finger a narrow well-curved scoop. With this the stone was turned out of the cyst, and was easily removed by forceps. The bladder for a few days was frequently washed out with Condy's fluid and water. Symptoms of peritonitis, however, quickly manifested themselves, and the patient died in about a week.

The post-mortem revealed the following facts:—There was no general peritonitis, but behind the pubes there was a sloughy cavity containing offensive pus and urine. In separating the recently adherent intestines this cavity was opened, but when the whole bladder and uterus were removed and dissected this was the relation of parts. The bladder proper was small and contracted; its muscular walls were quite three-quarters of an inch thick; the mucous membrane was deeply injected and slate-coloured, and its prominent folds were covered with adherent phosphatic matter. There were three sacculi, one large in the centre of the posterior wall; this was as large as the bladder itself, and was composed of its mucous and peritoneal coats only, no muscular fibres, and communicated with the bladder by a well-defined round opening, barely large enough to admit the tip of the forefinger. The whole interior of this sac was in a state of sloughy, almost gangrenous, inflammation; it contained, as I have said, fetid pus and urine, and was clearly the focus and starting-point of the inflammation which caused the death of the patient. The smaller sacs were situated close in front of each ureter; that on the right, from which the stone was removed, had for its walls both muscular and mucous coats; the orifice was contracted so as barely to admit a No. 10 catheter, but it was easy to see how the stone had fitted it. There was no sign of inflammatory mischief around the sac, and it contained only some creamy muco-pus. The ureters were rather enlarged, especially the left. The kidneys were healthy. The uterine walls contained a fibroma about the size of a walnut.

The patient had lived until the last two years in Gloucestershire; her father died after lithotomy, and her brother, who lives in Buckinghamshire, was at this time said to be suffering from stone. The cause of death in this case was localised peritonitis; and I may say that in nearly all the cases of death from encysted stone which have come under my notice—and they are not a few—the same thing has occurred; and the explanation, I believe, is easy: the urine in these sacculi is retained by the sphincter-like action of the muscular columns which surround their entrance. There is generally no muscular covering to these sacculi, and so they cannot contract and empty themselves; hence the urine is retained, it decomposes and sets up inflammation, which extends by contact to the parts around. In cases like this, in which an operation is performed, there is, for a time at least, an additional cause of general cystitis and fetid urine, and so the morbid condition in the sac is aggravated and hurried on to a fatal extension.

The cause—for I think there is only one—of sacculated bladder is not far to seek; and it explains why the condition is so very rare in the female. This cause is obstruction to the easy outflow of the urine, which in the male, in nineteen out of twenty cases, is due either to enlargement of the prostate or to stricture—and much more frequently to the former than to the latter. Stricture generally occurs in the young, producing hypertrophy of the bladder, but hypertrophy with contraction. Enlargement of the prostate always occurs in the aged when the natural powers are failing; hypertrophy is the result of the obstruction, but it is hypertrophy with dilatation; and it is precisely in these cases that the mucous membrane is forced between and through the thin muscular walls so as to constitute the sacculi. Many surgeons allege, and notably the late Mr. Crosse, that the presence of a stone, by inducing vesical tenesmus, produces sacculation; but I cannot agree in this theory, because, if there is no obstruction to the outflow of the urine, the irritated bladder at once relieves itself and there is no inducement to form sacs, and in the young, where neither stricture nor prostatic enlargement is found, we seldom, if ever, find stone and sacculation coexisting. But in the old, or in stricture cases, I can well believe that the presence of stone increases the tendency to sacculation. In the female, where there is no prostate and seldom any stricture, obstruction to the escape of urine is rare, and hence we should expect sacculation of the bladder to be, as it is, very rare indeed. If stone could cause it, we should expect it to be less rare, because stone in the female is not so very uncommon. How, then, is the combination to be explained in the case I have related? In my judgment the

stone had little or nothing to do with the production of the sacculi, but that the growth around the orifice of the meatus, as it seemed to close the opening and make it difficult to find it even with a probe, so it caused the obstruction to the outflow of urine, and this obstruction was the real cause of the sacculation. This obstruction, too, probably forbade the escape of the minute calculi when they first descended from the kidney, and thus, so far from the stone being the cause, it was at the most an aggravating condition of the bladder.

