

opinions are opposed to long-standing beliefs regarding the effects of quinine in malarial infection in India.

1. His recent work leads him to conclude that the malignant tertian parasite is more amenable to complete eradication under the curative use of the salts of the quinine alkaloid of cinchona bark than the benign tertian parasite. The reverse has been the opinion always held in India. In his paper he admits that he only dealt with 15 cases of malignant tertian infection, and I know from my own notes that there was only one case of malignant tertian infection in the first series—that is, Major Rennie's—which dealt with the Dagshai cases up to Sept. 30th, 1918. I do not include the diagnoses made regarding the cases on the plains anterior to their being sent to Dagshai, as I am of opinion that these latter diagnoses should not be included by Major Acton, who, I take the liberty of remarking, would be on safer ground if his deductions were based on observations actually made by himself and his colleagues. If this view is accepted, then one is disposed to consider that the number of malignant tertian infections dealt with by him and his collaborators is scarcely sufficient to justify a definite conclusion as to the relative curability of this type of malaria by quinine as compared with the effects of quinine in benign tertian infection. I would suggest that a much more extensive series of observations of the same kind on malignant tertian infection are necessary before arriving at any positive conclusions as to its curability compared with other types of malaria by quinine salts. I fully admit that Major Acton has made out a strong case in support of his statement that quinine acts as a steriliser of the blood in connexion with *Laverania malarie* infection—that it has a more decidedly specific action on this parasite than on the other two, and he has brought forward a series of facts not easy to refute.

2. My own experience in India and Burma, which goes over 28 years and includes something in the neighbourhood of 200,000 cases of malarial infection, indicates that benign tertian infection forms probably over 75 per cent. of the malarial infections met with, and not 50 per cent., as stated by Major Acton. My observations were carried out almost all over India. This is the general average for an entire year. The ratio of benign tertian and malignant tertian infections in many parts of India varies with the period of the year, the percentage of malignant increasing and that of benign tertian decreasing during the autumn. This is specially brought out in the malaria of our extensive N.-W.F. Provinces. I do not believe this difference to be due to raised atmospheric temperature, as appears to be the case in temperate climates; it is, I submit, owing to some undiscovered factor in connexion with the life-history of malaria-bearing anopheles. One has investigated malaria at all periods of the year in India, Burma, and Mesopotamia, and knows that it is necessary to continue observations for a whole year before anything like a true record of the ratios as to the types of malarial infection is to be obtained for each locality, district, or cantonment.

3. The difficulty of completely eradicating *Plasmodium vivax* from the blood appears to be proved by the work done in the Dagshai Malaria Dépôt; this was also well brought out by Professor J. W. W. Stephens and his co-workers during the great war, and should cause many to revise their opinion as to the innocence of this type of malarial infection. I have long been of opinion that benign tertian malaria is a much more important type of infection in India, both as regards direct and indirect mortality and the extent of morbidity it gives rise to, than malignant tertian infection. One published many statements to this effect years ago. The following is abstracted from "Prevention of Malaria in India," 1911, p. 100:—

The vast majority of the cases of malaria in India consists of simple tertian, which is considered by most authorities to be the easiest form of malarial infection to eradicate from a locality and from infected persons. This does not correspond altogether with the experience of many experts in India. Notwithstanding statements to the contrary, the largest number of relapses occurs in cases of simple tertian fever. During the last ten years one has had opportunities of watching and following relapses in localities where there is no initial malaria, and 79 per cent. of these were simple tertians, over 20 per cent. malignant tertians, and only a small fraction quartans. The generally accepted statement also that perniciousness is confined to malignant tertian is opposed to one's experience, as one has seen such conditions as algid paroxysms, choleraic attacks, cerebral attacks, and even hyperpyrexial phenomena associated with simple tertian infection, whilst a fair proportion of cases of repeated simple tertian relapses or reinfections end in malarial cachexia. In discussing the subject with medical men of wide knowledge of Indian malaria it has been ascertained that one's experience in this respect is not exceptional.

4. I am not altogether satisfied that an incontestible case has been made out by Major Acton for the exclusive use of the combined cinchona alkaloids (we used to call them the *mixed alkaloids* 30 odd years ago) in benign tertian infection, but he has shown rather forcibly that they should be subjected to a further extensive trial carried out on scientific lines. They were very largely used in the out-patient department of the Medical College Hospital, Calcutta, a

generation ago, where we had about 100 cases of malarial infection attending every morning, and one remembers that a fair proportion of these cases rejected their dose shortly after swallowing it. The use of tablets of the mixed alkaloids, as suggested by Major Acton, would probably to some extent remove this disadvantage.

Two sections of the paper under reference—The Effect of the Total Alkaloids of Cinchona Bark on Benign Tertian Infections and Effect of the Cinchona Alkaloids on Benign Tertian Infections—are of profound interest. They deal with an aspect of the therapeutic uses of the products of the cinchona bark regarding which we have been too long in ignorance, and they indicate the necessity of combining the work of a highly-trained chemist with that of clinicians and parasitologists in medical institutions carrying out original researches on malaria in India. It appears to me that the work which Major Acton has been doing in connexion with the mixed alkaloids might with great advantage be extended to inquiries regarding several of the individual alkaloids of cinchona bark.

