

whether infection may possibly or probably be conveyed by any given mode of administering holy communion is one for medical experts only; it is foolish for anyone else to give an opinion. It has, indeed, been suggested that communicants, obeying a Divine command, may commit themselves to the Divine protection against possible harm. I cannot put that suggestion summarily aside, because I believe in what is commonly called "miracle" as possible and not unfrequent; but I should meet it by a reminder of the theological commonplace that miracle must not be unnecessarily presumed either as an explanation of fact or as a guide to action; such presumption, therefore, is no excuse for doing a dangerous thing which is avoidable. It is technically presumptuous to hope for miraculous protection without taking all natural precautions. It is for medical experts alone to say whether any given precaution be adequate.

Is the peril, then, avoidable? If our actual practice be dangerous it is for ritual authorities to consider what may be substituted. There seems to be a tendency in some quarters to assume that no change can be made. I cannot understand this. Theologically speaking, it is to me inconceivable that God, Who reveals to scientific research the hidden perils of nature, has tied the administration of His Sacrament to a mode which that revelation shows to be harmful. Practically speaking, there are several modes of administration known in the Church. Careful consideration of the question will certainly be demanded. The *Church Times* was lately urging the English bishops to appoint a commission of inquiry. Even at the present stage of discussion it may be useful to recall the methods used.

In the greater part of the Eastern Church the method of *intinction* prevails, to which you have already referred. A kind of sop is made in the chalice, which is administered by a spoon. I believe the spoon is rinsed after each using, with what effect the proper experts may say. This method is not perhaps very likely to find favour with us.

In the West, before the withdrawal of the cup from the laity in the twelfth and following centuries, the sacred species was often administered by suction through a *fistula* or metal tube. This would probably be even more open to objection than our practice, in which the lower lip only comes into bare contact with the outside of the cup.

Early in the eighth century the Pope, St. Gregory II., forbade the use of many chalices on the altar for symbolical reasons, requiring all to communicate from one cup. This brought in the use of very large vessels, from which the sacred species was poured into smaller cups, called *scyphi*, for the communion of the people. This may show that no theological or ritual exception can be taken to the use of a separate cup for each person, advocated by your American correspondent, but there is an overwhelming practical objection where any large number is concerned. The rule of the English Church requires the total consumption of the consecrated species by the priest, alone or with the help of some communicants. This cannot be effected save by the usual practice of pouring into the cup some wine or water, or both, and drinking this. The treatment of several hundred cups passes imagination.

There is a practice, not formally adopted, so far as I know, anywhere in the Church, but sometimes followed by individual priests in difficult cases of clinical communion, which may be worth mentioning. A particle of the species of bread is slightly tinged in the chalice and then laid on the tongue of the communicant. This method of giving communion presents little practical difficulty and might conceivably be adopted by authority for general use.

Some of your readers may be glad to have this material to work upon in their character of medical experts.

I am, Sirs, yours faithfully,

Madingley Vicarage, Cambridge, Nov. 28th.

T. A. LACEY.

BERI-BERI AND CHRONIC ARSENICAL POISONING.

To the Editors of THE LANCET.

SIRS,—I trust that you will allow me to call attention in your columns to a point of some interest in connexion with tropical medicine raised by the present epidemic of peripheral neuritis in Chester and Manchester. The extraordinary similarity of some of the cases to certain phases of beri-beri seems to me to suggest the question whether many instances attributed to the latter disease may not really be due to chronic arsenical poisoning. Owing to

the kindness of several medical gentlemen I had the opportunity of examining pretty carefully 10 of the Chester cases, and also of seeing several of those which had occurred at Manchester. I certainly thought that Dr. W. A. Newall and Dr. J. R. Prytherch of Chester were abundantly justified in noting an extreme resemblance between their cases and cases of beri-beri. The local anæsthesias, especially of the feet, of the tibial skin, and of the finger-tips, the dropsy over the tibiae, the tenderness of the calf muscles, the loss of the deep reflexes, the ataxia combined with paresis, even the peculiar spacing of the heart-sounds in some instances, were all so suggestive that had the cases occurred in a beri-beri region I believe few medical men would not have returned them under that heading.

Assuming the cases to be due to chronic arsenical poisoning, we may well ask whether similar epidemics occurring in the East or on board ships may not often have been wrongly attributed to beri-beri. The possibilities of arsenical poisoning on vessels are too obvious to need reference in detail. Natives of India and Burma are very fond of lemonade which is manufactured from European chemicals—probably of the cheapest sort. They also often delight in tinned and bottled fruits. The occupants of jails, hospitals, and asylums may suffer from arsenical poisoning through half a dozen routes. The sudden disappearance of beri-beri from the Japanese navy after a general change of diet is certainly suggestive. It has long been thought on epidemiological grounds that beri-beri may be due to some form of food-poisoning; it now seems just possible that the poison may be, at least in some cases, nothing but arsenic. The sudden improvement occurring in most cases after removal from the house where they contracted the malady is certainly more suggestive of intoxication than of infection. In short, regarding such epidemics of beri-beri as I have had the opportunity of witnessing, I should be sorry to be called upon to give a definite statement to the effect that they were not due to chronic arsenical poisoning.