ON THE PHYSIOLOGY OF THE CARBOHYDRATES IN THE ANIMAL SYSTEM.¹

By F. W. PAVY, M.D., F.R.S.

By a mode of investigation I have been recently adopting, through which information is furnished of the precise nature of a carbohydrate existing in a product, I have been able to recognise and follow changes which have hitherto escaped observation, and have thus been afforded the opportunity of acquiring new knowledge regarding the physiology of the carbohydrates in the animal system.

In an "Introductory Note,"² presented to the Royal Society in April last, I gave a summarised representation of the main points that had been disclosed, and stated that I proposed to deal with the subject in detail in a series of communications.

In this communication, which constitutes the first contribution, I purpose dealing with the changes undergone by the carbohydrates in their progress after ingestion into the portal blood. When I first became aware of the significance of the information which was being revealed by the method of investigation I was pursuing, I was working at the changes occurring within the system, and more especially in connexion with the liver; but, seeing that the matter was presenting itself under a perfectly new light, it appeared to me advisable that the investigation should be carried to the commencing point at which the carbohydrates come into relation with the animal system,—viz., the alimentary canal. I therefore started upon a systematic inquiry into the changes undergone by the four chief carbohydrates of our food—grape sugar, cane sugar, lactic acid, and starch—in their passage towards absorption and arrival within the portal system of vessels. This inquiry has resulted, not only in new knowledge being supplied, but in confirmation being given to the knowledge that had been acquired regarding the phenomena occurring in other parts of the system. By thus beginning with the study of the phenomena at the starting-point, or the point where the carbohydrates first come into relation with the animal system, important help has been afforded, and all the information obtained stands in harmony, the one part with another, and conspires to show that the changes occurring in the principles belonging to the carbohydrate group proceed exactly in the reverse direction of that which has been generally supposed.

The means by which the truths to be adduced have been disclosed have consisted in the employment of extended measures for discriminating the precise nature of a carbohydrate having to be dealt with, and the application of the method of quantitative determination with the ammoniated cupric test which I introduced a few years ago.³ Without the aid of this test I am quite satisfied I could not have obtained the knowledge that has been acquired. One step has led to another, and it has been by a gradual process that the matter as it has now to be put forward has been evolved. If the original information obtained had not been before me I should have had nothing to suggest the course of investigation that followed. The process of differentiation adopted has been based upon the employment of alcohol, and the determination of the cupric-oxide reducing power of a body before and after treatment by boiling with sulphuric acid for conversion into glucose. By the employment of alcohol, starch and its animal congener, the so-called glycogen, are separable from the dextrins, maltose, and the sugars. Methylated spirit is what I have used, and it has

¹ Communication read at the Royal Society, Dec. 20th, 1883.

² Proceedings of the Royal Society, vol. xxxv, pp. 145-147.

³ Ibid., vol. xxviii, pp. 260 and 520.

invariably been submitted to distillation, to secure freedom from chance contamination that might vitiate the result. Actual experience has taught me the necessity of giving attention to this point. The specific gravity of the spirit has stood at about '8030, which corresponds with the strength of about 97 per cent. of alcohol. After separating by alcohol, and collecting the colloidal carbohydrate, its amount has been determined by conversion into glucose, by boiling with sulphuric acid, and estimating the glucose with the ammoniated cupric test. Observation testifies that the process yields extremely delicate and accurate results.

