counterbalances the above considerations, inasmuch as it is very largely responsible for the development of our existing hospital system.

Our own College had a large share in the initiation of a demand for better accommodation for the poorer classes. In the words of the celebrated Memorial moved by Dr. Anstie in 1874:

Physicians are brought much into contact with the poorer classes of the population, and are deeply interested in anything that concerns the welfare of these classes, not only on account of the poor themselves, but also because the evils engendered among them affect the whole of society It is well known that overcrowding, especially in unwholesome and ill-constructed habitations, originates disease.....

Rate-supported infirmaries and hospitals (I do not speak at the moment only of hospitals for infectious diseases) for the benefit of those whose living accommodation is too confined, when sickness appears in the household or family, are by some political economists regarded as a necessary result of the overcrowding of the population in large towns. It is from this point of view that the important subject of hospitals has lately been discussed. Messrs. Gordon Smith and Keith Young, architects of great experience and ability (the former of whom has for many years been the responsible adviser of the Local Government Board on the question of by-laws, plans, and buildings, hospitals, etc.), review the dwelling accommodation of the population of the United Kingdom. They show that with the great mass of the population of towns neither isolation nor decent nursing is possible, and that hospitals are therefore the necessary offshoot of the dwelling.

So also in country districts it is felt that hospitals are a necessity if the children of poor people are to have a fair chance of recovery from sickness. How impossible this must be under present conditions may be gathered from a recent report by the medical officer for Derbyshire (Dr. Sidney Barwise), who, in writing about the progress made in hospital provision, makes the following interesting and important statement:

In 1879, 296, or about one-fifth of the 1,510 provincial sanitary authorities, possessed some means of isolating cases of infectious disease, and that between that date and 1891, loans amounting to £448,769 had been granted to 100 authorities for the provision of isolation hospitals, so that in 1891 about 400 of the 1,510 authorities had made provision for their district, but more than two-thirds were without any means of isolation whatever. In some parts of the country not even that proportion of authorities are provided. For instance, in Derbyshire, out of a population of 426,768, only one district (Buxton), with a population of 7,500, has erected a permanent isolation hospital. That there is urgent need of them is apparent from the fact that over 5 per cent. of the tenements in the county have only two rooms, and between 10,000 and 11,000 families in the county occupy three rooms or less, so that the plea that there is not the necessity for isolation in country districts cannot for a moment be upheld.

We are, however, very far as yet from having reached the stage at which the general right to free hospitals can be said to be established, but in connection with isolation of infectious cases the problem may be said to have been worked out during the last twenty years. The infectious isolation hospitals of the metropolis and large towns are free to all cases; there is now no trace of the stigma of pauperism attached to their use; admission can be obtained for any sufferer provided there is a bed to spare.

The position of things has, therefore, undergone a wonderful change since 1872. The isolation hospital accommodation of this kind had only just been originated, and was limited to some thousand beds for a population of three millions. It is calculated that the present hospital provision of this kind for the metropolis amounts to 6,000 beds, considerably more than the 1 bed per 1,000 of the population, which is laid down by the Local Government Board as the standard requirement. With the multiplication of hospitals, due to the development of the isolation system, and especially stimulated by the introduction of the Notification Act in 1889, there has been a gradual disappearance of the signs of a pauper character attaching to the institutions. These have been replaced by the characteristics which distinguish our great voluntary hospitals, not the least important of which is the admission of students, who have the opportunity of regular courses of instruction in the hospital wards. In bringing about these great changes, the College of Physicians and the metropolitan Society of Medical Officers of Health had a large share.

In the lectures which I have had the honour to deliver be-

fore you I have endeavoured to describe impartially the evidence relating to the value of isolation, and at the same time to fairly represent its difficulties.

