

stomach in a variety of ways. The routine followed by myself is as follows:

1. Rest in bed until the ulcer has healed; the criteria of healing being three negative occult blood tests upon successive stools, and an absence on *x*-ray examination of both deformity and local tenderness.

2. Half a drachm of a standard alkali powder after each feed.

3. Strict Lenhartz diet. If healing is delayed, a second course is given, starting from the third day of the diet.

4. Belladonna is pressed, being given in full pharmacological doses—that is, just below the level of tolerance. The tolerated dose is often as high as *mxx* t.d.s. p.c. of tinct. belladonnae.

5. Olive oil ζ ss nocte. This both inhibits gastric secretion during the night and produces some laxative effect.

The total duration of such a course may be as long as ten weeks. This routine, or one of equal severity, is necessary before any group of cases can be regarded as having undergone adequate medical treatment, and no statistical information as to the value of medical treatment of gastric ulcer can be trustworthy unless based upon similarly treated groups of cases. Relief of pain and apparent healing can be obtained by less stringent means, but relapse is common.

A point of great importance arises if the above premisses be granted—namely, that treatment on these lines would soon result in the over-filling of the beds of general hospitals with cases of chronic gastric ulcer. It would therefore seem that the disease will not receive thorough treatment until there is available a large hospital whose whole activities are directed to this form of therapy upon a mass scale. To such an institution the surplus cases could be referred from general hospitals. Only in this way will the out-patient departments be freed from the reproach of inadequately treating patients with chronic gastric ulcer who are awaiting admission. At present their only means of obtaining adequate therapy are by producing a haematemesis, by suffering a perforation, or by prolonging an interminable existence dependent upon the doubtful vagaries of a long waiting list.—I am, etc.,

London, W.1, Jan. 5th.

GEOFFREY BOURNE.

COLPORRHAPHY FOR PROLAPSE

SIR,—I was much interested in the article in the *Journal* of December 26th, 1931, on displacements of the uterus and the pelvic floor, by Dr. S. Gordon Luker, and so pleased to find that he advises the operation of colporrhaphy as the treatment for prolapse of the uterus. Thanks to the pioneer work of my old chief, Professor A. Donald, this operation was firmly established in Manchester for the treatment of prolapse when I first entered the ranks of gynaecology twenty-seven years ago, and so good are its results that I have never employed any other.

I am sorry to see that Dr. Luker flirts with the idea of combining a ventral suspension operation with the colporrhaphy, and I am especially sorry to see the reason he gives for this combination. He considers it to be unnecessary in old people, because the vagina can be made much tighter in these than in younger married women, and apparently he looks upon the tightness of the vagina as a factor in the cure of the condition. This, I think, is quite wrong; and, whether the patient is old or young, we always consider an over-tight vagina to be an indication of miscalculation on the part of the operator, and our endeavour is always to cure the prolapse by firm suturing of the deep structures, without removal of an excessive amount of vaginal tissue. The one type of case in which ventral fixation might be necessary is the elderly virgin—a somewhat rare type, but one in which there is always deficiency in the muscular tissue of the pelvic floor. Even in this type of patient there is usually sufficient muscular

tissue to make a good pelvic floor, and in twenty-seven years of gynaecological practice I do not think I have found it necessary to combine the two operations on more than three or four occasions.

My chief reason for writing is to call attention to Dr. Gordon Luker's statement that "it is not advisable for a woman to have another child after colporrhaphy for prolapse has been done, as the vagina will be lacerated and prolapse recur." There is a chance of recurrence whenever a woman has a child after colporrhaphy, just as there is with every primigravida, and I doubt if the percentage of recurrences is greater in the one class than of the original prolapse in the other. In a recent paper of mine, published in the *Proceedings* of the Obstetrical Section of the Royal Society of Medicine (June, 1930), I quoted eighteen patients who went through labour after colporrhaphy was performed, and in only five (27.8 per cent.) was there any degree of recurrence.

