tained to the period on return from the war, when juniors had to rehabilitate and start building their contacts from scratch. Training, experience, and goodwill are in a profession the same as a client is to a business. Unlike now, these items had been paid for by the individuals themselves or their parents without grants and at their own financial sacrifice. It may be noted that in all other industries which were nationalized stockholders were reimbursed for their outlay.

During the first 10 years after the Act, when the extensive administrative structure was being set up, clinical work had to be continued. This proceeded along the same lines as before except that clinicians were given some paid sessions in hospital in lieu of the previous so-called voluntary work. The juniors still worked long hours for a few sessions as it would not have been proper to pay them more from public funds than their seniors. Gradually with retirement and reorganization their sessions became recognized and paid for as the so-called "interim arrangements" were realized. No back pay was given for the interim temporary arrangement, but most consultants thought that their pension rights, at least as far as years were concerned, were recognized. Pension regulations were interpreted differently by the Ministry officials to the professions, and from 1948 to 1960 many senior clinicians will find their total pensionable years are very much less than they expected, and they will now be short of pensionable service.

It is probably impossible to make a case for the unpaid work done as the old system gradually changed to the new, but it should not be beyond the actuaries to calculate in retrospect the cost added years at the rates of pay pertaining to the years 1948-60. If it was possible to buy back the years at the cost they were then, it might be possible for us to purchase a reasonable retirement pension.

I feel that this special case should be pressed by our negotiators in the British Medical Association. It is appreciated that it is most unpleasant for doctors to have to bargain and in the past this has been a major card in the hands of Departmental officials. If our negotiators are not able to be crude enough themselves, then they should hire somebody who is. It is obvious that we can only obtain results with Government departments by sheer rudeness, and more has been accomplished by the junior doctors in the past few years than we have achieved in a quarter of a century of negotiations. It should also be pointed out in general terms to the Department of Health that morale is an important factor and seniors do still influence their juniors in the presentation of the attractions of a career in the hospital service.—I am, etc.,

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Detection of Hypertension in Childhood

SIR,—With reference to your leading article (18 August, p. 365) no one could take issue with the view that treatment of equivocal hypertension in children and adolescents is presently unjustified. The same view holds

true for adults. It is, furthermore, perfectly true that the long-term effects on growth and development of the antihypertensive drugs are unknown and give cause for great concern.

What is troublesome about the article is that it seems to imply that blood pressure should not be measured routinely in children and adolescents since we do not anyway know what to do with patients who are found to have borderline hypertension. This seems to be illogical and hardly promotes progress in an area which has been already too long neglected. It is neither time-consuming nor expensive to measure blood pressure in patients aged 2-20 years during a routine physical examination. Furthermore, these measurements need not be performed by a physician and can be adequately performed by any paramedical person who has been properly trained. The mere act of checking blood pressure should be no more liable to produce hypochondriasis than the act of auscultating the heart or lungs.

If physicians would routinely measure blood pressure in patients under 20 years of age, true incidence data for hypertension in this age group might be obtained. In addition, by tracking the blood pressures of patients through childhood and adolescence it might be possible to eventually delineate what levels of pressure, at various ages, identify the hypertensive or prehypertensive child or adolescent. At present we know so little about what constitutes hypertension in children that we are reluctant to regard anyone with a diastolic pressure below 90 mm Hg as hypertensive. The basic information which we so badly need can never be obtained by ignoring blood pressure in childhood. Once the basic information is accumulated, further research into the aetiology and prevention of, for example, essential hypertension should follow.

Finally, there are some prevalence data^{1,2} which indicate that 1.4-11% of teenagers and young adults (12-24 years) have definite hypertension as defined by a systolic pressure of 140 mm Hg or more and a diastolic pressure of 90 mm Hg or more. In a third study³ 2.3% of children aged 4-15 years had blood pressures persistently greater than the 95th percentile for their age. These figures would doubtless vary from country to country and between races, but it would appear that hypertension in childhood is at least as common as congenital heart disease.

