Meanwhile the operation gained ground, and my own experiences constantly multiplied. But I always felt convinced that the popularity of the operation in England was due to the advocacy and practice of my two friends in Leeds, whom I had anticipated without having early enough made the fact properly known.

At the Manchester meeting of the Association in 1902, I gave an epitome of my work during the previous thirty years, comprising over 300 operations, reported in the BRITISH MEDICAL JOURNAL of October 25th, 1902. While referring to the prior date of my first operations, I paid what I thought due tribute to the great and important work of Allbutt and Teale. Having received from these gentlemen a handsome and generous vindication of my priority, the simplest way of stating this bit of history will be by giving some of the correspondence that passed between us. After writing, I received the following letter:

Cambridge, November 20th, 1902.

Dear Professor Rushton Parker,

My very cordial thanks to you for your very valuable paper on extirpation of glands, which would, I fear, have escaped my notice in the BRITISH MEDICAL JOURNAL. Up to its arrival as an offprint I had not seen it. I feel greatly to blame that I had not in my early papers on this subject looked up the history of the matter more carefully. But as no London surgeons seemed to know anything of the practice, and as at the International Medical Congress Mr. Teale and I read a paper herein, no one but Mr. Treves of Margate claimed to have excised systematically; and as we seemed to have done more and earlier than he, and more systematically—not in bad cases only—we assumed we were first in the field (after Billroth, of course; we turned up B.'s work some time after our own, which work was for us *relatively original*), and we ought to be ashamed of ourselves for not recognizing in our early papers that you were before us in publication.

Yours very truly,
T. CLIFFORD ALLBUTT.

The following is my reply, which he has preserved and lent me:

Liverpool, November 21st, 1902.

Dear Professor Clifford Allbutt,

I am much obliged to you for your kind letter and appreciation of my early work on glands. At the time I did not realize how absurdly inadequate, for publishing purposes, the journal was in which I published my early paper.¹ At the International Medical Congress in 1881, though present at the Surgical and Pathological Sections, I was unaware of your and Mr. Teale's communications in the Children's Section till they appeared in print. But if at any time I wished to state the case for operation I should quote word for word your opening paper, which I have elsewhere ventured to call a classical contribution to English medical literature. It was entirely my own fault and neglect that I did not better make known what I had done (a matter, however, of no importance in comparison with the generalization of the practice). But as I dealt with it at Manchester, I thought I might as well say a word for myself in the matter of priority, especially as, not being myself an originator of the method, I did not think I would thereby be open to the charge of undue boastfulness. I value the appreciation you have been good enough to express more than that of any one else concerned in the matter. I assure you that I hardly see how you could have been expected to know of work buried in such a manner, and nothing but the thoroughness of your literary character and integrity could have allowed you to express blame to yourself for not having unearthed it. . . . I feel that all surgeons who extirpate glands have reason to be proud of the exposition of their motives and reasons which you have furnished for them. With kindest regards, believe me,

Yours very truly,
RUSHTON PARKER.

At a later period came correspondence with Mr. Teale, who replied as follows:

Leeds, May 31st, 1905.

Dear Mr. Rushton Parker,

I return with many thanks your reprint. Two things are clear: 1st, that you have undoubted priority in England in the question of excision of tuberculous glands. You had worked on the subject and written before Allbutt and I seriously took it up. 2nd, That you and I are completely in accord as to the principles. Your illustrations of extreme cases are most encouraging. Another point which strikes me is that your experience coincides with mine as to the great rarity of death from phthisis after these operations. I am writing to Allbutt on your point.

Yours, very truly,
T. PRIDGIN TEALE.

Many letters passed, but these sufficiently explain the position. Arrangements were also made to deal with the subject in the same sense in the revision of their article on scrofula in vol. iv. of Clifford Allbutt's *System of Medicine*, and Mr. Teale sent me his remarks made for the purpose. But that article has had to be squeezed out of the new edition, and will not again appear.

There now only remains for me to again express my best thanks to Sir Thomas Clifford Allbutt, F.R.S., Regius Professor of Medicine in the University of Cambridge, and to Mr. Thomas Pridgin Teale, F.R.S., for their kind association with me in this joint historical record of our work in the treatment of tuberculous lymph glands.

REFERENCES.

¹ *Trans. Path. Soc.*, 1902, vol. lili, p. 307. ² BRITISH MEDICAL JOURNAL, October 25th, 1902; *Liverpool Med.-Chir. Journal*, July, 1903. ³ *Liverpool and Manchester Med. and Surg. Reports*, 1872. ⁴ BRITISH MEDICAL JOURNAL, November 25th, 1893.

ON THE ACTION OF CHLOROFORM ADMINISTERED BY DIFFERENT CHANNELS.*

BY

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Brief Summary.

