THE PHYSIOLOGY OF THE CARBOHYDRATES.

A REJOINDER TO DR. PATON'S FURTHER CRITICISM.

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THE issue of the BRITISH MEDICAL JOURNAL for February 1st contains a notice, from advance sheets, of Dr. Noël Paton's reply, in the February number of the *Edinburgh Medical Journal*, to my *Epicriticism*. As something requires to be said in response, I seek permission for my remarks to appear in the JOURNAL.

I am glad that Dr. Paton has published a letter I wrote to him in December, 1894, tendering him my thanks for a copy of the *Edinburgh Medical Journal* for December, 1894, which contained his original criticism of my book, *The Physiology of the Carbohydrates*. The letter was written on the day the publication reached me, and was founded upon the first impression conveyed to me by its tone and wording. Naturally time had not permitted me to do more than glance through Dr. Paton's article, and what I wrote shows the spirit in which I approached the consideration of the criticism. It was only when I came to consider the article in detail, and saw the misrepresentations which it contained that I became imbued with a feeling of resentment. I was forced to the opinion that by the use of unfair methods science was being trifled with. This was the ground for the condemnatory remarks contained in my *Epicriticism*.

Dr. Paton, in his reply, apologises for the misrepresentations I have pointed out in his previous article, and attempts to minimise them. As to what constitutes an incorrect representation, our ideas seem to be different, but I need not here enter into this matter, for by the words quoted in my *Epicriticism*, my position is placed beyond the range of doubt. The evidence, and my interpretation of it, is there, and everyone can form his own opinion.

Upon what ground has Dr. Paton assumed that my book was published specially for medical men and not for physiologists? I cannot allow this point to pass unnoticed. Dr. Paton referred to it in his original criticism, and now he writes:

But I did object, and do object most strongly, to his publishing a book for medical men, entitled *The Physiology of the Carbohydrates*, in which not a trace of the mass of evidence opposed to his views is considered. Had he published it as a contribution to the subject addressed to physiologists already familiar with both sides of the question, such omissions might have been pardoned; but in publishing the work *with such a title* [the italics are mine] he is dealing hardly fairly with the general medical reader.

Not a particle of evidence is afforded by the title of the book, by its preface, or by its text to support the assumption that it was addressed, as Dr. Paton suggests, to medical men and not to physiologists. It is, upon the face of it, a book on scientific questions falling in the departments of physiology—and therefore of medicine, for physiology is one of the institutes of medicine—of chemistry, and of vegetable physiology. Presentation copies were sent to foremost men in each of these branches of science. Dr. Paton did not receive a copy at the time of publication, owing to the accidental circumstance that I did not then know him, even by reputation, as a physiologist. But a few months later, and before I was aware of the criticism he was undertaking, I forwarded a copy to him.

The suggestion made is that I wrote for the medical man, who is regarded as standing in a position which renders him incapable to judge properly of what I am assumed to have prepared specially for him and not for others. Such a suggestion is simply grotesque. Does Dr. Paton expect any reasonable person to believe that I could be so misguided as to suppose for one moment that I could escape the deserved condemnation of those leaders of our profession who are also physiologists for making such an attempt at imposture?

Continuing the examination of Dr. Paton's recent article, I have to point out that what he says as to the extraction of sugar in the process of analysis applies to the work of all authorities, and would bring us to this position, that no re-

liable or valid evidence exists regarding the quantity of sugar present in the blood. Dr. Paton seems bent on upsetting everything in this department of physiology. The difference between my process of extraction of sugar and that ordinarily adopted consists in the employment of alcohol instead of water. Upon this point Dr. Paton wrote in his first criticism:

By Dr. Pavy's method of extracting the sugar by alcohol, two dangers are run—first, that all the sugar is not removed.—(Schenck, *Pflüg. Arch.*, Bd. xlv.)

This is all the reference then made to Schenck's work, and interpreting what was said to imply only a suggestion of imperfect extraction to which the alcoholic process was specially liable, I met the objection by stating, in *Epicriticism*, p. 38, that in my book "I enter in detail into the question of reliably securing the full extraction of sugar, and show by subsequent aqueous extraction that after the process recommended to be adopted no sugar is left. The matter was carefully considered by me many years ago, and dealt with at length in the *Proceedings* of the Royal Society for 1881. From this source I quote illustrative examples in my book." In his recent article Dr. Paton returns to this question, and founds much of his unfavourable criticism upon it; and I must therefore ask to be allowed to give full quotations with regard to it, more especially since it affords an example of the false position into which science is thrust by the system adopted by Dr. Paton of relying for the grounds of his arguments upon any recorded results he may come across in literature without taking the elementary precaution to pause and inquire into their accuracy and value. Dr. Paton writes as follows:

In regard to Dr. Pavy having shown, by subsequent aqueous extraction (p. 38), that all the glucose had been removed by his alcoholic extraction of blood and tissue, the results of Schenck must be borne in mind.