If the country generally should follow the example of the metropolis in respect of hospital provision, especially for infectious diseases, the isolation of which is, I believe, important to the public health, it will surely be spending money wisely and well. The evidence that I have submitted to you in the course of these lectures shows unmistakably that the lives of breadwinners depend on this new method of insurance; it shows that the health and strength of the rising generation must also in a large measure depend on the facilities for complete recovery which, in the case of the vast mass of the population, hospitals can alone afford.

THE FUTURE OF SANITARY ADMINISTRATION.

Human life is the most valuable kind of property. It is for "medicine" to say how it may be preserved in "public" as well as in "private" affairs of health. As I commenced these lectures by referring to the physician's influence and work in the politics of health, I will now conclude by quoting the words of a master on the subject:

For the further development of our sanitary institutions and their working, the educational onward influences may be expected to come from the members of the medical profession, and are perhaps not in any essential sense to be expected largely except from them. This is not meant only with regard to the abstract science of the matter, but equally with regard to administration. Regarding the science it has long been evident enough that for such new researches and observations as will supply increase to the body of exact knowledge on which the prevention of disease depends, the world cannot expect much help except from the work of the medical profession; and the same is now constantly becoming more and more clear in regard of Public Sanitary Administration.¹

THE PHYSIOLOGY OF THE CARBOHYDRATES.

A REJOINDER TO DR. PATON'S FURTHER CRITICISM.

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(Continued from page 586.)

THE question, in comparison with which all other points are of secondary importance, is whether it is possible for sugar, in accordance with the glycogenic theory, to reach the general circulation for transit to the tissues to be consumed in them, without an indication of such transmission being afforded by the urine. Experience shows that under normal conditions the urine is not influenced by the carbohydrate matter ingested; yet, of necessity, the carbohydrate is in some way or other disposed of. Carbohydrate matter which reaches the general circulation in the form of sugar gives evidence of so doing by passing into the urine. Since the carbohydrate matter of the food produces no effect upon the quantity of sugar in the urine, the natural conclusion is that the carbohydrate of the food does not reach the general circulation as free sugar. In diabetes the condition is the converse of that observed in health. The urine no longer remains uninfluenced by the carbohydrate matter of the food, but contains sugar in proportion to the carbohydrate matter ingested. As a corollary, since the urine is influenced, the blood of the general circulation must be influenced similarly, for the sugar which gets into the urine is derived from the blood. The fundamental difference between health and diabetes is that in the former the blood of the general circulation and, in consequence, the urine, escape becoming charged with sugar, while in the latter they do not escape. Obviously, therefore, a "stoppage action" must be exerted in the one case which is not in operation in the other. What is necessary in order to maintain the condition of health is the exercise of this "stoppage action."

First in my *Physiology of the Carbohydrates*, and subsequently in my *Epicriticism*, I have entered fully into a discussion of the conclusions which must be drawn from a consideration of the relation of the blood to the urine in connection with this question. To show in what position the matter stands I will here give a quotation from my *Epicriticism* (p. 59 *et seq.*):

Observation from every point of view shows that the urine affords an index of the state of the blood in relation to sugar. No one will dispute that the sugar of urine is derived from that previously existing in the

¹ *English Sanitary Institutions*, p. 473 (Simon).

blood. Conversely it may be said if the sugar exists in the blood it will appear in the urine, and it does so in proportion to the amount that is present. This is a well-established fact in connection with diabetes, the kidney constituting in reality the eliminating organs for the sugar, which is wrongly permitted to reach the general circulation. What applies in diabetes equally applies in health; and if from any circumstance whatsoever sugar reaches the blood of the general circulation, it will in proportion be discoverable in the urine. There is no tolerating capacity against elimination, as was at one time alleged. The erroneous notion arose from the elimination, when minute, being beyond the capacity or sensitiveness of the tests formerly employed to reveal it. With the tests now at our command, the sugar in the urine derived from the small quantity naturally existing in the blood is readily brought into view. There is no different kind of action with respect to sugar elimination in health and diabetes. The only difference is as regards the amount eliminated, and this, as I have said, is dependent upon the amount previously existing in the blood. It would be anomalous, indeed, if there were one law for a certain quantity, and another law for a quantity that simply happened to be less.

