the needle would be destroyed. At the same time the needle is oily, so that it goes through the skin more easily and remains sharp for a long time.—I am, etc.,

London, W.1. ALEXANDER FLEMING.

REFERENCES

1 Wright, Sir A. E., and Colebrook, L., *Technique of the Teat and Capillary Glass Tube*, 1921, London.
2 *Lancet*, 1946, **2**, 87.

SIR,—I have read with great interest the article on this subject by Drs. R. J. Evans and E. T. C. Spooner (July 22, p. 185). By a curious coincidence I have recently referred to the same problem in an article in the *Medical Officer* (July 15, p. 29), the point there being prevention of the possible transference of the virus of poliomyelitis from one individual to another. Without then having the benefit of the experiences described by the above workers, I suggested the following: (1) Only the amount of material required for one injection should be drawn up into the syringe. (2) Obviously a sterile needle is used for each case. (3) After discharging the contents, the plunger should be kept pressed firmly home until the needle is withdrawn from the arm.

I was not apparently far wrong, with the exception of the possibility of fluid being sucked up the bore of the needle in the act of removal from the syringe, owing to the vacuum created when disconnecting the boss.

Here, then, is a further suggestion. Slightly more fluid is drawn up into the syringe than is required for the injection. The correct amount having been inoculated (when convenient subcutaneously, to avoid built-up pressure from muscular contraction) the syringe and needle are withdrawn and the remainder of the fluid is expelled through the needle before removal of the latter from the syringe for changing. I think that the syringe should have a plunger which fits perfectly, like the good old "agla" once supplied by Burroughs Wellcome and Co., and it should be slightly stiff in operation. Further, pressure of the thumb or palm of the hand should be maintained evenly on the plunger during removal of the needle from the arm. Most of the pitfalls should be covered by this procedure.

Short of sterilizing numerous syringes, which is difficult in mass immunization, this is the most watertight technique that I can envisage. I would be glad to hear what Drs. Evans and Spooner think of this as the best compromise.—I am, etc.,

London, S.E.5. GUY BOUSFIELD.

SIR,—I have read with considerable interest the paper by Drs. R. J. Evans and E. T. C. Spooner on this subject (July 22, p. 185), and noted that, though using a bacteriological method, they confirmed my findings which were published in the *British Medical Journal* in 1946.[1]

I continued my investigations and published the results in the *Journal of the Royal Army Medical Corps* in 1946.[2] This further work made it clear that the pumping action on removing a needle from a syringe was of considerable importance, particularly with the large "Army" fitting. I found that with a Record fitting contamination with red cells occurred in 14% of instances after an intramuscular injection, whereas with an "Army" fitting it occurred in 47% of instances. As Evans and Spooner, using a very sensitive bacteriological method, have found contamination of the syringe in 81% of injections with a Record fitting, presumably if the experiments were repeated using their method contamination with an "Army" fitting would be almost 100%.

Syringe contamination would certainly seem to be an important factor in the transmission of infective hepatitis and possibly other diseases during mass inoculation in the Forces. It would be a pity to discard the "Army" fitting on this account alone, as the larger joint is much more convenient than the Record fitting—the needle does not tend to slip from the syringe, and one can use a large aspirating needle on the syringe with a good deal more security. In my opinion the "Army" fitting would be mechanically more satisfactory than the Record fitting for general use.

I suggested a simple method by which contamination of the syringe could be avoided during an intramuscular injection. In brief, this consists in interposing a removable tap between the needle and the syringe; when the injection is given the tap is turned off, the needle withdrawn from the tissues, and the tap and the needle removed for sterilizing. A further tap and needle can then be applied to the syringe for the next injection. This method has the great advantage that the needle and tap can be rinsed and dropped in a boiling sterilizer, to be sterilized in the minimum of time, unlike syringes, which are liable to breakage by unskilled personnel. I carried out a series of 99 intramuscular injections using this technique, and on microscopical examination for red cells I was unable to detect contamination of the syringe in a single instance. It would be interesting if this work could be repeated using Evans and Spooner's bacteriological technique; if the safety of the method is confirmed I think it might well be adopted during mass inoculations. The taps could be mass produced at a comparatively small cost: only about half a dozen would be required for each operator.

Incidentally, the author of your annotation on syringe sterility in the same issue of the *Journal* (p. 204) scarcely does me justice when he states:

"Hughes concluded that the pressure produced, especially by a rapid injection into contracted muscle, may force a little fluid back into the nozzle of the syringe. No suggestion is offered in his paper that any other mechanism may be concerned."

