some specimens are converted by ferments into sugar with great rapidity, while others are only changed by them after the lapse of hours, although these very specimens may be at once changed into sugar by boiling them with acids.*

It is to be observed that the liver is not the only organ in the body which contains glycogen, although it is the chief one. The muscles also contain this substance;† and it is found in considerable quantities wherever cell-growth is actively going on, as, for example, in foetal structures, or in the inflamed parts of the lungs in pneumonia. It is possible that it is first formed in the liver, and is merely carried to these other parts; but as it is readily changed into sugar in the blood, it seems much more likely that muscles and young cells possess also, to some extent, glycogenetic powers, and that the glycogen they contain is actually formed by them from sugar, or peptones supplied to them by the blood. The sugar they receive may either be derived from glycogen in the liver, or directly from intestinal digestion, for it is almost certain that, although a great part of the sugar and peptones is stopped by the liver, the whole of them is not.

Diabetes, from imperfect glycogenesis, then, is to be ascribed chiefly to the liver; but the possible participation of the muscles is to be borne in mind.

A LECTURE

ON

THE PROPER METHOD OF TREATING TAPEWORM.

Delivered at the Middlesex Hospital, Wednesday, December 10th, 1873.

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GENTLEMEN,—The treatment of that common form of helminthiasis technically called "tapeworm" unfortunately constitutes one of those departments of the healing art, about which people generally suppose that little or nothing need be said. Excellent practitioners will tell you that the administration of turpentine and castor oil, or the giving of half a drachm of the ethereal extract of male fern, suffices to dislodge the parasite; and, should the worm unhappily return—which is usually the case under such a rough-and-ready mode of treatment—all that you have to do is to repeat the remedy until the entozoon is finally destroyed. This kind of advice has the merit of being delightfully simple; but I have no hesitation in saying that, in the majority of cases, the practical results following its adoption are eminently unsatisfactory. I am not stating that the particular drugs in question are inefficient; on the contrary, viewed as tæniacides, I believe them to be superior to koussou, areca-nut, and other more or less well-known vermifuges. What I contend for is that, although in a small proportion of cases thus treated you may have the good fortune to be successful, yet, in by far the greater number of instances, the parasite will return, because neither the head nor even the neck of the worm will have been dislodged.

If I read the moral of professional conduct correctly, it should be our endeavour to effect cures as speedily as possible, thereby preventing the recurrence of the disease. The ordinary mode of prescribing is, to say the least, slovenly, and it fully accounts for the very incomplete results that are obtained. If it be urged that I ought to produce evidence of the truth of what has just been stated, I can only point to my own experiences in this connection, and assure you that nine out of every ten cases in which my opinion has been solicited have been cast-off or rejected ones, so to speak. I mean that they have been cases where the patients have long previously undergone treatment, experiencing only partial relief.

As observed in one of my former lectures delivered at this hospital, some persons think it fair and honest to record all cases as successful where the body of the parasite is eliminated from the bearer. Thus, one practitioner, desirous of extolling the merits of turpentine, had the politeness to inform me that this drug, with its appropriate aperient combination, "never failed to bring away the worm where it had been known to have been present." This statement was based on experiences obtained by the treatment of thirty or forty dispensary cases. The inadequacy, however, of conclusions drawn from such a source must, I think, be obvious, for in no single instance does it appear that

the head of the parasite was obtained, or even that it was so much as sought for by the medical attendant. On grounds, therefore, of this order, I maintain that it is unfair to speak of such cases as cures, and it is, further, an entire mistake to characterise the results thus obtained as successful.

Undoubtedly, now and then, cures have been effected without finding, or without any endeavour on the part of the practitioner to find, the head of the parasite; but I hold that no medical man is entitled to pronounce his patient as absolutely cured, unless he has dislodged and produced the head from the matters discharged, after and in consequence of the advice and remedies offered and administered. It may be, indeed, that the head, though expelled, escapes observation, however diligently sought for; and it is quite certain that much practical experience is often necessary to enable the searcher to find the small isolated and detached head in the midst of dark-coloured faecal matters. Repeatedly, I have obtained it after hours of search.

