simply be accounted for by the relaxation of the previously stretched lung surface afforded by its collapse, which allows the edges to appose sufficiently to close the gap even during inspiration. A simile would be the pin prick in a piece of stretched sheet rubber that closes when the tension is released. In the case of the punctured emphysematous bulla, on the other hand, elastic tissue is reduced in its wall and the lung relaxation will not assist the closure of the puncture to the same degree. Especially will this be the case on inspiration, when the lung surface will be more stretched than on expiration even when partially collapsed. Thus a "ball valve" mechanism is not difficult to visualize where air is admitted to the pneumothorax space during inspiration but cannot escape during expiration, producing an ever-increasing air pressure within the pleural space.

The opportunities for puncturing the lung at induction are great, and I think the incidence is high. Most tuberculosis workers must have been struck by the number of times the lung has obviously been punctured with no attendant ill effects. However carefully the induction is done, air may be withdrawn into the novocain syringe when making sure the needle is not in a vessel, or lung pressures may be obtained on the manometer with the induction needle itself penetrating the lung. I would suggest, therefore, that in the great majority of cases it does not matter if the lung is pierced, as the mechanism described above protects the patient from ill effects. Although my series is very small and no definite conclusions can be drawn from it, it is, I think, significant that emphysematous bullae were discovered in the two cases developing a tension element. Although the lung was pierced in many other cases (as proved at induction or later by screening) no bullae were discovered on the lung surfaces at subsequent thoracoscopy.—I am, etc.,

London, N.W.10.

PETER STRADLING.

Epidemic Gastro-enteritis

SIR,—I take the liberty of encroaching upon your space to make a few remarks about the recent outbreaks of "enteritis" which are occurring in maternity units throughout the country. We have had sporadic cases since August, 1944, though no case was fatal and all responded to treatment. It is mostly the nursing mothers who become affected, and it is also noticeable that the nursing staff, especially new pupil midwives, are susceptible.

The onset in mothers is sudden. The patient may be quite well in the morning and yet some hours later may complain of a feeling of nausea. This is seldom followed by vomiting (though occasionally, yes), but appetite is lost and the sight of food aggravates the nauseous feeling. This is followed shortly by a feeling of abdominal discomfort, which in turn is followed by abdominal pain of a colicky nature (umbilical region). Examination of the abdomen at this time shows a certain amount of meteorism to be present and also a "splashing" sensation in the caecal area. Watery stools are passed and may number anything from three to eight daily.

Patients who exhibit these symptoms are generally in the third to the eighth day of the puerperium. The condition clears up in about four days, and its cessation is abrupt.

For those who may be interested I give the routine treatment. At the onset of symptoms nothing in the way of diet is given except milk and soda-water for 24 hours or 48 hours. At the same time two tablets (1 gramme) of sulphasuxidine is given every four hours for 48 hours. If pain is prominent and the stools very frequent tincture of opium 15 min. (0.9 ml.) is given t.i.d. This generally leads to cessation of all symptoms in 36–72 hours. Occasionally treatment is preceded by the administration of ol. ricini 2 dr. (7 ml.) on the assumption of a toxin being present in the gastro-intestinal tract.

Bacteriological examination of faeces has been abandoned—all findings having been negative. Analysis of the water supply (to exclude metallic poisoning) was done, and a rough examination of the kitchen utensils failed to show any possibility of solvency of metals. We are ignorant of the cause, but are inclined to think that food—especially "frozen meat"—may be involved.

In regard to the newborn babies we have been fortunate (so far). Isolated cases have and do occur. Breast-fed babies are not immune, and indeed a relation can readily be established between "infected" nursing mothers and their babies. Our worst case was one of a breast-fed infant. My own opinion is that while the mother is "incubating" or developing the "toxin" a minimal amount may escape through the milk to the infant, and while the amount is not

sufficient to cause symptoms as yet in the mother, it is sufficient to cause disturbance in the baby. A careful watch is kept in the nurseries, and as soon as an infant develops an "unusual" relaxation of stools the following treatment is adopted.

No milk of any description is given. Ol. ricini 15 min. (0.9 ml.) is administered and 0.5% saline with glucose is given by mouth, sufficient being given to satisfy basic fluid requirements—that is to say, 2½ oz. (71 ml.) per lb. body weight in 24 hours. At the same time sulphasuxidine 0.25 g. is given 6-hourly for a minimum of 36 hours. When symptoms abate half-strength boiled breast milk is given (which is sometimes acidified by adding 2–3 min. (0.12–0.18 ml.) of B.P. HCl dil.). The feeds are increased daily.

It would be interesting to hear the treatment from other units.—I am, etc.,

Burnley, Lancs.

ANTHONY EUSTACE.

