steward alone. By pushing morphia and hyoscine to the extent of half or even three-quarters of a grain of the former, it is possible to remove even a difficult appendix under local analgesia, assisted perhaps in some cases by a few whiffs of chloroform while the appendix is being delivered.

One cannot help deploring the fact that in this country, where so many doctors have to practise singlehanded, no attempts are made in the majority of the teaching hospitals to use local analgesia and to impress its great advantages on the students.

I am, Sir, yours faithfully,

J. W. PORTER,
Bombay, August 22nd, 1922. Major, R.A.M.C. (retired).

DEATH IN THE ALPS.

To the Editor of THE LANCET.

SIR,—Reports continually appear of persons who are killed by falling in the Alps. Some of these cases occur through storms, avalanches, or other unfavourable events, but quite a large number cannot be attributed to any activity on the part of the mountains. They seem explainable only as in some way due to the acts of the persons themselves. Must we go as far as Stekel in saying that only a small part of the accidents in the mountains are actually accidents, and that they are for the most part really cases of involuntary suicide? Perhaps the following observations may suggest an answer.

The Geneva psychologist Flournoy reports the case of a man who consulted him because he lived in constant fear of dying from a fall in the mountains. This fear was persistent and overpowering, in spite of the fact that he knew he could avoid such a fate by not undertaking any dangerous climbs. For several years he made no mountain excursions at all. At last he undertook one with a feeling of perfect security. During a rest at a place where there was really no danger he went to sleep and fell into eternity. Why should he fall asleep in such a place, and why should he in his sleep make a movement that threw him into the abyss? Stekel tells of a passionate mountain climber who was in trouble. On one occasion, on the Rax, he was seized with the impulse to put an end to all his troubles by a fall. The impulse was not carried out, but shortly afterward at a dangerous point he became unable to proceed and was seized with violent giddiness. He afterwards described the condition as one of a struggle between two impulses, one to throw himself down and the other not to do so.

Many people undergo experiences like the one lately related to me by a patient. While climbing in the mountains around Badgastein he had to pass along a narrow path on a long steep slope. The path was perfectly safe, but he was seized by such an uncontrollable fear of falling that he could not proceed. Later, on coming to a ridge where the other side descended to great depths, he was obliged to throw himself on the ground, not on account of a fear of falling—of which there was no possibility as long as he remained where he was—but because he knew that if he made a step further he would throw himself over the ridge. This same impulse had all his life long made it impossible for him to approach the edge of any building or to cross a bridge without a rail to hold on to. It was quite frankly an overpowering impulse to commit suicide that lay in his unconscious mind and came to the surface only when there was a chance to fall. The fear in such a case is not a fear of falling, but a fear of yielding to the suicidal impulse. Such unconscious impulses often execute themselves during sleep. I have no doubt that the man of Flournoy's account fell asleep in that particular place because his unconscious mind wished to get the opportunity of carrying out its plan while consciousness was dormant. I further venture to assert that for many persons who are troubled by giddiness and fear in the mountains the cause is exactly the same—namely, an unconscious wish to commit suicide.

Mountain climbing has often been deliberately and consciously used as a means of committing suicide

respectably. The above considerations seem to indicate that it may be used unconsciously and involuntarily for the same purpose.

I am, Sir, yours faithfully,

E. W. SCRIPTURE.

New Cavendish-street, W., Sept. 6th, 1922.

PUERPERAL SEPTICÆMIA AND SCARLET FEVER.

To the Editor of THE LANCET.

SIR,—On July 7th I was called by a midwife to a puerperal woman with fever. A swab taken from the interior of the uterus was sent to Dr. E. Cronin Lowe, who reported a pure culture of *Streptococcus puerperalis*. On July 8th one of the patient's children developed scarlet fever, and was removed to the isolation hospital. On July 9th another child developed scarlet fever; in this case a swab from the throat showed *Streptococcus brevis*. On July 10th a third child developed scarlet fever. The throat was swabbed and a few streptococci found in the films, but these could not be isolated. The mother, who recovered, was treated on the first day with 500 millions detoxicated streptococci. The uterus was douched once with liq. sodæ chlorinatæ 1 in 3. 750 millions detoxicated streptococci were given on the second and 1000 millions on the fourth day. I am, Sir, yours faithfully,

J. HARGREAVES ROBINSON.

Southport, August 29th, 1922.

THE SCOPE OF VACCINE THERAPY.

To the Editor of THE LANCET.

SIR,—In your issue of Sept. 9th Dr. C. E. Jenkins in his paper on this subject makes two statements which should not in my view be allowed to pass without protest.

(a) He says: The number of injections (apparently of any vaccine) should not exceed eight. In the practice of the laboratory which I direct, we have found that in respiratory diseases (including prophylaxis) and the various ailments arising from intestinal stasis we have obtained much better results by diminishing the dose and increasing the number of injections; in the vast majority of our cases the prescribed course consists of 16 doses, and we find that the improvement in the patient's condition is usually greater during the second half of the treatment than the first.

(b) The old type of vaccine is not, in Dr. Jenkins's opinion, preferable or even equal to the residual vaccine in the improvement it produces. Here it is, perhaps, doubtful whether he uses the word "residual" as synonymous with "detoxicated," as he evidently does in the sentence immediately preceding it. If so, I can only say that in gonococcal and catarrhal vaccines quite half of the cases which I see in consulting and laboratory practice are sent by their medical attendants for an autogenous vaccine, because they have derived little or no benefit from the "detoxicated" variety.

Both the points on which Dr. Jenkins is dogmatic are, in my opinion, open to discussion, and it is better for the pathologist to treat each case on its merits.

I am, Sir, yours faithfully,

A. KNYVETT GORDON, M.B.

Bedford-square, W.C., Sept. 12th, 1922.

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