when groups of people with pneumoconiosis are compared with others the differences in symptoms, function tests, and expectation of life are small. What I object to is the assumption that all patients with simple pneumoconiosis respond to dust in their lungs in the same way. Can the statisticians tell us how many may, for example, develop secondary emphysema and disability without upsetting the statistical conclusions that, as a group, they are no worse off than non-pneumoconiotics? It is time we accepted that the post-mortem studies of Lyons *et al.*¹ are the best available instead of trying to discredit them, mainly, it seems, because they do not support current dogma. Does Professor Morgan really believe that they are more biased than some of his studies in which ex-miners and retired miners are not included?

Most of the letter from Dr. McGowan and his colleagues deals with the aetiology of chronic bronchitis in miners and is a distraction from my main theme. As they seem to feel that my criticisms of panels are unjust I would like to offer four helpful suggestions which they have the power to implement.

(1) That when recording the results of ventilatory tests the expected readings should be added. This should reduce the frequency with which abnormal readings are reported to be normal. Dr. T. J. G. Phillips may recall a recent case in which readings of 70% of the expected were repeatedly claimed to be normal.

(2) That the frequent practice of writing statements such as "disability slight" under the readings should be abandoned—that is, disability should not be determined solely by these.

(3) That chronic bronchitis of various degrees of severity should not be diagnosed on naked-eye examination of post-mortem material unless criteria for making such a diagnosis and distinctions can be given.

(4) That when rejecting pneumoconiosis as a factor in disability or death panels should not use as the main argument such statements as "this degree of pneumoconiosis should not prevent him from carrying out his regular occupation" and "insufficient pneumoconiosis was present to have caused or accelerated death." The present philosophy is that pneumoconiosis does not matter until x cm of progressive massive fibrosis are present; x is bad, x-1 is harmless. In the latter case there may be a lot of scratching around for alternative explanations and practice (see (3)) is then often invoked.—I am, etc.,

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¹ Lyons, J. P., *et al*, *British Medical Journal*, 1972, 1, 713.

Traumatic Hindquarter Amputation

SIR,—Your leading article (5 October, p. 4) states that "there has . . . been one case of traumatic hindquarter amputation, and the patient survived." This presumably refers to Gordon-Taylor's series, but it could be interpreted as indicating that you know of only one case of traumatic hindquarter amputation in which the patient has survived. McLean¹ was under a similar impression when he reported his case in 1962.

Recently at a meeting of the Holdsworth Club I showed a patient of my own who had survived a traumatic hindquarter amputation and mentioned another case treated by Mr. Bruce McCulloch in Bishop Auckland. Neither of these cases had previously been recorded. Two members of the Holdsworth Club stated that they had seen similar injuries. McPherson² reported one case in 1960, Wade and Macksood³ reported two cases in 1965, and a single case was reported from Budapest in 1969 by Lasslo.⁴ The most recent case reported is that of Ganapathy⁵ in 1973.

This injury is perhaps not so common as we had previously thought.—I am, etc.,

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¹ McLean, E. M., *Journal of Bone Joint and Surgery*, 1962, 44B, 384.

² McPherson, J. H. T., jun., *Journal of the Medical Association of Georgia*, 1960, 49, 494.

³ Wade, F. W., and Macksood, W., *Journal of Trauma*, 1965, 5, 554.

⁴ Lasslo, P., *Orvosi Hetilap*, 1969, 17, 970.

⁵ Ganapathy, D. H., *Injury*, 1973, 5, 51.

Cold and the Heart

SIR,—Your leading article (17 August, p. 430) gives inadequate advice, I believe. True, the hypertensive stress reaction to sudden cold may cause myocardial hypoxia in a person with ischaemic heart disease, and avoidance of sudden exposure to cold will, of course, prevent such occurrence. Also I agree that those who use nitrates would do well to take an extra dose before exposure and perhaps they should indeed increase their daily measure of beta-blocking medication during the cold months. But such advice is stop-gap, remedial, and fails to make any attempt at correcting the heart's ability to cope with the increased work cold stress evidently demands.

Anginal patients will experience myocardial hypoxia during exertion. But do we advise them to stay in bed and take extra medicine when they must walk? On the contrary we urge them to strengthen the heart muscle and to improve coronary oxygen supply by gradually increasing exercise. The same applies with regard to temperature changes and needs attention when one must face winter each year, as in the U.K. By routinely taking a cool to cold shower after a warm bath the circulatory stress response itself will become attuned or numbed to the sudden cold stimulus and the heart will be trained to cope with the response. In contrast the use of beta-blockers suppresses the heart's compensatory action and will thereby tend to weaken the heart. I believe that adaptation through training is a therapeutic principle of prime importance for the long haul, while avoidance and medication must be considered temporary, short-term necessities.

A second point: stepping out into the cold is one way of looking at this cold stimulus. But the other way is emerging from the heat. We are dealing here not so much with exposure to cold or winter as with the impact of a sudden change. I advise my patients to bring their indoor temperature in better harmony with the outdoors, both in summer and in winter, and to dress indoors accordingly. For the winter that means an indoor temperature in the low

60s and being warmly dressed. This greatly reduces the "insult" of stepping out into the crisp cold.

