

diffusible, we have a method by which the requirements for colloid absorption can be delicately adjusted. It has been suggested by Woolley¹ that the non-diffusible colloid is converted into a diffusible crystalloid.

In Graves's disease the changes in character of the vesicles—viz., the tubular structure, the infolding of the walls, and the high columnar epithelium—can be largely explained by an absence of distension. The glands, however, show indications of hypertrophy in that they are larger and more numerous. The excessive absorption of colloid can be explained by two factors, an increased diffusibility of the colloid and an increased blood-supply. The former is probably the more important.

In a previous paper² I pointed out that the changes in the thyroid gland in some cases of primary Graves's disease are focal in character. The distribution, to go back to the analogy of the lungs, is like that of a broncho-pneumonia. The areas affected vary from the size of miliary tubercles to nodules half an inch in diameter and larger. It is impossible to explain such a distribution of the changes by any theory which attributes the condition to alterations in the blood or nervous system, for it is evident that such alterations would affect the whole of the gland tissue or, at any rate, that of one lobe. We are left, therefore, with the explanation that at the centre of each of these foci there is some substance being formed which renders the colloid diffusible and possibly increases the blood-supply locally. Associated changes often found in the centre of these areas are lymphocytic and plasma-cell accumulation. In some cases these are slight, in others marked, and they are similar to the changes of chronic inflammation.

It is suggested that these points indicate an agent as the cause of Graves's disease which can lodge in capillaries and set up changes in the surrounding glands leading to absorption of colloid. The toxic action of such an agent must be slight. It is possibly a micro-organism. Other facts suggesting the same idea are the enlarged glands of the neck, the lymphocytosis, and the well-known fact that many cases of Graves's disease end with myxœdema. I have been endeavouring to test whether the gland in cases of Graves's disease contains a ferment which can render colloid diffusible. The experiments are not yet completed. The advisability of inoculating suitable animals—e.g., monkeys—with Graves's disease thyroid should be considered.

I am, Sir, yours faithfully,

HELEN CHAMBERS, M.D. Lond.,
Clinical Pathologist, Royal Free Hospital.

Feb. 24th, 1912.

To the Editor of THE LANCET.

SIR,—In your excellent abstract of the discussion on Partial Thyroidectomy and Local Anæsthesia (i.e., analgesia) my remarks on the employment of ether in such cases are not given quite correctly. May I extend your abstract?

I said that provided atropine was injected before ether was used, that anæsthetic was suitable for many of the cases. My experience with the open method of etherisation and of intravenous infusion of ether has led me to this conclusion, but as I pointed out the employment of ether by *any* method is open to one objection. The stimulation of the ether gives a spurious appearance of well-being to the patient whose real condition is thus masked. The operator may be misled and induced to undertake a more serious surgical procedure than he originally contemplated. The patient presents such a favourable aspect that even against his better judgment he continues his work, anxious as always to do the utmost for the patient.

When the ether administration is stopped collapse sets in and in some cases the patient cannot withstand the depression and sinks. If chloroform is employed in the way I suggest both the anæsthetist and the surgeon are better able to estimate the degree of operative shock, and the latter can restrict his procedures to the immediate necessities of the case. There should be no post-operative shock due to the anæsthetic unless over-dosage or asphyxial complications have been allowed to complicate the normal course of narcosis.

I am, Sir, yours faithfully,

DUDLEY W. BUXTON.

Mortimer-street, Cavendish-square, W., March 2nd, 1912.

¹ Woolley: Bulletin Johns Hopkins Hospital, February, 1912.

² Observations on the Pathology of Innocent Goitre, Brit. Med. Jour., Sept. 25th, 1909.

THE PRESENT POSITION OF SALVARSAN.

To the Editor of THE LANCET.

SIR,—In view of the controversy at present raging as to the benefits to be expected from the arsenical treatment of syphilis one is forced to ask the question, Is it really as efficacious and curative in the *long run* as mercury and should we discard the latter drug as the routine treatment? I have had 29 years' experience of treating syphilis and conscientiously believe from the proved results of my treatment of many scores of cases which have been under my care from start to finish that when mercury is properly and adequately administered it will actually cure syphilis. I am in touch with many patients who were infected and treated years ago. I know them, their wives and children, and in a few instances the grandchildren. They are all free from any clinical signs of the disease and in those cases which have allowed a Wassermann to be done they have given negative results. My observations refer to patients who were infected from 15 to 40 years ago, or even longer. I think that one should be chary in expressing a dogmatic opinion upon the *cure* of a case of syphilis until time has assisted one to that end. When one knows from practical experience what can be done with mercury one does not feel inclined to entirely discard it in favour of another preparation which, regarded from the standpoint of time, cannot be said to have stood as lengthily a "curative" test. That arsenic is of undoubted value in certain cases there can be no question. But is it advisable to use it as a routine treatment, and can it be truthfully said to the patient that one will eradicate and cure syphilis by its use *alone*, or advise that it should be regarded as the premier drug?