It is well known that hypertension may produce no symptoms until diastolic pressure has been sustained at a high level for a relatively long period. This means that mild to moderately hypertensive youngsters may be labelled as "normal" if blood pressure is not routinely checked and may eventually be identified only in the accelerated phase of their disease. This has been a not infrequent experience in this institution. If it is worth routinely auscultating hearts for murmurs then, based on the available prevalence data for definite hypertension, it should be worth measuring blood pressure routinely.—I am, etc.,

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Biochemical "Desensitization" to Violence

SIR,—The shift from an exaggerated sympathetic state to vagotonicity is very clearly demonstrated in the impressive paper by Drs. Malcolm Carruthers and Peter Taggart (18 August, p. 384). Though the expression "desensitization" is not in accordance, owing to the absence of antigens and/or blocking antibodies, with the postulates of older and newer allerge-immunological concepts, linguistically and in the strict sense of the observed events there can be no objection to its use. The authors' interpretation of the clinical phenomena experienced seems, however, questionable.

The discrepancy in time-lag between changes in "pre" and "post" plasma and urinary values, as well as clinical signs, warrants consideration of the possibility of phase differences—in particular the supposition of compensatory or rebound tendencies rather than decrease of the reactivity of target organs. The same phenomenon can be observed in the successful administration of sympathomimetic drugs to revert paroxysmal tachycardia, a maximally sympathetic state, to sinus bradycardia.¹ It was also recently seen in our clinic in a case in which status asthmaticus elicited a bi-directional tachycardia² (this latter event had previously been observed as a consequence of isoprenaline injection³). If this is the operative mechanism, there is little hope that frequent exposure would result in "vaguely mediated aversion to violence."—I am, etc.,

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- 2 Pálóssy, B., *Congress of the Hungarian Society of Allergy, Kékestető* 1973.
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Exposure to Radiation

SIR,—Your recent leading article on fall-out from bomb tests (8 September, p. 510) pointed out that the assessment of radiation risk to the individual from occupational exposure, radiological diagnosis, or fall-out suffers from the fact that the problem lacks a firm scientific foundation and that, because of this, there is little point in becoming too emotive about the effects of fall-out. This is undoubtedly true, but there is also no doubt that the situation, scientifically speaking, is far worse than it need be, partly because of the generally confused methods of presentation of such facts as are known and partly because of a lack of information which should be readily available.

Figures frequently quoted for the general radiation exposure of the populace per year are: background radiation 0.10 rem; diagnostic radiation 0.02 rem; fall-out radiation 0.002 rem; occupational radiation 0.001 rem. Such a method of presentation is misleading since it does not distinguish between the external and internal dose. Further, background radiation represents a reasonably constant dose which applies to every member of the population, but diagnostic doses vary considerably and it is not reasonable to average this figure out as though the distribution was uniform. This is equally true of occupational exposure. Dose averaging may perhaps be acceptable in consider-

ing genetic effects where pooling of genes will occur, but it is not acceptable in assessing radiation carcinoma and leukaemia risk.

However, perhaps the worst anomaly lies in the collection of radiation records. Radiation workers in establishments subject to the Factories Act^{1,2} must, by law, have correct and accurate radiation records which are transferred with them when they change jobs. Other establishments such as universities and hospitals generally adopt the same procedure for their radiation workers,^{3,4} and recently the National Radiological Protection Board has set up a computerized record system which represents the first attempt to institute some form of central register.

The irony of the situation is that a large proportion of radiation workers will receive doses of less than 0.25 rem per year, this being the limit of measurement, and such doses will be carefully recorded. Doses obtained from medical and dental exposures for particular individuals on the other hand, which will in many cases considerably exceed these figures, will in general not be precisely known and will not be recorded. Such a situation is palpably absurd and it is obviously time that all radiation doses received by any individual from any source, whether it be occupational or from medical or dental diagnosis, should be accurately known and centrally recorded. Only when this is done can any reasonable evaluation of radiation risk be made.—I am, etc.,

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Polyunsaturated Fatty Acids and Cancer

SIR,—In your editorial "Are PUFA Harmful?" (6 October, p. 1) you mention the results of Pearce and Dayton,¹ revealing an increased incidence of cancer in people taking a prudent diet enriched in polyunsaturated fatty acids (PUFA). Our previously reported results^{2,4} indicate a dose-dependent regulatory effect of PUFA on the antigen-induced lymphocyte response as measured with the macrophage electrophoretic mobility test (see fig.) and in the lymphocyte

The inhibitory effect of various concentrations of linoleic acid (c 18:2) on the PPD-induced lymphocyte response, measured by the macrophage electrophoretic mobility test.

transformation test. Therefore an increased serum PUFA level might suppress the reaction of the immune system to cancer and favour tumour development and growth. If further investigations confirm our findings then a therapeutic lowering of the serum PUFA level might be a supporting factor in the treatment of cancer. Owing to an inverse relationship in the metabolism of single unsaturated fatty acids and PUFA, as shown for example by Belin *et al.*⁵ for oleic (C 18:1) and linoleic acid (C 18:2), such a decrease in the serum PUFA level can readily be produced by an increased C 18:1 intake.