While there are yet other points to consider (did exposure to cold happen after a hearty meal or on an empty stomach; did the patient drink coffee or tea or whiskey; is he obese or lean; did he argue with his wife?), adapting oneself to the seasonal temperatures and training the adaptation process deserve, I believe, our major attention.—I am, etc.,

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Geriatric Policies

SIR,—Professor G. F. Adams says that "the geriatric physician with a high turnover and no longstay problem is . . . suspect as a gerontological spiv. Somebody, somewhere must carry the can for him" (28 September, p. 789). I find this denigration of colleagues who strive for high turnover in order to run their service without a waiting list regrettable.

The high turnover strategy does not make the longstay problem disappear, but it can give a more efficient service based on one's limited resources.^{1,2} Those of us who use this approach feel that the tougher discharge policy is a good exchange for abolition of the wait for admission which the patient, the relatives, the general practitioner, and the community services would otherwise have to bear. Somebody has to "carry the can" either way; what needs to be properly examined and discussed is which of the alternatives imposes the lighter burdens. Should we not argue the relative merits of the high turnover strategy and of the conventional system without unnecessary emotionalism or insults or implying that those of one's own persuasion have the monopoly of "warmth, feeling, compassion, humour, patience, integrity and understanding"?—I am, etc.,

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¹ Hodgkinson, H. M., and Jeffreys, P. M., *British Medical Journal*, 1972, 4, 536.

² O'Brien, T. D., Joshi, D. M., and Warren, E. W., *British Medical Journal*, 1973, 4, 277.

Experts and Child Abuse

SIR,—Dr. Selwyn M. Smith and Mrs. Ruth Hanson are I feel breaking new ground with their paper (14 September, p. 666) which draws together the psychological, community paediatric and psychiatric fields into the joint study of battered children. Professor S. D. M. Court and his colleagues (28 September, p. 801) have pointed out that the doctor cannot intervene uninvited. Hence it is all the more important for him to become invited by meeting the parents-in-need half-way, through the context of the ordinary and routine child health service. Dr. Smith and Mrs. Hanson report that many (50) of the 134 children studied had no doctor as a general practitioner, yet many were already "at risk" since developmental quotients were significantly lower for battered children of low birth weight or with failure to thrive. There were then clear reasons for parental concern. The authors

rightly draw the conclusion that lack of professional concern is partly due to failure to appreciate the use of well-validated developmental tests on the child.

Psychologists and clinical medical officers have a great deal to offer in relation to the measurement of mental growth, even in the very young child, through testing and the observation of the status of the child's behaviour and play. The physical effects of abuse and deprivation are there to be seen even postmortem, but equally observable are signs of overall behavioural immaturity or specific immaturity.

Whether the aetiological causes arise from within the child or from without secondary social problems arise. The parents, who may have limited understanding and no insight with their problem, sometimes react highly and emotionally or primitively. It is strange that society lays less blame at the door of more careless parents—for example, those involved in road traffic accidents.

It is generally accepted that all children ought to be subject to brief developmental screening. Those with special needs, be they physical, social, or to do with mental ability, can be separately assessed in great depth on the Griffiths Mental Developmental Scale, as Dr. Smith and Mrs. Hanson have demonstrated. Given astute professional advisers we may expect advice to be available to those parents and children in need. It is usually only psychologists and specially trained (mostly self-trained) doctors who have the impartiality which makes them accessible. At least it should be part of the new community paediatric service to make such advice available where and when it is most needed—that is, in the context of services available to all children. Only in this way will advice be available soon enough or often enough.—I am, etc.,

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Tests of Thyroid Function

SIR,—We were interested to read the paper by Dr. C. W. H. Havard and Miss Margot Boss (14 September, p. 678) in which it was stated that similar information to that provided by the thyroid-stimulating hormone (TSH) stimulation test may be obtained by the measurement of serum TSH levels before and after the injection of thyrotrophin-releasing hormone (TRH). In our paper,¹ to which they refer but unfortunately misinterpret, a single estimation of serum TSH, and not the TSH response to TRH, was as valuable as the time-consuming TSH stimulation test in the assessment of thyroid reserve. In our experience, in the investigation of suspected hypothyroidism no additional information is gained by estimating the TSH response to TRH unless to attempt to differentiate between pituitary and hypothalamic hypothyroidism.

In the following issue (21 September, p. 708) Dr. E. G. M. D'Haene and his colleagues did not find as good a correlation between the effective thyroxine ratio (E.T.R.) and the free thyroxine index F.T.I. as previously reported. They did not, however, assess the diagnostic accuracy of either test on the grounds that an objective measurement would be compared with a subjective clinical impression. They there-

fore have no basis for assuming that the F.T.I. is the better clinical test. In a recent report² we could find no difference in diagnostic accuracy between the E.T.R. and the F.T.I. measured in 100 consecutive patients referred to the thyroid clinic. The thyroid status in these patients was determined not only by subjective clinical means but also by other in vivo and in vitro tests of thyroid function such as plasma TSH estimations and radioiodine uptakes with triiodothyronine (T-3) suppression and TSH stimulation if required. Since the E.T.R. has the advantage over the F.T.I. of being both a one-stage procedure and yielding a more rapid result we still prefer it to the F.T.I. as a screening test of thyroid function. Indeed, we have stopped using serum T-3 uptake measurements for some two years, these being perhaps indicated only if one suspects a diagnosis of binding protein abnormality that has not been induced by pregnancy, oral contraception, or other medicaments.—We are, etc.,

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- 1 Irvine, W. J., et al., *Clinical Endocrinology*, 1973, 2, 135.
- 2 Toft, A. D., et al., *Clinical Endocrinology*, 1973, 2, 127.

