is so narrow? (Maximum in the one = £1,005 and in the other = £1,172.) The career prospects in radiography are shown to be poor indeed when comparison is made with those of physiotherapists.

In 1958 the scales were: physiotherapists top grade—£750 to £960; superintendent radiographer—£810 to £920. (Maximum £40 less for superintendent radiographer.)

In 1969 the scales were: physiotherapists top grade—£1,570 to £2,050; superintendent radiographer III—£1,286 to £1,668. (The £40 less for the superintendent radiographer in 1958 has now grown to £382 on the final increment.)

What inducement really is there to seniors to stay in radiography—particularly the married men, whose skills are so often sought by industry? These figures speak for themselves and illustrate clearly the point which Dr. Grainger was making.—I am, etc.,

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Hearts—Real and Artificial

SIR,—Your leading article (14 June, p. 649) is correct in emphasizing that, regrettably, artificial heart development has been confined virtually to the United States, but is mistaken in appearing to separate research into artificial hearts from research into total body perfusion, of which it is an inevitable extension, and in which Britain has an extensive clinical and research experience.

Interest in the small orthotopic prosthetic heart in the United Kingdom has been slight mainly because such a device has been thought of as a chronic implant and not as an extraordinarily useful adjunct to clinical practice when used as a short-term circulatory support. The reasons for this are presumably mainly because this type of pump naturally borrows heavily from technologies outside the experience of most medical trainees whether clinicians or researchers. The situation in the United Kingdom, therefore, in striking contrast to the U.S., has been the failure of clinicians in this field to interest industry imaginatively in their problems, and, in addition, the Department of Health must bear some responsibility in failing to identify an area of mutual research endeavour whose benefits in human and economic terms are potentially enormous.¹

The Houston experience² has demonstrated quite clearly the feasibility and usefulness of short-term circulatory support using a prosthetic orthotopic heart that was relatively unsophisticated in the design of its valves and pumping chamber. The success of the techniques used showed quite clearly that the surgeons involved were entitled to use their extensive clinical experience of total body perfusion, and were correct in their prediction of a perfusion time of 60–70 hours (3 May, p. 310).

Human cardiac homotransplantation has strikingly demonstrated the feasibility of removing and reinserting hearts that remain capable of maintaining a normal circulation as well as reacting appropriately to physiological demands. Your leading article is therefore probably unnecessarily pessimistic in confining the use of artificial hearts to the field of cardiac homotransplantation. The

repair of the acutely damaged heart becomes for the first time a real possibility and one that does not await the development of a new technology. For this reason it is almost certainly wrong to assume that the development of a prosthetic heart has in its turn to attend further successes in the control of tissue rejection.—I am, etc.,

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- ¹ Task Report, National Institutes of Health, 1967, 1. Baltimore, Hillman and Associates.
- ² Cooley, D., and Liotta, D., *Proceedings of the American Society for Artificial Internal Organs*, 1969, to be published.

Bilateral Posterior Dislocation of Shoulders

SIR,—Dr. N. E. Miller's letter about bilateral posterior dislocation of the shoulder joint caused by electric shock (14 June, p. 694) prompts me to report a similar case as a result of an epileptic fit.

This produced bilateral anterior dislocation of the shoulder joint, and happened to a middle-aged man with no previous history of fits, who, during the course of a febrile illness, while watching television, had his first epileptic fit. This was immediately followed by pain and immobility of both shoulder joints, and when seen at an outpatient department six weeks later he had bilateral wasting of all the muscles of the shoulder groups, with gross immobility and bilateral circumflex nerve sensory loss. These dislocations have been difficult to reduce, and one will need an open operation before reduction is possible.

In the course of neurological practice I have seen four unilateral dislocations of the shoulder joint as a result of severe epileptic muscle spasm. All these cases have been posterior dislocations and not anterior. Many doctors still remain unaware that dislocation can occur owing to epilepsy, and it is a complication of epilepsy which rather escapes notice in most textbooks.—I am, etc.,

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Sequential Oral Contraception and Clotting Factors

SIR,—In its recent warning on the use of sequential oral contraceptives the Committee on Safety of Drugs "on the somewhat limited evidence,"¹ was unable to state that these preparations carry less thromboembolic risk than combined preparations. We have previously shown that oral contraception with conventional combined oestrogen-progestogen preparations causes rises in blood clotting factors VII and X.^{2,3} We therefore have

examined our records to see whether sequential contraception produces similar clotting changes to conventional oral contraceptives.

