CORRESPONDENCE

Correspondents are urged to write briefly so that readers may be offered as wide a selection of letters as possible. So many are being received that the omission of some is inevitable. Letters should be signed personally by all their authors.

Specialties within community medicine

SIR,—There is concern among medical graduates wishing to pursue a career in epidemiology in university departments or research units about the requirements that may be imposed upon us for specialist accreditation within the terms of the European Economic Community.

It appears that we may be forced to seek membership of the Faculty of Community Physicians, but there are objections to this as regards the present attitudes of the faculty. It seems to promulgate the view that "community medicine" is, in its own right, a discipline and that this discipline is founded upon certain "basic sciences"—that is, epidemiology, statistics, sociology, administrative theory, and health economics. Prospective community physicians are required to take a written examination (part I) which covers the above-mentioned subjects. I maintain that community medicine must not be viewed as a single composite subject but that it is merely the conglomeration of disciplines mentioned. The depth of knowledge of any one of those subjects required for the examination is of necessity superficial. The understanding of sociology, which figures largely in the examination, must be merely the uncritical assimilation of received opinion. Sociology is completely at odds with the spirit of epidemiology, where the concept of the randomised controlled trial makes hypotheses objectively refutable. The administrative theory is, if anything, even worse than sociology. It is obvious from the sad state of our industry and the incipient collapse of the Health Service that administrative theory as yet has little to offer. It seems that the faculty wishes its candidates to absorb the woolly concepts and noxious jargon contained within the set books. Good administration merely requires that people of initiative, talent, and a capacity for hard work be given the authority to get on with their jobs without constantly referring to various "structured" or "unstructured" committees, groups, etc. (The distinction between a committee and a group eludes me.) A cynic might say that the faculty is using current administrative theory as an "intellectual justification" for the sorry plight of the Health Service.

I believe that a wide but superficial knowledge is useful to the aspirant administrator but that the faculty must recognise that knowledge in depth of one or more of the basic sciences is of equal value. They should not seek to create a new discipline but rather to act as an umbrella organisation looking after the interests of a multiplicity of related specialties and promoting cross-fertilisation wherever possible. I ask them to consider the following proposals, which would have the effect of reorganising the special needs of those who do not desire to be Health Service administrators but who nevertheless will contribute usefully to the intellectual life of the faculty.

(1) Make the part I requirements more flexible. For example, the exemptions should be extended to all who have a higher qualification in one of the "basic sciences" such as epidemiology, statistics, or economics. There would not be a large number of suitable candidates for exemption; hence the faculty would not lose much revenue. Alternatively, keep the present part I for aspiring administrators and have a different version for pure epidemiologists and others.

(2) Retain the part II thesis for all candidates.

(3) Give recognition to certain institutions as being suitable places for epidemiologists, medical statisticians, etc, to gain experience and to count this experience towards specialist accreditation.

It is widely believed within the medical profession that community medicine does not on the whole attract the best of medical graduates. I believe that this must be true since entrants to community medicine have not registered any protest at the unstimulating and undemanding diet they are being fed. The faculty must encourage those who desire knowledge in depth about one of the "basic sciences"; otherwise it will become merely a club for mediocre "Jacks-of-all-trades" while academics and researchers will seek to go their own way.

The faculty must be adaptable. I would like to see it strong and becoming intellectually respectable so that one day it may sever its link with the Royal College of Physicians and gain royal college status of its own.

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Early detection of growth hormone deficiency

SIR,—Your leading article on this subjecf (31 January, p 245) highlights the problems ot diagnosing short stature due to growth hormone deficiency and initiating treatment at the earliest possible stage so that maximum benefit may be obtained from replacement therapy. Professor J M Tanner and his colleagues have made major contributions to our understanding of this condition over the years, but there are three points that at this stage we would like to make.