Again, in proof of the statement which I have advanced, respecting the comparative unfrequency of cure by the ordinary methods, I may mention that not a few of the numerous cast-off cases to which I have adverted have been those of patients who had undergone more or less severe drugging at intervals extending over a period of several years; moreover, as bearing on the question of diagnosis, it is also my duty to observe that patients have come to me for advice about their tapeworm disorders under far graver circumstances. I mean, that they have been subjected to the trying discipline of taking nauseous and drastic vermifuges for the expulsion of tapeworm and other parasites which, to my certain knowledge, never had any existence, excepting either in their own imaginations or in those of their medical advisers. This is a painful statement to have to make, but it is nevertheless strictly true. One of the most remarkable cases of this description is already recorded in my published *Lectures on Practical Helminthology*, case xxiv, p. 44. Others of a similar kind are given in the volume referred to, where it will be seen that the majority of such purely imaginary cases of tapeworm occur either in overworked men or in hysterical women. Certainly, they are amongst the most painful cases that we have to deal with; but, taken in connection with the phenomena of true parasitism, they have served to convince me that special knowledge on this subject is not without its practical value.

To return to the particular point previously urged, I have further to remark that, with more care in the exhibition of the ordinary remedies backed by a more thorough appreciation of the general economy and modes of development of the human tapeworms, a much larger professional success will be obtained. Lengthened experience justifies me in making this statement. I can say, indeed, that, during the year now closing, it has been my good fortune to obtain unusual success in the treatment of tapeworm. Some of these helminthiasis were regarded by the patients not merely as obstinate, but as incurable cases. However, to cut the matter short, let me say that, in all instances, without exception, where I have this year had an opportunity of actually examining the faeces and other matters passed by stool, as a consequence of remedies prescribed by myself, I have succeeded in procuring the head of the worm. In every instance, therefore, I have been enabled to assure my private patients that they were permanently and absolutely cured. This success, unique, perhaps, of its kind, of course only holds good in the case of those persons who have properly placed themselves under my personal care. In the more numerous instances where my opinion has merely been sought, I cannot tell what results may have followed the advice given.

Here, it is only just that I should likewise remark that, in all the cases above referred to, I have had to deal with the very prevalent beef-tapeworm. Had it been otherwise, a similar degree of success could not possibly have followed. It seems to me that the pork-tapeworm (*Tenia solium*) becomes rarer and rarer, whilst that derived from beef (*T. mediocanellata*) is extremely common. I am, indeed, informed by a practitioner of great experience that, of late years, the tapeworm disorder has very sensibly increased in Somersetshire, and it would not surprise me to learn that such was the case in other counties. Be that as it may, I can now only find time further to observe that, in the diagnosis and treatment of tapeworm, there are many other practical points which I should have been glad to have brought under your notice. For these, however, it must suffice me to refer you to the work already quoted.

By way of bringing the subject to an appropriate conclusion, I will mention one interesting case which came under my care last July. It was that of a gentleman who had carried the cestode entozoon about with him for a period of sixteen years. Previously to my seeing him, he had been repeatedly dosed with male fern, as much as one drachm and a half of the extract being taken by him at a single dose. The body of the tapeworm had several times come away; nevertheless, the

* Kühne, *Lehrbuch der Physiologischen Chemie*, p. 63.