To the work of this observer I have already called Dr. Pavy's attention, but he still continues to ignore it entirely. Coming, as it does, from Professor Pflüger's laboratory in Bonn, it must be received with respect, and cannot simply be ignored as Dr. Pavy tries to ignore the admirable work (it is critically considered and discussed, pp. 125-28) of Panormoff, the "Privat-Dozent of the University of Kazan." It has a most important bearing on all Dr. Pavy's work, and is worthy of his careful study and consideration.

By adding weighed quantities of pure glucose to defibrinated blood, to serum, and to each of the two proteids of the serum, Schenck found—I quote his words—"that after coagulation only a part is to be found in the filtrate. If the coagulum has been washed with water or alcohol until the watery or alcoholic extract contains no more trace of a reducing substance, and the coagulum is then boiled with dilute hydrochloric acid, reducing substances may be recovered in the fluid, which, in successful cases, corresponds to the amount of sugar lost. It is highly probable from this that the glucose is combined [the italics are mine] with the albumins of the blood." In the course of the paper he indicates the difficulty introduced by the sugar naturally present. It must be remembered, too, that the deficits of sugar are not trifling, but amount to as much as 46 and 47 per cent. of the amount added, and the difficulty in the extraction of added glucose will also be experienced in extracting that normally present. Thus the failure to get sugar on subsequent aqueous extraction does not prove that all the sugar has been removed.

A distinct stand is here taken. I am reproved for not having before given attention to Schenck's work, and am told, that, coming as it does from Professor Pflüger's laboratory in Bonn, it must be received with respect; and that, further, it has a most important bearing on all my work, and is worthy of my careful study and consideration. I respond to the call thus made, and have given the work the study and consideration asked for. To give a full and adequate exposition I shall have to enter into detail, but time and space, I consider, will not be thereby uselessly expended, as a forcible illustration will be afforded of the sort of material relied upon by Dr. Paton in his endeavour to overthrow the work brought forward in my book. The point is one of fundamental importance in connection with the whole question under consideration, by exemplifying the kind of basis upon which his criticism is founded.

Schenck's experiments consisted of adding a known amount of sugar to definite quantities of defibrinated blood and serum, coagulating by boiling with water and slight acidification with acetic acid, filtering and washing, and then determining the sugar in the evaporated filtrate and washings by titration with Knapp's solution—a mercuric test. He also dealt with the separated proteids of the serum. No determination was made of the sugar contained in the blood employed, and this being omitted from the sugar reckoned to be originally present would carry the loss beyond what it is represented to be. Placed in a tabular form, the results come out as follows:

TABLE I.—(Schenck).

	Sugar added in grammes.	Sugar found in grammes.	Loss per cent.
I. 50 c.cm. blood of calf	0.260	0.143	45.0
II. 100 c.cm. " " " " " " " "	0.475	0.095	80.0
III. 50 c.cm. horse's serum	0.240	0.200	16.6
IV. 50 c.cm. " " " " " " " "	0.240	0.187	22.0
V.a. 50 c.cm. " blood	0.240	—	47.0
V.b. 50 c.cm. " serum	0.240	—	46.2
VI. 50 c.cm. globulin solution from 50 c.cm. of serum	0.240	0.212	11.7
VII. 50 c.cm. proteid solution, representing 1st fractional precipitate by ammonium sulphate from 25 c.cm. of serum	0.235	0.216	8.0
VIII. 50 c.cm. proteid solution representing 2nd fractional precipitate by ammonium sulphate from 25 c.cm. of serum	0.235	0.214	9.0
IX. 100 c.cm. globulin solution representative of 100 c.cm. of serum	1.000	0.926	7.4
X. 1 litre serum, filtrate and washings amounting to 15 litres, evaporated to a few c.cm., taken up in alcohol, evaporated and treated with lead acetate. Sugar by polariscope	2.77	0.94	66.7

I need not follow the details of the paper into greater length. It is sufficient to say that there is noticeable throughout a want of appreciation of the proper method of experimenting, and as a consequence an extreme inconsistency in the results.

Before proceeding further I must ask how it comes that Dr. Paton has given the representation he has done with reference to the extent to which Schenck failed in recovering added sugar. Dr. Paton says: "It must be remembered, too, that the deficits of sugars are not trifling, but amount to as much as 46 and 47 per cent. of the amount added." Is this a true representation? Certainly it is not the whole of the truth. When it is stated that the deficits "amount to as much as 46 and 47 per cent.," the conclusion which would naturally be drawn is, I think, that they did not amount to more. Why were the 80 per cent. and 66.7 per cent., noticeable in the above table, kept back? In an examination of the series of experiments the 80 per cent. had to be passed by. It may be said that losses of 46 and 47 per cent. were sufficient to create a feeling of distrust, but had the loss of 80 per cent. in one experiment been mentioned, would it not have at once produced on the reader the impression that there must have been something radically wrong with Schenck's methods? Loyalty to science should have prevented such an example of "inadvertence" as this. So, again, with regard to the sugar naturally present in the blood, Dr. Paton says: "The difficulty in the extraction of added glucose will also be experienced in extracting that normally present."