Quite naturally we prefer to operate upon patients who have completed their families, but in the case of a young woman who has considerable discomfort from prolapsus uteri after one child, it is not necessary to condemn her to pessary treatment for a long period of years, or to consider sterilization along with the operation of colporrhaphy. Colporrhaphy is an operation with very slight risk, and such a patient can have a colporrhaphy performed and be made quite comfortable, with the knowledge that any further labour has no more risk of producing a recurrence than had the original labour, and that the difficulty of the labour itself is not increased.—I am, etc.,

WM. FLETCHER SHAW, M.D., F.C.O.G.

Manchester, Jan. 6th.

THE PROBLEM OF TUBERCULOSIS

SIR,—Dr. Middleton Martin's interesting collection of "salient facts" (published in your issue of January 9th) seem to me to lead to other conclusions than those he draws from them. His final sentence reads: "Our aim should be to reduce the infection to the minimal protective dose—for example, by the commercial pasteurization of milk," etc.

In an earlier paragraph (7) he says: "7 per cent. of the samples [of cow's milk] are reported to be infected with the bovine form of the bacillus," and, later, "it is more or less generally accepted that immunity, more or less complete, is afforded by early infection." If an early bovine infection safeguards against adult developments, why neutralize so benign a possibility by pasteurizing milk? Another "conclusion," and a serious one, states: "Tuberculosis schemes do not appear to have had appreciable result in reducing the amount of tuberculous illness." Surely, if that is true, undue expenditure is going on all over the country by maintaining a costly service, both residential and domiciliary, even if it does, as he claims, have an indirect effect "to stimulate healthy living among the general population."

Present economic stringency calls for careful scrutiny of all public spending, and the fact that tuberculosis has a particularly lavish distribution of public funds allotted to it for certain definite reasons should cause any modification of those reasons to be considered with special care. The arguments set forth in Dr. Martin's paper might reasonably suggest certain curtailments or alterations, and so serve a national need. If, for instance, we can believe (a) that the fall of the death rate from tuberculosis is likely to proceed independently of our campaigning, (b) that the factor of direct personal infection has been unduly emphasized, and (c) that the commercial pasteurization of milk would lessen the toll of childhood tuberculosis, then surely a new orientation of outlook is definitely called for.

How may we best "reduce infection to the protective minimal dose"? In discovering the right answer to this question much of our present work may be found of doubtful value, but the "problem of tuberculosis" is still a sufficient burden to demand a revised investigation in the light of fresh knowledge and accumulated experience.—I am, etc.,

Peppard Common, Oxon, Jan. 9th.

ESTHER CARLING.

CATARRHAL INFECTION AND FORMALIN VAPOUR

SIR,—The memorandum by Dr. Wells on the above subject, in the *Journal* of January 9th, recalls to my mind an instructive, and perhaps interesting, incident which occurred during the war; but it is now so many years ago that I cannot pledge myself to more than the substantial accuracy of the facts I state.

We were experiencing a sudden and severe epidemic of influenza in the camp at Wimbledon to which I was senior medical officer, and the men were disabled by the score—this, of course, threw a great strain on my medical officers, and we decided to disinfect with formalin. Regarding the matter as urgent, I got into a light ambulance next morning and was driven to—let us say—Depot "A," where I indented for one gallon of formaldehyde. They were sorry they had run out of it; no doubt if I would go to Depot "B" I would be supplied, but it would be wise to ask for three gallons. The courteous officers at Depot "B" were in the same position; but I would certainly get it at Depot "C," provided I did not ask for less than ten gallons. At Depot "C" I was told that it would be delivered that afternoon. I got back to camp feeling very ill with influenza, unable to eat any luncheon, and only fit for bed, to which I betook myself. About 3 o'clock my sergeant woke me to say that a lorry had delivered twenty gallons of formaldehyde, what was he to do? My only reply was "Disinfect and be —"; and here I fell asleep. The next minute, as it seemed to me, I wakened coughing, spluttering, and feeling half-suffocated. Jumping out of bed I discovered my sergeant, in much the same condition as myself, vigorously scrubbing the floor with a stable broom, which he was dipping in a pail of pure formaldehyde. Postponing, for the moment, any observations I wished to make, I rushed out for the fresh air, when soon, to my astonishment, I began to feel much better. I dined in mess that night. I was well next day, and, taking the hint, less heroic but equally effective, from the sergeant's method, we cleared the camp of influenza in less than a week.—I am, etc.,

GRAHAM GRANT,

London, W.C., Jan. 11th.

