If this be so,—if the number of the dismissals may serve like that of the deaths to appreciate the severity of disease, it will be seen that these results corroborate those which the study of the numbers of the discharged has already afforded, relative to the duration of the diseases. In effect there is a correspondence between the statistical data relative to the duration, and the data relative to the severity of diseases. A great number of patients are admitted in January; many do not go out until April; the conclusion has hence been drawn that the mean duration of the diseases furnished by the month of January, was considerable. But in the two intermediate months, that is to say, in February and March, the *proportion* of deaths is large; the affections were therefore severe; and thus severity attends length of duration, as indeed generally happens. In May, when many patients were admitted, the *proportion* of the discharged remains nearly the same as in April. Instead of increasing it slightly decreases, which is a sign of a certain degree of severity, but less than in the preceding cases. In June the *proportion* of the discharged rises all at once, even to a degree that is not easily explained by the number of admissions during the months of March and April. It was therefore, in a great measure, supplied by the patients admitted in May, or at the beginning of the month of June, and the mean duration of the diseases was consequently rather short. Now precisely in May the proportion of deaths is less than in February, March, and April; and in June it is much less than in May, so that June occupies the bottom of the scale. The severity therefore of the diseases brought into the dismissals in May, and received for the most part in April, is less than that of the diseases brought into the dismissals of April, and received for the most part in January. In the same manner the severity of the diseases brought into the dismissals of June, and received for the most part in May, is still less than those brought into the dismissals of May. Finally, the large number of admissions in June is not sensibly felt in the dismissals until August; and we have thence concluded that the reigning diseases of June were of rather long duration; and the proportion of the deaths in the month intervening between the admissions and the dismissals reveals a certain degree of severity in the diseases;—that is to say, that everywhere the degree of the severity of the diseases rises or falls with their mean duration.

(To be continued.)

CASE OF DIABETES, WITH OBSERVATIONS.

By H. S. BELCOMBE, M.D.,

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W. S., aged 50, an upholsterer, a man of very quiet industrious habits, requested my advice, February 15th, 1846. He complained that he had for six months back felt very weak, but as his appetite was good, he thought it could only arise from the pressure of business; presently, however, he became aware that he was very thirsty, and was constantly seeking cooling beverages. The great quantity of water he was now passing did not arrest his attention, as he attributed it to a natural consequence of the fluid taken. Feeling,

however, that he was becoming weaker, with a dry tongue, ceaseless thirst, dry and hot skin, he thought it best to apply for advice. I found him, as I expected from his preface, labouring under well marked symptoms of diabetes mellitus: these were uneasiness, restlessness, disturbed digestion, loaded tongue, thirst, dry hot skin, &c., risings from the stomach of a sour fluid, and a sense of constriction and of burning heat at the scrobiculus cordis. The pulse was quick, small, and irritable. The urine, of which between four and five quarts were passed in the twelve hours of the day, was limpid, of a pale greenish straw colour, of a peculiar hay-like odour, and of a very sweet taste. All the tests usually employed gave the corresponding character, and I am here happy to acknowledge the value of Mr. Moore's test, which I had previously doubted. On obtaining some very pure liquor potassæ, and subjecting the urine to its influence, under the spirit lamp, the melassic acid was produced; and I am bound to say I consider it a very valuable test, though perhaps inferior to the sulphuric acid, which is very quick and decisive.

The case was sufficiently unpromising. Under the use of opiates, however, he recovered for six weeks surprisingly; the urine was reduced to two quarts in the twelve hours, but never lost its distinctive character; the quantity passed at night was also considerable, perhaps equal, but he always forgot to measure it. Being obliged to take some purgative medicine, the whole train of symptoms returned, nor were they again relieved; the hectic fever increased considerably, perspiration now appearing, and the quantity of water passed, *pro tanto*, diminished, but no other mitigation. Finally, he expired somewhat suddenly about three weeks back, in a state, as the practitioners called to him informed me, bordering upon apoplexy. No examination took place.

Dr. Thomas Willis is, I believe, the first English physician who has written explicitly upon this disease; and though his theory of its cause will be now rejected, yet his account of the symptoms leaves nothing to add. He relates a case, where, after considerable time and suffering, the patient was cured by "water of slaked lime." Dr. Mead afterwards stated that "alum whey was a cure for diabetes;" and in these days we all know how much is trusted to diet, however various the remedies that medical men favour. It is not my intention to pass these under review, but rather to make a few observations on the effect of diet. In regard to the hectic fever that accompanies the later periods of the disease, I think it a beautiful elucidation of John Hunter's doctrine,—“Hectic fever is an habitual universal sympathy of the constitution, struggling with a disease it is unable to overcome.”

Gay Lussac and Thenard have laid down the following three laws, with regard to vegetable substances:—

1. If the oxygen and hydrogen exist in the exact proportion necessary to form water, the vegetable substance is neither acid nor resinous, but it is sugar, honey, manna, starch, gum, lignin, or some such body.
2. If the oxygen be to the hydrogen in a proportion greater than is necessary to form water, the substance is a vegetable acid.
3. If the hydrogen be to the oxygen in a proportion greater than is necessary to form water, the substance is oil, resin, wax, alcohol, ether.