LATE poisoning is now a well recognized, although fortunately a remote, danger in the administration of chloroform as an anaesthetic. The object of the present investigation is to throw light upon the etiology of the condition by a study of the changes produced in the protein metabolism of the body, with more special reference to any interference with the hepatic metabolism. Such an interference is indicated by disturbances in the distribution of the waste nitrogen excreted in the urine, and chiefly by a decreased conversion of ammonia into urea. Hence the proportions of these two constituents have been more specially studied, although in the later experiments the nitrogen in uric acid and in creatinin also have been determined, as well as the sulphur in the oxidized and in the non-oxidized condition.

BY INHALATION.

Dogs were used throughout the investigation. The animal was first put on a fixed diet of oatmeal porridge and milk, the urine being collected in a suitable cage. When nitrogenous balance was established, in the first six experiments chloroform was administered by the respiratory passages for one, two, or three hours, and the urine again collected for several successive days. No attempt was made to measure the amount of chloroform administered, as the work of previous investigators has shown that to produce anaesthesia the quantity of chloroform in the blood must be something between 30 and 50 mg. per 100 c.cm.

I had expected to find that chloroform thus administered would act like alcohol and sulphonal, decreasing the proportion of urea nitrogen and increasing the ammonia nitrogen. But this was not the case. In four of the experiments the proportion of urea nitrogen, by Möerner and Sjöqvist's method, was increased 3, 6, 10, and 10 per cent. In two it was practically unaltered. The increased excretion of nitrogen observed by previous investigators was manifest in three of the six experiments. This quantitative change was not always associated with the qualitative. Thus in two experiments there was an increased excretion of nitrogen with no disturbance in the proportion of urea, and in two there was no increase with a marked disturbance in the proportion.

These results seemed to indicate a stimulation of hepatic metabolism rather than a depression.

Protein was present in the urine in appreciable amount, in only one experiment, that in which the administration had been continued for three hours, and in mere traces in two other experiments. In two there was a deposit of fatty cells resembling renal epithelium, but in none were tube casts observed.

Apparently in the dog the disturbances in the hepatic metabolism produced by the inhalation of chloroform, even for periods of three hours, are very trifling, while the kidneys are little affected.

* A full report of these researches is contained in two papers: (1) On the Effects on the Metabolism of Chloroform Administered by Different Channels, by D. Noël Paton; and (2) On the Rate of Assumption and Elimination of Chloroform Administered by Different Channels, by Dorothy E. Lindsay and D. Noël Paton. Published in the *Proceedings of the Royal Society of Edinburgh*, 1908, vol. xxviii, pp. 414, 439.

BY THE STOMACH.

In the next two experiments 25 c.cm. of chloroform in oil were administered by the stomach on two successive days. In both experiments a distinct increase in the total nitrogen excreted was observed, and in both there was a distinct fall in the nitrogen in urea, a rise in the nitrogen in ammonia and in uric acid, no marked change in the proportion of nitrogen in creatinin, and a very marked rise in the nitrogen not in these combinations. This "remainder nitrogen" may be taken as probably chiefly nitrogen in amino-acids, and it is of interest to observe that its chief rise preceded the chief rise of ammonia nitrogen. The proportion of oxidized sulphur showed a fall. In both experiments proteins, granular cells, and tube casts were present in the urine, and in one experiment jaundice was present.

HYPODERMICALLY.

In the next two experiments 10 c.cm. of chloroform were given hypodermically on two successive doses. No marked rise in the total nitrogen excretion was found. The proportion of urea nitrogen fell markedly in the first experiment, but only to a small extent in the second. The ammonia nitrogen showed a rise, the proportion of uric acid nitrogen was increased, but the proportion of creatinin nitrogen was not markedly altered. The nitrogen not in these combinations rose markedly in the experiment in which the urea nitrogen was decreased. The sulphur was estimated in only one experiment, and the oxidized sulphur showed a marked decrease.

Proteins and granular cells were present in the urine.

ASSUMPTION AND ELIMINATION IN THE BLOOD.

Is this difference in the mode of action of chloroform administered by these different channels merely the result of difference of dose, or do some other factors play a part?

To answer this question the assumption and elimination of chloroform by the blood when it is administered by different channels, was studied in conjunction with Miss Dorothy Lindsay. For this investigation rabbits were used, and the chloroform in the blood was determined by Nicloux's method. It was found that, generally speaking, when chloroform is inhaled the amount in the blood rapidly rises to something like 40 mg. per 100 c.cm., and that it is rapidly eliminated, so that at the end of three hours it has largely disappeared. Several cases showing a marked divergence from this rule were observed. It was found that, if the rabbits were kept in a small cage after the administration, the elimination of chloroform took place much more slowly, and that at the end of two hours there might still be as much as 24 mg. of chloroform present. The observations of Tissot and of Nicloux showing that there is no fixation of chloroform in the liver were confirmed.

