

rence whatever of the symptoms worth mentioning, although she leads a very active intellectual life.

It is certainly instructive, as showing the state of chaos which the treatment of fibroids has reached to find Dr. Murdoch Cameron once more reviving, and giving his imprimatur to an operation which has been universally condemned by operating gynaecologists—an operation which does not affect the pathological conditions present, and merely induces a hurried and premature menopause.—I am, etc.,
Florence, October 14th. T. GERALD GARRY, M.D.

CAESAREAN SECTION.

SIR,—To convince Dr. Edmunds that I have no desire to write simply to make copy, I only remark that his advice to abolish uterine sutures in Caesarean section is not likely to be followed by any modern operators. Regarding Dr. Herbert Spencer's note on my statistics, I explained to the meeting at Manchester that the death-rate under heading 2 was solely due to outbreaks of sepsis in the Maternity Hospital. Since then I have only operated in the Western Infirmary, and have not had a death for nearly seven years.—I am, etc.,
Glasgow, Oct. 21st. MURDOCH CAMERON.

SIR,—If, as the papers on Caesarean section in the BRITISH MEDICAL JOURNAL of October 11th seem to promise, the time has at last come for the abandonment of the antiquated and barbarous operation of embryotomy, is it not a pity that Dr. Galabin should discourage the general practitioner from attempting the abdominal section? It is in the general practitioner's hands that the great bulk of midwifery work lies, and if obstetric practice is to keep pace, as it should, with the advance of modern surgery, it seems to me that he above all must be encouraged to adopt conservative methods.

There are many men in country districts who would readily enough operate on a hernia, for example—and yet to whom the idea of performing Caesarean section would never even occur, or, if it did, the traditional feeling of awe which clings to the words "abdominal section," combined with the unfamiliarity of the operation itself, would at once negative it. Yet I feel sure that no one who has once seen a Caesarean section and who has reasonable confidence in his asepsis and surgical skill—and without these his embryotomy will not be a strikingly safer alternative—would or need hesitate, even without the resources of a hospital behind him. The instruments required are very few, the operation itself, comparatively speaking, simple and straightforward. The technique, as I saw it carried out in two cases in Chrobak's clinic last January, was even simpler than that described so graphically by Dr. Murdoch Cameron.

In both cases it had been decided on early in pregnancy on account of deformed pelvis, which had necessitated previous craniotomies. The time chosen was three to four hours after labour had begun. The abdomen was opened in the middle line from the highest point of the uterus to within 3 in. to 4 in. of the symphysis pubis. The uterus was brought out through the opening, the edges of which were then carefully packed with gauze sponges. An assistant encircled it firmly with both hands as low down as possible, and it was then opened by one cut, beginning at the fundus and following the back of the child, which in both cases lay in the dorso-anterior position, as far as the groove of its neck. The blood and liquor amnii which followed this incision with a gush were prevented from reaching the abdominal cavity by the sponges and the position of the uterus. The child and placenta having been removed without difficulty, the uterus rapidly contracted and was closed by a continuous suture. It was squeezed once or twice and replaced. No peritoneal toilette being required the abdomen was immediately sewn up, also by a continuous suture, and the dressings and a binder applied. The children were both saved, and the mothers made uneventful recoveries. The sutures, I may perhaps mention, were of Pagenstecher's cellulosed thread, which in Chrobak's clinic has almost entirely superseded all other material. The tubes were not tied in either case. Dr. Galabin's opinion on this point is exceedingly interesting. The ethical problem involved is a wide and difficult one, but one which nowadays most gynaecologists at any rate have, in some form or other, very early to face. Such being the case, the views of other men who, like Dr. Galabin, are well qualified to speak on this question

would, I feel sure, be greatly valued at least by junior members of the profession.—I am, etc.,

Belfast, Oct. 16th.

MARION B. ANDREWS, M.B., B.Ch.

THE DISCUSSION ON SCURVY AT THE ANNUAL MEETING.

SIR,—In the discussion on the prevention of scurvy which is necessarily closely connected with its causation, there appears to be a tendency to underestimate the valuable work on the subject which has been contributed to medical literature during the last fifty years or more in favour of finding some new theory. That it originates in an inadequate supply of fresh food—both as regards quantity and quality—is an established fact, and it is immaterial whether the nutriment is of animal or vegetable source. I agree with Fleet Surgeon Kirker (BRITISH MEDICAL JOURNAL, p. 1024) who stated in his reply to Dr. Turnbull's discussion at this year's meeting of the British Medical Association that the disease cannot be due to decomposing food. I am aware that Vaughan Harley and Jackson¹ have demonstrated in monkeys fed on decomposing meat, symptoms which are identical with those found in true scurvy, and that they were able to produce these symptoms even when a certain amount of fresh fruit was added to the monkeys' diet. But that such a diet is a common antecedent in human scurvy is opposed to all experience. We know, for example, that overheating and desiccation of food favour the onset of the disease, whereas such processes are hardly likely to favour putrefaction. Similarly, the onset of scurvy in the infantile form—which in its pathological anatomy is identical with the adult type only modified in accordance with the altered activity of the infantile tissues, notably the highly vascular periosteum and which yields to the same therapeutic measures—has been traced in numerous instances to prolonged heating of the natural foodstuff, namely, milk. To quote the words of Sir Thomas Barlow, "it seems fair to say that the further we get from a living food the less is the antiscorbutic power; fresh vegetables are more powerful (as antiscorbutics) than preserved or cooked, and raw meat is more powerful than cooked meat," or one may add than salted or corned meat. Further, the proof of identity of adult and infantile scurvy opens up a fresh line of investigation in the study of the disease seeing that an adequate supply of fresh milk—human or otherwise—is an antiscorbutic.