I was very pleased to notice that Major Acton considers that the microscope is not sufficiently employed in the diagnosis of malarial infections in India.

It is specially gratifying to learn that Major Acton is now labouring at home endeavouring to give practical application to the excellent work he was doing at Dagshai.—I am, Sir, yours faithfully,

P. HEHIR,

London, June 14th, 1920.

Major-General, I.M.S. (retired).

ABDOMINAL EMERGENCIES.

To the Editor of THE LANCET.

SIR,—I beg to be excused for sending so belated a reply to Mr. A. G. T. Fisher's criticism (April 17th) of my paper on Abdominal Emergencies published in THE LANCET of April 10th. Several points struck me at once on reading it.

1. He had evidently not read the cases carefully.

2. He had missed the chief point of the paper—viz., a plea for an exact diagnosis, and, the natural outcome of this, to act on the diagnosis made.

3. The cases he mentions convey quite a different impression in his description than they do from reading my notes of them; in other words, he gives them the complexion he wishes them to have.

4. His opinion appears to be based entirely on statistics he has been able to collect, and in no place does he lead me to suppose that he has had any personal experience in "civil" abdominal emergencies.

5. I feel that his remarks are, to say the least, ungenerous when he applies such terms as "apparently," "gambler's throw," and "considered to be" to what we considered at the time as definite diagnoses.

Apart from this, perhaps I may be allowed to comment on his letter. In ruptured duodenal ulcers he has apparently not seen a case such as we diagnosed No. 15 to be, although they are by no means rare, and within a fortnight of writing the published paper I had another. This was a patient, aged 25, admitted six hours after perforation. He was diagnosed correctly in detail by the house surgeon and myself independently; he was operated on at once and the diagnosis confirmed, the ulcer presenting a minute perforation which was already closed. The fact, too, of Case 15 being perforated 36 hours seems to mean nothing to him, and in his consideration of statistics he has overlooked the relative recovery-rate, according to the interval between perforation and operation.

From his criticism of Case 4, the appendix abscess which was left alone, two conclusions are obvious: (1) in such cases the frequent passage of small quantities of mucus conveys nothing to him; and (2) he did not accept the diagnosis that the abscess was "pointing" in the upper part of the rectum, although this was verified within 24 hours.

In reference to his criticism of Case 2, the female child with pneumococcal peritonitis, I should like to ask him what operation he thinks would be of any use in such a case. If he is thinking of drainage, I should have thought that his experience as surgical specialist, R.A.M.C., would have taught him the futility of a drainage-tube in a diffuse infection of the peritoneal cavity, where no large quantity of free fluid was present.

Perhaps, too, it is permissible to remind him that the peritonitis in such cases is only one of the manifestations of the pneumococcal infection, and possibly the incision of the belly wall may be even harmful, as interfering in some measure with the respiratory movements.

Finally, may I recall to him my second concluding remark—viz.: "No hard-and-fast rules can be made; general principles ought to be laid down and followed, and special features of individual cases must be taken into consideration before a final decision as to treatment can be adopted."—I am, Sir, yours faithfully,

CHARLES F. M. SAINT,

Professor of Surgery, University of Cape Town.

May 20th, 1920.

GASTRO-ENTEROSTOMY: DIETETIC VALUE OF ALCOHOL.

To the Editor of THE LANCET.

SIR,—As a confirmed gastro-enterostomist of some years' standing I wish to express admiration for Mr. H. J. Paterson's lucid exposition, at a recent meeting of the Section of Surgery of the Royal Society of Medicine,¹ of the rationale of this operation; my personal experience is in accordance with that of Mr. Paterson as to the application and success of the procedure. Also I was deeply interested in reading Mr. A. J. Walton's dark roll of surgical iniquity, which I am sure will command profound study by all who are responsible for the efficient training of surgeons. At the same time I do not wish to infer that, although I happened to escape from inclusion in that roll, I have not had my share of compensatory, post-operative trouble, the principal one being a two-year plague of diffuse, dense iodine peritoneal adhesions, some of which caused intestinal obstruction, with three fatalities. The abandonment of iodine skin disinfection and return to old friends—alcohol mercurial lotion, and careful adjustment of isolating towels in and around parietal wounds by Mr. Marmaduke Sheild's through-and-through retractor silk sutures—relieved a situation which was really becoming intolerable.

It is not my desire to occupy space in reiteration of what I have already published on this subject further than to mention that my ideas on the operation of gastro-enterostomy are recorded in THE LANCET of Oct. 26th, 1912, and it is possible that the method therein described may now merit some attention. I think it a duty to repeat that in my opinion the employment of clamps in gastro-enterostomy violates the rudiments of operative surgery, in that they entail a needless traumatic insult to the delicate visceral structures, and a fatuous, if not culpable, obfuscation of potential hæmorrhage from many important vessels which must be injured in the course of the necessary incisions. I have no hesitation, judging from the number of fatalities which occur from post-operative hæmatemesis, in craving for the attention of surgeons to what I maintain is a preventable catastrophe—and, moreover, I consider it a disgrace to surgery and a parody on human reason that the term "hæmostatic through-and-through stitch" should ever have been countenanced in the surgery of the stomach. And I strongly recommend anyone who has not time or nerve openly to tackle the dozen or so bleeding vessels which usually demand ligature before the insertion of the inner circular catgut suture to transfer such cases to a colleague.

