tory difficulty at birth and pulmonary function tests were normal.

I would like to point out that my colleagues and I^{1,2} have suggested that the pre-delivery amniotic fluid lecithin concentration of 3.5 mg/100 ml is associated with practically no risk of neonatal respiratory distress syndrome. As far as I can judge this is confirmed by the findings of these workers. This critical lecithin level indicates the earliest time when the fetus in jeopardy can be delivered without the risk of developing the respiratory distress syndrome in the neonatal period. One would not advocate premature induction (such low lecithin levels are likely to be encountered before 38 weeks gestation) unless definitely indicated in the maternal or fetal interest. In fact, amniotic fluid lecithin concentration for assessing the potential risk of respiratory distress syndrome should be determined only when delivery seems indicated within the next few days. Postponing delivery in such cases may occasionally prove disastrous, for intrauterine fetal death may occur. It would be worth while to follow up the infants with pulmonary hypoperfusion for any predisposition to recurrent chest illnesses. One could then, perhaps, strike a balance between the risk of intrauterine death on one hand and subsequent ill effects of pulmonary hypoperfusion on the other.

Dr. Fisher and his colleagues feel that the margin of safety provided by the bubble stability test is necessary. They attribute five of their 11 false negative bubble stability tests to excessive amniotic fluid volume, but provide no explanation for the rest. That the test gives too many false negative results has also been shown by others.³

Finally, I would like to take this opportunity to add that with borderline lecithin concentrations (3-4 mg/100 ml) certain factors such as the time interval between amniocentesis and planned delivery, mode of planned delivery (vaginal or caesarean section),¹ and amniotic fluid volume ought to be considered for assessing the potential risk of respiratory distress syndrome in the neonate. The fetal size might be another factor, as a large fetus with possibly a large pulmonary alveolar surface area is likely to require a larger quantity of lecithin for alveolar stability than a comparatively small fetus.—I am, etc.,

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¹ Bhagwanani, S. G., Fahmy, D., and Turnbull, A. C., *Lancet*, 1972, **1**, 159.

² Bhagwanani, S. G., Fahmy, D., and Turnbull, A. C., *Lancet*, 1972, **2**, 66.

³ Whitfield, C. R., Sproule, W. B., and Greene, E. M., *British Medical Journal*, 1972, **4**, 362.

Laterality of Fractures

SIR,—In a recent unpublished study of 51 forearm shaft fractures treated by operation over a three-year period at this hospital we have found that exactly two-thirds occurred in the non-dominant forearm. We seek to explain this.

The tendency to injure the non-dominant side was most marked in males and in fractures of the radius alone or both forearm bones together, especially when sustained as a result of falls on the outstretched hand. The tendency was, however, also present,

though less marked, in women and in forearm fractures due to direct blows (see table).

	No. of injured forearms on:	
	Dominant side	Non-dominant side
<i>Sex Incidence</i>		
Male	11	26 (70%)
Female .. .	6	8 (57%)
<i>Mode of Injury</i>		
Fall on outstretched hand	7	20 (74%)
Direct blow to forearm	10	14 (58%)
<i>Type of Fracture</i>		
Radius only .. .	1	7
Ulna only .. .	2	0
Both forearm bones .. .	10	22 (69%)
Monteggia .. .	2	2
Galeazzi .. .	2	3
Total	17	34 (67%)

The prognosis in terms of early return to work and lack of residual symptoms was better in the non-dominant group, as was expected. However, we also found that there was a significantly better recovery of objectively measured rotation in the non-dominant forearm.

We wonder if this tendency to injure the non-dominant forearm has been noted previously, and suggest that it is due to more frequent or more awkward falls to that side.—We are, etc.,

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Enteroviruses in Heart Disease

SIR,—Our investigations of patients suspected of viral carditis during the years 1959 to date have confirmed the importance of enteroviruses in acute myocarditis and pericarditis.^{1,2} The numbers of investigated cases finally classified as "other cardiac diseases" now suffice to give an indication of the significance of enteroviruses in some subgroups of this class also. Considering as "positive" cases in which virus was isolated and/or tests for Coxsackie B virus neutralizing antibodies showed a four-fold or greater rising titre or a static titre of 256 or more to any of the six types, our results may be summarized as follows:

Disease Group	No. Tested	No. Positive (%)
Acute myocardial infarction	48	7 (15)
Acute rheumatic carditis ..	14	3 (21)
Other acute diseases ..	12	1 (8)
Myocardial ischaemia ..	46	1 (2)
Other chronic diseases ..	38	4 (11)

By the same criteria "positive" results were found in 52% of 67 cases of acute myocarditis and 31% of 86 cases of acute pericarditis cases but in only 11% of 72 cases in which the final diagnosis was of non-cardiac disease. In none of the tabulated categories did the percentage of positives significantly exceed the 11% control value of non-cardiac cases; all three cases of acute rheumatic carditis had elevated antistreptolysin titres, indicating streptococcal aetiology.

Our observations, which will be presented more fully elsewhere, suggest that enteroviruses have little if any aetiological significance in myocardial infarction and myocardial ischaemia and that virological

tests for enterovirus infection are not worth while for patients with these types of disease.—We are, etc.,

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and Regional Virus Laboratory,
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¹ Grist, N. R., *Postgraduate Medical Journal*, 1972, **48**, 750.

² Bell, E. J., and Grist, N. R., *14th European Symposium on Poliomyelitis and Other Virus Diseases, Ankara 1973*. In press.

Radiological Evaluation of Pulmonary Metastases

SIR,—I thank Drs. A. W. O'Malley and Michael Shaw for their letter (25 August, p. 454) in which they correctly point out an error in the legend of fig. 3 in our report on epipodophyllotoxin VP 16213 (28 July, p. 199). This should have read "incomplete remission" instead of "complete remission." From the text and tables in the article it is evident that only one incomplete regression was obtained in kidney tumours.—I am, etc.,

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Prescription Charge Anomalies

SIR,—Dr. G. M. Brown (1 September, p. 503) mentions one anomaly in the rules for exemption from prescription charges. There are many others. Consider the patient who is disabled by chronic respiratory and cardiac complaints who may be on four or five different drugs, unable to work, not sufficiently impoverished to qualify for supplementary benefits, not sufficiently disabled to be unable to leave the house without assistance, who has a varicose ulcer requiring dressings but not a permanent fistula. For this patient there is no exemption. On the other hand, someone who has suffered from pernicious anaemia and is now cured need not pay for the monthly injection of Cytamen while able to work full time. A patient with Addison's disease need not pay for his cortisone. He has a deficiency disease but may well be able to work full time. His neighbour, crippled by rheumatoid arthritis, must pay the prescription charge for the same drug.

I would like to suggest a much simpler criterion. All patients who have been certified as unfit to work for periods exceeding four weeks or who have retired from work should be exempt. No doubt there are other categories that should be added to this but clearly the rules as they are now are illogical and unfair.—I am, etc.,

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Superannuation and the Elderly G.P.

SIR,—I write in support of the letter of our Plymouth colleagues (15 September, p. 595) regarding the superannuation pensions of doctors who retired between 1969 and