Upon these premisses it follows that it is permissible to look to the urine for affording us knowledge regarding the entry of sugar into the blood of the general circulation. Under natural circumstances the blood contains a certain small amount of sugar, which may be considered as constitutionally belonging to it, just as sugar, it would seem, belongs as an integral part to the other constituents of the body. Authorities are agreed that the amount of sugar in the blood is not a fluctuating one, or associated with the amount of carbohydrate ingested. Within ordinary limits both blood and urine remain uninfluenced by the character of the food in relation to carbohydrates. It matters not whether only the comparatively insignificant amount of carbohydrate existing in an animal diet, or the large amount existing in many kinds of vegetable food be ingested, the result is the same. But if in accordance with the glycogenic doctrine, the carbohydrate has to reach the systemic capillaries for disappearance in some unknown way, the position as regards the blood and consecutively as regards the urine cannot be the same. True, it is said that by the transformation into, and storage as, glycogen in the liver, means exist for regulating the transit into the blood, so that only a small quantity passes at a time—a quantity, it is alleged, which may be insusceptible of definite recognition in consequence of falling within the limits of liability to error to which the analytical procedure is open. No matter, however, whether or not there is any validity in this suggestion, the carbohydrate some time or other must undergo transportation. If the carbohydrate is ingested, and if it has to pass to the systemic capillaries for destruction, there must be within a given time a transit under a diet including farinaceous articles out of proportion to that occurring under a diet of animal food.

Now, the state of the urine constitutes, as I have pointed out, a very delicate indication of the passage of sugar into the general system. My daily experience in connection with diabetes for a large number of years has taught me that the slightest dietetic deviation on the part of the patient is immediately revealed through the condition of the urine. It is surprising with what exactitude the result occurs. Under the restricted diet for diabetes the urine with ordinary testing shall give no indication of sugar. Let there be a deviation to ever so small an extent, and within a very short time sugar will be discoverable, and discoverable in proportion to the extent to which carbohydrate matter has been taken.

The experiments I have given in my book on the introduction of sugar into the jugular vein and into the subcutaneous tissue speak exactly in the same way. Even with the employment of half a gramme per kilo. body weight a decided influence is producible.

How does this fit in with the glycogenic theory? The sugar discoverable in the blood during subsistence upon animal food (which in reality stands as representative of the sugar naturally existing as a standard constituent of the blood) reveals itself by the presence of a certain amount of sugar in the urine, but the large amount of sugar derivable from vegetable articles of food, and assumed to pass to the tissues for destruction, fails to reveal itself, inasmuch as no increase is observed in the amount of sugar present in the urine. Whatever goes to the tissues must also go to the kidney. It might only, it is true, pass continually in small amount, but nevertheless the fact remains that in a given time a certain quantity infinitely larger than that which could be derived from animal food would have to pass. Yet this is to happen without giving evidence of its doing so. Although the passage of a certain quantity is revealed, the additional quantity goes for nothing as far as indication of transit is concerned viewed through the urine.

From the amount of sugar escaping in a case of diabetes I drew an illustration of the amount which in health would have to undergo transit in the circulation from the liver to the tissues without the transit being revealed through the urine, were our carbohydrate matter conveyed as sugar to the tissues for destruction. When the patient was upon a diet including 16 ozs. of ordinary bread and other carbohydrate-containing food, the sugar eliminated amounted to 685 g.—rather more than 24 ozs.—in 24 hours. It must be remembered that the food which led to this result in the diabetic would have had no effect upon the urine in the healthy. Yet we are asked to believe that this carbohydrate can reach the general circulation in the form of sugar without the slightest indication of its transit being afforded by the urine. I then continued as follows:

Any system, it may be said, which involves the functional transit of carbohydrate matter as sugar to the tissues for destruction is negatived by the absence of evidence of this transit. When the carbohydrate matter of our food does, indeed, reach the circulation as sugar, it becomes conspicuously revealed by the presence of sugar in proportionate amount in the urine. Such entry, therefore, means glycosuria, and what is

wanted for the production of the state belonging to health is that the carbohydrate should be disposed of in a manner to be prevented reaching the general circulation as sugar.

