On the whole the last sort of diet on which to develop chronic constipation, if diet can do anything to arrest its acquisition.

Anxious to investigate this apparent anomaly I asked him to what he attributed his constipation. He replied that it commenced from the time when he first went to a school; he was so anxious to learn that he deliberately inhibited the calls of Nature in order that he might have more time for his studies. In the early morning there was always the rush to get away to school and the moment he had swallowed his food the same hurry to return to his books.

From this habit thus acquired in early life he never afterwards escaped.

When I saw him he was supposed to be dying from an inoperable malignant growth of the bowel, an exploratory operation having taken place some 5 months earlier.

The case is instructive in that it illustrates the fact that of all the factors contributing to produce this state inhibition is the most powerful.

It matters little how well balanced and physiologically perfect a diet may be if the calls of Nature are systematically disregarded, the result will be the same as though white bread and fish and chips were the daily fare.

The unlettered agriculturalist on the other hand has few temptations to neglect the calls of Nature; he has never been exposed to the rush and hurry of town life, never had to submit to the exigencies of school training, never had to wait his turn at the latrine, for, wheresoever he may find himself in field or jungle, his latrine is at hand.

The intention of my article was to approach the subject from the dietetic point of view. I can only speak for the Punjabi whose diet, of course, varies widely from that of the Bengali. I should imagine that the rice-eater would be more prone to suffer from stasis than the whole-meal wheaten flour eater, but I have no first-hand evidence on the subject.

As regards the educated Punjabi his diet differs greatly from that of the illiterate villager; the town dweller and the babu class generally are coming more and more to the use of steel rollered Delhi flour and do not eat a great deal of raw fruit and vegetables.

Constipation seems to be very common amongst the clerical and town-dwelling class, but very rare amongst the country people.

Hence in recording cases of acute appendicitis in Indians very definite information is wanted as to diet, occupation, literacy, and previous medical history as regards the existence of constipation or not.

The same applies to the connected diseases such as gall-stones, gastric and duodenal ulcer, visceroptosis, etc.

If it is true, as I believe, that the uneducated villager enjoys an immunity from the group of diseases under discussion, then his habits are as well worth investigation as were those of those Gloucestershire milk-maids whose immunity from small-pox suggested a line of research to Edmund Jenner which led to the discovery of vaccination, the most momentous advance in preventive medicine which had been made up to that date.

I cordially agree that what is wanted are accurate figures, but in all such returns and reports I suggest that dietetic habits, literacy or the reverse, and social position should be clearly indicated.

This is, I fear, a long letter, but the importance of the subject must be my excuse.—Yours, etc.,

H. HALLILAY,
Lieut.-Colonel, I.M.S.

LAHORE,
17*th November*, 1924.

[*Note.*—We would welcome a draft of a proposed enquiry circular or card in connection with this subject, asking for all details required, for circulation to all civil surgeons in India. The information thus collected, when analysed, might prove to be of considerable evidential value.—EDITOR, *I.M.G.*]

INTRAMUSCULAR QUININE.

To the Editor, THE INDIAN MEDICAL GAZETTE.

