

an interval of seven years between the operation and the onset of symptoms, but two or three is more usual. I have had the opportunity of examining some cases that have had no return of symptoms, and I find that this rapid emptying does not occur.

Cynically I have said that the gastro-enterostomy which is the most successful is the one which does not ultimately work. The operation in most cases is wonderfully successful considering the disturbances of the physiological processes which must take place, and it is a pity that an operation has not yet been devised which would avoid these disturbances.—I am, etc.,

Leeds, May 12th.

LEO. A. ROWDEN.

PROPHYLACTIC USE OF QUININE IN MALARIA.

SIR,—The remarks of Sir Ronald Ross (May 3rd, p. 558) regarding quinine prophylaxis are of great interest, and especially the statement that he is inclined to abandon the theory of the direct action of quinine on the malarial parasites and to adopt the hypothesis that quinine acts by stimulating the production of antibody.

When working at this subject in 1910–11 I assumed the following working hypothesis to explain the action of quinine on the malarial parasite:

(a) That when the sporulating forms break up and liberate their spores in the plasma the liberated spores are provided with an exotoxin by which they are protected against the normal defensive mechanism of the blood.

(b) That during this protected period the spores enter the red blood corpuscles.

(c) That they then cease to act as foreign bodies in the plasma, being enveloped by the host.

(d) That the diffusion of their toxin is the cause of the paroxysm of fever.

(e) That the beneficial effect of quinine on the disease depends upon its ability to remove the protection afforded by the enveloping corpuscle to the contained parasite.

(f) That quinine effects this by acting as a haemolyzer of the infected red corpuscles.

(g) That, its protective envelope being removed, the parasite is dealt with by the normal defensive mechanism of the blood.

In support of this hypothesis I showed experimentally that quinine administered to a healthy person (who had previously had malaria, and who was exposed to daily infection) was followed by a marked increase in the excretion of urobilin in the urine.¹ My article concluded with these words, "To exclude the possible influence of malarial infection, a similar experiment in a non-malarious country on persons who have never suffered from malaria is very desirable."

As a substitute for the above hypothesis, the theory that quinine acts by stimulating the production of antibody is attractive, but it would fail, I believe, to explain the increased excretion of urobilin shown to follow the administration of quinine to a patient who had previously suffered from malaria.—I am, etc.,

Kidlington, Oxon, May 8th.

W. M. GRAHAM, M.B.

SIR,—Colonel G. T. Rawnsley's letter on this subject in your issue of April 19th is interesting, but I trust no one will consider it conclusive as to the prophylactic use of quinine for malaria.

Let it first be recognized that there is no acute malaria (of any type) that does not usually react to a timely dose of 10 grains of quinine. If this be a fact (and I say it is), then a prophylactic dose does not require to exceed this amount. But the frequency and the division of the total dosage depend upon the local degree of infectivity—that is, the possibility of infection by mosquitoes, say, daily. Consequently one can easily understand why, in 1916, 5 or 10 grains given on two successive days in the week proved failures. Infection occurred in the intervals, and while acute attacks were prevented time was permitted for new spores to carry on the infection. This holds good for 1917—namely, (a) 10 grains on two successive days weekly, and (b) 10 grains on two successive days twice weekly. Now we come to the larger doses employed in 1917—(c) 10 grains daily, (d) 15 grains daily, (e) 20 grains daily. In each of these cases these doses were given temporarily, that is, a few days before and a few days after coming out of highly malarial parts of the front trenches. Whilst excluding the necessity for giving 20 grains and permitting the two former doses, I submit that the short period of

carrying out these prophylactic doses is in itself complete evidence for the failure of the incidence of malaria. It is not surprising that after "some weeks" (why the definite number of weeks is not stated I do not know) there was no reduction in the incidence of malaria, and I presume none of these cases went to an absolutely free malaria area, and consequently any spores in any of them or reinfection from their area would suffice to account for the "80 to 90 per cent. of the units infected." What should have been done was the daily administration of quinine to all of these men for a period of "some weeks" after they returned from the trenches, and quinization of them on alternative dates up to the fourth month from the trenches or from the malarial area.

We are not told the incidence of malaria during the dosage whilst under campaigning conditions. Therefore, to conclude that "no dose could be tolerated that had any protective value to troops under campaigning conditions" is unjustifiable. The failure resulted not from dosage, evidently, during campaigning, but from failure of regular quinization for a definite and sufficient period thereafter.—I am, etc.,

London, N., April 19th.

A. G. NEWELL, M.D., D.P.H.

SCOPOLAMINE-MORPHINE IN CHILDBIRTH.

SIR,—Mr. Webb-Johnson's letter in your issue of April 26th, p. 531, leads me to suggest that a safe method of relieving the severer pains of labour, which is all that really matters in most cases, lies in the hands of every practitioner, however busy he may be. My method—which, no doubt, has been adopted by many others besides myself—is as follows. Any idea of producing "twilight sleep" from first to last is abandoned. The patient is encouraged to believe that she will not be allowed to suffer, at all events for any length of time, any pain beyond what she can bear with reasonable ease. Now, it is well known that the alkaloids fail to act as desired if the initial dose is administered alone after the pains have become strongly established, but if chloroform is given in sufficient quantity to produce "surgical anaesthesia" for a few minutes after the initial dose of alkaloids, "twilight sleep" is readily established as the effects of the chloroform pass off.

As soon, then, as the pains begin to become severe the initial dose of alkaloids is administered, and, the doctor having been sent for, puts the patient under chloroform. As the effect of the latter begins to wear off he can leave the case, all being well, for two and a half to three hours. Among the majority of normal multiparae, and in many primiparae, this is all that is necessary as far as the alkaloids are concerned. The second stage is now often far advanced, and may be completed with light chloroform anaesthesia as in ordinary cases. Otherwise one or more minute doses of hyoscine, repeated every two hours and reinforced, if necessary, by momentary "surgical anaesthesia," is all that is needed to relieve severe pain. In delayed labour this treatment may be continued until the perineum begins to distend, when resort may be had, as before, to light chloroform anaesthesia, the alkaloids being stopped, or, if necessary, forceps may be applied under deep anaesthesia, as in ordinary cases.

When producing "surgical anaesthesia" for the purpose of initiating or reinforcing the effect of the alkaloids, it is usually necessary merely to induce, not to maintain this state. Any delay to labour caused by chloroform thus exhibited is so transient as to be negligible. One does not claim that this method is "fool-proof" nor that it is always infallible, but the margin of safety to mother and child, always reasonably wide, is increased practically to infinity in the hands of any practitioner accustomed to administer chloroform at childbirth, while the cases in which severe pain is not prevented are very exceptional. It will be noted that the use of the alkaloids is reduced to a minimum.—I am, etc.,

V. CHASTEL DE BOINVILLE, M.D.,

Capt. R.A.M.C.(T.).

Late Honorary Anaesthetist David Lewis Northern Hospital, Liverpool.

April 28th.

URETERS AND THEIR ORIFICES IN GUNSHOT WOUNDS OF THE SPINE.

SIR,—From the correspondence under the above heading it appears that the treatment of the bladder by avoiding all catheterization and allowing the bladder to overflow,

¹ *Ann. of Trop. Med. and Parasitology*, vol. v, No. 3, December, 1911.