I submit, then, that the evidence to be drawn from a reading of the information supplied by Nature is, through the train of reasoning deducible from it, absolutely fatal to the glycogenic theory.—Epicriticism, p. 63.

When once we have recognised that the entry of sugar into the general circulation is revealed by the state of the urine, we are placed in a commanding position, and find ourselves in possession of the key of the situation. Other considerations and the points of physiological detail upon which so much of a contradictory nature has been written sink into a subordinate place. In my *Epicriticism* I showed that the ground upon which Dr. Paton based an attack on the question of relation of sugar in the urine to that in the blood was untenable. Nevertheless, in his second criticism he returns to the point, and in spite of what I said about the effect of dilution of the urine having to be taken into account, as well as the percentage composition, in the consideration of the amount of sugar escaping, founds an argument simply upon percentage composition, notwithstanding he has fuller information before him. After what I have previously said about not considering it necessary to enter further into the general points of detail in dispute, I should not have gone out of my way to refer to the point in question did it not call for attention on account of its bearing on the subject under discussion. Dr. Paton writes as follows:

According to Dr. Pavy, whenever the amount of sugar in the blood increases, a proportionate increase in the urine takes place; in fact, the sugar (p. 63) "is simply filtered through the system, and escapes with the urine" (see also Croonian Lectures, p. 78).

Now Seegen finds in one case of diabetes 0.254 per cent. of sugar in the blood and 2.4 in the urine, and in another 0.314 per cent. in the blood and only 1.4 in the urine. Such results are clearly opposed to Dr. Pavy's contention. Even his own figures (Croonian Lectures, p. 80) fail to support his conclusion. In one case he finds 0.2789 per cent. of sugar in the blood and 4.549 in the urine, and in another 0.1848 per cent. in the blood and 4.811 in the urine.

The object with which these examples are adduced is to show that sugar may exist in the blood without escaping in proportionate quantity by the urine. Now it is obvious that the percentage of sugar in the urine will be influenced by the degree of dilution or concentration of the urine. We are concerned to know the amount of sugar which leaves the body by the urine, not simply its percentage in a particular specimen of urine. This gives information of a certain value, but not for the purpose for which Dr. Paton employs it. In support of this position let us look at the examples quoted by Dr. Paton. As to Seegen's results I need only say that as only percentage compositions are given, we have no data for checking the information supplied by them. Though most of the observations in Seegen's table are presented in this form, yet he gives four in which, together with the sugar in the blood, the sugar eliminated in the urine during twenty-four hours is shown. In the case of these it happens that, excluding one in which there is a considerable deviation from the rest, a striking consistency is observable in the relation of the sugar eliminated to that present in the blood.

With respect to my own results, fuller information is given than is the case with those of Seegen. I not only give the percentage amount of sugar in the urine, but likewise the amount of urine for the twenty-four hours which permits of the amount of sugar eliminated during the period, the real information required, being supplied. These figures stand in the table by the side of those representing the percentage amount. There are the right figures to take for the line of argument and the wrong. The wrong set fit in with Dr. Paton's requirements, and we find that they are taken. The right set oppose what he is endeavouring to prove, and are allowed to pass unnoticed. It is perfectly true that, looking at percentage composition, we have in one case 0.2789 per cent. in the blood, with 4.549 in the urine, and in the other 0.1848 with 4.811; but then the amount of urine passed in the latter case was about a third less than in the former, which would lead to increased concentration, and thereby a correspondingly increased percentage. If instead of percentage we take the sugar passed, the figures presented are 115.8 g. of sugar eliminated with 0.2789 per cent. of sugar in the blood in the one case, against 84.1 g. in the urine with 0.1848 per cent. in the blood in the other, relations which are in striking harmony with the view that the sugar eliminated in the urine is proportional to that existing in the blood.