SIR,—I see that the *Indian Medical Gazette* is once more reviving the stunt of the dangers and inutility of intramuscular injections of quinine; in fact the crusade is so strenuous that we may soon expect to see the subject filmed, and the sadistic character of the exponents of this method of treatment shown up by lime-light. I am surprised that hitherto no one has had the temerity to break a lance on the subject, and I can only conclude the reason to be that the practice of intramuscular injections appears to be almost universal in malarial countries, but that those who use it have little to reply to theorists but continue to use it, satisfied with their clinical results. I have used intramuscular injections of quinine for nineteen years, being taught how to give them by Dr. Valentine, then medical officer to the North Cachar Tea Estates, and in that time I have given many hundreds, for in my early days I gave them much more freely than I now recognise to be necessary, and in no single case have I experienced any distressing result. Many hundreds also have been given by my subordinate staff, and I have never seen an injection go wrong, save when, against my instructions, it has been given in the arm. There are few operations, or treatments, which cannot be shown on occasion to result in failure or consequential damage, and such will be more frequent if treatment is carried out improperly. An intramuscular injection is a minor operation, and if not done correctly, or sepsis not properly guarded against, naturally, serious consequences may ensue. For instance, the arm should never be used, it appears to possess a peculiar liability to form a sloughing ulcer, and it is difficult to understand why injections are still given in that limb, for as far back as 1912 I was taught in the London School of Tropical Medicine that the practice was dangerous. I have always given injections in the gluteal region, choosing the highest point where the muscle is deep enough to sink the needle in, and the needle must not be the ordinary short hypodermic needle, but long enough to make sure of giving the injection well into the muscle. I have always used Burroughs and Wellcome's hypodermic "tabloids," and an ordinary 20 to 30 minim glass hypodermic syringe. In spite of laboratory experiments some years ago, in which the danger of tetanus and other evils was supposed to be proved, I think there were very few practitioners in Assam who did not continue to use the intramuscular method, being satisfied of its safety and the benefit derived, and that absorption was fairly rapid. I had confirmation of the last point in the case of a patient who, in three to four hours of each ten grain injection, developed a quinine rash. It appears to be generally accepted now that the action of quinine in malaria is not as simple as was at one time supposed, and if certain complex changes must first be effected, perhaps in the muscular tissue, it is open to doubt if intravenous injections have much, or any, advantage in rapidity of action. My impression is that cases receiving no further treatment relapse sooner after intravenous injections.

The ordinary case of malaria is amenable to quinine given in solution by the mouth, and the indiscriminate giving of injections of any kind is to be deprecated. They have their uses, in cases of heavy infection, when it is necessary to quickly control the disease, in cases of vomiting, especially the bilious type, in the case of those not absorbing the quinine, or not taking it as prescribed, and in some chronic cases, in which the extra stimulus appears to be helpful. Intravenous injections ought certainly to be tried in the cerebral form,—but does experience show that they are of much avail?

It is probable that there is little to choose between the two methods as regards efficacy. The intramuscular is more painful, but the pain is not great; the patient feels as if he was bruised, but this seldom lasts for more than a couple of days. I have known patients play polo on the next day but one. Intravenous injections have a pull in the matter of pain, but they require more care and skill in their administration, and the risks are undoubtedly greater. It has to be remembered that the treatment of malaria in India is largely carried out by comparatively untrained and unskilled men; it is difficult to realise the ignorance of asepsis that obtains, and I think most of us, in spite of gruesome pictures, would infinitely prefer, if the occasion arose, to be given an intramuscular injection, by such, in preference to an intravenous. A mishap with the latter and one might not live to be photographed!—Yours, etc.,

R. A. MURPHY.

LUSKERPORE TEA ESTATE,
CHANDPORE BAGAN P. O.,
SOUTH SYLHET,
18*th October*, 1924.

[*Note.*—We publish Dr. Murphy's letter with much pleasure on the principle of *audi alteram partem.* With regard to the question of tetanus, it does not rest solely on Sir David Semple's work. A very sad tragedy which occurred several years ago in Central India was the death from tetanus of a young English girl, just out from Home, a few days after an intramuscular injection of quinine. The injection had been prepared at a British Station Hospital with every possible precaution taken; but investigations shewed that tetanus spores were present in the distilled water used, and had probably got into it from the straw packing in which the bottles of distilled water had been received by the hospital.

We agree with Dr. Murphy that the arm is an entirely unsuitable site. Not long ago an adult female Anglo-Indian patient was admitted to the Carmichael Hospital for Tropical Diseases in Calcutta. Having gone down with fever she had consulted a practitioner who had given her a course of intramuscular injections of quinine into the deltoids, without apparently even examining the blood. On admission there was a large and foul ulcer in the right arm, at the bottom of which the humerus lay exposed; in the left deltoid an abscess was developing. The case turned out to be one of typhoid fever, and ultimately proved fatal; the fatal issue being almost certainly hastened by—if not due to—the exhaustion of the patient's strength by the prolonged suppuration.—EDITOR, *I.M.G.*]

THE PSYCHOLOGY OF FREUD.

