	Solid residue by Henry's formula.	Solid residue by Christison's formula.
1	1016.	39.36	26.40	44.28	37.28
2	1016.	43.00	26.40	41.28	37.28
3	1018.	43.00	29.70	46.44	41.94
4	1021.	52.20	34.65	54.18	48.93
5	1021.5	50.00	37.47	55.47	50.09
6	1028.5	52.24	47.02	73.53	66.40
7	1029.5	60.05	48.67	76.11	68.73
8	1029.7	70.20	49.00	76.63	69.20
9	1030.22	71.53	49.94	77.97	70.40
10	1030.44	72.10	50.22	78.53	70.42
11	1032.	70.15	52.00	82.56	74.56
12	1032.37	80.41	53.41	83.51	75.42
13	1432.5	76.57	53.62	83.85	75.72
14	1032.27	80.17	54.40	85.06	76.82
15	1035.	81.50	57.75	90.30	81.55
16	1035.3	80.00	58.24	91.07	82.25
17	1036.0	90.40	59.40	92.88	83.88
18	1037.1	80.52	61.21	95.72	86.44
19	1038.0	94.70	62.70	93.04	88.54
20	1038.9	94.11	64.18	100.36	90.72
21	1039.	93.45	64.35	100.62	90.87
22	1039.8	96.20	65.65	102.68	92.73
23	1040.	103.26	66.00	103.22	93.20
24	1042.	68.80	69.30	108.36	97.87
25	1043.5	100.80	71.40	112.23	101.35
		1859.06	1310.82	2051.86	1847.19

Although, from my knowledge of Dr. Christison's accuracy of observation, I was led to expect that his formula would give more correct results than those of Becquerel or Henry, yet I must confess that the extreme minuteness of the error is much more striking than I had anticipated. There can be no doubt that although formulæ of this nature cannot be legitimately used by the chemist in the manner that Becquerel has done throughout his "*Semeiotique des Urines*," still, if they even approximate to the accuracy of Christison's diabetic formula, they will be instruments of no mean importance in the hands of the practical physician who may not have time or opportunity for delicate chemical investigation.

Southwick-street, Hyde Park-square.

ON THE TREATMENT OF ACUTE RHEUMATISM

LARGE DOSES OF NITRATE OF POTASS AND SULPHATE OF QUININE.

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AMONG the various subjects which have the privilege of occupying, at the present time, the attention of the medical profession, the treatment of acute rheumatism stands prominent. The rules laid down by Sydenham have long since lost their "prestige," and a dozen different plans of treatment are urging their claims to notice and adoption. There can be no doubt but that the researches which many talented physicians are now prosecuting will tend to establish, on a more firm and rational basis, the therapeutics of rheumatism; and, in order to

* Dr. Christison in the "Library of Medicine," vol. iv., p. 248.

facilitate their investigation, it is the duty of all who are acquainted with data calculated to assist the elucidation of truth, to lay them before the public. Impressed with this idea, I have written the following remarks, which may be considered as the sequela of my former communication (LANCET, Feb. 10) on the physiological and toxicological effects of large doses of the nitrate of potass.

In the paper to which I allude, I stated that I had seen the nitrate of potass used in large doses in the treatment of nearly all the cases of rheumatism (from seventy to eighty in number) which were received into the wards of M. Gendrin, at La Pitié, during the years 1838, 1839, and 1842; indeed, that having been house-physician (*interne*) to that physician during a great part of that period, I had myself been actively concerned in the treatment of these cases, which were more immediately entrusted to my care. Since then I have also lost no opportunity of testing the efficacy of the nitrate of potass in rheumatism, and have thus been able to form an opinion on the subject from personal experience.