Aspiration of Acute Pleural Effusions

SIR,—Experience in consulting practice and in hospitals, civil and military, suggests that there is not a little uncertainty as to the indications for aspirating an acute pleural effusion. It may be due to the natural desire to prove an effusion diagnosed on clinical signs, coupled with faulty teaching of the inexperienced. Removal of a sample for demonstration to students and for pathological examination is legitimate in a teaching hospital. Aspiration is quite another matter and may be detrimental to the patient. Liaison between clinical teachers should enable every student to see aspiration of the pleural cavity (for this or for some other condition) at least once in his career, and most house-men to perform it. The indications should be clearly taught. I believe them to be as follows:

- (1) Dyspnoea at rest, particularly if increasing.
- (2) Circulatory embarrassment from mediastinal displacement, particularly if increasing.
- (3) Any suspicion of pus—for example, unduly toxic appearance, coated tongue, rapid loss of weight, swinging temperature. It should be noted here that the temperature may be very high with clear effusions, but that it seldom varies by more than two degrees in the day.
- (4) An effusion which shows no sign of absorption at the end of three weeks, particularly if fever is not diminishing. Removal of part will often lead to absorption of the remainder, or examination of the liquid may raise the question of tuberculous empyema or of neoplasm.
- (5) An underlying pulmonary condition requiring artificial pneumothorax treatment. Then it is important to air-replace the effusion before pleural adhesions develop.

I have seen a rise of temperature and increase of effusion so frequently after needling the chest in the acute stage that I prefer not to withdraw even a sample unless one of the above conditions exists, or unless there are strong reasons to suppose that the pleurisy is due to something other than tuberculosis.—I am, etc.,

St. Leonards-on-Sea, Sussex.

E. A. WOOD.

Intra-articular Injection in Osteoarthritis

SIR,—Over the last eight years at the Charterhouse Rheumatism Clinic and in their private practice members of the staff have treated several thousand cases of arthritis by Grant Waugh's method of intra-articular injection.¹ Some hundreds of these were for disease of the hip-joint. I entirely agree with Dr. Vernon Hetherington's warning (Dec. 28, 1946, p. 1007) that for these a prolonged treatment is necessary, and also with his remarks on Dr. Mawson's paper (Nov. 9, p. 691), which is certainly too optimistic.

The object of this letter is to draw the attention of your readers to the advantage of using acid potassium phosphate (A.P.P.)² and acid magnesium phosphate (A.M.P.)³ rather than lactic acid for the larger joints, as the effect of the injection is more prolonged; consequently the treatment, which is by no means painless, need not be administered so often. I make no claim that the salts are more effective than the acid, as we have made no comparative tests, but merely that the effect is more prolonged.

In practice, for a hip-joint, 20 ml. of A.P.P. is used on the first occasion, as A.M.P. is sometimes followed by considerable local reaction. A fortnight to three weeks later 10 ml. of A.M.P. is injected, and thereafter the interval is judged by the tendency to relapse, which may be from a month to six weeks. Usually six or eight injections are sufficient.

Personally I now always use the anterior approach to the joint. In about 10% of cases some fluid is present, which should first be aspirated before the injection is made. As much as 35 ml. has been withdrawn. For the smaller joints A.M.P. should not be employed, but it is excellent for knees and shoulders.—I am, etc.,

London, W.1.

H. WARREN CROWE.

REFERENCES

- ¹ Grant Waugh, W., *Lancet*, 1938, 1, 487
- ² Crowe, H. W., *ibid.*, 1944, 1, 563.
- ³ — *ibid.*, 1946, 1, 590.

Chemotherapy in Virus Diseases

SIR,—May I, greatly daring, be allowed to question the accuracy of a statement in the leading article on "The Chemotherapy of Tuberculosis" (Dec. 7, 1946, p. 862)? You say: "There is so far scarcely a hint that viruses are susceptible to any such [chemotherapeutic] attack, and nothing that we know of their properties and behaviour encourages belief to the contrary." Viruses are large protein bodies, and while little is known as to how they get into a living cell it seems probable that they do not ordinarily multiply except in the intercellular phase. Human beings after attack seem able to develop some method of sterilizing the viruses and acquiring an immunity which is usually permanent—e.g., measles and smallpox—though sometimes—e.g., influenza and the common cold—this immunity is lamentably transient, which suggests that there may be at least two methods of inactivating a virus. There is however a third possible way of attacking the problem besides those of actually killing the virus or in some way inactivating it. If, when once a virus has gained access to a cell, it could be imprisoned within that cell or (more hopefully) other cells have their walls made impermeable to the virus, either the virus would die out or the body have time to develop its defensive mechanism—that is, unless the original dose were already fatal. Is there any possibility that such a thing can be done by chemotherapy or any other means?