Another point of importance is that well-cooked potatoes are strongly antiscorbutic, and it seems to me that any discussion on scurvy must take cognizance of these two facts, namely, that fresh milk alone is an antiscorbutic, and likewise cooked potatoes, provided both are taken in sufficient quantity.

To come to more precise analyses of the tissues of scorbutics it was found by Ralfe² that the alkalinity of the blood was diminished in scurvy, a result which would follow the removal of the organic salts of potash from the food, as suggested by Dr. Buzzard.³ Experimental observations on the production of acid intoxication were made by Walter⁴ in 1870, working under Schmiedeberg. He found dissolution of blood corpuscles, ecchymoses, degeneration of muscles, that is, symptoms of scurvy in animals after injection with mineral acids; and Ralfe suggested that the same sequence of events occurred in human scurvy. Actual acidity of blood is, of course, not produced, such a condition being incompatible with life; the condition is one of diminished alkalinity.

Professor Wright⁵ has prosecuted the study of scurvy and other conditions characterized by a tendency to bloody and serous effusions on similar lines, but more accurately in so far as by improved technique he makes accurate quantitative observations of the blood reaction. He finds that under the conditions named the blood alkalinity is reduced, but that the exhibition of alkaline and organic salts tends to effects a cure, and that the improvement in the patients' condition progresses concurrently with restoration of the blood to its normal alkalimet-

¹ *Lancet*, 1900.

² *Lancet*, 1877, and reprint.

³ Reynolds's *System of Med.*, art. Scurvy.

⁴ *Arch. f. Exp. Path. et Pharm.*, 1877.

⁵ Army Medical Report, 1895.

rical state.⁶ He has also shown that the scorbutic value of food stuffs may be predicted from a study of the reaction of their ashes after incineration, a process which corresponds with what occurs in our own bodies. Why then, we may ask, has the acid-intoxication theory of scurvy been ignored latterly? I believe the answer lies in the statement of Ralfe himself, writing in conjunction with Bradshaw,⁷ that "chemical explanations are inconsistent with the fact that the antiscorbutic properties of foods are destroyed by desiccation and overheating, processes which do not affect the proportions of the acids and bases they contain." In spite of this very impartial dictum which aims a blow at his own hypothesis, there is an explanation which suggests itself as a cause of scurvy, which in no way invalidates the acid-intoxication theory as elaborated by Ralfe and Wright.

A consideration of the organic salts of foodstuffs which contribute to the alkalinity of the blood shows that they belong in the majority of instances to the class of optically active chemical compounds in virtue of their containing one or more asymmetric carbon atoms. As van't Hoff pointed out in his work, *Dix Années dans l'Histoire d'une Théorie*, 1887, such compounds are capable of existing in different modifications—stereoisomers—differing in their action on polarised light, and, where crystalline, in their crystallographic shape. Moreover, they are reciprocally convertible in definite proportions at definite temperatures until chemical equilibrium is established, and differ in their digestibility by living cells. Now, assuming that the varieties which occur in natural foodstuffs are the most readily assimilable, it would follow that prolonged heating in the process of cooking or desiccation would, in the production of stereoisomers, be tantamount to the withdrawal of an important ingredient from the dietary of the patient—the prospective patient—namely, an amount of the organic salt depending on the temperature and time of the process, resulting, possibly after a long period only, in diminished blood alkalinity below what is consistent with health. More recent knowledge also teaches us that milk, which for a long time was considered to contain no organic acids, really does, namely, lactic (Marchand), citric (Corlette, Soxhlet) in the form of calcium citrate and acetic acids. On the other hand these acids are optically inactive, but appear to be precipitated partly during desiccation, calcium citrate granules being thrown down in the preparation of condensed milk.

Again, the fact that well-cooked potatoes are highly antiscorbutic accords with this hypothesis, for the citric acid of potatoes is incapable of existing in any other modification, as it does not contain an asymmetric carbon atom, and we should not *a priori* expect any loss of antiscorbutic properties by boiling potatoes.