It is a singular enough fact that nitrate of potass was used against rheumatism in the middle of the last century, by Dr. Brocklesby, and by other physicians, in doses of from one to two ounces in the four and twenty hours; and that, nevertheless, the impression that in doses of a few drachms it is a poisonous salt, has kept possession of the minds of all writers on therapeutics and toxicology up to the present day. As I have previously shown, however, it is only when a large quantity is administered at one dose that it sometimes, not invariably, produces serious toxic effects. If the quantity I have named (from one to two ounces) be sufficiently diluted in any ordinary fluid, it may be given with perfect safety. From the discussion which took place at the Académie de Médecine, last October, on M. Martin Solon's reading his memoir on the treatment of rheumatism by nitre, it would appear that it has been given in large doses, casually, by several Italian physicians, and by two or three French practitioners during the present century. However this may be, when, in 1837, I became acquainted with M. Gendrin, and followed his practice; he was, as far as I could learn, the only physician in Paris who used large doses of nitre in the treatment of rheumatism. M. Martin Solon's claim of having revived the mode of treatment I believe to be unfounded. In 1840, when he commenced his experiments, my friend and late colleague, Dr. Aran, was his "*interne*." The year previous Dr. Aran had been *interne* to M. Gendrin, and he it was who requested M. Martin Solon to try the effect of the agent he had seen so successfully employed by M. Gendrin. The result of the first experiments of M. Martin Solon were published by Dr. Aran in the "*Journal des Connaissances Medico-Chirurgicales*."

The cases in which I have found the nitrate of potass to be the most successful are those of acute rheumatic fever; in chronic rheumatism it appears to be but of little avail. M. Gendrin generally begins by four drachms in the twenty-four hours, with an adult female, and six drachms with an adult male, and rapidly increases the dose to eight, ten, or twelve drachms, seldom carrying it further. This is also the practice which I have followed. The nitre must be given dissolved in a large quantity of weak lemonade, or barley-water, properly sweetened, in the proportion of about four drachms of nitre to a pint and a half of gruel. This solution should be the only beverage, in order to ensure its being drunk by the patient. In the great majority of cases, when thus administered, it is from the first tolerated. Sometimes, however, the ingestion of the solution at first occasions slight vomiting, but this I have always observed soon to give way on merely continuing its use. M. Martin Solon states that in these cases he has found that the addition of a little syrup of poppies effectually arrests the vomiting. During the first twenty-four or thirty-six hours, very little change is produced in the state of the patient, but, generally speaking, about that time, sometimes a little later, the pulse diminishes in strength and in number, and that without any perceptible change in the state of the patient. M. Martin Solon says that the quantity of the urine is seldom if ever increased, but in this I believe he is wrong, for I have certainly seen many cases in

which the renal secretion has been decidedly increased. Sometimes the excretions of the skin are also much increased, abundant diaphoresis setting in on the second or third day, and lasting for a day or two. When this occurs I have remarked that the pulse falls much more rapidly than when the depressing effect of the salt alone is experienced. M. Martin Solon does not think that the diaphoresis, which is thus occasionally observed, is the result of the administration of the nitre; he considers it merely a symptom of the malady. According to his views the nitre merely acts as a sedative on the circulation. I agree with him, that the sedative action is the most powerful, and the one which is generally observed, but I think he underrates the influence of the salt on the secretion of the kidneys and of the skin. I have often seen them considerably increased under its administration.

At the same time that the general febrile symptoms abate the local inflammation also begins to subside. The swelling and pain gradually become less and cease to pass from joint to joint. When this is the case, if the ingestion of the salt is continued, there is seldom any relapse. The local and general symptoms gradually subside, and by the fifth, sixth, or seventh day the patient may be considered to be nearly convalescent, the cure being completed by the tenth or twelfth day. I have remarked that the tongue nearly always remains white and rather loaded as long as the nitre is administered, and that it only begins to assume its natural appearance when it is suspended. Then only, also, does the appetite return. When I have given nitre in large doses to persons not labouring under febrile symptoms, I have invariably found that it slightly disordered the functions of the stomach. It seldom occasions diarrhoea, being, as M. Martin Solon remarks, carried into the system and eliminated by the urine. This physician, who is a good analytical chemist, and has paid considerable attention to the analysis of the urine in disease, states that he has always found all the nitre he administered to his patients in their urine, which accounts for its being necessary to continue its administration for some time if we wish to keep up the action it exercises on the economy. Although nitre, as far as my experience goes, is a decidedly efficacious therapeutic agent in the treatment of acute rheumatism, it does not always succeed. Occasionally cases occur in which it produces no effect whatever, the disease continuing to progress, or, at least, not diminishing under its influence. If, after administering it for four or five days, we find such is the case, that no favourable effect has been produced, we are warranted in presuming that we have to deal with one of these rebellious forms of the disease, and then it is as well at once to change the medication.