In my experience one might almost say that there are two types of syphilis: that as seen in the better-class private patient, in contra-distinction to the disease as met with in hospital cases. I never see in private the gross ravages of the complaint which are so common in institutional clinics. Moreover, in the private cases after, certainly, six weeks' treatment it would be difficult for anyone to assure himself that the patient had got or had ever had syphilis, and if they will only follow out instructions they usually never have any further sign of the trouble. The hospital case, owing to loss of time in attending, which usually means loss of work and wages, is too frequently content to stop treatment with the disappearance of symptoms and does not seek further advice until a relapse compels it to do so. Their syphilis simmers within them. Mercury has received much unmerited blame, partly from the way it has been tendered and accepted. One meets with cases who complain that they have been under Mr. So-and-so's care for two years and are not yet well! Cross-examination reveals the fact that they may have visited the surgeon in question twice or thrice. They certainly have not accepted the proposed treatment or obeyed as regards alcoholic moderation. Consequently, they blame the surgeon and the drug. Again, I have had cases which were treated as syphilitic with mercury and no beneficial result ensued—for this reason, they were tubercle.

Certain cases of syphilis react magically to arsenic, whilst others do not respond satisfactorily. The same may be said of mercury. That arsenic does clear up certain specific lesions with remarkable rapidity is common knowledge. But mercury will do ditto. Hospital practice is one thing and private practice another. The better class, educated patient is apt to ask awkward questions and to want some guarantee as to the effect and results of proposed arsenical treatment. One of the usual questions is, Is it quite safe, no risk, danger, or ill after-effects? Well, I take it that such a question requires a "qualified" answer. The next is, Can you assure me that the effect of the injection will be a permanent cure and that the disease will not break out again? Of course, one must answer that query according to one's experience *and conscience*. We know but little about the propagation or multiplication of the spirochæte within the human body, whether it does so, for certain, by fission or sporulation. Certain microphotographs would lead one to presume fission, others sporulation. However much we know about the effect of arsenic or mercury on the mature spirochæte, I do not think we can speak otherwise than theoretically as to their action upon the presumed embryo, the subsequent maturation of which is conceivably the explanation of a "relapse." It has been suggested

that one should give one or two intravenous injections of arsenic so as to clear off the existing spirochætae and then to follow up with a more or less prolonged mercurial régime. But if mercury is really necessary to effect a cure, why not start with it and continue with it to the final medication of the case, and not subject the patient to even the small percentage of risk which would seem to attend the intravenous injection of arsenic for what seems to me to be a transient and somewhat problematical benefit?

I have no hesitation in saying that mercury when adequately and judiciously administered cures syphilis. I would think twice before making such a dogmatic statement in regard to arsenic. It would now appear that aural symptoms are not uncommonly met with during the early stages of syphilis, or rather since arsenic commenced to displace mercury in the treatment of the disease. I would like to apologise to the many students to whom I have taught the rudiments of syphilis for not having mentioned this seemingly common symptom, for the simple reason that I was unaware that it occurred.

I am, Sir, yours faithfully,

March 4th, 1912.

CAMPBELL WILLIAMS.

THE SIGNIFICANCE OF THE SYMPTOMS IN CASES OF DUODENAL ULCER.

To the Editor of THE LANCET.

SIR,—I desire to point out that the explanation put forward by Mr. C. Mansell Moullin in THE LANCET of March 2nd of the group of symptoms which he associates with duodenal ulcer is really no explanation at all, and incidentally to protest against the tone of his references to the views held by physicians on this subject. He says: "It is not consistent with scientific work to assume that a thing does not exist because it has not yet been seen," but it is at least as unscientific to assume that a thing does exist until it has been seen. His contribution to the problem is that the symptoms hunger-pain, pyloric spasm, hæmorrhages, and hyperchlorhydria are caused by an intensely hyperæmic and hyperæsthetic state of the mucous membrane, and in illustration of this he actually cites the case of the interior of the stomach of Alexis St. Martin.

The description to which he refers was given by Beaumont as follows: "The inner surface of the stomach showed extensive erythematous livid patches, from the surface of which exuded small drops of grumous blood, large and numerous aphthous ulcers, the whole covered with thick mucus"; but if I am not misrepresenting Mr. Mansell Moullin in supposing that this is the condition to which he refers the symptoms just related, it is noteworthy that Beaumont expressly states that St. Martin suffered no pain and had a good appetite, so that in the one case to which Mr. Mansell Moullin appeals in which the alleged condition was present there were no such symptoms as those for which it is now given as the suggested explanation.