I would draw his attention to the final paragraph of my paper, published in the *Journal* in 1946, which is as follows:

"The limited number of investigations carried out suggests that contamination occurs as follows. A small amount of fluid is forced back into the needle after the injection, or traces of blood are left on the tip of the needle when it is withdrawn; this blood tends to spread slowly along the needle, and on removing the latter from the syringe the needle contents are aspirated, leaving a contaminated drop of fluid on the tip of the syringe. This drop contaminates the next injection, despite the changing of the needle."

If he still doubts whether I mentioned the importance of aspiration of fluid from the needle when it is removed from the syringe, I would refer him to my paper in the *Journal of the Royal Army Medical Corps*.[2]—I am, etc.,

Liverpool. ROBERT HUGHES.

REFERENCES

1 *British Medical Journal*, 1946, **2**, 685.
2 *J. R. Army med. Cps*, 1946, **87**, 156.

Haemorrhage from Peptic Ulcer

SIR,—The discrepancy between mortality figures in haematemesis and melaena given in the *Journal* of July 15 by Drs. C. D. Needham and J. A. McConachie (p. 133) from the Aberdeen Royal Infirmary and by Drs. A. G. Ogilvie and I. O. B. Spencer (p. 138) from the Royal Victoria Infirmary, Newcastle-upon-Tyne, is so great that a further comment on them may not be out of place. There will probably be general agreement with the statement of Needham and McConachie that their mortality rate of 13.9% for 476 cases corresponds to results obtained for a similar period in most general hospitals. The achievement of Avery Jones[1] (7.8% mortality in 615 cases) shows clearly how large is the room for improvement, but I submit that the very low mortality claim by Ogilvie and Spencer (2.4% in 170 cases) may be misleading.

To begin with, the total number with which these authors deal is relatively small—they work out at two patients per month, while general physicians in hospitals of comparable size where no restriction is placed on admission of such cases will often handle many times that number. This low turn-over suggests that only a small proportion of cases of haematemesis and melaena occurring in the populous area of Newcastle-upon-Tyne found their way into the Royal Victoria Infirmary, probably because of needs for teaching students. The figures given by Ogilvie and Spencer bear out the idea that such a selection was indeed made, because they show that only 16% of their patients were over 60 years of age, most of these between 60 and 70, whereas the records of Needham and McConachie, Avery Jones[1] from the Central Middlesex Hospital, and Baker[2] from the Selly Oak Hospital, Birmingham, dealt with about 30% of patients over 60, a heavy proportion of whom were over 70.

Without knowing what happened to the other patients with bleeding peptic ulcers in Newcastle-upon-Tyne, it is difficult to accept Ogilvie and Spencer's mortality figures at their face value.

On the larger issue, haemorrhage from peptic ulcers ceased some time ago to be solely the preoccupation of physicians, and now even the most conservative recognize that surgery has a place in the treatment of that emergency. Few will be found to subscribe to the idea that all, or even the majority of, cases of such haemorrhage should be operated on as a matter of course, and the balance of present-day opinion inclines to the view that there is a definite, though not very large, percentage of cases in whom operation would be a life-saving measure.

Most clinicians now feel that an emergency operation should be offered to some of the patients in whom severe haemorrhage recurs after admission to hospital. Obviously a number of such patients will not be fit for operation for a variety of reasons, while others may refuse surgical aid, and the crux of the matter is to balance in the remainder the probability of death without and with operation. The problem would be easy if only there were a way of divining whether bleeding comes from an opening in a sizeable artery, often eroded tangentially in the floor of a chronic ulcer, because it is then that an imminent threat to life exists. Few will disagree that in such instances an operation, preferably a partial gastrectomy, should be performed.

In the various series of cases of haematemesis and melaena recorded I have yet to find a reference to the type of patient who is admitted—sometimes shocked, sometimes in a fair condition—who in spite of recognized treatment continues to lose ground in a rather insidious way. The blood count of such patients goes on dropping to a level far below that for which haemodilution would account; frequently they do not vomit or pass melaena stools, and one must assume that in them continued seepage of blood into the bowel is taking place. I am sure others will recognize in that description a large group of cases whose place is between those who go all one way and give rise to little anxiety and, at the other extreme, those in whom a sharp recurrent haemorrhage gives clear warning of impending disaster. The patients in this middle group often benefit from blood transfusion, and with experienced management they will rarely need an operation.