I am not of those who discount the importance of time,² yet I wish to emphasise that I am unable to follow the mentality that admits, in gastric surgery, of the repudiation of the cardinal rule—see the bleeding point and tie it. With all deference I beg of the authors of operative text-books to give this view some more reflection in the hope that they may see their way to delete some of the irons which now adorn their illustrations, and to bring a needle and thread into better perspective with an artery forceps and ligature well in the foreground.

There is another item which has for years baffled my sense of comprehension—the inordinate haste to get these patients out of bed and to introduce solid food into doubly lesioned stomachs. Apart from direct and peristaltic traumatism, apart from the very negation of Hilton's law, apart from the fact that it takes at least 14 days for *restitutio ad integrum* of any solution of continuity in the human body, the early administration of solid food is a physiological anachronism in that it induces the gastric glands to pour forth an abundance of acid secretion at the moment when the organ is extra-handicapped with an additional six-inch gastric sore, and when, by all the rules, an alkaline state is necessary for its salvation. Recently, I was gratified to read that Dr. T. G. Moorhead, of Dublin, had called attention to this matter, and I hope his opinions will exact the consideration which they always deserve.

It has been my plan for many years, a few days after operation, to hand over these cases to the sister's personal care, with the injunction that they shall be kept in bed and on a rigid milk diet (plus mist. carbonatis ter die) until the 28th day. Before discharge each one receives instructions as to what he should eat and drink during the ensuing year, and invariably some alcoholic stimulant is recommended at lunch and dinner.

It may not be reckoned irrelevant if I touch on the prophylactic treatment of this too prevalent malady, and in order to avoid prolixity or getting out of depth I will briefly mention "Ten Commandments!" which I have found of great service to candidates for gastric ulcer, who, perforce, have to lead a sedentary existence with comparatively little out-door exercise.

I. As a general rule, indulge only in two meals a day, and take only two plates to each meal—meat with vegetables, and fruit with or without light milk pudding, or cheese.

II. Masticate thoroughly, have rotten "stumps" extracted.

III. Take—in moderation—whatever alcoholic beverage is most congenial to your stomach—i.e., which does not, hours afterwards, create excessive acidity or a feeling of "liver."

IV. Whenever practicable, rest mind and body for one hour after a meat lunch in order to allow the digestive machine to get the wind up before an extra call for blood is made by brain or muscles.

V. Allow at least a six-hour interval between mid-day and evening meals.

VI. Do not pollute the refreshing cup of afternoon tea with bread and butter, cakes, scones, or other decomposing fermenting carbohydrate messes which deprive the unfortunate stomach of all chance of a rest before it has to tackle its dinner problem.

VII. Abjure the use of any form of alcoholic refreshment except at your lunch and dinner table.

VIII. Whenever feasible take a stroll in the evening after dinner, and sleep in a room with a large open window—not in a draught—so that the blood may be properly oxygenated during the eight hours of repose.

IX. Make it a creed to take regular morning exercise. Begin with a hard snap of shadow-boxing (commence with 20 and work gradually up to 100 double clouts) in order to extend the heart muscle and elastic tissue of the lungs. Follow with, e.g., Muller's eight physical exercises, then have a cold shower-bath, and promptly conclude with Muller's excellent towel drill before some open door or window.

X. "Dulce est desipere in loco"—but not too often.

I am aware that I tread on delicate ground in advocating the general moderate use of alcoholic drink with meals. In response to the "Hallelujah" cantations of ascetic mummies whose psychological diapason is solely comprised of one hyphenated falsetto—to eradicate an abuse it is necessary to lop off the use—I have no compunction in affirming that the rational dietetic use of the fermented juice of the grape, of the hop, and of some cereals is a "god-send" to man's digestion, and that while the indiscriminate use or bestial abuse of same unquestionably speeds down to the grave, universal total abstention would tend, in a comparatively short cycle of time, to depopulate the earth, and, in all probability, would compel whatever might remain with human form ultimately to seek refuge in the Simian freedom of an arboreal existence.

The die is cast—the test is now being applied on a large scale—may I beg my readers and their descendants carefully to note and compare, in the respective wet and dry zones, the future incidence of tuberculosis, cancer, lunacy, diabetes, resistance to infection, addiction to drug habit, and frequent and prolonged tours abroad for business, health, and pleasure purposes.

I am, Sir, yours faithfully,

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April 14th, 1920.

¹ THE LANCET, March 13th, 1920, p. 598.

² Operating to the Clock, Brit. Med. Jour., Dec. 21st, 1918.