I am not one of those who regard hyperchlorhydria as a satisfactory explanation of hunger-pain, but I protest against Mr. Mansell Moullin's language where he suggests that it is used by physicians as "a long word which hides our ignorance from our patients," followed by the remark that "the days of the early Victorian physician have not gone by yet," although I confess I do not understand what he means by that, except by using the words "early Victorian" in the contemptuous sense in which we hear this qualification applied to dress and furniture of that period which does not happen to satisfy the modern taste.

I suffered from hunger-pain for almost the whole of the first half of my life; it had no relation to "cold" or catching cold, I never had "jaundice," I never suffered from "septic poisoning," I certainly had it in childhood and up to the age of 35 or 40. It was distinctly brought on by fatigue and by excitement; for example, in my early manhood I always had a bad attack after making a speech; it was relieved by alcohol, a circumstance that I discovered on one occasion, but having a horror of such a remedy I never repeated it; I could always relieve it by a small dose of morphia; it was also relieved by a large meal, but not by a small one; in fact, small quantities of food seemed often to make it worse. I was undoubtedly neurasthenic and often suffered from overwork, and to this alone I attribute the symptom. I call it gastralgia for want of a better name, and I think it is

possible that Soupault was right in attributing the actual mechanism to a spasm of the pylorus, but I have no doubt of its essentially nervous origin, and I have treated many cases successfully on the principles suggested by my own condition.

As one who has welcomed the assistance of surgeons and has fully admitted the great advance in the treatment of stomach diseases which has followed from that assistance, I regret that any surgeon should allow himself to adopt such an arrogant attitude towards his medical colleagues as is displayed by Mr. Mansell Moullin.

I am, Sir, yours faithfully,

Birmingham, March 4th, 1912.

ROBERT SAUNDY.

OPERATION FOR PHLEBITIS WITH THROMBOSIS.

To the Editor of THE LANCET.

SIR,—I have read with interest Mr. Skene Keith's communication in THE LANCET of Feb. 3rd *re* the excision of thrombi in cases of phlebitis of the superficial veins of the lower extremities. I have had several cases where this operation has been done with excellent results and greatly to the gratification of the patients. All that is necessary is to make a skin incision over the inflamed and thrombosed segment of vein, tie above first and then below the thrombus, and dissect out the intervening diseased part. This can be done *cito, tuto, et jucunde* under local anæsthesia, saving much time, relieving pain, and preventing all risk of embolism.

In "Clinical Memoranda" (Baillière, 1909) I have written the following paragraph:—

The treatment of phlebitis and thrombosis of the veins of the lower extremities by the "masterly inactivity" of rest in bed, and local applications, is not only intensely irksome to the patient, but is fraught with the danger of sudden death from pulmonary embolism. All danger is averted by, and a speedy recovery follows, the surgical treatment by excision of the affected veins at the earliest opportunity. Moderate inflammation is no bar to operation.

I am, Sir, yours faithfully,

A. T. BRAND, M.D., C.M.

Driffeld, East Yorks, Feb. 27th, 1912.

THE BEARING OF SIR DAVID SEMPLE'S TETANUS RESEARCHES ON SURGERY.

To the Editor of THE LANCET.

SIR,—I have lately been discussing with a number of surgeons Sir David Semple's researches on Tetanus,¹ and have been rather surprised to find that they do not consider them to have any practical bearing on surgery. They do not think that he has sufficiently proved his case to cause us to consider the matter from a practical point of view. If any of these men had been unfortunate enough to have had a case of tetanus ensuing on a surgical operation, as I have had myself on two occasions (hernia), or after a hypodermic administration of remedies, perhaps they would be more disposed to consider the matter. To my mind Sir David Semple has proved his case. It is a far-reaching case; I regard it as one of the most important papers of modern times in its bearing on surgery, especially in India, where tetanus is very prevalent. (In all that follows I am not referring to tetanus following mill injuries and such accidents.)

A few years ago there was considerable correspondence in the British medical press *re* tetanus following surgical operations, especially hernia. There was no definite conclusion arrived at as to the causation, but there was a very definite impression that some cases at least could not be explained by direct infection of the wound (through ligature or so forth). I am personally satisfied that Sir David Semple's paper, though not written directly to bear on surgery, demonstrates the proximal cause of tetanus following surgical operations where infection from direct sources can be excluded.

I have seen two cases of tetanus myself follow hernia operations; the same ligatures, the same lotions, &c., and technique were used for these cases as were used on the same day in several other major operations, including another hernia without tetanus following, and the same

¹ See Scientific Memoirs of the Government of India, No. 43 THE LANCET, June 10th, 1911, p. 1589.