This, in fact, is the essential point in his argument, and yet, though he gives to Schenck's work so much prominence, no allusion is made to the experiment in the same paper in which the sugar naturally present in the blood was determined with the surprising result that only 0.005 g. were obtained from 50 c.cm. of blood. This would be a relation of 0.1 per 1,000 (weight per volume), a result which, standing as it undoubtedly does at variance with the results obtained by others, and having regard to the purpose with which Schenck's work is quoted by Dr. Paton ought certainly to have been mentioned by him as a matter of scientific loyalty, even though the statement must inevitably have had a damaging effect upon the value of the evidence upon which reliance was placed.

After reading Dr. Paton's paper, and finding the importance which he wished to attach to these results of Schenck's, my first step was to make similar experiments in order to ascertain what result would be yielded. The blood used was obtained from a horse slaughter-house, and was taken from the vein of an animal which, it so happened, had been killed a minute or two before my messenger arrived. Two portions of blood, each measuring 50 c.cm. were taken, and to each was added 10 c.cm. of a sugar solution (from honey), in which the amount of sugar had been determined previously by titration.

In one, the sulphate of soda process, in the other the alcoholic, was adopted for coagulation and preparing for sugar determination with the ammoniated cupric test. Separate analyses of the blood employed in the two experiments were made, and the figures as seen in the table below happened to come out with only a slight difference in the fourth decimal place. Upon this point it may be remarked that the products for titration were brought to different bulks, and nothing was known until the calculations were made of how the figures would stand.

TABLE II.—(Pavy).

	Recovery of Sugar from Blood. Grammes.				
	Original Sugar in Blood.	Sugar added.	Total Sugar.	Sugar found.	Loss per cent.
Sulphate of soda process	0.0595	0.3217	0.3812	0.3690	3.2
Alcoholic process	0.0597	0.3217	0.3814	0.3637	4.6

These results, it will be observed, stand diametrically opposed to those of Schenck, and show that under the adoption of a proper method of procedure sugar added to blood is to be recovered with only a comparatively insignificant amount of loss. Part of the error in Schenck's experiments was evidently due to the fact that simple washing of the coagulum is insufficient to extract the sugar. The coagulum, as a matter of fact, requires to be repeatedly washed and squeezed. Where the proteids were employed, and the coagulum was much less in bulk, there was much less loss. But more error than that arising from defective removal by washing must have occurred to account for the enormous discordance presented. Look at Experiment X, and see if it is to be wondered at that sugar was missing. No less than a litre of serum was taken, which yielded so unwieldy an amount of coagulum that it had to be dealt with in three portions; 15 litres of filtrate and washings had to be evaporated down, and this, we are told, was done over an open fire (flame?) till a small bulk was attained.

I venture to assert that no physiologist accustomed to practical work could read Schenck's paper without being at once struck by the fact that his experimental methods were obviously faulty, and his conclusions from them therefore worthless. Yet this is the paper which Dr. Paton has not hesitated to pick out and to say of the experimental material, opposed to my views, which it contains that it "must be received with respect."

No sooner was Schenck's paper published than a stir was made against it. The volume of *Pflüger's Archiv* containing it must have been published in the early part of the year 1890. In the *Centralb. f. Physiol.*, vol. 4, No. 1 (April, 1890) Röhmman attacked it, and in the number for July an attack upon it was also made by Seegen. In our own country Dr. Vaughan Harley attacked and condemned it in the *Journal of Physiology* in 1891. Schenck himself, in the succeeding volume (*Pflüg. Arch.*, Bd. xlvii) to that in which his original paper was published, unreservedly acknowledged his work to be unsound. To this acknowledgment I will next direct attention.

(To be continued.)

MEDICAL MEN AS WITNESSES.—A Bill has been introduced in the United States Senate providing that in the District of Columbia no physician shall be permitted, without the consent of the person affected, to testify to any facts coming to his knowledge in his professional capacity, and which were necessary to enable him to act in that capacity, whether such information shall have been obtained from the patient or his family or from the person or persons in charge of him. This is not to apply to evidence in criminal cases, and the disclosure shall be required in the interest of public justice.

Messrs. Arch. Constable and Co. announce a new edition of *The Death of Lord Nelson, October 21st 1805*, by William Beatty, M.D., Surgeon of H.M.S. *Victory*.