To the Editor, THE INDIAN MEDICAL GAZETTE.

SIR,—I have read with much interest the correspondence appearing in your journal on the psychology of Freud. Lieutenant-Colonel A. W. Overbeck-Wright has a distinguished record in alienist work, and his remarks against the Freudian standpoint certainly deserve a fair consideration. Freud's doctrines have not received universal recognition as yet, and there are bound to be two sides to the question. It is, therefore, easy to quote authorities either favourable or otherwise, according to the leanings of a particular writer.

To condemn a controversial point, something more substantial than the mere quoting of authorities is necessary. One is disappointed to find that Colonel Overbeck-Wright's arguments consist only of vague assertions; "condemned by the majority of British alienists," "discarded by all reputable psychiatrists," etc. The "public denouncement by the British Medical Association" is no better. By the way, one becomes curious to know on what occasion this august body passed such a weighty judgment. Even if the whole world stands against a doctrine, it does not necessarily prove its fallacy.

One looks in vain for a criticism based on personal experience of the subject in Colonel Overbeck-Wright's letter. Most of the critics denouncing psycho-analysis belong to the armchair type. It is, of course, presumptuous on my part to refer such a distinguished alienist as Colonel Overbeck-Wright to the history of hypnotism with which he is no doubt familiar. Hypnotism was repeatedly condemned by successive scientific commissions in different parts of Europe, and there were many distinguished persons on them. In spite of all hostile criticisms, hypnotism has held its own.

Psycho-analysis has been in the field now for more than a quarter of a century, and there have been adverse critics who have foretold its end before Colonel Overbeck-Wright, but much to the chagrin of all these prophets, psycho-analysis is still going strong. Colonel Overbeck-Wright in the course of his alienist work must have seen persons foretelling the end of the world and daily expecting to see their prophecy fulfilled. The wish in such cases is father to the thought, and although one may sympathise with such a person's mentality, one may as well be excused for refusing to set any value on such assertion.

There is, however, no reason to become impatient of Colonel Overbeck-Wright's attitude. The history of all religious and political movements amply proves that the most violent critic is apt to become the most ardent convert.—Yours, etc.,

G. BOSE, D.SC., M.B.,
President, Indian Psycho-Analytical Society,
Lecturer in Abnormal Psychology,
University of Calcutta.

14, PARSIBAGAN, CALCUTTA:
26*th October*, 1924.

"PHENOLAINE."

To the Editor, THE INDIAN MEDICAL GAZETTE.

SIR,—With reference to an enquiry regarding 'Phenolaine' in the July number of the *Indian Medical Gazette,* I have to inform you that the drug is a powerful liquid local anæsthetic, and can be employed in operative work of every description.

It can be had from Messrs. W. Martindale, 10, New Cavendish Street, London, W. in 1 dr. and 1 oz. bottles. —Yours, etc.,

VALLABHDAS N. MEHTA, L.M. & S.

VIRAMGAM:
2*nd November*, 1924.

CHUNAM CANCER.

To the Editor, THE INDIAN MEDICAL GAZETTE.

SIR,—In the report on the surgical work at the Government General Hospital, Madras, published in the October issue of your journal, Colonel Bradfield evidently lays much stress on the part played by *chunam* in the causation of cancer of the cheek and lower jaw in South Indians.

The local irritant effects of *chunam* should not make us lose sight of the fact that there is another irritant which contributes an equal share, if not more, to the causation of the said cancers. I refer to tobacco. In the majority of such cases of cancers, on careful enquiry there is invariably a history of the tobacco habit. The tobacco, either alone or more usually with betel leaves smeared with a little *chunam* is taken chewed, and retained between the cheek and the lower jaw. Some people retain this bolus for hours together every day. Thus ample opportunity is offered for the tobacco to work its mischief and exert its irritant action