Lieut.-Col. R.A.M.C.T. (ret.).

OXYGEN AND EVEREST

SIR,—Mr. Odell, in a letter to *Nature* of December 19th, 1931, re-emphasized conclusions drawn from the Everest expeditions as to the value of oxygen-breathing apparatus, and upheld the importance of acclimatization. He stated that acclimatization at 27,000 to 28,000 feet should be aimed at, and then oxygen used for the last 1,000 feet of ascent. As it takes weeks for the party to reach 27,000 feet, experiments in pressure chambers conducted on normal men for a few hours' duration are of no significance. We know, however, that natives do not go and live in the summer months above 18,000 feet, and the question arises whether it is the altitude or the extent of the pastures which sets this limit.

It was on account of this that Dr. Argyll Campbell carried out many experiments with animals while I was in charge of the Department of Applied Physiology at

the National Institute for Medical Research. He used monkeys, cats, rabbits, guinea-pigs, rats, and mice, enclosing these animals almost continuously in chambers for many weeks, either the oxygen partial pressure or the barometric pressure being lowered gradually to resemble the conditions on the mountains. In the latter case use was made of a chamber at Messrs. Siebe Gorman's works. His results have been published, and clearly show that at 20,000 to 29,000 feet the oxygen tension in the tissues is not kept at normal, but is markedly subnormal; further, there result pathological changes, particularly in the heart, liver, and brain. Also, he found that breathing oxygen at normal pressure for one hour daily did not help these animals materially.

Thus in the so-called acclimatization at these great altitudes the members of the expeditions will be deteriorating continuously; that they did so in the past expeditions is proved, by the facts both that climbing became most difficult, and that they all lost weight and appetite. The climbers who persevered at 28,000 feet or above all came to grief.

Another expedition of the same type is likely to end similarly unless other precautions are taken. Dr. Campbell found that out of more than a hundred healthy animals only about ten could tolerate the low oxygen partial pressure which pertains at 29,000 feet for any length of time. A few animals survived for eight days—a record—under this pressure, and recovered their health again when put under normal conditions.

It is obviously very difficult to pick men able to tolerate such a low oxygen pressure for such a prolonged period as is necessary on the mountain. The climbing party should be increased in numbers, and thus the chance of one of them carrying on after 28,000 feet, with or without the use of oxygen-breathing apparatus, will be increased. All the evidence from the expeditions and from these experiments on animals leads to one conclusion—namely, that above 28,000 feet deterioration is rapid and excessive, and no acclimatization prevents this. If the climbers were continuously supplied with oxygen at normal pressure they could, of course, proceed straight up to the summit. Some day, perhaps, a liquid oxygen apparatus will be established at the 18,000 feet level, and Everest thereby conquered in safety.—I am, etc.,

Chalfont St. Peter, Jan. 7th.

LEONARD HILL.

THALLIUM TREATMENT OF RINGWORM

SIR,—I have read with interest the article on thallium acetate in the treatment of ringworm of the scalp, by my friend Dr. J. T. Ingram, in your issue of January 2nd, and am in general agreement with what he writes. At the same time I think that he dismisses a little too lightly the possibility of serious toxic effects, even when errors of dosage have been eliminated. I could have recorded a series of 100 consecutive cases treated by this method without a single alarming sequel, and with a very high percentage of cure, but I have since had one patient, a boy of 8, who almost died, and took many months to return to normal, as I have recorded elsewhere (*Glasgow Med. Journ.*, 1931, cxvi, 57). The case recorded by Twiston Davies and Andrews (*British Medical Journal*, 1927, ii, 1139) also gives food for thought. I believe with Dr. Ingram that those who have condemned this method have done so without full and fair trial. The very slight risk of poisoning no more than balances the risk of permanent alopecia following x rays, the latter being a real one in clinics where only an occasional epilation is undertaken. Possibly the ideal method will be found in a combination of thallium and x rays—that is, 5 mg. thallium per kilo, and 1/2 Sabouraud—as suggested