Professional Autonomy

SIR,—As a new member of the B.M.A., and one who joined only shortly before the recent controversy over the general practitioners' remuneration and terms of contract, I feel bound to say that I have felt annoyed and embarrassed by several of the "statements" issued on behalf of the profession from B.M.A. House. It has been inadequately emphasized that this question affects by no means the bulk of the profession, and the attitudes adopted by general practitioners and their representatives do not necessarily reflect the opinions of all doctors in this country. It is regrettable that confusion of this kind in the public mind has been allowed to arise at all, because of the resultant harm to the good will and respect which the profession has hitherto enjoyed. The harm already done is incalculable, and cannot be measured in the short term.

Within the profession it is easier to see that, so far as general practitioners are concerned, "the pain goes deeper than the pocket," but this is less easily seen by the public, who reacted in the columns of the popular press with understandable anger and resentment when our profession "collectively" rejected the award and findings of an independent review body. One is tempted to ask what the profession's reactions might have been if, say, transport workers or dockers had acted in a similar way. It is precisely because the profession has traditionally had the greatest difficulty in communication that resentment and hostility are aroused, and the fault surely is ours.

I am surprised that what seems to me to be the main issue at stake in this matter has received such scant press, either from you or from independent observers. This is more remarkable, since it was most beautifully highlighted in the *B.M.J.* at the time of the Belgian doctors' strike, when the philosophical issue implicit in "service by contract" as it applied to doctors in modern industrial society was raised. The conclusions were that if a doctor is to be deprived by the nature of his work (described as a *sacerdote*) of the right to strike he should be given considerable autonomy in deciding the way the profession should be organized and paid. This accepted, the doctor then has to translate this principle into political terms and reality, something he conspicuously failed to do when the National Health Service was born.

It was encouraging to read the leader in the *Sunday Times* of 28 February in which a reappraisal of the means test in social welfare was advocated. Most people accept in principle the idea of paying according to one's means. This is, after all, the way in which income tax is levied, and, while cumbersome, can patently be seen to be fair. If the principle of the means test were to be adopted into the Health Service, it is possible to see how the autonomy of the profession in directing its own affairs could be guarded while patients' expenses would be borne, in large part, by the State. This system would only require that a doctor charged his patient according to the service rendered (there could be a national scale of charges for particular services), and the patient in turn would claim the charge from the State, paying according to his means, and perhaps the chronicity of his illness. If the

B.M.A. had given the press and public some insight into the nature of the doctor's philosophical dilemma a little of the bitterness and reproach might have been avoided.

It has been often said but much more often implied that one of the principal reasons for young doctors emigrating is inadequate financial remuneration in general practice. However true this may be as an explanation, it is too facile, and, moreover, carries with it the strong suspicion that those who propound it are merely using it as a prop for their own arguments for more money for themselves. Perhaps an equally important reason why general practice appears unattractive to young doctors is because it offers a life in which merit appears to go unrecognized and where promotion is virtually non-existent. It is a non-competitive career with all the disadvantages that stem from this. It is to be hoped that the career structure for general

practice as advocated recently¹ may be given serious consideration.—I am, etc.,

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REFERENCE

- ¹ Crombie, D. L., *Lancet*, 1965, 1, 368.

The Review Body, a Correction

SIR,—I see from the copy of my typescript that "disputable or controversial" appears as "disreputable or controversial" in the final sentence of my letter (13 March, p. 718). I hope you will correct this. The Review Body has had much criticism since its award; no one, least myself, would suggest anything "disreputable" about its actions.—I am, etc.,

Barnsley, Yorks.

