It is, of course, essential that both the polynuclear count and the Schilling haemogram should be investigated in every case, but I can see no reason to suppose that either can justifiably be regarded as the more important.

May I also confirm Arneth's statement that there is a shift to the right in lymphatic leukaemia, although I do not know whether this is invariably so? At the same time there is almost always a general immaturity in Class I of the polynuclear count. Similarly, in some, perhaps all, cases of Hodgkin's disease there is a shift both to the left and to the right, although this usually becomes less noticeable after *x*-ray treatment.—I am, etc.,

Maldon, Essex, April 6th. A. PINEY.

DEEP *X* RAYS AND THE CELLS OF THE BLOOD.

SIR,—Drs. Knott and Watt raise many important issues in their interesting preliminary note of their work on opsonic indices in radiated patients (March 23rd, p. 542). Much clinical work, however, showing the benefit of radiation in many acute and chronic septic conditions—work often confirmed fully by bactericidal blood tests—has shown that only in a very restricted sense is their statement to be accepted that "actively septic patients, leukaemic or otherwise, are not usually favourable subjects for irradiation."

Dr. Cave has pointed out (April 6th, p. 664) that, particularly in some acute skin infections, many good results have been obtained. But many other deeper acute and chronic infections have been successfully treated by radiotherapy. The report of Heidenhain and Fried (*Langenbeck's Arch. f. Chir.*, 1924, pp. 624–665) shows about a 75 per cent. rapid and excellent result in a series of 243 cases of such conditions as acute inflammatory glands of the neck and groin, acute mastitis, cellulitis of various regions, post-operative pneumonia, and parametritis: and these clinical results were confirmed by a series of bactericidal blood tests, which showed the beneficial effect of *x* rays on the patient as evidenced by the inhibited growth of stock and, more particularly, autogenous cultures.

Such bactericidal tests may be better than opsonic estimations for estimating the effect of radiation on septic conditions. But, apart from this point, the work of Heidenhain and Fried has shown clearly that radiation, to be successful in acute septic conditions, must not only be very closely watched twice daily in its effect, but must be very carefully applied *in doses about 20 per cent. of the mild skin dose*. Larger doses will not lead to the desired effect, and may even be dangerous.

The experience of many can confirm these results. It is not primarily a matter of the *quality* of the radiation used, as Dr. Cave suggests, but of the *quantity* absorbed, in comparison with some standard such as the skin dose, that is of importance to the result. For example, Berven, in his just published report on the "Treatment of tumours at Radiumhemmet" (*Acta Radiol.*, 1929, vol. x, p. 1), says that in ulcerated tumours "we have had great use of a resorbent Roentgen ray treatment prior to the radium treatment." A sixth to an eighth of a skin dose (high voltage) makes a great improvement in some days in the condition of cases of very septic cancer of the lip, and the subsequent radium treatment is facilitated.

I have had similar results in septic cancer cases; also with many non-malignant infections. If small but adequate doses are used, Dr. Knott and Dr. Watt's statement will be found to be far from a complete summary of the recently opened-up subject of the effect of radiation in acute and chronic infections.—I am, etc.,

London, W.1, April 12th. J. H. DOUGLAS WEBSTER.

DOSAGE OF THALLIUM ACETATE.

SIR,—Thallium acetate has been in use in the schools of the Brighton guardians for about eighteen months. The calculation for determining the right dose was rather complicated, involving the conversion of weights from the English system into the metric system and back again. On one occasion an overdose, fortunately not severe, occurred as the result of a mistake in the calculation. I then tried to find a simpler calculation. I found that *the weight of the child in pounds divided by 18 equals the dose of thallium in grains*. As an additional safeguard the weight of the child is put on the top of the prescription so that the dispenser can check the calculation. In view of the recent unfortunate disaster in London I thought perhaps that this simpler calculation might be useful to others, who may be alarmed at the risk of a mistake in estimating the dose.—I am, etc.,

Hove, Sussex, April 10th. H. J. MCCURRICH.

SIR,—There have been so many mistakes and discrepancies in the press reports on the inquest on the three boys who died from an overdose of thallium acetate that I would like to state the plain facts of the case. The strength of the solution should have been 7.8 mg. per fluid drachm, and was based on a prescription handed to the hospital dispenser about two years ago. The prescription was written in the hospital pharmacopoeia, and it has always been understood that the solution sent up from the dispensary would be that strength. Acting on this assumption, the correct volume of the medicine was measured out so that each child should receive a dose of 8.5 mg. per kilo of body weight. Unfortunately, the solution was not correct; a decimal point had been misplaced and each child received ten times too much—instead of 8.5 mg., 85 mg. per kilo were actually given.

A further point which was not made clear was that on analysis the solution from the small bottle "B" was only 5.6 times, and not 10 times, too strong. This is accounted for by the fact that after the children had received their medicine from the larger bottle "A," only about one ounce of the solution was left in the bottle, and on being returned to the dispensary it was emptied out into the smaller bottle "B," which already contained about one ounce of the solution left over from a previous occasion, and which was, of course, the correct strength.

One further remark I would like to make. In one paper I was reported to have admitted that one of the patients to whom I had administered the same medicine had afterwards gone off into a "trance." What I really said was that in all the series of cases we had treated only one reported having slight transient pains, and these did not occur until a week had elapsed after taking the medicine.—I am, etc.,

J. M. SPENCER SCOVELL, M.D., Ch.B.Ed.

London, W.1, April 15th.

THERAPEUTIC VALUE OF ULTRA-VIOLET LIGHT.

SIR,—In the introduction to the recently issued annual report of the Medical Research Council certain general comments with regard to light treatment are made. As these remarks are only in the general introduction it would be helpful to have an authoritative answer to the following question: Had the comments on light treatment been submitted for approval to the Medical Research Council's Committee on the Biological Actions of Light? If the answer is in the negative further observations are unnecessary.—I am, etc.,

London, W.1, April 12th. C. B. HEALD.

ESTIMATION OF HEPATIC EFFICIENCY.

SIR,—I hope you will kindly allow me to comment on the letter on the above subject in your issue of March 30th (p. 621), which referred to my report on Roch's test, recently published.

1. Your correspondent does not state how the results he quotes were interpreted as an answer to the question set.
2. No experiments seem to have been made on healthy subjects as controls for the results obtained with patients.
3. My experience with Roch's test on myself and other three healthy subjects gave positive results, and as similar results were obtained with patients having unhealthy livers this was regarded as proof that the elimination of salicylic acid in the urine is a normal result after its ingestion, whether the liver was diseased or healthy.
4. Your correspondent's positive results cannot therefore be accepted as proof that the liver's antitoxic function was disturbed in any single case, nor can much be inferred from the negative results, except that probably they arose from incomplete extraction of the salicylic acid, as was proved in my experiments.

These results need not be more fully analysed, but as a whole I consider they confirm the conclusion reached in my article—namely, that Roch's test is of no value in diagnosing disease of the liver.—I am, etc.,

London, E.1, April 12th. W. RALSTON.