Of course there may be, and very probably so, a true beri-beri, but the points of distinction do not seem to be very clearly established at present. I think I remember bullæ and even pigmentation in India. Certainly cases approximating to the "wet" form of beri-beri do not seem to have been noted in Chester; but this is, I think, a later stage of the disease, and may perhaps have been overlooked in chronic arsenical poisoning. Lastly, our knowledge of the epidemiology of beri-beri cannot be said to be so complete as to enable us to exclude all possibility of arsenic being concerned in the causation at least of many cases attributed to the disease.

In conclusion, I hope that it will be understood that I am only suggesting a possibility and that I do not presume to erect a theory. The next ship which arrives infected with beri-beri at an English port will afford an opportunity for an examination of the question.

I am, Sirs, yours faithfully,

Liverpool, Nov. 30th, 1900.

RONALD ROSS, D.P.H.

"THE TREATMENT OF SPRAINS AND OF SOME FRACTURES."

To the Editors of THE LANCET.

SIRS,—For the last 20 years I have been treating sprains by the application of adhesive plaster and, like Mr. Christopher Heath, I have found this plan far more satisfactory than the older methods. Since I first became aware of its value I have never omitted to use this plaster in some stage or other of the treatment of these injuries, even in sprains accompanying fractures. I was first led to realise the advantages likely to result from so firm and steady a support as that effected by adhesive plaster when studying oedematous tissues microscopically for the purpose of the "Atlas of Histology." The plasma which in oedema is infiltrated into the connective tissue, separating almost every individual bundle from the others, seemed likely not only to prevent repair by its pressure, but also (from lack of complete absorption) to produce permanent thickening of the tissues by becoming organised.¹ This condition pointed to the desirability of preventing, if possible, this infiltration from taking place, and if it had already taken place of promoting its absorption, and the firm support without undue

¹ Atlas of Histology, Plate VIII., Fig. 14.

pressure given by adhesive plaster seemed just what was required.

In using adhesive plaster for this purpose certain precautions are necessary. No attempt must be made to produce pressure; the plaster must be laid on very evenly, being held only just sufficiently firmly to allow it to adhere without creases. It is necessary to use a plaster which does not require heating, and there are several so-called rubber plasters now made which are suitable. Strips from one to two inches wide are best, and they should just overlap each other. After a few days, or sooner, the plaster should be removed and fresh strips applied. If the deep tissues are not very severely injured it is well to precede the application by about five minutes or more of gentle massage (towards the heart). Massage is also very useful in the subsequent treatment whenever the plaster is removed. In addition to massage passive movement is generally beneficial. I have applied strapping in a case in which I feared gangrene of the tissues round an ankle, the joint not only having been sprained but the soft parts also severely crushed. The patient made a good recovery. I have also applied strapping in the case of a Pott's fracture which necessarily involved a severe sprain. The effect was to prevent swelling and thus greatly to hasten the cure; and as there was consequently very little infiltration of plasma subsequent stiffness of the ankle was prevented. If a sprained joint is already swollen when seen by the surgeon it is well to raise the affected part above the level of the body for a few minutes to allow gravitation of the blood and during this process the massage may be carried out and will help in reducing the oedema.

Although it is best to use the plaster from the first, yet the beneficial effect of this treatment of sprains is very noticeable when applied to patients who have undergone some days of treatment by the old plan—i.e., rest of the affected limb and the application of evaporating lotions. The swollen joint will be rapidly reduced in size by absorption of the infiltrating fluid after application of the plaster. In these circumstances we should remove and re-apply the plaster as soon as the oedema has visibly lessened, and this may be necessary every day, until very soon the joint will have assumed its natural size, in perhaps as many days as it would otherwise have taken weeks. The advantages of the adhesive plaster treatment are immense. Often the patient may begin to walk about directly the plaster is applied, and there can be no doubt that early use of the part as well as passive movement and massage assist the cure.

Dislocations.—The difference between a sprain and a dislocation is chiefly a matter of degree as regards the injury to parts surrounding a joint, and therefore the same considerations in respect to treatment should prevail. Without entering into details with which many of your readers must already be familiar I would sum up the treatment of dislocations as follows: 1. Reduction under an anæsthetic, in order to avoid unnecessary and detrimental disturbance of the injured tissues. 2. Support by means of adhesive plaster and light bandaging. 3. Early *passive* movement, commencing 24 hours after reduction and repeated daily. 4. Massage. 5. Early *active* movement.

