The figure compares the costs inherent in each mode. One third of a consultation's costs are allocated to administration; this is taken as one cost unit, which happens to approximate to the average cost of one item on prescription. No account is taken of opportunity costs by the patient through loss of earnings or by the fact that the specialist clinic continues to function. Nevertheless, the model shows that referral as an outpatient, for example, may be seen to be a highly expensive management decision.

Morbidity studies of general practice show that the referral option is exercised with a high degree of discrimination (about 1 in 10). The general practitioner thus plays a key role in "sparing" expensive specialist resources. Nevertheless, there is a wide range of variation, suggesting that there may be scope for creating a greater awareness of cost effectiveness in primary care. Issuing a prescription may be obviated by using the counselling mode, but at the expense of consulting time. The figure shows the relative insignificance of prescribing as a cost, yet this management option has received so much attention. We need a re-appraisal of primary medical care costs in a fuller context, to include the general practice-hospital interface and the public.³

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SIR,—There are many reasons why the concept of clinical budgeting is misguided, but the fundamental one is easily identifiable in Mr John Appleby's leading article (7 February, p 326). Mr Appleby is a district health economist and seems to be unaware of the role of the children. He states that "the separation of clinical judgment from financial responsibility will soon end." This notion is based on a confusion, evident when he describes the clinician as making decisions about "using society's resources."

As a clinician, the only activity I am engaged in is doing the best for the person with whom I have an implicit contract: the patient. To describe what the clinician, qua clinician, is doing in any other way is to be confused and will lead to dilemmas and conflict for those who imagine that they can wear a clinician's hat at the same time as the budget holder's. To weigh the virtue of doing the best for the patient against that of saving money is not only immoral but will lead to a lowering of standards and, sooner rather than later bad medicine.

There is no question that money can be saved; paradoxically, this can be achieved even by improving medical treatment. How much of what we do is ritual? How often are our clinical judgements examined critically and the usefulness of what we do questioned? Instead of spending time on administration, committees, and trying to run the service (our aptitude for and success in these endeavours over the years is far from obvious) we would be better occupied in discussing clinical problems, sorting out how best to deal with the common conditions that comprise most of our work, and regaining our optimism about our chosen subject: clinical practice.

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Moderate sodium restriction with angiotensin converting enzyme inhibitor in essential hypertension

SIR,—The study by Dr Graham A MacGregor and others (28 February, p 531) shows that reduced dietary sodium is associated with a fall in blood pressure in hypertensive patients treated with an angiotensin converting enzyme inhibitor. The authors failed to comment, however, on the fact that treatment with slow sodium increased urinary sodium excretion by 50 mmol/day, a value identical with the reduction during placebo treatment.

The trial compared the effects of sodium restriction and sodium loading on blood pressure in patients treated with captopril. It was implied that the response to sodium restriction was greater in patients treated with captopril (13/9 mm Hg, supine) than in untreated patients (10/5 mm Hg), these figures being taken from an earlier report.¹ No evidence of the precision of these estimates was given, however, and it is difficult to be sure that the small difference between the experiments was not merely due to chance.

The findings are used by Dr MacGregor and colleagues to advocate managing mild hypertension with salt restriction followed by treatment with an angiotensin converting enzyme inhibitor. This policy was not evaluated in their study, and we cannot be certain that the addition of treatment with an inhibitor to salt restriction would produce an effect on blood pressure comparable to that observed. Furthermore, treatment of patients who have a restricted salt intake with such an inhibitor might cause a significant incidence of first dose hypotension.² Before this approach can be recommended for general use its efficacy and safety must be evaluated extensively. More pertinently, it will be necessary to show that such a regimen is superior to current practice (stepped care based on thiazide diuretics and β blockers), which large trials have shown to be safe and to prevent cerebrovascular events.³ Experience with angiotensin converting enzyme inhibitors is much more limited, and concern that they may not prevent strokes has been expressed.⁴

Until we have evidence that new treatment policies have significant clinical advantages over stepped care, hypertension should continue to be treated by measures proved to have a beneficial influence on long term outcome.