In the case of the dextrins, maltose, and sugars, the differentiation is effected by dividing the product into two equal parts, and taking the cupric-oxide reducing power with the ammoniated cupric test of one part in its unaltered state, and of the other part after treatment with sulphuric acid, to convert into glucose. From the information supplied regarding the condition of the two parts, the nature of the particular carbohydrate present is shown. When such a plan of procedure is not adopted, and the cupric-oxide reducing power is simply taken, no correct information can be obtained as to the nature and quantity of the body present. For instance, the portal blood may contain a low cupric-oxide reducing dextrin, and if the cupric-oxide reducing power, as it at first exists, were taken as expressive of glucose, which is what in reality has hitherto been done, the amount of carbohydrate actually present would be very much under-estimated. At the same time, no information is supplied that the body consists of glucose. It is only by looking at the cupric-oxide reducing power before and after treatment with sulphuric acid that a correct knowledge can be acquired, concerning not only the nature but likewise the amount of the principle which is present. For conversion into glucose, sulphuric acid, to an extent to give a 2 per cent. strength in the liquid operated upon, is added. A point to keep in view is to prevent the development of colour, as this interferes with the delicate determination of the result in the subsequent process of titration with the ammoniated cupric test. Hence, as it has been found by experience, it is necessary to use the pure acid. The condition of the product must be kept in view to secure that the sulphuric acid added is not appropriated by displacing a weaker acid from any saline combination present. The product if alkaline must be brought to the neutral state before the sulphuric acid is added, or more sulphuric acid added in order to ensure the 2 per cent. strength. The process of boiling is carried on for different lengths of time, according to the nature of the product dealt with. Information about this will be given when the products are being spoken of. When, as with cane sugar and derivatives of cane sugar, boiling for a few minutes only is required, this can be done in an open flask; but when boiling for a long time, as for one hour or an hour and a quarter, is required, it is necessary to use an inverted condenser to prevent loss by evaporation, which entails the risk of the higher degree of acid strength attained occasioning a destruction of carbohydrate. Observation has taught me the necessity of giving attention to this precaution. What, of course, is wanted is a complete conversion into glucose by the acid treatment. If such is not obtained a vitiated result is given. With some products, as will be seen further on, on account of the resistance offered to conversion, it is advisable to employ a high pressure boiler and raise the temperature to 300° F. (140° C.). After the conversion has been effected, the liquid must be brought to the neutral state before titrating with the ammoniated cupric test. This requires to be done cautiously. A strong solution of potash is used for the purpose, and care must be taken to have the liquid cold before adding the potash, and to see that the temperature does not rise to any material extent during the process, in order to avoid leading to a destruction of glucose. Potash at a high temperature very readily destroys glucose, and by not giving attention to the point referred to error may occur. Sulphate of soda is used for more effectually precipitating albuminous matters at a boiling temperature than otherwise can be done. It is necessary to give attention to the sulphate of soda being pure. The commercial sulphate of soda I have found to be so adulterated with carbonate as to effervesce on the addition of an acid; and this, if used, would entirely frustrate the object for which the agent is employed.

With these preliminary remarks I will now proceed to deal with the principles in detail.

GRAPE SUGAR.

Grape sugar is characterised by producing the same cupric-oxide reducing effect before and after treatment with sulphuric acid. If after contact with a ferment a product is presented which does not possess the same cupric-oxide reducing power before and after treatment with sulphuric acid, it must be inferred that the grape sugar has been transformed into another body. Hitherto the carbohydrates have been known to be transformable by increased hydration attended with the acquirement of increased cupric-oxide reducing power into grape sugar or glucose, which appears to occupy the position of a final product in the carbohydrate group. The results to be detailed will show that within the animal system, contrary to existing notions, transformation in the opposite direction occurs, and glucose can be made to move into a product of less cupric-oxide reducing power. By boiling with sulphuric acid this product is carried back again into glucose. A ferment exists in the stomach and intestine of the rabbit which has the property of moving glucose into a body of less cupric-oxide reducing power. This statement is substantiated by the following experiments. With saliva and the pancreas, I may first mention, no alteration has been observed to be produced.

Grape Sugar placed in Contact with Strips of Stomach and Intestine.

Experiment.—The stomach of a recently killed rabbit (at a period of digestion) was removed, emptied of its contents, washed and divided into two portions along the curvatures. One-half was then cut into small strips and placed into a beaker with about 50 cub. centims. of water, and 20 cub. centims. of a solution of grape sugar containing '138 grm. of glucose, and exposed for one hour and a half to a temperature of 120° F. (48·8° C.). The contents of the beaker were now boiled with some crystals of sulphate of soda to precipitate the albuminous matters and prepare for testing, filtered (through glass wool), washed, and the filtrate made up to 100 cub. centims., and divided into two equal parts. One part was titrated at once with the ammoniated cupric test, whilst the other was subjected to boiling for one hour and a quarter, with 1 cub. centim. of sulphuric acid (giving a 2 per cent. solution) in a flask affixed to a tube connected with an inverted condenser to prevent concentration through loss during boiling. When cold the contents of the flask were neutralised with potash, and brought to a known volume, and this liquid then titrated.