This letter is not the place for giving an account of the routine treatment of haemorrhage from peptic ulcers adopted in the hospital at which I work—this has been done already by Baker in his report—but one of the points I insist on in my patients is the avoidance of any aperients or enemata in the first week after admission, or even longer if melaena continues. I have found from prolonged observation that, as in typhoid, constipated patients with haematemesis or melaena rarely die. I feel that injudicious stimulation of the bowel in a precariously balanced patient may start bleeding again. It is true that an occasional subject has to pay for my view with faecal impaction, but, while this may give rise to considerable discomfort, it can be relatively easily relieved and does not endanger life.

Finally, those charged with the care of patients suffering from haematemesis and melaena will agree with the views expressed in your leading article (p. 153) that there is a real need for further clinical studies of this serious and all too commonplace problem.—I am, etc.,

Birmingham. A. M. NUSSEY.

REFERENCES

1 *Guy's Hosp. Rep.*, 1947, **96**, 1.
2 *British Medical Journal*, 1947, **2**, 441, 477.

Abortion

SIR,—We should like to offer the following comments on the review of 2,665 cases of abortion by Mr. Albert Davis (July 15, p. 123). He suggests that the frequent finding of organisms such as *Staphylococcus albus* and diphtheroids is indicative of active infection.

In the former London County Council puerperal sepsis unit at the North-western Hospital, Hampstead, a strict bacteriological survey of all cases of post-abortal sepsis was carried out between 1937 and 1946, and the infecting organisms were classified as follows:

Infecting organism	Cases	Percentage
Haemolytic streptococcus	134*	13.9
Staphylococcus pyogenes	110	11.4
Bact. coli	239	24.8
Other aerobes	119	12.3
Cl. welchii	190	19.7
Anaerobic streptococci / *Bact. pseudonecrophorum* ..	125	12.9
Negative cultures	48	5
Total cases	965	100

* 60 strains (45%) belonged to group A.

While a series of 50 consecutive cases of abortion are not strictly comparable with cases of definite sepsis, nevertheless we did not consider that the presence of organisms such as *Bact. coli, Cl. welchii*, anaerobic streptococci, or coagulase-negative staphylococci was always associated with clinical signs of infection, although putrefying products of conception may certainly encourage their proliferation.

The greatest difficulty in the bacteriological assessment of these cases is the determination of the pathogenicity and invasiveness of the various organisms found on cervical culture. The wide variation in the toxigenicity of *Cl. welchii* strains has been emphasized in a previous study.[1] Experience has shown also that the finding of coagulase-positive staphylococci on cervical culture carries with it a considerable chance of pyaemia, whereas coagulase-negative organisms can be disregarded.—We are, etc.,

A. MELVIN RAMSAY.
J. VAHRMAN.

London, N.W.3.

REFERENCE

1 Ramsay, A. M., *J. Obstet. Gynaec. Brit. Emp.*, 1949, **56**, 247.

SIR,—In his interesting report on abortion Mr. Albert Davis (July 15, p. 123) describes a method of causing the uterus to empty itself while the cervical canal is distended by a Hegar's dilator. Ten units of oxytocin are injected into the uterus. Since I have seen severe and prolonged circulatory collapse following the administration of ten or even five units of "pitocin," I am prompted to ask if the risk of this alarming complication is justified in order to achieve a more or less bloodless evacuation of the uterus.—I am, etc.,

Birmingham. HUGH C. MCLAREN.

D-tubocurarine Salts and Derivatives

SIR,—The letter from Drs. G. A. Mogey and J. W. Trevan (July 22, p. 216) rightly calls attention to the need for exercising caution in using derivatives of D-tubocurarine. The variation in the relative potencies of D-tubocurarine and its dimethyl ether from species to species is, as the writers point out, not surprising, nor indeed are the variations observed with different methods of testing and modes of administration; for it would be very unusual if a phenol and its methyl ether were absorbed and metabolized in precisely the same way.

The experiments carried out by Dr. H. O. J. Collier and Mr. R. A. Hall (June 3, p. 1293) were designed to show whether dimethyl tubocurarine differed in any important respect from D-tubocurarine. The results obtained with different species of animals were important because they indicated that the relative potencies of the two substances varied from species to species, suggesting that there might also be a difference in the relative potencies of the two substances in humans. Such a difference was in fact found, and was favourable to dimethyl tubocurarine, which, according to Drs. H. B. Wilson, H. E. Gordon, and A. W. Raffan (June 3, p. 1296) and Pelikan. Sadove, and Unna,[1] is two to two and a half times as potent as D-tubocurarine in humans. The pharmacological investigation showed furthermore that for equipotent doses in the cat less histamine was released by dimethyl tubocurarine than by D-tubocurarine and that there was less paralysis of the autonomic ganglia. Further, in the rabbit and the rat there was less paralysis of respiration by dimethyl tubocurarine than by