I have also found the nitrate of potass an extremely efficacious and valuable remedy in the slight forms of rheumatic affection, which are not unfrequently met with in damp weather in this climate, and which manifest themselves more by local than by general symptoms. I allude to the cases in which first one joint and then another becomes slightly painful and swollen, without any, or, at least, much general disturbance existing, and that for weeks or months at a time. These forms of rheumatism, although often the forerunners of a severe attack of rheumatic fever, are generally neglected, because neither the physician feels inclined to prescribe or the patient to bear an active treatment; and yet the half measures which are often adopted are not sufficient to ward off the impending danger. I have treated by nitre several cases of this kind this spring. In every instance an ounce taken daily for a week has entirely eradicated all the symptoms which the patients presented.

The question that naturally presents itself to us is,—how does nitre act to produce the very decided effects which I have described? This, however, is a point on which I have but little to say. We are not as yet sufficiently acquainted with the state of the blood in rheumatism to be able to form even a well-founded supposition as to the intimate effects of the introduction of nitre in large doses into the economy: I shall, therefore, confine myself to the enunciation of the above therapeutical facts. Heart disease has been a very rare phenomenon

among the patients whom I have treated or seen treated by the nitrate of potass. Indeed, I have long considered its frequency, as propounded by Bouilland and other writers, to have been grossly exaggerated. This I attribute to their mistaking the bellows-sounds of the heart, which are so common in persons who have been rendered anemic by loss of blood, for the symptoms of endocarditis. This belief is founded on my having met with many cases in which the mistake has certainly been made by disciples of Bouilland's school, and on my not being able to reconcile what these pathologists have written and said with what I myself have seen.

The treatment of rheumatism by nitre in large doses has hitherto appeared to me to present great advantages over the other plans of treatment which are generally adopted, and more especially over the extreme depletion system. Whenever a patient loses much blood during the treatment of a disease, be it artificially or naturally, the convalescence is long, and during that convalescence he is much more exposed than a person in health to morbid agencies. I have known persons remain months, nay years, in an anemic state, weak, and sickly, after having been cured (?) of rheumatism by Bouilland's plan of bleeding *coup sur coup*. A treatment, therefore, which overcomes the disease without exhausting too much the sources of life is decidedly preferable to any other; and such is the treatment of rheumatism by nitre. I have many a time seen a strong muscular man, between twenty and thirty, brought into the wards, presenting the most marked symptoms of acute rheumatism: the pulse 100, full, bounding; the ankles and knees, wrists and elbows swollen, painful, and red; the skin hot, the face flushed: such a case, in a word, as a practitioner would consider himself called upon to bleed to deliquium. An ounce of nitre has been administered daily, and in two or three days the pulse has fallen and become soft, the skin moist, the joints less painful and less swollen, and in less than a fortnight the patient has been out of bed, allowed to eat, and able, after a week or two's rest, to resume his labours. The remedy, also, is cheap, an important consideration in the treatment of the poor, and is easily administered.

The above remarks must be taken for what they are worth. They are merely the impression produced on my mind by the observation of a considerable number of unrecorded cases, and are given more with a view of stimulating my professional brethren to try the agent recommended than of settling a disputed question. I am quite willing to acknowledge that it is not at all impossible that were I to give the nitrate of potass on a larger scale, and to record the cases, I might find reason to alter several of the opinions I have expressed. There is one important fact, however, connected with the administration of this substance, which what I have stated must have established, viz., that the nitrate of potass, given diluted, in very large doses, is not a toxic substance, and that although it be the "gunpowder salt," as an enlightened physician facetiously termed it before me, a short time ago, it does not give rise either to inflammation of the stomach or of the kidneys.