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Inadequacy of oleic acid in erythrocytes as a marker of malignancies

SIR,—We believe that several points on the use of stearic:oleic acid ratios as markers of malignancy need clarification. In 1985 Wood *et al* suggested that the erythrocyte ratio was a very good tumour marker, giving a positivity of almost 100% in disease.¹ More recently, this hypothesis has been questioned by Dr O Sørreide and coworkers (28 February, p 548), who stated that there were

no differences between the erythrocyte ratios in normal subjects and cancer patients. Questions arise about the methods used by both groups. Even after careful examination of all the references cited by Wood *et al* we could not ascertain the details of the methods they used. The methods quoted by Dr Sørreide and others were equally perplexing—for example, no details of interbatch and intrabatch variations or recoveries were given. In addition, Dr Sørreide and others stated that "oleic and stearic acids were separated almost completely by gas liquid chromatography." What exactly "almost completely" means in relation to gas liquid chromatography is difficult to understand and accept. Furthermore, the quoted erythrocyte stearic:oleic acid ratio for the control group was 0.68, which contrasts strongly with the values usually obtained.²

We have also assessed the use of the erythrocyte stearic:oleic acid ratio as a marker of malignancy. Our work differs from that of Wood *et al* and Dr Sørreide and coworkers in that we evaluated the method used to determine erythrocyte ratios before assessing its use as a tumour marker.³ We found the erythrocyte stearic:oleic acid ratio to be of no use in the diagnosis and monitoring of patients with malignant tumours. There was a small, but significant, difference between the mean (SD) erythrocyte ratio in 21 patients with untreated bronchogenic carcinoma (1.23 (0.17)) and that in 27 healthy adults (1.38 (0.14)). Six patients with lymphoma also had lower ratios (1.11 (0.19)), while five patients with hepatoma showed no difference (1.46 (0.44)). Using a cut off value of 1.00, the erythrocyte stearic:oleic acid ratio would have a sensitivity of less than 12.5% in the diagnosis of malignancy. Furthermore, the ratio may be lower in patients with other diseases, such as diabetes.⁴

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Dose dependent response of symptoms, pituitary, and bone to transdermal oestrogen in postmenopausal women

SIR,—Though Drs Munro Peacock and Peter Selby (14 February, p 440) responded to some of our criticisms (17 January, p 181), most were not answered.

In Coope's study group¹ the average weekly number of hot flushes was 44 before treatment; the average one, two, and three months after withdrawal of oestrogen was 24, 56, and 60, respectively. Thus one month without oestrogen was insufficient for the pretreatment value to be regained. We do not understand the comment by Drs Peacock and Selby about the values in Coope's placebo group as we were discussing the effects of oestrogen withdrawal, not placebo treatment. Washout periods ranging from four to six weeks are clearly inadequate.^{2,4}

Drs Peacock and Selby have misrepresented our study.⁵ In agreement with all other investigators, we

observed a significant suppression in the frequency of flushing episodes after three weeks of treatment (60% reduction). Unlike all other investigators, however, we extended the duration of oestrogen treatment to three months and observed a further 30% reduction, which suggests that three week study periods are too short.

With regard to the postmenopausal score, we understand that this questionnaire is scored by the observer. It is still not clear whether the observer knew the oestrogen dose given to each patient—hence our question about patch sizes. This is a potential source of bias, and further bias could have been introduced by variability between the observers.

Drs Peacock and Selby have still not answered the statistical points raised in our original letter. They stated that the analysis was “based on a paired *t* test between untreated and treated values” in their original paper. We pointed out, however, that to use a paired hypothesis test in circumstances that clearly require a more rigorous, multitreatment approach, which is necessary to show a dose-response effect, is inappropriate.

As the authors pointed out, the phrase “arbitrary drawn through the means” is indeed a solecism, which makes even more surprising the fact that they attribute so much importance to the so called congruency of the lines. If we take the literal sense of the word congruency as implying agreement then we must also consider the meaning of an “arbitrary” line, specifically one that is “not bound by rules” or that has arisen from “accident rather than from rule.” We cannot accept an “agreement” based on something that is by definition (etymologically and by the authors’ implicit insistence) “not bound by rules.” With a quantitatively determined function (non-linear least square regression) rather than subjective guesswork the authors could have presented their case with reasonable statistical validity. As it stands, the study does not confirm a dose dependent relation between transdermal oestradiol and climacteric symptoms, gonadotrophin release, or bone resorption. The authors cannot claim that these three actions of transdermal oestrogens become asymptotic at plasma oestradiol concentrations above 150 pmol/l as the curves to which they refer are arbitrary.

Though Drs Peacock and Selby have not detected changes in calcium regulating hormones with oestrogen administration, others have done so.⁶⁻¹⁰ Thus an indirect action of oestrogen on bone, mediated at least in part by calcium regulating hormones, cannot be excluded. Certainly, there is no evidence to support their hypothesis that oestrogen affects bone resorption by a receptor mediated action. In contrast to calcium regulating hormones, oestrogen receptors have not been detected in bone tissue.^{11,12} Similarly, there is no evidence that oestrogen resets the secretion thresholds of calcium regulating hormones without changing their plasma concentrations and no reason why such an effect should influence bone resorption.