Our present knowledge about living cell membranes has recently been conveniently summarized by Jevson and Danielli in their book *The Permeability of Natural Membranes*, and I have good grounds for believing that what follows is at least agreeable to such knowledge. The cell membrane is semi-permeable, which means that it retains certain electrolytes and other substances while it is freely permeable to others. By virtue of this property the cell contents exert an osmotic pressure which distends the cell with fluid. When a child is in full health, and particularly after a seaside holiday, its flesh has a full firm feel which is quite different from that of oedema. The difference is that in the healthy flesh the fluid is contained in the tissue cells, while in oedema it is in the tissue spaces—i.e., it is intracellular instead of intercellular. What has happened is that something has occurred to the cell walls that has enabled them to retain products to which they were previously permeable, and so their osmotic pressure has risen with the consequent firm feeling typical of robust health. (It is of course also possible that the cells might have manufactured some new metabolite which had raised the osmotic pressure, but on many grounds this seems unlikely, particularly as the reverse process in shock, wherein the erythrocytes soon become permeable to potassium, which is then excreted, is well known.) Now the cell wall consists of what one might consider a very fine network made up of lipoids, unravelled protein molecules, and other bodies, making the fine semi-permeable sieve; and it is also probable that there exist some larger spaces or "pores" and that it is through such pores that the viruses and the large molecules of toxins find entrance. If therefore we could in some way diminish these pores we might expect not only to exclude stray viruses but also toxins as well; in fact we should produce health. Can this be done?

I have written of the typical health of a seaside holiday, but we know that clinically many of the benefits of such a holiday can be reproduced by liberal dosage with cod-liver oil or vitamins A and D; and vitamin D at least is directly produced in the body at the seaside. These two fat-soluble vitamins are chemically closely allied with substances already known to exist in the cell walls, and it is no far-fetched hypothesis to suggest that an ample supply of these vitamins

would, in fact, adsorb on to the existing cell wall network, so producing that finer sieve which is required to enable the cell to retain other contents and so raise its osmotic pressure; it might even materially alter the physical, chemical, or electric formation of the larger pores. (When one gets down to molecular phenomena, spatial, chemical, and physical or electrical conceptions largely merge into one another, but in this case the process is most easily visualized as a net or sieve.)

Now I believe that chemotherapy on these lines—i.e., the injection of large doses of vitamins A and D—does give us just the effect that I have suggested as required to limit and inhibit the invasion of the body by viruses. I have mentioned in various communications, and do not here repeat, the effect of such injections on bacterial conditions—i.e., presumably on the large toxin molecules—and also their effect on cases of influenza in a letter which attained most unexpected publicity, a publicity which has, I fear, prevented many doctors from testing the truth of what I then stated and have since proved many times over, not least in my own person. Most cases of measles are hardly bad enough to warrant giving the child an injection, but in those cases where for one reason or another I have given such injections (I use "dekadexolin," which was first made for me some sixteen years ago) the subsequent improvement has been rapid and marked. Chicken-pox is even less in need of chemotherapy and I have only had two cases in which "dekadexolin" seemed justifiable. They were children in whom the chicken-pox had got complicated with an impetigo with the result that nearly every pock became impetiginous, a distressing complication I have not seen recorded hitherto. To both these children I gave some "dekadexolin," and the way the infection cleared up was startling in its rapidity.

The simplest and easiest virus infection in which to study the effect of "dekadexolin" is herpes zoster, a condition so painful that the occasional discomfort caused by the injection is of little account. In my experience these cases all respond the same way. If the case is seen in the erythematous stage before any vesicles have formed, 2 ml. of "dekadexolin" will abort it completely. Even after many vesicles have fully matured before the case is seen, I find that administration of "dekadexolin" stops the threatening areas from developing as one would normally expect, and the whole disease runs a more rapid course. I have used "dekadexolin" very little for mumps, but one adult to whom I gave it made such a rapid recovery that I doubted the diagnosis until his children later developed the disease, which he insisted I should treat in the same way and with the same happy results. I have had no opportunity to treat any cases of smallpox, but during the recent epidemic at Birkenhead I found that there was no known treatment for the very severe cases, so I wrote suggesting the use of "dekadexolin." My letter only finally reached the doctor in charge while he was treating the last severe case. I was told verbally that there was dramatic improvement for 48 hours after "dekadexolin" was started, but then the improvement ceased and the child rapidly died. No further case occurred on which to try the treatment, but apparently this was the only therapeutic measure which had been followed by any improvement in any such case and the child was very bad when it was instituted. I have not seen full notes of the case so I cannot comment on it further. I am well aware that one swallow does not make a summer, but it certainly seems a treatment worth further investigation.

All this may seem a meagre result for sixteen years' patient study. My reasoning may be wrong, but of the correctness of my observations I am quite convinced, and where I have been able to obtain controls or the opinion of better clinicians than myself I have availed myself of them. All too often, however, such help has been of little use. For example, during the war I had two children brought to me with muscular pains and tenderness and weakness of one leg, just like a very early poliomyelitis. The obvious thing to do was to prevent development if I could, so I gave a dose of "dekadexolin" and arranged for the mother to take the child next day to the Manchester Children's Hospital. In each case the physician there could find no trace of anything wrong. Were they cures, as according to my hypothesis they might well be; or was my provisional diagnosis wrong? These are the conditions under which a general practitioner works. I have just picked up straws as I found them in the course of a busy practice.