As I formerly stated, I have given nitre in the above manner in several cases of puerperal fever, and in other inflammatory affections, and think I have observed the same general depressing effect as in rheumatism, although in a less marked degree. May not nitre, thus administered, prove a valuable sedative medicine in febrile and inflammatory diseases?

During the winter of 1843 I had an opportunity, at St. Louis, of trying, under the superintendence of M. Emery, the effects of the sulphate of quinine in the treatment of rheumatism. We gave it in doses varying from fifteen to forty-five grains in the twenty-four hours, in fourteen cases of rheumatic fever, but without perceiving it to produce any amelioration in the state of the patients. Indeed, the disease appeared to progress as if nothing had been done, and, after a fair trial, we were obliged to resort to other remedies. I had under my care that winter several cases which had been treated out of the hospital for two, three, or four weeks by quinine, without the disease having been modified. In a large proportion of our cases we were obliged to suspend the use of the

medicine owing to the manifestation of peculiar symptoms of intoxication,—cephalalgia, giddiness, deafness, slight difficulty of speech, &c. These symptoms appeared mostly two or three days after the dose had been increased to thirty or forty grains. M. Monneret has recently published the result of his trial of this agent in twenty-two individuals, and arrives at pretty nearly the same results. M. Monneret has also given the nitrate of potass in eight cases. His report is unfavourable, but he does not appear to me to have allowed the remedy a fair trial, so that I am not inclined to attach much importance to the opinion he has expressed. I trust that what I have stated will induce some of my professional brethren to test the efficacy of nitre in their practice on a sufficient number of patients to render the result of their trials of value. I would, however, remark, that in order for a correct opinion to be formed of the powers of nitre in the treatment of rheumatism it ought to be administered alone.

Cambridge-square, Hyde-park, May, 1844.

ON THE
DETECTION OF POISONS, GENERALLY,
IN MEDICO-LEGAL INQUIRIES, AND ON A NEW AND
PERFECTLY SATISFACTORY METHOD FOR THE
DETECTION AND QUANTITATIVE DETERMINATION OF ARSENIC.

BY

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[Read before the Chemical Society of London April 1, 1844.]

OF all the various branches of practical chemistry the detection of poisons, in chemico-legal cases, has, perhaps, occupied most the attention of chemists; the reason of this is evident, considering the great importance of the subject in its social relations.

It appears, however, to me, that there still remains much to be done in this department of chemistry, both as respects the discovery of improved methods, and also in determining precisely what may be fairly demanded of the chemist,—what, in fact, are the limits of his power in answering legal inquiries respecting the presence or absence of poisons.

It is not my intention to attempt, by a complete treatise, to exhaust this important subject; I shall, in this paper, merely endeavour to point out the position which the chemist, charged with medico-legal investigations, occupies relatively to the law and to the public; to indicate and limit the questions which may properly be addressed to him; and to demonstrate the expediency and usefulness of legislative enactments, to lay down certain tried and confirmed methods for the detection of poisons, as rules which every chemist entrusted with the investigation of a case of poisoning should be compelled to follow. And, finally, I shall give, in the second part of this paper, a description of the detection and quantitative determination of arsenic, which I myself, in connection with my friend, Dr. von Babo, have endeavoured to establish, and which may serve as an example and model of such a method—safe under all circumstances, and applicable in all cases.

I.

If it were my design to write a treatise on the detection of poisons in general, I should be obliged to premise some preliminary remarks concerning the definition and the usual classification of poisons; the variety and diversity of their action; the symptoms indicative of poison having been administered or partaken of, &c.; but as I merely intend to develop my views upon certain points, I think I cannot be charged with treating my subject imperfectly, or entering upon it abruptly.

The first question which I purpose to consider is, *What is chemistry able to perform with regard to the detection of poisons? what may properly be demanded from the chemist?* And, on the other hand, what is there belonging to this matter which cannot fairly be expected from him.

I have already mentioned that it is not my intention to expatiate upon the classification of poisons. But in