I do not know whether I ought to attribute Dr. Paton's line of argument to a want of acquaintance with an elementary principle taught to every student when he is instructed to pay attention to the amount of urine voided in estimating the quantity of urea excreted, or whether this is another error from inadvertence, or whatever else it may be called, to be added to the list already given in my *Epicriticism*.

To argue against the relationship of sugar in the urine to that in the blood is to argue against that which is self-evident when in looking at the matter we take into account the unquestionable relationship existing in diabetes between the food taken and the amount of sugar passing off with the urine. Everyone knows that according to the food so is the elimination of sugar in diabetes. How can the carbohydrate ingested reach the urine except through the blood? If the relationship exists in the one case, it follows as a logical sequence that it must also exist in the other. I said upon this point in my *Epicriticism* (p. 117):

In addition to evidence of this kind, all experience in connection with diabetes conspires to show that the urine is most sensitive to, and speedily and proportionately influenced by, the ingestion of carbohydrate matter. For the urine thus to become influenced it follows either that the blood must become correspondingly so, or that the sugar must reach the urine in some mystic way without undergoing intermediary transit.

With the utmost confidence I reassert the proposition that according to the sugar encountered in the urine so has sugar pre-existed in the blood; and, conversely, in proportion as sugar is present in the blood so will it reveal itself in the urine, and this alike for the small quantities belonging to health and the larger quantities belonging to diabetes.

I do then in the most distinct manner affirm, and I am prepared to sustain the affirmation, that, in the words used by Dr. Paton, "whenever the amount of the sugar in the blood increases, a proportionate increase in the urine takes place." I further say that what is wanted to keep sugar, derivable from our food, out of the urine, is to keep it out of the blood, and that if the facts stood as they are assumed to stand under the glycogenic doctrine, the result would be universal diabetes. The difference between health and diabetes, in reality, is that in the one case the carbohydrate of our food is disposed of in such a manner as to prevent its reaching the general circulation in the form of sugar, whilst in the other the power of accomplishing this, in other words what may be spoken of as the assimilative power, is at fault. In proportion as it is at fault, so will the carbohydrate ingested be permitted to reach the general circulation as sugar, and thence the urine.

The fact that the experiments on the injection of sugar into rabbits showed that the sugar underwent modification, whilst within the system, does not affect the fact of elimination after introduction. The experiments simply show that in the healthy system there exists a power tending to convert carbohydrate matter into a lower cupric-oxide-reducing form. As I have already made known, I have found in some cases of diabetes that the sugar encountered in the urine has possessed a lower cupric-oxide-reducing power than that of glucose.

(To be continued.)

THE TREATMENT OF AORTIC VALVULAR DISEASE.¹

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THE treatment of aortic valvular disease recurs from time to time as a subject of somewhat uncertain cogitation in medical literature. So long ago as 1829² Dr. Hodgkin, writing on Retroversion of the Aortic Valves, inferred from his experience that depletion in these cases, whether by blood letting or by other means, among which he seems to have included digitalis, was unsuitable for these cases. Sir Dominic (then Dr.) Corrigan, in his classical paper on Permanent Patency of the Mouth of the Aorta, or Inadequacy of the Aortic Valves,³ contrasts aortic with mitral lesions, and speaks very strongly of the dangers of digitalis in the treatment of such cases, remarking that cases injured by its use