Coagulation studies were performed in 19 women before starting a sequential preparation (Dista Sequential). They were given appointments at monthly intervals for three months, then quarterly for the next nine months. In addition 11 women already taking the following sequential products for an average of 14 months were referred for coagulation studies while the trial was in process and included in the final analysis: Dista Sequential 5; Sequens 4; Feminar Sequential 2. The technique for prothrombin time, cephalin time, factor VII, and factor X assays was as described previously. To eliminate the variables in coagulation studies arising during an extended period of time, results of the sequential group were recorded as a clotting time in seconds and compared with the parallel controls, as in our earlier studies. To ensure sufficient data for statistical analysis they were grouped on a time basis as follows—results at three months, results between six and 12 months and results over 12 months.

The results are given in the Table. There was no significant change in cephalin times. The factor VII rise became significant at three months ($P=0.05$) and showed a progressive increase subsequently. The factor X increase became significant from six months onwards and appeared to be progressive subsequently. The results were also expressed as a percentage of the parallel normal readings in the Figure.

Owing to difficulties in getting the women to attend for follow-up blood tests, far fewer studies were performed in this sequential group than with the volunteers in the combined low-dose trial (reported elsewhere). The results nevertheless show the same trend—that is, rises of factors VII and X, which occurred after conventional oral contraception. There is therefore no suggestion that sequential oral contraception will reduce the tendency to raised clotting factors which follows conventional oestrogen-progestin administration. This, although not proof of an equal thrombotic risk, raises the suspicion that this may be the case.

	Total	Cephalin Time		Factor VII		Factor X	
		Mean (sec.)	S.D.	Mean (sec.)	S.D.	Mean (sec.)	S.D.
Women before starting and parallel controls ..	32	67.28	3.4	18.7	0.88	18.63	1.0
3 months ..	16	66.94	3.8	16.47	0.34	18.35	1.47
6-12 months ..	18	70.94	6.9	15.85	1.11	17.53	1.96
Over 12 months ..	9	65.78	5.5	15.75	1.32	15.56	1.13

The pattern of change is seen to be similar to that of a concurrent combined trial reported elsewhere.⁴

The study was performed while in receipt of a thrombosis research grant from Manchester Regional Hospital Board. We are also indebted to Manchester Family Planning Clinics, who sent the women for testing, and Dr. W. Lye, of Dista Products Ltd., for supplying the Dista Sequential preparation used in this study.

—We are, etc.,

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- 1 Committee on Safety of Drugs, *Adverse reactions series*, 1969, No. 8.
- 2 Thomson, J. M., and Poller, L., *British Medical Journal*, 1965, **2**, 270.
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Asparaginase

SIR,—In your leading article (24 May, p. 465) you state: "Asparaginase is now known also to affect normal cells, particularly those of the thymus and lymphoreticular system. Whether this effect, which has so far been seen only in animals, will prove an added hazard to therapy in man is not yet known."

I recently treated a patient with acute lymphoblastic leukaemia who developed a profound leucopenia following asparaginase treatment. The findings in life and the post-mortem examination strongly suggested that the normal cellular elements of the bone marrow, as well as leukaemic cells, had been depressed. A detailed report on this patient by Dr. M. J. Oehlers, Dr. W. Fetwadjeff, and myself will be published elsewhere.—I am, etc.,

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Acute Infective Gastroenteritis

SIR,—I should like to comment on the letter by Dr. Mary J. Wilmers and others (31 May, p. 573) advocating the wholesale use of neomycin and other antibiotics in their gastroenteritis ward with the expressed purpose of preventing cross-infection with pathogenic *Escherichia coli*.

They give as their rationale the failure of careful barrier nursing techniques to contain cross-infection with these organisms. We have developed a system of barrier nursing at this hospital which has proved highly effective,^{1,2} but it must be emphasized that in order to operate the system efficiently it is essential to have properly constructed and equipped cubicles and a sufficiency of nurses for both day and night duty.

There are a number of possible disadvantages and dangers in the indiscriminate use of suppressive antibiotic treatment in babies. (1) It may engender in the minds of doctors and particularly nurses a false sense of security. Dr. Wilmers and her colleagues continue to get periodic bouts of cross-infection despite the approach they are recommending. (2) This practice encourages the emergence of resistant strains of *E. coli*. (3) Neomycin and antibiotics such as oxytetracycline may have undesirable and occasionally serious side-effects in small babies.

The question of whether or not to use antibiotics for babies seriously affected with gastroenteritis is a separate and debatable

matter. For the last four years or more we have seriously restricted the use of antibacterial preparations in our gastroenteritis unit, and have found rather to our surprise that very ill infants not given antibiotics generally respond promptly and well to accepted measures directed at restoring and maintaining electrolyte and fluid balance coupled with skilled nursing.