So that sugar, starch, gum, &c., may be considered as composed of carbon and water only; vegetable acids, as composed of carbon, water, and oxygen; oils, resins, &c., as composed of carbon, water, and hydrogen. Now, as the great bulk of vegetable food consumed consists of starch, or gum, or sugar, it is not at all improbable, that from an imperfect, or deficient assimilation in the stomach, or even from no assimilation having taken place, a morbid process may convert such substances into sugar; hence the rationale of an animalized diet among the means of cure.

The pathology of the disease is still very obscure, but the following facts may be mentioned as evidently connected with it, and as indicating that the original disease is not in the kidney.

1. *Its morbid anatomy.*—Dr. Prout states, “the most constant diseased appearances I have noticed after death, in diabetic individuals, have been rather of a medico-chemical nature than strictly organic; and, generally speaking, hypertrophy of the kidney has been principally the morbid appearance exhibited in it; also it has been found as frequently complicated with organic disease of the lungs, liver, and lymphatic system.”

2. *The chemical relation between urea and sugar.*—This remarkable relation seems to afford much explanation of the phenomena of diabetes, and to prove that it consists in a depraved secretion of urea. Thus, urea and sugar contain exactly the same quantity of hydrogen, while the quantity of carbon and oxygen in sugar is twice that of those substances in urea; a morbid change therefore takes place in the composition of urea, and instead of its being eliminated from the system, as healthy function demands, sugar takes its place.

The morbid phenomena of diabetes may be considered to consist in a depraved condition of the assimilatory processes by which the aliment is converted into the nature of our body.

The deductions are,—that it is a highly dangerous complaint, and that when formed, it shews a very depraved condition of organic life;—that the mode of treatment should be regular and systematic, not depending too much upon diet in the way of regimen, or upon chemical combinations as regards medical means, but on the steady endeavour for the re-establishment of the vital powers.

The sympathetic and splanchnic nerves have been found enlarged; and this is not surprising when we remember how the nerves are concerned in secretion.

ON AMORPHOUS QUININE AS IT EXISTS IN THE SUBSTANCE KNOWN IN COMMERCE AS QUINOIDINE.

By BARON LIEBIG.

In the preparation of sulphate of quinine, after all the crystals which can be obtained are separated, a dark-coloured mother-liquor remains, having an extremely bitter taste. On the addition of an alkaline carbonate, this liquid loses its colour and bitter taste, depositing, at the same time, a yellowish-white, or brownish precipitate, which, after being rinsed with water, and exposed to a gentle heat, agglutinates into a coherent mass, exhibiting the appearance of resin.

From the experiments of Sertuerner, Thiele, Bucholz junior, Koch, and other chemists, it has been long known that this resinous substance possesses the properties of a base, that it neutralizes acids perfectly; but the salts which are formed by these combinations with acids, have baffled all attempts at crystallization.

Sertuerner, who was the first chemist to separate this resinous substance from the mother-liquor of sulphate of quinine, considered it to be a distinct and peculiar organic base, existing in yellow and red cinchona barks, associated with quinine and cinchonine. He assigned to this, as he supposed, new substance, the name quinoidine, and greatly extolled its medicinal efficacy, in which he declared it was in all respects equal to quinine. In his journal (*Ueber die neueste Fortschritte in der Chemie, Physik und Heilkunde*, Bd. iii., No. 2, page 269,) he terms it “a true fever-destroyer.”

Subsequently, this substance, under the term quinoidine, has been employed medicinally in many places, and even introduced into the list of commercial articles or price-currents of many of the druggists of Germany.

In certain mother-liquors of quinine left in the preparation of the sulphate, which were analyzed by Henry and Delondre, and also a sample of quinoidine examined by Geiger, these able chemists discovered an amount of quinine and cinchonine, accompanied by a resinous substance, which they considered impeded the crystallization of the sulphates of the two bases, and which in their experiments they failed to separate. The results obtained by these chemists, and the inferences obviously deducible from these results, rendered it indubitable that the medicinal efficacy of quinoidine must vary according to the greater or less proportion of quinine it may happen to contain. Now, there cannot be a doubt but that this uncertainty with respect to the relative amount of quinine in commercial quinoidine, has prevented many physicians from prescribing the latter as a remedy, notwithstanding the testimony borne to its efficacy.

Having occasion, some time since, to pass through Coblenz, I procured from Messrs. Jobst and Co., of that town, a sample of quinoidine, for the purpose of employing it for the preparation of quinoleine—a substance discovered by Gerhardt to result from the transformation of quinine, and to which much scientific interest attaches, in consequence of the recent discovery of Professor A. W. Hofmann, that quinoleine is identical with leucol, a body which is one of the components of the essential oil of tar, prepared from anthracite coal. It then occurred to me, that if the sample of quinoidine which I had procured contained quinine, it must yield a corresponding amount of quinoleine, and that, consequently, a very simple method of testing quinoidine for the amount of quinine it may contain, might be based upon this property of quinine to be transformed into quinoleine.

On subjecting the sample I had obtained, (which amounted to several ounces,) to distillation with strong potass ley, I confess I was surprised at the large amount of quinoleine produced, which proved the presence of a far larger proportion of quinine than could have been anticipated. This unexpected result induced me to subject quinoidine to a stricter examination; and in order to avoid being misled by accidental circumstances, I procured, beside the Coblenz