† Bernard, Kühne, and M'Donnell; Kühne's *Lehrbuch der Physiologischen Chemie*, p. 307; Nasse, *Pflüger's Archiv*, vol. ii, p. 97.

patient was not cured. Like some other patients I have seen this year, he not unnaturally despaired of my ever finding the head. It appeared to him absurd that I should obtain better results than others; and the more so, since I proposed to employ the same drug that others had used. However, an admirable preparation of the extract, prepared by Corbyn and Co., had the desired effect. Recommending my patient to observe certain preliminary rules, I succeeded with much smaller doses of the drug than he had been accustomed to take. To be brief, the head of the worm was at once obtained; and thus, host and guest, after sixteen years of intimacy, were for ever separated, to the astonishment of the former and to the final discomfort of the latter. During the residence of this truly obstinate parasite, I reckon that my patient must have been the means of distributing something like five hundred millions of tapeworm germs; and yet, possibly, scarce a score of these germs have since arrived at sexual maturity. Be that as it may, I judge that in the course of a few more years the parasite itself would have died a natural death. It was an ill-nourished specimen, and evidently feeble. How long a tapeworm, if left to itself, may be capable of living I do not know; but I lately saw a patient who assured me that he had played the part of tapeworm-bearer for a period of no less than twenty years.

ON THE TREATMENT OF *POST PARTUM* HÆMORRHAGE.

By T. SNOW BECK, M.D.Lond., F.R.S.

I HAVE no doubt Dr. Robert Barnes exercises a wise discretion "not to enter upon any discussion which might bear the semblance of controversy," knowing, as he must, that his clinical experience, when he ventures upon the record of cases, has ill borne the test of practical examination. In the course of last year, when going over the subject of retro-uterine hæmatocele, I had occasion to examine twenty-seven cases, published by Dr. Barnes, where "effusion of blood into the peritoneum" was said to have occurred through the Fallopian tubes during menstruation; yet in not one of these twenty-seven cases was there sufficient evidence to show that any effusion of blood had ever taken place. (*Obstet. Trans.*, vol. xiv, p. 260.) And, unless I am much mistaken, his clinical experience on the subject of the injection of the perchloride of iron to arrest *post partum* hæmorrhage will prove as little to be relied upon. During the recent discussion upon the subject at the Obstetrical Society, I adduced evidence to show that the employment of this substance was fraught with the greatest danger—that fatal results had followed the employment of it in several cases—that the deaths had been caused by the transmission of the perchloride through the pervious canals of the uterine veins into the general system, producing blood-poisoning. And I would not have now returned to the subject had not Dr. Barnes, in some recent remarks, referred pointedly to this discussion "to point my (Dr. Barnes) assertion that the 'evidence adduced' cannot be trusted."

If I rightly apprehend the subject under discussion, it is not whether *post partum* hæmorrhage has been successfully arrested by the injection of a solution of perchloride of iron into the cavity of the uterus, or whether fatal cases have occurred after this substance had been so injected, for there cannot be a doubt that both these results have followed this plan of treatment. But the moot point appears to be, whether the injection of strong styptics into the uterus directly after parturition is, or is not, the correct principle of treatment. For it must be remembered that styptic injections have been used, not only to arrest *post partum* hæmorrhage, but also to anticipate the occurrence of it when no hæmorrhage was present. To advocate this "new plan of treatment," Dr. Barnes asserts that the "old way" of arresting hæmorrhage by procuring contraction of the gravid uterus is not to be trusted; that the means we have been accustomed to depend upon are insufficient, and frequently useless; and that it becomes necessary to adopt "a new principle," a "new power," to chemically seal up the mouths of the vessels and corrugate the inner surface of the uterus, by the direct application of styptics to the bleeding surface. I, on the contrary, venture to maintain that the whole of these assertions made by Dr. Barnes are erroneous; that the only principle by which we can arrest *post partum* hæmorrhage is by procuring the contraction of the uterus; that the "good old ways" are sufficient for this purpose, when properly and efficiently applied; that the injection of styptics into the gravid uterus neither seals up the mouths of the blood-vessels, nor corrugates the inner surface of the uterus; and that this practice involves the serious risk of causing the inevitable death of the individual upon whom it is employed.