M. E. TAPISSIER.

Sodium Fluoride and Cell Growth

SIR,—Dr. W. D. Armstrong and his colleagues (20 February, p. 486) have recently published experimental data for two strains of human cells *in vitro* which they interpret as refuting the evidence of Berry and Trillwood¹ for inhibition of cell growth by small amounts of sodium fluoride incorporated into the culture medium. From this they "... draw conclusions which do not impugn the safety of use of properly fluoridated water." However, in order to demonstrate inhibition of cell growth, it is first necessary to show *significant cell growth*; this they fail to do. Fig. 1 compares growth observed in control cultures of HeLa S-3_{Oxf.}, using the methods previously described, with the inadequate growth obtained by Dr. Armstrong and his colleagues in their experiments. The Oxford HeLa growth data are plotted both for increase in total cell number, as determined by trypsinization of attached cells at various times after plating and counting them electronically, as previously described, and for increase in mean number of cells per individual clone when small numbers of single cells (50–125/ml.)

are allowed to grow into macroscopic clones. In each case growth is maintained at a high rate for at least seven days, refuting the assertion of Dr. Armstrong and his colleagues that "hypermaturation" cells could have been lost from the glass surface of the culture bottles. The maximum doubling time under our culture conditions is about 30 hours, and the total growth in seven days produces 9–15 times the number of cells in the original inoculum. By contrast, Dr. Armstrong and his colleagues show a total growth of only 2.6 times the initial inoculum, and *no* significant net growth after the first three days' incubation. A similar comparison is seen in Fig. 2 between the growth observed in Oxford strain L mouse fibroblasts and that observed in Minn. EE cultures by Dr. Armstrong and his colleagues. The same comparisons as for HeLa growth are valid here. As the total growth observed by Dr. Armstrong and his colleagues amounts to only about one cell division, and even heavily x-irradiated cells destined later to die can complete one or more cell divisions,² the conclusion by Dr. Armstrong and his col-

FIG. 1

FIG. 2

leagues from these experiments that no growth-inhibitory effect of sodium fluoride was seen must be regarded as meaningless.

The data of Albright,³ however, are significantly different from ours; mouse-leukaemia cells grown in suspension were not affected in their growth by higher NaF concentrations than the 0.1–10 p.p.m. used by us. The attachment to a glass surface which is a *sine qua non* for the growth of HeLa and the strain L fibroblasts is not required by Albright's mouse-leukaemia cells, and this attachment may be the step at which the cell is most sensitive to the action of fluoride. We demonstrated that although growth rate was decreased in cultures containing 0.1–10 p.p.m. NaF, the absolute reproductive capacity (ability of single isolated cells to grow into macroscopic clones) was unaffected unless much higher fluoride concentrations were used.

Finally, we have never interpreted our results in respect of the safety or hazard of public-water fluoridation; despite the flurry of press and scientific comment upon our work, we have merely presented a biological observation, not refuted by subsequent studies, which remains to be confirmed and explained or validly refuted.—We are, etc.,

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- Albright, J. A., *Nature (Lond.)*, 1964, **203**, 976.

Efficacy of Intra-articular Steroids

SIR,—In the *Annals of the Rheumatic Diseases*^{1–3} several articles have appeared by the group at Leeds and Harrogate in which they apparently “prove” that intra-articular corticosteroid injection has no more beneficial effect in osteoarthritic knees than placebo injections. Their papers are studied with “controls” and statistical analyses. I have recently gone over them in revising my chapter on intra-articular corticosteroid therapy, wondering why this group alone could be so completely out of line with all other reports. I have found the answer, and have checked it in our clinic.

Hartfall and his colleagues have used the technique of injecting knees with the patient sitting up, knees bent, and attempting to inject through or beside the patellar tendon. We had never used this approach, discarding it years ago as completely inadequate. They describe no fluid obtained by this technique, and the reason is because no fluid can be obtained with this method from any osteoarthritic knees—i.e., the joint space has not been entered, and injections were only made into the infrapatellar fat pad, not into the joint. We checked this on 12 patients with osteoarthritis, could obtain no fluid by this approach, but obtained it easily by our method. They have therefore proved that, no matter what is injected into a knee, it will have no palliative effect unless it enters the joint space, and nothing more. Rheumatoid arthritic knees, of course, usually contain large effusions, and fluid can be obtained

even by this method in them, so their rheumatoid injections could have been effective as described.

In earlier papers published in the *Lancet*⁴ and the *British Medical Journal*⁵ the group claimed that half the patients showed marked destruction of joints from such therapy, including even “Charcot joints.” They used large doses of steroids; repeated them frequently; and do not say that they cautioned the patients to moderate activity. Our own incidence of such joint destruction has been less than 1% of patients.