The two titrations showed a cupric-oxide reduction reckoned for the whole product equivalent to that producible by the presence of the subjoined amounts of glucose :—

Before sulphuric acid		'080 grm.
After ,, ,,		'134 ,,

Thus '138 grm. of glucose was taken, and by contact with the strips of stomach was converted into a body possessing a cupric-oxide reducing capacity equivalent only to '080 grm. of glucose. By boiling with sulphuric acid this body was carried back into glucose, and it will be observed that the amount of glucose discovered after conversion and reconversion was '134 grm., against '138 grm. started with. The cupric-oxide reducing power of the body produced was not far removed from that of maltose, the figures presented, '080 and '134, standing in the relation of 58 to 100, instead of 61 to 100, the figures for maltose. As a check the other half of the stomach was also cut into strips. These, however, instead of being used at once were first boiled with the 50 cub. centims. of water, and then the 20 cub. centims. of the solution of grape sugar containing '138 grm. of glucose were added. The beaker was exposed alongside the other for an hour and a half to 120° F. (48·8° C.). The same subsequent process of procedure as that which has been described was carried out, and the results yielded stood as follows :—

Before sulphuric acid		'130 grm.
After ,, ,,		'134 ,,

The figures here show that glucose existed. Hence with exposure of the strips to the temperature of boiling water previous to being placed in contact with the glucose no effect was afterwards exerted. The small intestine of the animal was also used in identically the same manner. All the products, indeed, belonging to the experiments were placed side by side and treated alike; as there was enough intestine for the purpose, a duplicate observation was made in

that part of the experiment where no preliminary boiling was resorted to. The same solution of grape sugar was used, and the same quantity—viz., 20 cub. centims., containing .138 grm. glucose. The results of titration yielded the following figures as representing the cupric-oxide reducing power of the product before and after subjection to the converting action of sulphuric acid expressed as glucose.

Intestine not subjected to Boiling before Employment.

OBS. 1. Before sulphuric acid	...	.086 grm.
After " "	...	.138 "
OBS. 2. Before " "	...	.086 "
After " "	...	.134 "

Intestine subjected to Boiling before Employment.

Before sulphuric acid	...	.130 grm.
After " "	...	.124 "

With the intestine, therefore, as with the stomach, no change was induced in the glucose where the strips were previously subjected to the influence of a boiling temperature. On the other hand, where the strips were employed before their activity was destroyed, the glucose was moved so as to make the product possess a reducing power (expressed in percentage relation) of 62, instead of 100, in the one case, and 64, instead of 100, in the other.

Experiment.—A similar experiment was performed with the employment of a larger quantity of glucose; 20 cub. centims. of a solution of grape sugar, containing .400 grm. of glucose, were used, and the time of exposure to 120° (48.8° C.) was two hours.

Duplicate observations were made in the case of the stomach and intestine taken previously to being boiled. The results obtained stood as follows:—

.400 grm. of Glucose in Contact with boiled Strips of Stomach (Rabbit).

Before sulphuric acid	...	.384 grm.
After " "	...	.370 "

.400 grm. of Glucose in Contact with the fresh Strips of Stomach.

OBS. 1. Before sulphuric acid	...	.232 grm.
After " "	...	.384 "
Relation 60 to 100.		
OBS. 2. Before sulphuric acid	...	.216 grm.
After " "	...	.384 "
Relation 56 to 100.		

.400 grm. of Glucose in Contact with the boiled Strips of Intestine.

Before sulphuric acid	...	.422 grm.
After " "	...	.370 "

.400 grm. of Glucose in Contact with the fresh Strips of Intestine.

OBS. 1. Before sulphuric acid	...	.222 grm.
After " "	...	.370 "
Relation 60 to 100.		
OBS. 2. Before sulphuric acid	...	.200 grm.
After " "	...	.384 "
Relation 52 to 100.		

