of the farce, if indeed it is one, was given in the same issue by Mr. M. J. Buxton and Mr. R. E. Klein (p. 345), who conclude their paper on the distribution of hospital provision by criticizing reorganization because it has created too many administrative tiers and suggest that one of them may be dispensable.—I am, etc.,

HARVEY GORDON
District Community Physician,
Mid-Surrey Health District

Epsom, Surrey

SIR,—I wrote to you last year (31 August, p. 581) drawing attention to the urgent need to establish an Association of District Community Physicians. Such an association was formed in London on 7 February 1975 and I warmly welcome its inauguration.

A situation now exists in which there is a meeting of regional medical officers, an Association of Area Medical Officers, and an Association of District Community Physicians, and it is being said that the specialists in community medicine in child health, social services, and information services may be establishing their own small groups. Next it will be the health care planners who will do the same thing.

The time is now ripe for the next move. Before it is too late and the new specialty of community medicine is irrevocably shattered into factions, those in office in these new organizations should meet together to negotiate the formation of one single Society of Specialists in Community Medicine which would have within it each of the distinct sections which have been formed. Such a combination of common and specialized interests would be a much more successful and powerful representation of the specialty, giving effective voice as appropriate to its collective and individual needs. At least one other health management team profession has had the vision to do this.

It is suggested that the chairmen of these new organizations should take the initiative in getting in touch with one another to arrange a meeting in the near future to discuss what could be achieved along these lines. Might the Faculty of Community Medicine act as a catalyst?—I am, etc.,

R. G. DUNNING

Department of Community Medicine,
Somerset Area Health Authority,
Yeovil

Linoleic Acid in Multiple Sclerosis

SIR,—The report by Millar *et al.*¹ that sunflower seed oil (active principle linoleic acid (L.A.)) exerts a beneficial effect on the number and severity of acute attacks of multiple sclerosis (M.S.) is clearly of first importance, since it constitutes the only positive claim (based on a properly conducted double-blind trial) for a long-term treatment in the otherwise bleak 100 years the disease has been known. As such, it needs confirmation (and reconfirmation), especially as the effect of L.A. was clear only in the London ($P < 0.01$) and not in the Belfast series of results ($P = 0.1$) reported in their paper.

A blind clinical trial of this type inevitably involves a number of factors out-

side the observer's control. An important one, however, the effective level of linoleic acid-like activity (L.A.L.A.) in the serum, may easily and usefully be monitored.² A curious inversion of lymphocyte sensitivity to L.A. occurs when the concentration of L.A. used in the *in vitro* test for M.S.³ is 0.04 mg/ml instead of the standard 0.08 mg/ml (see table), and if this reflects the *in vivo* situation then clearly an important variable may creep in. Some factor (perhaps dependent on diet—see below) may well have operated in producing the Belfast/London differences. With 0.04 mg/ml in the *in vitro* test the suppressive effectiveness of L.A. is actually less on M.S. lymphocytes than it is on cells from normal subjects or patients with other neurological diseases (O.N.D.), the reductions being 3.6, 10.6, and 12.4% respectively. In other words, M.S. lymphocytes are less susceptible than normal or O.N.D. cells to suppression by low levels of L.A. *in vitro* (and presumably *in vivo*). It therefore becomes important to ensure that an effective L.A.L.A. level is kept up in the serum. While chemical estimation is expensive, complicated, and time-consuming, the functionally important L.A.L.A. may be very simply determined by the macrophage electrophoretic mobility method.² It would therefore be highly advisable to check that a level capable of suppressing lymphocyte activity is indeed being maintained during a trial and that per contra no control patient has an excessively high intake of unsaturated fatty acids.

Mean Percentage Inhibition ($\pm$ S.D.) of Lymphocyte Response to Thyroid Antigen (F1) *In Vitro* by Linoleic Acid

	Concentration of linoleic acid	
	0.08 mg/ml	0.04 mg/ml
Normal subjects not in M.S. families (17)	57.23 $\pm$ 2.51	—
in M.S. families (52)	57.25 $\pm$ 1.46	12.41 $\pm$ 1.28
M.S. patients (17)	91.02 $\pm$ 2.85	3.55 $\pm$ 2.85

Crawford and Hassam⁴ have suggested that the outcome of the L.A. test for M.S. may be influenced by (undisclosed) sunflower seed oil intake. The discrepancy between the results of Field *et al.*³ and those of Mertin *et al.*⁵ in this test is readily explicable on technical grounds.⁶ Circumstances beyond our control have prevented us from observing the effect of long-term exhibition of L.A. on lymphocyte reactivity, which would have been studied as the L.A.L.A. of serum was being monitored in the course of trials.

It has been found that intake of much saturated fat at the same time as L.A. reduces the level of L.A.L.A. in serum as well as delaying its rise, so that a cut-down in animal fat ingestion at the time of taking L.A. is desirable.² Data obtained by L.A.L.A. estimations might indicate the critical level of suppressive activity in serum necessary for effective treatment of M.S. and would so add significantly to the information to be gained from prolonged and time-consuming clinical trials.