Undoubtedly, there may exist complications which will necessitate a modification of this line of treatment, but in the majority of cases the above rules may be followed with advantage; the period of treatment will then be considerably shortened from that pertaining to the older methods, and stiff joints will almost certainly be avoided or much reduced in degree.

I am, Sirs, yours faithfully,

Queen Anne street, W., Nov. 24th, 1900.

NOBLE SMITH.

OLIVE OIL IN TYPHOID FEVER.

To the Editors of THE LANCET.

SIRS,—In view of the enormous mortality from enteric fever amongst the British troops in South Africa may I again bring before your readers a method of treatment advocated by myself in your paper about two years ago? ¹ It consists in slowly administering every 12 or 24 hours an injection of olive oil per rectum. The quantity used for adults should be about a pint at a time, and should be retained in the bowel from 12 to 24 hours if possible. If not returned in this time it may be brought away with an

ordinary soap-and-water injection, a fresh dose of olive oil being administered two or three hours later. After a week or 10 days the daily use of the injections can be discontinued, and they are given only when the temperature rises or the bowels are confined. With diarrhoea it is imperative to give olive oil, and those who try it will find it more efficient than opium or any other drug in bringing about a natural discharge of fæces. Occasional small doses of calomel from one-tenth of a grain to half a grain are useful if the oil is not sufficiently stimulating to make the bowels act. I use it now instead of giving olive oil by the mouth, which was found to be too nauseating for better-class patients. My only medicine for typhoid fever, in addition to olive oil and the above-mentioned occasional doses of calomel, consists of carbonate of ammonia, glycerine, and decoction of cinchona given every three or four hours. Whether this preparation has any beneficial effect or not I am unable to say; it has the virtue of being probably harmless, and without the glycerine was a favoured preparation of Percival Pott. With oil injections the bismuth preparations are quite unnecessary. Patients treated in the above manner in my opinion cannot die; for careful observation has led me to believe that typhoid *per se* is a harmless fever, it is the accompanying sapræmia or ptomaine poisoning which produces the ill-effects. Under this line of treatment there are no sequelæ, tympanites, perforations, or heart failure. No cold baths are necessary. The most unskilled nursing is sufficient. Its only drawback occurs in private practice, where patients get well so easily that no credit accrues to the physician. Medical friends in Australia and Canada endorse my opinion as to its value.

I have heard that olive oil given per rectum has been found post mortem in the stomach less than 24 hours after administration; if this is true it is of extreme interest as showing that the beneficial effects of the oil extend above the ileo-cæcal valve. During a visit home last year a bacteriologist hinted that my patients were not suffering from true typhoid fever, but from some fever due to a closely allied bacillus. I can only state that my friend Dr. W. T. Dermer examined a large series of cases here and obtained Widal's reaction in nearly all of them. Large bodies of men, as armies or gold-prospectors, who invade an undeveloped country, are invariably attacked with typhoid fever—witness West Australia, Klondyke, the Cuban campaign, and South Africa. My own hypothesis is that the bacillus present in virgin soil acquires virulence by passage through a series of hosts. An old prospector informed me that he knew of numbers of typhoid-stricken places in Australia which had completely cleared themselves in a few years without any attempt at sanitation. Do the inhabitants and their children acquire immunity or does the bacillus lose its virulence? People alone in the bush, living on apparently virgin soil, can develop typhoid fever. In the case of my own child, aged two and a half years, who nearly died from enteritis (not typhoidal) accompanied by a recrudescence of malaria, and who on recovering from the acute stage daily passed casts of the intestine, the condition was completely cured in from two to three weeks by injections of olive oil, protargol and other remedies having been used in vain.

I am, Sirs, yours faithfully,

OWEN F. PAGET, M.B., B.C. Cantab.

Fremantle, Western Australia, July 21st, 1900.

"PROFESSOR LUSTIG'S PLAGUE SERUM."

To the Editors of THE LANCET.

SIRS,—I am much indebted to your special Indian correspondent for excusing my "personal attacks"—so he is pleased to call the repudiation of his erroneous assertions—on the plea that "such attacks may be common in debate on the continent of Europe," and in return I cordially sympathise with him on "an extraordinary thing" which happened to him in his reply in THE LANCET of Sept. 22nd, page 903—viz., to pose as a martyr correspondent "who cannot criticise without ignorance or wilful misleading being put forward to explain his opposition," and at the same time to substantiate in a most creditable manner the first of the two alternatives he is complaining of. The present controversy arose from your special correspondent's insinuation in THE LANCET of June 2nd, page 1608, that in the second series of the experiments with Professor Lustig's plague serum *selection or exclusion* of patients was practised. Although from my letter in THE LANCET of

¹ THE LANCET, August 13th, 1898, p. 409.