When, on the other hand, chloroform is given by the mouth or hypodermically in doses of 1 c.cm., the amount in the blood slowly rises to reach about 22 mg. at the end of five hours, and then slowly falls. The elimination thus takes as long as six hours. The amount of chloroform in the liver is as great as, or greater than, the amount in the blood.

The difference between the effects of the administration of chloroform by inhalation on the one hand, and by the stomach or hypodermically on the other, is not, then, due to difference in dose, but to its rapid elimination after inhalation, and to its slow elimination after the latter modes of administration.

The greater effect produced in these experiments by the administration by the stomach as compared with the administration hypodermically is probably a result of the difference in dose—50 c.cm. as against 10 c.cm.—for upon rabbits we find that it is equally toxic when given by the mouth and when given hypodermically, and that the course of accumulation in the blood and elimination from the blood is about the same.

The rarity of late chloroform poisoning after its inhalation seems to be due to rapid elimination, and the absence of fixation in liver tissue. But the occurrence of so many cases of slow elimination in rabbits seems to show how, when this occurs from any cause in the human subject, the same fixation may take place, and the metabolic and renal changes characterizing late chloroform poisoning may ensue.

A NOTE ON THE ADMINISTRATION OF ETHER
BY THE OPEN METHOD.

By G. H. COLT, M.A., M.B.,

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HOSPITAL.

WHEN testing the principles of ether anaesthesia last summer I removed the small bag from the Clover's inhaler and allowed the patient to breathe to and fro without it. The disadvantage of this method is that half the ether is wasted and the purity of the surrounding air is vitiated; the cooling of the inhaler is also excessive. Messrs. Barth and Co., therefore, made me a small valve box for introduction between the inhaler and the facepiece, and so arranged as to allow inspiration through the inhaler, but to direct the expiration into the air without passing back through the inhaler. The valves were of rubber, and the condensation of water on the metal due to the cooling caused by the ether vapour was excessive, so that the valves stuck and were inefficient. The next step was to interpose between the facepiece and the inhaler one of Barth's aseptic 3-way gas stopcocks, patent 1902, in which the valves are of mica seated on ring knife edges. The piece was adapted by means of a reducing collar. The water of condensation caused no sticking of the valves, and flowed off readily through the expiratory one, which, with this method, is the most dependent part of the apparatus.

The induction and maintenance of anaesthesia in this way is very simple. The small bag, with gas connexion, being attached to the ether inhaler, the valve pointer being turned to "air" and the patient's head being on its side, preferably the right, the facepiece is applied and gas admitted, the pointer is gradually turned to "valves," and the gas in the bag continuously replenished. The proceeding, in fact, precisely resembles the ordinary method with the large bag, except that the valve chamber is placed between the facepiece and the inhaler, instead of between the bag and the inhaler, and that the small bag is used throughout. The induction is the same, and needs no description. When the patient is fully under ether and the gas has been disconnected, the pointer being at this stage usually at "no valves," and the patient rebreathing from the small bag, the latter is removed, the pointer turned to "valves," and the inhaler rotated on its stem so as to increase the percentage of ether vapour. The patient continues to inspire fresh air laden with ether vapour through the inhaler, and to expire through the valve, the passage of the expiration through the inhaler being prevented. Should he at any time show signs of returning consciousness, all that is necessary is to reapply the small bag, turn the pointer to "no valves," and to decrease the percentage of ether on the inhaler. When he has now rebreathed "four and one" or "five and one" for two or three minutes, the sequence may be repeated.

There seems to be no doubt that there is, on the whole, less cyanosis, less secretion of mucus, and less oozing of the tissues by this method, but the anaesthesia almost always tends to become light, as is evidenced by the advent of shallow, occasionally thoracic, breathing, lateral movements of the eye, and the contracted (2.5 mm.) pupil. Perhaps this would be obviated by using an inhaler which presents a greater evaporating surface for the ether. At no time, however, is the oozing of blood less than with anaesthesia maintained with chloroform. The advantages of the method are:

1. Some attempt at measurement is introduced and undoubtedly much less ether is used than would be required to produce the same effects by pouring it on lint. The inhaler is kept upright. Ether is given by the open method, but the ether is kept in a closed chamber so far as waste by evaporation is concerned.
2. Apparatus at present on the market can be used, thus reducing the amount of apparatus that need be carried. Messrs. Barth and Co. will supply a reducing collar for the necessary adaptation, or the proximal limb of the valve chamber can be itself adapted, whereby the distance from the lower border of the inhaler to the centre of the valve chamber is reduced to 1½ in. In such case, however, the fitting to the large bag has to become an inside instead of an outside fitting, a matter of no disadvantage, or a collar may be used for this. The whole stopcock may be boiled without injury.