In addition, there is now increasing evidence for an involvement of the immune system in the process of ageing and an immunoregulatory effect of PUFA may also be considered in premature ageing caused by excessive PUFA consumption.⁶—I am, etc.,

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- 1 Pearce, M. L., and Dayton, S., *Lancet*, 1971, **1**, 464.
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Anaesthetic Safety Devices

SIR,—We have been following this correspondence with interest because we have been and still are actively working on the problem. Though our results are not yet ready for publication, our interim conclusions may be useful to others.

Dr. T. H. Howells's proposal (13 October, p. 105) for the use of the reserve cylinder is not new, and careful consideration has shown that it does not eliminate human error and could well make things worse. As Dr. Howells himself indicates, it is possible for both oxygen cylinders to become exhausted; the anaesthetist may fail to notice that he has transferred to the reserve cylinder, or may decide that it is not worth changing the cylinder till the end of the operation and then forget, or be prevented from doing so. He will then run out of gas and find he has no reserve. In addition, the adoption of Dr. Howells's system would require anaesthetists to make a substantial change in their habits, and the resulting confusion might well cause more accidents than it would prevent.

Dr. R. Parfitt (22 September, p. 635) and Dr. R. L. Wynne (13 October, p. 105) have both shown how unreliable oxygen failure alarms can be. Though a simpler and more reliable oxygen failure alarm might perhaps be made, it would not eliminate human error, because experience with all kinds of monitoring systems has shown repeatedly that audible alarms are liable to be switched off or otherwise put out of action when, as is bound to happen, they give a false alarm. In our opinion, therefore, there is only one approach to the problem which is likely to be effective and that is not to try to prevent oxygen failure, but to prevent asphyxia resulting from it.

Both Dr. Parfitt and Rosen and Hillard¹ have included this requirement in their criteria for a "perfect" safety system, and as they have recently reported (20 October, p. 167) the latter's device is now being marketed, in a modified form, by the British Oxygen Company as a successor to the Bosun. We feel that some of their requirements are unnecessarily stringent and their device correspondingly unduly complex, so we have designed a simpler one which we believe is capable of preventing serious anoxia being produced by oxygen failure in almost all circumstances, and which can be fitted easily to any gas-and-oxygen anaesthetic machine. The device is a valve fitted between the patient and the machine which shuts him off from the machine and vents him to air if the oxygen pressure falls significantly. It also diverts the other anaesthetic gases through a separate vent, and both vents can be fitted with whistles. Thus if the patient is breathing, he will come to no harm and his whistle will draw attention to the oxygen failure. If he is on a ventilator, the other whistle will give warning long before serious anoxia occurs. A patient being ventilated with oxygen and air will also be safe, and only a paralysed patient being ventilated with pure oxygen will still be at risk. A device of this kind has been made and is now being evaluated, and we hope to be able to publish a full description and account of its performance shortly.—We are, etc.,

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- 1 Rosen, M., and Hillard, E. K., *British Journal of Anaesthesia*, 1971, **43**, 103.

Selection of Patients for Clinical Trials

SIR,—In advancing his thesis that the results of clinical trials may be influenced by the natural prognosis of the patients selected Dr. J. P. Leff (20 October, p. 156) refers to the recently reported M.R.C. multicentre trial of continuation thymoleptic medication in depressed patients.^{1,2} He suggests that the difference in outcome between Glasgow and the other centres may be due to the inclusion of more "good prognosis" or less severely depressed patients in the Glasgow sample, taking as his index the differences in relapse rate in patients receiving placebo. He may indeed be correct, but I think there are other explanations for the differences between centres which deserve consideration.

Firstly, in the trial as a whole, relapse rates were much higher in those patients who were not completely well when maintenance therapy was commenced, especially in those patients who drew placebo. Fewer Glasgow patients had such residual symptoms and it is therefore not surprising that their relapse rates were lower. These figures may of course be open to other interpretations but it may well be that many patients were admitted to "maintenance" therapy too soon and were, in consequence, more likely to relapse.

Secondly, high relapse rates may also have been due to the failure of patients to take their medication, and this would have been so for those taking placebo as well as those on active drug. Tablet counts were