I am grateful to those who place “dangerous curve” signs on the roads I drive, but I am not grateful for “detours” when a mere warning would be adequate. Physicians should be warned of dangers from abuse of any therapy, but this group seems to suggest that intra-articular steroid therapy is not palliative in osteoarthritis, and is so dangerous in rheumatoid arthritis that it should be abandoned. I have had more experience than they, having administered more than 150,000 intrasynovial steroid injections to more than 5,000 patients in our clinic since 1951. No other therapy has given so much relief to so many for so long.—I am, etc.,

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Reactions to Intravenous Iron Dextran

SIR,—I too was interested in the article by Dr. Barbara E. M. Clay and her colleagues (2 January, p. 29) and also in the comments by Drs. R. S. Lane and Jean M. Scott (13 February p. 449), who suggest that folic-acid deficiency may trigger the sensitivity reaction.

Recently I have infused 101 cases with iron by this method with two severe reactions of the anaphylactoid type but without collapse. All the cases were associated with pregnancy but the reactions were both in puerperal patients. I have fairly detailed information concerning the folic-acid status of one of these two women. It is shown in Table I.

The serum-folic-acid activity (S.F.A.A.) is suspiciously low, but the marrow was normo-

TABLE I.—Serum Folic-acid Activity in Patient Reacting to Iron Dextran

Gestation (weeks)	27	31	35	38 (Delivery)	7th Day Puerperium
Hb (%)	78	79	84	83	55
Total serum folic-acid activity (m μ g./100 ml.)	2.8	3.0	3.3	1.6	2.6

TABLE II.—Serum Folic-acid Activity in Patient Not Reacting to Iron Dextran

Gestation (weeks)	25	29	38	39 (Delivery)	7th Day Puerperium	6th Week Puerperium
Hb (%)	74	65	77	87	83	84
Total serum folic-acid activity (m μ g./100 ml.)	2.4	1.7	1.2	1.4	1.2	1.8

blastic on the seventh puerperal day, that of the infusion. Neither folic acid nor any iron therapy had been administered.

In contrast, five of twenty cases showed low S.F.A.A. levels throughout pregnancy. Of thirty readings from these five patients in the second and third trimesters, only two were greater than 2.0 m μ g./100 ml. The following case serves as an example (Table II).

The S.F.A.A. was consistently low and megaloblastic anaemia was excluded. Infusion took place at 30 weeks. Based on the 6th-week post-delivery level this woman, even in the non-pregnant state, has a low serum folate.

Of the other four cases mentioned, three were normoblastic and the fourth, a twin pregnancy, subsequently developed a megaloblastic marrow at full term. None had the slightest reaction to infusion.

Thus on the basis of the S.F.A.A. it becomes apparent that a deficiency is not necessarily a trigger or even predisposing factor. Once again one is confronted by that age-old problem—individual idiosyncrasy.—I am, etc.,

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I. W. MANSON.

Drug Advertisements

SIR,—May I congratulate Professor O. L. Wade on his letter (27 February, p. 585)?

This is a most important subject. I have amused myself from time to time collecting advertisements relating to conditions in my specialty which are not only misleading but devoid of any scientific basis. Surely if the printing of such clap-trap is worth while we cannot be such a learned profession as we would like to think. Many must be mere tradesmen recommending advertised remedies.

It is a pleasure to read such a well-thought-out letter with constructive ideas.—I am, etc.,

Tunbridge Wells,
Kent.

W. H. GERVIS.

Booster Tetanus Toxoid

SIR,—Authorities offer different advice on the timing of a booster dose of tetanus toxoid following the completion of a standard course of three injections. Reinforcement is variously advised after three years or five years. A report by Goldsmith, Rosenberg, and Pollaczek (*New Engl. J. Med.*, 1962, **267**, 485) suggested that this period could be extended to 18 years.

We have under our care 17,000 to 18,000 workers who occasionally suffer injuries requiring first aid at the medical centre. As many are ex-Servicemen it seemed interesting to find out how many of those who had been immunized in the Forces show evidence of active immunity. We therefore gave a small dose of tetanus toxoid to a random sample of workers who had previously been immunized against tetanus, to determine the type of antibody response (primary or secondary). We