(1) Growth hormone treatment schedules almost always involve twice or thrice weekly injections of relatively large doses of growth hormone. As a result of the pharmacokinetics of growth hormone absorption and metabolism plasma growth hormone concentration has usually fallen to baseline values within 24 hours of the injection, however large (see fig). The metabolic clearance rate of growth hormone in man averages 3 ml/kg/min[1] and with an average plasma growth hormone concentration of 3 μg/l the daily production rate is in the order of 1 mg in a 70-kg adult, being reduced or increased according to body weight. The

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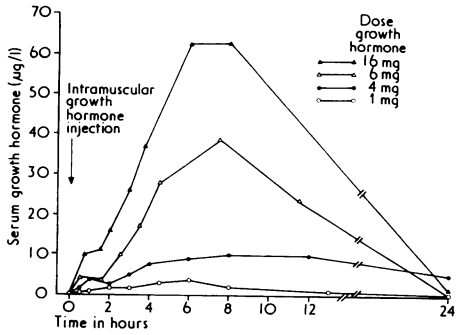

implications of this is that logical replacement therapy should probably be based on the production rate; this amounts to approximately 15 μg/kg/day.

(2) Although it is obvious that clinicians should want to reduce the number of injections given to children to the minimum, this should not be allowed to compromise the efficacy of treatment. The situation is similar to that with insulin in diabetic children, and it is never advised that this be given only twice weekly. Since growth hormone can be given subcutaneously it is logical that children with growth hormone deficiency should be taught to give their own injections of growth hormone in the same way as diabetic children. By the very nature of their problem it will be necessary to continue only until epiphysial fusion occurs and not for life. This treatment schedule, if it were shown to be effective, would mean a considerable saving in growth hormone and could make it more widely available for countries not quite so fortunate as our own.

(3) All new therapeutic substances go through a stage of assessment of efficacy and safety before being made generally available. With growth hormone we feel that this stage was passed several years ago and that treatment and assessment of response to the hormone can now be carried out by a much larger group of hospitals, especially those with recognised paediatric and endocrine units with facilities for diagnosing growth hormone deficiency. This would involve making supplies of the growth hormone available to these units (perhaps the participation of the DHSS in growth hormone production is a signal that is about to happen) and would improve the convenience of follow-up and ensure continuity of care for the patients.

This does not mean that the growth assessment centres are no longer necessary; there remains much to learn about the best ways of treating these patients. We would like to see a trial of growth hormone treatment given by frequent and smaller dosage regimens assessed both by development and measurement of somatomedins. This is a trial that could conveniently be arranged through the existing Medical Research Council structure but would allow paediatricians and endocrinologists to treat their patients without having to refer them to the few and unevenly distributed "recognised" assessment centres.

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[1] Owens, D, *et al*, *European Journal of Clinical Investigation*, 1973, 3, 284.

Psychiatric aspects of shoplifting

Sir,—Dr M Segal's long list of psychiatric causes for shoplifting in his thoughtful letter (28 February, p 523) is impressive. Unfortunately, he has omitted the commonest cause of all—namely, simple stealing for gain. I prefer to call it stealing rather than shoplifting, a term which has developed an aura of respectability, and is almost a medical diagnosis. Whether rich or poor, famous or unknown, we all have the desire to obtain something for nothing. The fact that someone has a large sum of money in her purse or £50 000 in his bank when caught shoplifting does not imply (as many probation officers seem to think) that he or she must be mentally ill. He is simply doing what the modern supermarket openly invites us all to do—help ourselves. It is a basic impulse which has even been given the dubious honour of being called "the acquisitive instinct."[1] However, greed does not justify dishonesty.

In many years experience of psychiatry I have seen a steady stream of these cases referred by the courts. Very rarely indeed have I found any psychiatric disorder in these cases. True, there is the occasional case of a depressed menopausal woman, the odd schizophrenic, and the infrequent confused organic dementia. In over 30 years I can recall only one case of true compulsive stealing.