In every clinical trial uncertainties creep in and occasional simple monitoring of L.A.L.A. in serum can be useful. Recently one of us (E.J.F.) encountered (in a totally different context) a patient taking part in an L.A. trial who was simultaneously in-

dulging a private passion for sunflower seeds.—We are, etc.,

E. J. FIELD
B. K. SHENTON

Department of Pathology,
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Newcastle upon Tyne

- 1 Millar, J. H. D., *et al.*, *British Medical Journal*, 1973, 1, 765.
- 2 Field, E. J., and Shenton, B. K., *British Medical Journal*, 1973, 4, 738.
- 3 Field, E. J., Shenton, B. K., and Joyce, G., *British Medical Journal*, 1974, 1, 412.
- 4 Crawford, M. A., and Hassam, A. G., *British Medical Journal*, 1975, 1, 150.
- 5 Mertin, J., *et al.*, *British Medical Journal*, 1974, 4, 567.
- 6 Field, E. J., and Shenton, B. K., *British Medical Journal*, 1975, 1, 92.

Diagnostic Test for Multiple Sclerosis

SIR,—Dr. J. Mertin in his letter (8 February, p. 334) points out that in the macrophage electrophoretic mobility test the patient's lymphocytes are washed free from plasma. This implies that the possibility of interference by high plasma linoleate levels in multiple sclerosis patients taking sunflower seed oil (18 January, p. 150) is removed.

Surely the point is that the lymphocytes of these patients would have been living in a linoleic-acid-rich medium, and washing them would not remove linoleate and its derivatives incorporated into the cell structures and organelles. In our routine analysis of red cell or platelet structural lipid washing procedures are employed; this does not remove the lipids and one would expect the same would be the case for the lymphocytes. The lipid composition of the lymphocyte will be determined by its past history and could affect the test.—We are, etc.,

MICHAEL CRAWFORD
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Human Tumour Xenografts

SIR,—I cannot help feeling that your leading article (16 November, p. 366) on this subject presented a rather negative picture.

In extensive studies on the immunobiology of HeLa tumour survival in mice treated with antithymocyte serum good tumour growth was obtained in 67% of the animals, often in excess of 25 mm.¹ In view of this finding, in which the tumours were implanted subcutaneously, my colleagues and I investigated the growth of human tumour xenografts in thymectomized, irradiated mice reconstituted with syngeneic bone marrow cells (T—B+).² Unlike other workers,³ who have used the renal capsule as the implant site, we continued to implant the tumours subcutaneously.

It is our experience that the growth rate of tumours is largely governed by two factors: the size of the initial implant and the completeness of the immunosuppression. In unpublished work, in which the immunosuppression of the T—B+ mouse has been "tightened up," enhanced tumour growth has occurred as compared with control mice. I would think it is difficult to relate this to growth rates observed in man. Some tumours (carcinoma of the human bladder), in our hands, have shown a threefold in-

crease in size within a 35-day period. Furthermore, it is incorrect to say that "the tumours appear to lose their property of metastasizing in the system." In June 1974 my colleagues and I reviewed our work at a Royal Society of Medicine meeting, during which we reported a case of metastatic growth of a melanoma to a mouse para-aortic node. We believe this was the first reported case of metastases in the T- B+ mouse.⁴ We have subsequently reported metastases from human colon, rectum, and HeLa tumours, all to mouse lung.⁵ These observations are comparatively recent and may in part be due to the use of mice from the specific-pathogen-free unit at Mill Hill.

In conclusion, I cannot in principle see the objection to using this system for chemotherapeutic sensitivity studies of tumours. Why should it be necessary to establish that actual growth is occurring? After all, the T- B+ mouse can be used to maintain tissues in vivo without having actual growth. In a pilot study my colleagues and I examined the sensitivity of a breast tumour to chemotherapeutic agents and compared the treated mice with untreated mice. During the study period, in excess of 40 days, the implanted tumours gradually regressed (we have demonstrated growth in other studies), but we compared the regression rates between the treated and untreated mice. Nobody would be foolish enough to suggest at this stage that the system should be used as the ultimate criterion for chemotherapeutic treatment. However, it can provide very useful guidelines. Anything which can do that in a branch of medicine, which largely relies on trial and error cannot be a bad thing.—I am, etc.,

C. R. FRANKS

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London S.E.1

¹ Franks, C. R., Curtis, K., and Perkins, F. T., *British Journal of Cancer*, 1973, 7, 390.

² Franks, C. R., Perkins, F. T., and Holmes J. T., *Nature*, 1973, 243, 91.

³ Castro, J. E., *Nature New Bio'logy*, 1972, 239, 83.

⁴ Franks, C. R., Perkins, F. T., and Holmes, J. T., *Proceedings of the Royal Society of Medicine*. In press.

⁵ Franks, C. R., *et al.*, British Association for Cancer Research Meeting, September 1974.