As regards the value of lime-juice it seems at present to be in danger of losing its time-honoured place as a specific remedy. I think, however, that a study of Lind's *Treatise on the Scurvy* will confirm the impression that the value of lime-juice has been made manifest without any improvement in diet, to which Dr. Turnbull would attribute the improvement in the health of the navy which followed Sir Gilbert Blane's memorable recommendation. I do not think that we can afford to neglect the lessons taught us by Buzzard, Ralfe, Walter and Wright, or the identification of Barlow's disease with the adult type of scurvy when we attempt to elucidate the problem of scurvy and its prevention.—I am, etc.,

Harlesden, Oct. 4th.

O. EICHHOLZ, M.B.

THE ETIOLOGY OF SCURVY.

SIR,—In an interesting article on "Should Milk be Boiled," in the *BRITISH MEDICAL JOURNAL* of February 22nd Dr. Ransom concludes that scurvy is a ptomaine poison due to eating tainted meat. In support of this view he quotes the case of two polar expeditions when men consuming fresh meat remained free from scurvy, while those consuming tinned meat and lime juice suffered severely, and Jackson and Harley who brought experimental proof in support of this theory. I have had much experience of scurvy on the N.W. Frontier of India during the past twenty-five years which absolutely disproves the theory of ptomaine poisoning.

⁶ *Lancet*, 1900.

⁷ Quain, *Dic. of Med.*, art. Scurvy

The fact that men living on tinned meats and so-called lime juice got scurvy only proves that what is called lime juice even if taken honestly will not keep off scurvy when there is no fresh food—but to expect such a thing is to expect a great deal too much. It does not require much reflection to see that lime juice itself is a thing that becomes stale, and that when stale it is not then likely to be drunk honestly or to keep off scurvy if so drunk. That ptomaines have nothing to do with the matter, though I should be quite prepared to hear that slow ptomaine poisoning was indistinguishable from scurvy, the following account will prove:

In the year 1885 I was in medical charge of the 23rd Pioneers in the Hurnai Pass. Knowing that no fresh vegetables would be procurable, I had arranged that a native dealer should open a shop and get up supplies from India. One day a man came to hospital with an obscure swelling of the leg which suggested scurvy, and I said, "You have not been eating vegetables." To this the man replied that there were none to be got; in short the native dealer had found it so difficult to supply vegetables that he had given up the attempt. I then examined some of the sepoys and followers, and found in nearly every case that the gums were showing signs of scurvy. Looking round the barren hill sides I found a little cruciferous plant. This I showed to a native officer, who said he knew it well, and it was rank poison. I said, "That may be, but we officers are going to have a spinach made of it, and we are going to eat it in the mess to-night." On my return to camp the same native officer met me, and said he had in the meantime boiled and made into spinach some of the plant, and that it was very good. The result of my trial of it was that I had a regimental order published stating that this plant was good to eat, and that any one found with scurvy would be punished. During the rest of our stay there, which extended to several months, I sent out two men per company daily to collect herbs, which after inspection by me were cooked for the companies. The result of this was that we had no more scurvy. Here was a body of men, the fighting part of whom had one pound of meat twice a week eaten immediately after being killed, and the non-fighting part no meat at all. They all began to get scurvy when fresh vegetables were stopped, and they all at once recovered when fresh vegetables were provided. There is no place at all here for ptomaine poisoning. In the Chitral garrison in 1895-96 every man in the garrison was affected with scurvy more or less severely by Christmas, in spite of our having a good supply of so-called lime juice. Here among the young wheat we found herbs which were collected and eaten as spinach by the men, and stopped the ravages of the disease; but when men are as badly affected as the men in Chitral were, then it requires a liberal supply of fresh nutritious meat and milk to eradicate the disease, and these were not procurable in Chitral, rinderpest having swept the valley clean of its cattle.

The fact that in the polar expeditions those who got fresh meat remained free from scurvy only proves that the fresh food required to keep off scurvy need not be fresh vegetable food. This would almost seem self-evident; of course the pound twice a week that the sepoys had was not enough.

If I have not proved that scurvy is not due to the presence of ptomaines nor to the absence of potash or citrates or malates or any other salts, but to the absence of something that exists in fresh animal and vegetable foods, I have merely failed to marshal my facts aright.—I am, etc.,

Peshawur, Oct. 3rd. H. HAMILTON, Lieutenant-Colonel I.M.S.

MOTOR CYCLES FOR MEDICAL MEN.

SIR,—I have read with great interest the article "Motor Cars for Medical Men" by Dr. E. Phillips in the *BRITISH MEDICAL JOURNAL* of September 27th. I have not had the benefit of his experience with both types of motor, that is, with both car and bicycle, as I have never been on the former, but I have daily used the latter for professional work for the past six months. Now, I take it that the majority of my professional brethren practising in purely rural districts have an income say from £400 to £600 a year, while in many cases although the income is small the mileage is great, necessitating the keeping of two horses, especially in men "getting on in life." It is this class, I think, that would willingly take Dr. Phillips's excellent advice and get a motor