We still cannot conclude from their study that oestradiol concentrations higher than 150 pmol/l will not confer additional benefit.

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SIR,—As the author of the study quoted by Dr Malcolm I White and colleagues and Drs Munro Peacock and Peter Selby (14 February, p 440), I would like to comment on the design of this study¹ and other trials of oestrogen and placebo treatment.

Our patients experienced flushes about two weeks after the withdrawal of oestrogen treatment.² These symptoms did not reach their maximum level, however, until at least three months after withdrawal of oestrogen at the cross-over point. In our second trial we incorporated a washout period of two months between the two halves of the trial: half of the patients received six months of treatment with oestrogen first and half received six months of treatment with placebo.³ Again flushes recurred during the first month after withdrawal of oestrogen but did not reach maximum until six months after withdrawal.

In both trials the effect of placebo was different in the two halves of the trial. This may be because patients who stopped receiving oestrogen guessed this because of the flushing that they subsequently experienced.

Both of these trials suggest that a washout period of one month (as in the study by Drs Peacock and Selby) is too short to allow the effects of oestrogen treatment to disappear completely.

The total duration of Selby and Peacock's study was short. The duration of our trials was extended from six months to 14 months, and we found that oestrogen treatment reached its full effect after three to five months. The washout period of two months in the second trial could have been longer, but we believed that with a longer gap some patients would lose motivation and drop out of the study.

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Poor start for Health Education Authority

SIR,—Those who have expressed concern about the motives behind the Department of Health and Social Security's “takeover” of the Health Education Council (14 March, p 664) have emphasised the government's need to stifle critics within the

council who have spoken out against “anti-health” vested interests. This may well have been one of the reasons for the move towards direct ministerial control of the organisation, but a statement made by Mr Tony Newton, Minister for Health, in the debate on the reconstitution of the Health Education Council suggests a different reason for the takeover—namely, the department's desire to have greater control over the Health Education Council's advertising machinery so that politically motivated campaigns can be mounted without opposition from professionals.¹

In the debate Mr Newton said: “. . . the proportion of the council's income spent on advertising, which I suppose is the most direct form of communication with a mass public, had fallen, not steadily but quite a bit, from 55% of its total budget in 1978-79 to 14% in 1984-85. . . . Therefore, we doubted whether that body was using its money effectively for the purpose that we all want achieved.”²

The first point that must be made about this statement is that mass advertising has been found to be the least effective form of health education. A review of 250 evaluation studies from 27 countries, funded by the Health Education Council, showed that individual instruction is the best form of education, followed by a group approach.^{3,4} Impersonal, generalised instruction was found to be the least effective means of influencing knowledge, attitudes, and behaviour.

Secondly, the decline in the proportion of the council's income spent on advertising was a direct consequence of the council's attempts to improve its effectiveness, as a result of the findings of these evaluation studies. In 1978 the council held a “retreat” for council members, at which the limitations of mass advertising and the implications of this for the council's policy were discussed.⁵ Given the mounting evidence, the Health Education Council had no alternative but to reduce its dependence on mass advertising.

Mr Newton expressed doubts about whether the council had been spending its money effectively, but clearly he cannot be judging the organisation in terms of its educational effectiveness. In fact, the Department of Health and Social Security has a long history of using advertising campaigns for political reasons^{6,7}—that is, when there is pressure to do something about a health problem an advertising campaign is a convenient, quick, and highly visible response. As a civil servant in the department commented some years ago: “Why bark when you've got a dog?”

In recent years, however, the Health Education Council has been increasingly reluctant to “bark” for the Department of Health and Social Security, and it has sometimes refused to carry out politically motivated advertising campaigns. This is why, for example, the department has occasionally had to run campaigns through the Central Office of Information and why civil servants have taken charge of the *Look After Your Heart* campaign, which one Health Education Council official described as “the campaign to help re-elect the Tories.” The department has therefore had to take the final step of bringing the council “to heel.”

Judging by what Mr Newton said, we can look forward to an increase in the new Health Education Authority's advertising budget and an increase in the number of campaigns carried out for political reasons. When this happens, and when the controversy that has surrounded health education campaigns for many years begins to intensify, perhaps criticisms should be directed at the ministers who set up the Health Education Authority rather than at the authority itself.

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