Lassa Fever

SIR,—Your leading article on Lassa fever (25 January, p. 173) draws attention to the highly infectious nature of this dangerous disease and to the difficulty in diagnosing and treating it. After quoting the W.H.O. recommendation that patients suspected of having the disease should not be moved out of the endemic area you go on to qualify this by adding, "if for medical reasons the patient must be moved to another country. . . ." I find it very difficult to visualize in the light of the information given in the rest of your article what these medical reasons might be.

On the question of movement by air it would be interesting to know what "available apparatus" is referred to which can diminish the risk of airborne contamination. There is no equipment available to the Royal Air Force which will isolate a patient completely from the rest of the aircraft cabin environment. In any simple patient isolator air circulation would be to the aircraft cabin

and therefore to the crew and medical attendants. I know of only two occasions on which patients suffering from Lassa fever have been intentionally moved by air. In the first a German aircraft was fitted with a simple isolation fitment. It is surely incautious to conclude from this single experience that air transport of Lassa fever patients is safe when, for all we know, the patient may not have been infective. In the second case the complex Apollo astronaut isolation system was used. This would hardly be available to us and we could not airlift it if it were. I am of course aware that aircraft exist—for example, the U.S.A.F. C-9A Nightingale—in which the patient isolation facility ventilates to the atmosphere. Aircraft of this kind are specially adapted wholly for aeromedical use and are unfortunately not available to the Royal Air Force.

Given the nature of the disease we are dealing with and the present state of our knowledge, surely air transport would be better utilized in taking whatever skills and therapy we possess to the patient.—I am, etc.,

D. W. ATKINSON

Director of Health and Research (R.A.F.)

Ministry of Defence,
London W.C.1

Proton Beam Therapy for Acromegaly

SIR,—We were interested to see your leading article on "Assessment and Management of Acromegaly" (7 December, p. 549) and are writing about your comments on proton beam therapy. We agree that the results reported for this form of treatment¹ seem to be at least as satisfactory as those of invasive techniques currently in use. We suggest, however, that proton beam therapy may therefore be *superior*, for it lacks the inevitable complications of invasive procedures and involves very little time in hospital. The matter will not be resolved until controlled trials have been undertaken.

The synchrocyclotron at Harwell is of similar design to that used at Harvard for proton beam therapy, and the Medical Research Council has agreed to support the

development of its use for controlled trials of the treatment of pituitary disorders. There are still a number of problems to overcome, but we are hopeful that eventually this treatment will become available in Britain.—We are, etc.,

T. D. R. HOCKADAY

Radcliffe Infirmary,
Oxford

A. H. LAING

Churchill Hospital,
Oxford

R. B. WELBOURN
Chairman

Royal Postgraduate Medical School,
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M. HARTOG
Secretary,

M.R.C. Working Party on Proton Beam Therapy

Bristol Royal Infirmary,
Bristol

¹ Kjellberg, R. N., and Kliman, B., in *Diagnosis and Treatment of Pituitary Tumours*, ed. P. O. Kohler and G. T. Ross, p. 234. International Congress Series No. 303. Amsterdam, Excerpta Medica, 1973.

Children's Worms

SIR,—In your leading article (5 October, p. 3) you state that "it is hard to believe that worms 2-12 mm long would cause abdominal pain." I most respectfully disagree.

In 54 years of clinical practice of paediatrics I have seen several patients with heavy infestations with oxyuris who have episodes of severe abdominal pain which promptly disappeared after effective vermifuge treatment. The latest of these was my own 6-year-old grandchild, who would come home from school and lie down screaming of abdominal pain. (She did have considerable histrionic ability but the pain was undeniable.) Physical examination was negative, but a stool specimen was a solid mass of pinworms, literally thousands. Symptoms promptly and permanently disappeared following appropriate oral therapy.

Surely some of your readers have had similar experiences.—I am, etc.,

EDWARD B. SHAW

University of California Medical Center,
San Francisco

Other Health Services

SIR,—I was interested to read the letter from Dr. N. Kaye (21 December, p. 722) on the Canadian Health Service. I should like to make one or two comments on his statement that "hospital outpatients have virtually disappeared." In Ontario this is not the case, especially for those who work in a university setting. The outpatient service in university-affiliated hospitals is much the same as in the U.K., the patient being referred by the general practitioner to the consultant (specialist). However, there is one important difference. There is an unwritten rule that when a new patient is seen by a specialist in internal medicine, for example, the time spent on examination and initiating treatment will be *at least an hour*. The Ontario Government has taken this into account when fixing the rate of payment at \$40 for each new consultation by the internal medicine specialist, the fee for a follow-up visit being approximately half that amount.

The most important benefit from allowing an unhurried consultation is that the patient receives first-class treatment.

I would agree with Dr. Kaye that there is very little abuse of the fee for item of service system of payment, and very high earners have their billing procedures investigated. In the university hospitals full-time consultant staff are paid a salary plus an additional earnings ceiling from seeing patients. Money earned in excess of that ceiling usually goes to the university or to a special fund from which such "perks" as car rental and subscriptions to professional bodies are met. Needless to say, the "average" consultant's earnings are some four to five times that of his contemporaries in the U.K.—I am, etc.,

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