A marked consistency runs through all these results, except to a certain extent in the figures given by the titration of the product from the boiled strips of the intestine before treatment with sulphuric acid. Here the result comes out rather too high, but absolute accuracy can scarcely be expected to be obtained in every instance. The little loss otherwise noted throughout from the .400 grm. of glucose used may not unnaturally be looked for.

No further comment with regard to these experiments is needed. Looked at as a whole they speak in unmistakable terms, and furnish a check within themselves.

(To be continued.)

A POLICE CONSTABLE who had been placed on fixed duty near one of the open sewer ventilators in the public road recently died in the Croydon Infirmary of typhoid fever, followed by pleuro-pneumonia, supposed to have been caused by his having inhaled the escaping gas from the sewer.

CASE OF MANIACAL DELIRIUM TREATED BY THE COLD DOUCHE.

By THOMAS BARLOW, M.D., F.R.C.P.

ON the evening of March 30th I was asked by Mr. Gabe, of Farringdon-road, to see with him a man who was apparently suffering from a severe attack of delirium tremens. He was thirty years of age, and a barman by occupation. He was said to be a "moderate drinker;" there was no history of any drinking bout immediately preceding this illness, but the alcoholic supply had been unlimited. He had not slept for three nights and four days; he had had many delusions, was much troubled with various objects in the room, more especially the bedclothes, which he was anxious to get out of his way. During the last twenty-four hours he had become much worse; it was doubtful if he recognised anybody; he had struck his wife and threatened to murder her; two men were required to keep him in bed. His bowels had acted, and he had taken a little food; but, as I have said, he had had no sleep, notwithstanding the administration of large doses of bromide, chloral, and morphia. The patient was an active, fairly developed young man, sitting up in bed when I saw him at eleven o'clock, with a wild, bothered look, muttering incoherently and pulling violently at a sheet which was in front of him. He was hardly in a mood favourable to investigation by ordinary methods. It was impossible to feel his pulse as he would not keep his hand still. On my making some slight attempts at physical examination, he told me he should "do for me" if I did not leave him alone. There was some tremor about his lips; he was much flushed; there was no rash; but his skin everywhere was dry, hot, and pungent to the touch. This struck me as noteworthy, because in all the cases that I could recall of very severe delirium tremens, the skin had been bathed in sweat. It appeared undesirable to order him any more narcotics, and my inclination would have been to let him alone, aiming simply at giving him food, but that evening I had read in THE LANCET¹ Dr. Broadbent's record of "two cases illustrating the successful employment of the cold douche." One of the two was a case of delirium tremens, and I suggested to Mr. Gabe that we should follow on the lines indicated by Dr. Broadbent. The bed and bedclothes were removed, and a large mackintosh coat was spread over the mattress, and over this a piece of sack-ing was placed. The patient was then stripped, and a spongeful of cold water squeezed down his neck and back, and then freely splashed over his face and chest. It quieted him immediately. He said it was nice, and he was well pleased to have it repeated. He was then vigorously rubbed and dried, and some strong broth was given to him, which he thoroughly enjoyed. During the rubbing process I noticed that he gave one short loose cough. The bed was soon made; there was not the slightest difficulty in getting the patient to lie down and close his eyes, and presently he dropped into a quiet sleep. We left his friends with the instruction that if he became violent again the same treatment was to be repeated, and that he was to have no physic, but only some egg and milk or broth whenever he should awake. His wife was also told by way of precaution that it was by no means infrequent in such cases (even when not treated by cold douche) to get pneumonia, and that therefore this was one of the things for which to be prepared. The patient slept quietly for four hours, then woke and was violent again. He was doused by the friends in the same way as before, and once more lay down and slept quietly. He had no delirium after this. A day or two subsequently, as I learnt from Mr. Gabe, the patient had a very slight cough and once expectorated a small quantity of blood-stained sputum. There was a little bronchial breathing at the left base, which soon cleared up. Thirteen days later, for my own information, I saw him with Mr. Gabe. He was perfectly rational; his tongue slightly granular. He had a good appetite, but looked thin, pale, and pulled down, like one recovering from a severe illness. His temperature was 99°. He had a very slight occasional dry cough, and then complained of a little pain, referred to

¹ March 24th, 1883.