reference care of worried and "difficult" patients and the handling of particular problems such as the social and psychological management of terminal cases, we need the services of trained social workers whose job is primarily personal contact with the patient. We should value the essential and excellent help that they can give in overall patient care when encouraged in this function by members of the medical profession who do not find their presence threatening.—We are, etc.,

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Intracranial Venous Thrombosis and the Pill

SIR,—I was surprised to see only the progestogen component of two oral contraceptives mentioned in an article on cerebral thrombosis attributed to their use (16 June, p. 647).

Both contraceptive pills contain oestrogen as well: mestranol 0.1 mg in Ortho-Novin, and ethinyloestradiol 0.05 mg in Minovlar. So far as I know, no episodes of thrombosis have been reported as due to progestogen-only contraceptives. On the other hand, oestrogens are known to affect several blood clotting factors, and the increased incidence of thromboembolic disease in women taking combined preparations is accepted as due to the oestrogen component.—I am, etc.,

P. N. HOLBERTON

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Smoking Hazards to the Fetus

SIR,—Space does not permit a full analysis of the letter from Dr. R. J. Hickey and his colleagues (1 September, p. 501) in which they discuss the role of possible causative factors influencing the well-known statistical association between maternal cigarette smoking in pregnancy and birth weight.

We are concerned, however, that they cite our work¹ only to dismiss it as in "error." We did not, in fact, make the elementary error of suggesting that an association by itself proves a causal relationship; nor, so far as we are aware, do other reports in the literature. We did in fact suggest that it might be possible to test the causal hypothesis by a controlled trial in which women were persuaded to give up smoking during pregnancy.

Dr. Hickey and his colleagues appear to dismiss a causal relationship because not all babies of smokers are of low birth weight. Is this really what they imply? The "alternative" hypothesis which Yerushalmi² is purported to have tested (namely, that babies born to women *before* they become smokers will be lighter than those of non-smokers) suffers from severe methodological shortcomings,³ making it clearly untenable. We would, however, agree with them that the "causal" question is still not settled, but we feel that it would be unfortunate if the progress so far made in discouraging mothers from smoking in pregnancy were to be inter-

rupted by the kind of arguments used in their letter.—We are, etc.,

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- ¹ Butler, N. R., Goldstein, H., and Ross, E. M., *British Medical Journal*, 1972, 2, 127.
- ² Yerushalmi, J., *American Journal of Obstetrics and Gynecology*, 1972, 112, 277.
- ³ Goldstein, H., *American Journal of Obstetrics and Gynecology*, 1972, 114, 570.

office bearers who are coloured as well as white; and (3) that the Municipality of Johannesburg has this year decided that it will no longer continue the differentiation of its large municipal staff and has levelled up salaries with no racial discrimination.

It is thus to be hoped that the weight of public as well as professional opinion may yet bring about a change while the Nationalist government continues in power.—I am, etc.,

HUGH WOODMAN

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Co. Wicklow, Eire

Doctors in South Africa

SIR,—Apparently Mr. I. N. Bernadt (22 September, p. 632) reads only the advertisement pages of the *South African Medical Journal*, otherwise he would know that the Medical Association of South Africa has done considerably more than utter verbal condemnation of racial discrimination in the medical field. It has repeatedly sent deputations to the Minister of Health requesting abolition of discrimination in salaries, resulting at one stage in an (admittedly only slight) improvement. Over a year ago in its journal it publicized the establishment of a Salary Equalization Fund for the receipt of voluntary contributions from non-African doctors towards supplementing the official salaries of their African colleagues. (I hope the fund will receive many contributions as a result of Dr. Bernadt's and this letter.)

The fact that coloured doctors cannot, in South Africa, examine white patients is not due to the attitude of the medical profession in that country or even to the Government, as Dr. Bernadt surely must know. With rare exceptions, white persons would refuse—generally with vehemence—to be examined by non-white doctors; and even if they agreed, white nurses in attendance upon them would refuse to assist in such examinations, generally with no less vehemence and with the support of every member of parliament (government and opposition), except perhaps one, the vast majority of their white nursing colleagues, and South African Whites as a whole. These attitudes of white patients and nurses long antedate 1948, the year in which the present political rulers of South Africa came first to power.—I am, etc.,

G. W. GALE

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SIR,—Anyone with knowledge of the practice of medicine in South Africa will be acquainted with the injustice of the two salary scales for doctors and nurses, based on colour, to which Dr. I. N. Bernadt refers in his letter (22 September, p. 632). Over the years there have been demonstrations, protests, even resignations over this question, and agitation mounts in the press and elsewhere against this system.

Apartheid remains dominant politically but it may be noted (1) that the South African Medical and Dental Council, equivalent to the G.M.C. at home, registers doctors in alphabetical order with no reference to colour or recognition or apartheid, all on the register having equal voting rights; (2) that the Medical Association of South Africa has similarly maintained this attitude and has

Alcoholism and the G.P.

SIR,—In view of the considerable publicity and controversy associated with the "Helping Hand" report on this matter¹ I would be grateful for an opportunity to identify my position. In the lecture of mine quoted in the report I said, "Studies suggest the general practitioner is frequently not an effective agent for picking up alcoholics. The reasons would appear to be two-fold. Firstly, the patient or the patient's relatives are unwilling to bring this problem to the doctor. Secondly, the general practitioner is likely to miss cases of alcoholism if his stereotype of the alcoholic is of a skid row figure—that is, the end state of the disease becomes the only form recognized and his lack of awareness that in the early stages alcoholism presents with primarily social, rather than medical pathology."

Unfortunately in the majority of press reports only the second reason was quoted, suggesting that I was unaware of the great difficulty the general practitioner has in dealing with patients who deny their problem. I am of course fully aware of this and had no intention of criticizing general practitioners at large in relation to their difficulty in identifying the alcoholic. I was trying to emphasize the importance of early detection and how alcoholism will often present to a doctor in social rather than medical terms. In any comprehensive treatment service for alcoholics the general practitioner has a major role in diagnosis, and it is hoped that the recent upsurge in interest among general practitioners in the last two years, as evidenced by requests for postgraduate lectures, articles in journals, and research studies, will be maintained.—I am, etc.,

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¹ *Alcoholism and the G.P.* London, Helping Hand Organisation, 1973.

Shake Test on Amniotic Fluid and the Respiratory Distress Syndrome

SIR,—I wish to comment on the suggestion of Dr. P. M. Fisher and others (19 May, p. 423) that, for assessing fetopulmonary maturity and the risk of neonatal respiratory distress syndrome, a critical amniotic fluid lecithin concentration of 3.5 mg/100 ml is too low. Their suggestion is based on the evidence of pulmonary hypoperfusion in two newborn infants with predelivery amniotic fluid lecithin levels of 5.70 and 7.35 mg/100 ml respectively and a negative bubble stability test though the infants had no respira-

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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B. P. HEATHER

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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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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.,

D. W. SMITH

Wellborough, Northants

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