frequently recover on abandoning the drug and giving stimulants. Professor William Henderson, of Edinburgh, came to the same conclusion with respect to the use of digitalis in these cases, and agreed with Dr. Corrigan as to the cause of the unsuitability of the drug, namely, the prolongation of the diastolic interval. Dr. Easton, of Glasgow, to whose views Dr. G. W. Balfour, as recently as 1892,⁴ refers approvingly, contested Professor Henderson's views and endeavoured to maintain the opposite thesis, namely, that the longer the diastolic interval the less the opportunity for regurgitation. He appears to have ascribed aortic regurgitation to "contractions of the aorta." On this supposition he seems to have concluded that the more frequent such contractions were the greater was the regurgitation, and *vice versa*. Many subsequent observers whom it is not possible to enumerate on this occasion have either condemned or spoken hesitatingly of the benefit to be derived from the use of the digitalis group of remedies in aortic incompetency. Others again—a smaller body at present—have spoken with an emphasis in the inverse ratio to their number, as is the manner of minorities, of the unquestionable good to be derived from these remedies in these cases. Dr. Barrs, in a paper read before the Therapeutical Section of this Association so recently as 1891, maintained that there was no essential difference in the kind of failure in aortic and mitral disease, very justly held that digitalis could not be viewed as acting in one way in one case and in another way in another, and concluded that that drug was a valuable remedy in aortic valvular disease. It is almost too well known to be mentioned that the weight of Dr. G. W. Balfour's esteemed authority is cast in the same scale, but, as he very well observes,⁵ this is a question rather of "physics" than of "statistics." If, therefore, there be a difference in our conceptions of the physics involved, there will of necessity be a difference also in our interpretation of phenomena and in our views of treatment. That the mechanism both of failure and of relief differs in mitral and in aortic disease I shall endeavour with necessary brevity to maintain, and to explain, I hope satisfactorily, among other points the cause of the reliability of the digitalis group of remedies in mitral disease, and their still questionable reputation in the treatment of aortic, and especially of aortic regurgitant, lesions with lost or unattained compensation.

In the experimental investigation of the physics of the circulation, it is impossible to maintain in their entirety the conditions existent in the normal circulation, and this fact has an influence upon phenomena and upon deduction from an observation of them. Dr. Halford pointed out in 1860 (The Actions and Sounds of the Heart) that removal of the pericardium and evacuation of its contained fluid rendered tumultuous the beating of the heart which was acting smoothly and systematically, even after the chest was opened. From the days of Stephen Hales until now, the relations between artery and manometer have not been such as obtain in Nature, and the argument from quadrupeds to man as to the power of the heart and pressure in the blood vessels are fallacious, for those physical reasons which bring about a difference in the circulatory apparatus of animals habitually and respectively horizontal and erect. The latter is a point which I considered in some details in 1878, and Mr. Hill has recently read an interesting paper on the same subject before the Royal Society. Much more fallacious is it to assume that an experiment such as that by which Pascal burst a barrel is exactly applicable to the forces which tend to dilate the cardiac ventricle. It is in no spirit depreciatory of experiment that I make these remarks, but merely to emphasise the admitted fact that conclusions based on all such experiments have only an approximate value. It is true as Dr. G. W. Balfour and others have remarked⁶ that there is neither air nor a vacuum within the vascular system, but, in considering problems affecting blood pressure in the heart and blood vessels, sufficient emphasis has not, in my opinion, been laid upon the influence of respiration, and the consequence of the importation into the circulation of a greatly enhanced aspirative force after the commencement of aërial life. Not that the subject has been overlooked by physi-

¹ Read in the Section of Medicine at the Annual Meeting of the British Medical Association held in London, July-August, 1895.

² London Medical Gazette, vol. iii, p. 442.

³ Edin. Med. and Surg. Journ., April 1st, 1832.

⁴ BRITISH MEDICAL JOURNAL.

⁵ Diseases of the Heart and Aorta, 1st edition.

⁶ BRITISH MEDICAL JOURNAL, 1892, vol. i, p. 1132.