C-Film as a Contraceptive

SIR,—In April 1973 the Family Planning Association started a clinical trial to try to assess the efficacy and acceptability of C-Film used as a sole method of contraception. This trial was carried out at the request of the manufacturers of C-Film, Potter and Clarke Ltd., and with their financial support for which we are most grateful.

To be eligible for recruitment to the study a woman had to be aged 16-35 years, to have a stable sexual relationship, and to have had no exposure to oral contraceptives for at least one cycle. All those who entered the study volunteered to do so. On admission a history was taken from each subject, paying special attention to vaginal discharge, pruritis vulvae, and venereal disease. Any history of balanitis, genital pruritus, or venereal disease in the subject's partner was also recorded. After a gynaecological examination the woman was taught how to insert C-Film and written instructions on the use of the contraceptive were given to her. These included the following points: (a) that one C-Film should be used for each act of intercourse, (b) that no other contraceptive should be used during the period of the study, and (c) that a monthly record card should be kept to note the dates of menstruation, the days on which intercourse took place, whether C-Film was used for each act of intercourse and which partner used it, the presence of vaginal symptoms, and whether

other methods of contraception were used (despite instructions to the contrary). Finally, each woman was asked to return to the clinic at three-monthly intervals. Sixty-three subjects entered the study. Of these, 18 were not included in the analysis—three because they never used C-Film, six because they were lost to follow-up, and nine because they failed to keep their record cards satisfactorily. It may be noted that the nine subjects in the last category completed at least 19 months of use of C-Film between them and that, to the best of our knowledge, none became pregnant.

Of the 45 subjects included in the analysis, all had regular menstrual cycles, 23 were nulliparous, and one was lactating at the time of enrolment. Nine of these women experienced an involuntary pregnancy during a total of 175 person-months of exposure to risk, representing a use-effectiveness pregnancy rate of 62 per 100 woman-years. Two pregnancies occurred during the first month of use, one during the second, two during the fourth, and the remainder during the fifth or later months. In only one case did the woman admit to having had unprotected intercourse; the remaining eight women stated that C-Film was always used. In addition to the nine women who became pregnant, a further 25 withdrew from the trial—four because of side effects (irritation and discharge), two because of the difficulties with the method, and 19 for reasons not connected with the method.

During a total of 185 cycles of use C-Film was always used by the female in 109 and always by the male in 15. In six cycles C-Film was always used by both partners, in 47 the pattern of use was "mixed," and in eight it is unknown which partner used C-Film. It has been suggested that male use of C-Film offers less protection against pregnancy than female use. Accordingly, in the assessment of the nine involuntary pregnancies particular attention was paid to which partner had used C-Film during the cycle of conception. In five cases C-Film was used exclusively by the female, in three the pattern of use was mixed, while in only one case could we establish exclusive male use during the cycle of conception. Though the subjects were instructed that no other method of contraception should be used for the trial period several did use other methods on odd occasions during a total of 18 cycles. In these instances other contraceptives were generally used instead of rather than in addition to C-Film. In none of the nine involuntary pregnancies was another method of contraception used during the cycle of conception.

In spite of the fact that the women were carefully taught how to use C-Film, several complained of difficulties in inserting the film properly. This and the fact that C-Film may not always dissolve completely may be responsible at least in part for the high failure rate.

In view of the unacceptably high failure rate the F.P.A. decided to terminate the study in June 1974. The association does not recommend the use of C-Film as a sole method of contraception to women attending its clinics. Whether C-Film has a role to play as an adjunctive to other methods of contraception remains to be seen.—We are etc.,

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M. P. VESSEY
W. BOUNDS
J. WARREN

Economies in the N.H.S.

SIR,—I am prompted to write in support of the letter by Dr. H. P. Hughes (5 October, p. 41) suggesting that there should be a reappraisal by the Law Society, B.M.A., and medical defence societies on what might or might not be considered negligence in failing to x-ray an accident case. There are, without doubt, a great number of unnecessary examinations carried out at the request of inexperienced casualty officers because they fear litigation. Apart from the waste of money, these requests are a source of further unrest among radiographers, who

often have had considerable experience of trauma and who know that the x-ray examination requested is unnecessary. In fact, during the radiographers' strike many of the accident cases sent away never came back again.

Another factor seldom considered is the question of exposing the patient to unnecessary irradiation. *The Code of Practice for the Protection of Persons against Ionising Radiations*¹ states: "The clinician requesting the [x-ray] examination of investigation must satisfy himself that it is neces-