It is evident that these questions involve the consideration of the whole subject of the treatment of *post partum* hæmorrhage, which would be much too lengthy for the pages of this JOURNAL; but it may not be improper to advert to one or two points which appear to be causing considerable interest. Everyone will admit the remark of Dr. Barnes, that these practical questions cannot be determined by "the subtle argumentations of Zoilistic critics"—though many may doubt the good taste of anyone who thus compares himself with the immortal Homer—and that these questions "must eventually be settled by the results of clinical experience." But the value attached to clinical experience presupposes accurate observation at the bedside, and correct deductions drawn from the cases observed; whilst imperfect observation and unfounded assertions cannot be accepted in the place of the former. For it is equally true in medicine as in other things, that "there is no untruth so misleading and so mischievous as a half-truth."

Referring to Dr. Boyd Mushet's remarks, which Dr. Barnes considers "to express some current errors as to fact and theory," I may say that I entirely endorse them. Dr. Mushet may commit a slight mistake in supposing that others have not used the injection of cold water; but he is not alone in attributing much value to the injection of cold water freely into the uterus, and in having no compunction as to any ill-effects which may follow the use of it. When conversing with some of the most successful practitioners in midwifery, I have been surprised how many had been led to adopt a similar plan in their own practice—to administer one or two doses of ergot of rye when the head was low down in the pelvis, and no obstacle hindered its exit; and to douché the cavity of the uterus with cold water thrown up by a long elastic tube attached to an India-rubber bottle, capable of holding about one pint. Dr. Sells of New York lately informed us it was the practice in the Vienna Hospital that, provided ergot and the injection of cold water did not arrest any bleeding, a weak solution of the sesquichloride of iron was gently injected, and repeated until the hæmorrhage ceased. And, in a communication which I have received from Dr. Hermann Biegel, within the last few days, he confirms this account, adding, "This solution of the sesquichloride is too weak to have any effect; it is the cold water which acts."

But Dr. Barnes has undertaken to correct the errors in "fact and theory" into which we have fallen, for I endorse Dr. Mushet's remarks. He terms the local application of cold water and ice to the interior of the uterus, by which it was "struck with a sudden chill," as "a graphic clinical description of reflex action." Reflex action, when the uterus itself was struck with a sudden chill by the local application of ice. In the desire to correct the errors of others, Dr. Barnes appears to forget there is such a thing as local impression produced by the direct application of cold. Reflex action, he elsewhere informs us, may be excited by "flapping the abdomen with the corner of a wet towel," which "is the most certain to evoke contraction," and which, I would add, would be equally effective if another and more posterior part of the body were subjected to a similar treatment. But, in this "graphic clinical description" of reflex action, he appears to have forgotten that "enormous supply of nerve-force," which he informs us, in his Lumen lectures, is necessary to complete the work of parturition, and which is of such essential importance as to require the pregnant female to become afflicted with "physiological hypertrophy of the spinal cord" during each pregnancy; and, as this "physiological hypertrophy" is said to be "analogous to that which takes place in the heart," it necessarily follows that a woman who has been six or eight times pregnant, must have "physiological hypertrophy of the spinal cord" six or eight times increased. It does seem "the unkindest cut of all" that Dr. Barnes should forget this idol of his own creation, and be led away by "a graphic clinical description" to attribute the local effects of ice applied to the interior of the gravid uterus—to reflex action.

It is a curious fact which has been brought out by these discussions, that there is only one cause of *post partum* hæmorrhage—the want of contraction, or relaxation, of the uterus; and one means by which this hæmorrhage can be arrested—by procuring the contraction of the organ by which the arteries are compressed, and the flow of blood prevented. This want of contraction usually arises in two ways—either the contents of the uterus are removed in the absence of contraction, or relaxation occurs after the completion of the labour, the hæmorrhage coming on fifteen or twenty minutes afterwards. I am not unmindful that so able and practical a physician as Dr. R. Gooch has described "a peculiar form of hæmorrhage" which he believed to take place when the uterus was contracted, and to be caused by the violent action of the heart and arteries. But a reference to the cases published by Dr. Gooch shows that, in each case, he passed the hand into the uterus, which he could not have done unless the organ had become relaxed after the completion of the labour. The examination of the uterus in cases which have proved fatal, after the injection of the perchloride of iron, has also established