The truth about shoplifting seems to lie not so much in an esoteric diagnosis but more in Oscar Wilde's comment that the simplest way of dealing with temptation is to yield to it.

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[1] McDougall, W, *An Outline of Psychology*, 6th edn, p 161. London, Methuen, 1933.

Not so double-blind?

Sir,—Professor T W Anderson and his colleagues (21 February, p 457) suggest that a clinical trial can be called double-blind only if active and placebo preparations are indistinguishable and that evidence by a "taste committee" or similar group should be included before editors accept the description of "double-blind" for published trials. This implies that trials are double-blind only if all medicaments under test appear to be identical, which is just not true.

May I refer to the recent report on clinical trials by the Medico-Pharmaceutical Forum,[1] which states: "*Double-blind Trials*—A double-blind trial must ensure that neither patient nor observer recognises a treatment and preferably does not identify a treatment as being the same that he or others receives or prescribes or has received or prescribed in the past. Such lack of recognition (blindness) avoids bias by either patient or clinical assessor. . . . '*Identical*' *Medicaments*—The simplest way of preventing recognition of medicaments is to make them appear identical for both test and control groups. While this is an established method, it may involve some risk of an increased carry-over effect in cross-over trials for an active treatment to a matching dummy or vice versa, resulting in a potential loss of sensitivity."

This makes it quite clear that the use of "identical" medicaments is just one method of organising a double-blind trial. Any method that prevents recognition of a preparation under test by patient or doctor permits a valid double-blind trial and it is not essential for active treatment and placebo to appear to be identical.

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[1] Medico-Pharmaceutical Forum, *A Report by the Forum's Working Party on Clinical Trials*. London, Medico-Pharmaceutical Forum, 1974.

Sir,—Professor T W Anderson and his colleagues (21 February, p 457) express concern at the loose way the term "double-blind" is often used in reports of clinical trials and consider that "active and placebo preparations should be indistinguishable (short of chemical analysis) by any patient, nurse, or physician of average intelligence and curiosity." They recommend that the active and placebo preparation should be examined by a "taste committee" of 20 or 30 colleagues.

We have undertaken a study[1] using a panel of four observers to compare the matching qualities of 22 pairs of agents which have been employed in double-blind investigations. Our study demonstrates that Dr Anderson and his colleagues' concern is fully justified but also shows the adequacy of a *small* panel of assessors. Five pairs of substances were virtually indistinguishable, but in seven there were differences obvious to all the panel members. For this reason we recommended that matching properties should not be evaluated casually, but should be investigated in a formal way along the lines of our study, as Joyce[2] has also suggested.

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[1] Hill, L E, Nunn, A J, and Fox, W, *Lancet*, 1976, 1, 352.
[2] Joyce C R B, in *Psychopharmacology Dimensions and Perspectives*, ed C R B Joyce, p 215. London, Tavistock, 1968.

Progression and regression of atherosclerosis

Sir,—Your leading article on this subject (28 February, p 481) omits all mention of important experimental work carried out over the past 20 years on non-human primates. Recent visits to Chicago and to Iowa were immensely impressive, and it is very difficult to believe that anyone familiar with this work could doubt its relevance to man.

Rhesus monkeys are very similar to man in cardiovascular anatomy and cardiopulmonary physiology and in their lipoproteins and are not susceptible to spontaneous atherosclerosis. By feeding an ordinary American diet prepared by the chief medical nutritionist of the Chicago University Hospital and containing a mixture of 25 of the most commonly eaten constituents, such as beef, pork, fish, poultry, eggs, butter, margarine, cheese, cereals, cake, potato, vegetables, fruit, orange juice, and so on, Wissler and Vesselinovitch[1-6] have produced severe atherosclerotic lesions, both in their earlier and later stages, which closely resemble those seen in humans. Characteristic complex changes occur, with proliferation of collagen, fibroplasia, central necrosis, ulceration, thrombosis, and calcification, followed by "clinical" complications, including coronary occlusion,

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