Needless to say, faced with an outbreak of hospital-acquired gastroenteritis in a paediatric unit or maternity department, it would be folly not to use an appropriate antibiotic in an attempt to stem the spread of infection.—I am, etc.,

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- 1 Gibson, M., and Mann, T. P., *Nursing Times*, 1965, **61**, 1309.
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Contaminated Chloroform

SIR,—For at least 15 years we have used analytical grade chloroform for the extraction of the dithizone complex with lead when estimating this metal in biological fluids. During the last few weeks these estimations have given anomalous values which have appeared to be too high. Careful investigation ruled out contamination of the apparatus as a cause. The source of the error has been traced to certain batches of chloroform labelled as being suitable for use with dithizone and free from metallic impurities. Some batches of chloroform gave a red colour with dithizone which was equivalent to a positive error of 20 µg. of lead/100 ml. whole blood.

The impurity can be removed from the contaminated chloroform by washing with 10% sulphuric acid. The washings give an apparent lead content of 1.9 µg. of lead in 100 ml. of contaminated chloroform, in the presence of citrate/cyanide buffer. The metallic impurity is not lead, as by taking the same amount of wash liquor and omitting the cyanide buffer, the complex gives an optical density of 1.75 at 520 mµ in 0.5 cm. cells in 10 ml. of chloroform. It should be noted that if the contaminated chloroform is used to "delead" cyanide buffer solution then the cyanide becomes contaminated with the impurity which is not removed by dithizone.

Similar high optical densities can be obtained in the presence of thiosulphate, of iodide, and of citrate buffers. Thus, severe interference can be expected in the measurement of copper, cadmium, cobalt, and zinc when contaminated chloroform is used in the dithizone methods for these elements.

It is strongly recommended that laboratories which use chloroform for the extraction of dithizone metal complexes should test each batch of every consignment of chloroform before it is used in micro-analytical methods. It appears that the chloroform became contaminated during the filling process owing to a faulty filling line.—We are, etc.,

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J. STEEL.

R. W. ELLIS.

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Relapsing Amoebic Colitis

SIR,—The report by Drs. S. R. Kanari and R. Knight (7 June, p. 613) will, I hope, stimulate awareness of the possibility of amoebic disease in patients who have lived or served abroad.

Army personnel have been particularly at risk in certain campaigns. In 1965 I diagnosed amoebic colitis in over a dozen soldiers who had been infected at the same source in Borneo, but there were many others, both British and Australian, who were at risk. Should any of these attend doctors, military or civil, with a complaint of piles, or recurrent diarrhoea, or rectal bleeding, it is to be hoped that amoebiasis will be considered. My patients were frequently referred with "piles." In the acute case I noticed that the abundant mucus always contained myriads of tiny bubbles; it was almost effervescent. I have not seen this described and it may not be of significance, but in that particular outbreak led me to repeated searching and examination of the specimens myself in the laboratory if a negative report was received from a technician.

As has been pointed out many times, the geographical history is the most important clue to the possibility of "tropical disease."—I am, etc.,

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Neonatal Serum Bilirubin Estimation

SIR,—Dr. T. Hargreaves (17 May, p. 448) indicates that false "low" results could occur using a diazo-coupling method during serial estimations of neonatal bilirubin levels and be reported as "normal" results, thus leading to complications arising over any proposed treatment by exchange transfusion.

Recently, while comparing two methods for the estimation of serum bilirubin levels in neonates, using a micro spectrophotometric method,¹ and a diazo-coupling method,² both methods were found to be in close agreement, the spectrophotometric method tending on average to be 1 mg.% higher than the comparable diazo results. However, in one case, a premature jaundiced infant, there was a marked discrepancy between results obtained using both methods, the diazo method producing consistently higher values than the spectrophotometric method over a period of several days (see Table). After careful rechecking of both methods, further inquiries resulted in the information that the baby was being treated with the antibiotic ampicillin (50 mg. twice daily). During a subsequent exchange transfusion samples of blood before and after transfusion were obtained and the serum separated, 2 ml. was acidified and extracted with ethyl acetate. The extract was evaporated to dryness and the residue subjected to two-dimensional chromatography, first in isopropyl alcohol, ammonia, water (80:10:10) overnight, and secondly in Anisole/acetic acid (70:30) for five hours. The chromatograms were dried, sprayed with 10% sodium carbonate, redried, and developed in diazotized *p*-nitro aniline to detect phenolic compounds. Both pre- and post-transfusion chromatograms developed an intensely staining orange/red spot which did not appear to correspond, position-wise, to any known phenolic acid and was presumed to be either ampicillin or a metabolite of ampicillin. On stopping treatment with ampicillin the marked discrepancy between the diazo and spectrophotometric values for the bilirubin levels which had occurred reverted to the expected level. Blood was also taken from full-term in-