

legitimate work of public health medicine and happen daily.

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1 Whitty P, Jones I. Public health heresy: a challenge to the purchasing orthodoxy. *BMJ* 1992;304:1039-41. (18 April.)

SIR,—Paula Whitty and Ian Jones's article questioning the role of public health doctors in the purchasing of health care services is challenging.¹ It lacks, however, a broader perspective on the problems faced by the specialty of public health medicine in procuring health gain.

Firstly, the authors mention just briefly the impact of fundholding by general practitioners on the organisation and planning of health care in a population. With post-election hindsight (and a new secretary of state for health, whose ambivalence towards trusts and whose dogged determination over fundholding is well publicised), this "wild card" in the NHS reforms deserves greater and urgent attention.² Public health doctors with a purchasing role in their district health authority have generally ignored the purchasing power of fundholders, especially those practices operating in consortiums. In so doing they may have tipped the delicate balance between the need to remain influential in the district health authority and the need to retain independent professional responsibility for the health of the whole population too far in the former direction. In addition, there are no incentives for fundholders to take regard of departments of public health medicine with a view to obtaining rational advice on purchasing.

Secondly, the authors assert that NHS services are poor at promoting health gain in comparison with fundamental changes in socioeconomic conditions. Historically most NHS resources have been focused on secondary health care at the expense of primary health care services. But primary health care services are rarely regarded as potential providers of cost effective, integrated, and locally responsive health care; rather they are considered merely to be efficient gatekeepers. Although, like secondary health care, primary health care remains essentially unevaluated, the reorientation of medical care towards preventing disease, promoting health, and primary health care is an inevitable but, hopefully, worthwhile strategic trend.³

Thirdly, the purchasing role of family health services authorities has at best been marginal (in terms of absolute money available) yet their potential for influencing health gain is unrealised. Some faltering steps are being made by a few public health doctors in health care planning and developing strategy in primary health care. The relationship between medical advisers or directors and public health medicine is of paramount importance, yet there is little evidence that it is being managed proactively. I suspect that the future success of public health medicine depends more on its central involvement in primary health care than it does on any other purchasing role.

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SIR,—Paula Whitty and Ian Jones's article on public health medicine is misleading and over-pessimistic.¹ The authors fail to acknowledge the growth throughout the 1980s of the new public health movement, which seeks to influence from

within the NHS as well as operating more widely through structures such as the healthy cities movement and external organisations such as the Public Health Alliance.^{2,4}

It is erroneous to attribute the current division of working time of those in public health to the NHS and Community Care Act. Since 1974 public health has been anchored in the NHS management structure, and much of the time and energy of public health physicians has been devoted to health care services. The authors produce no evidence that more time has been spent on health care services and less on health promotion since the implementation of the act.

Though there are serious problems with the NHS and Community Care Act, we see particular advantages to the split between purchasing and provision. Health authorities are now responsible for the health of a defined population, and, as partners in purchasing teams, public health physicians have a major role in the distribution of resources. We are now able to require information on outcomes and effectiveness as part of service contracts.

The fear that assessment of health care needs will not improve the public's health derives from the work of McKeown⁵ and assumes that the only important outcome measures are reductions in mortality. We suggest that a reduction in distress and disability due to the activities of health care services is equally important. Indeed, Whitty and Jones are in danger of belittling the daily contribution that the one million employees of the NHS make in ameliorating inequalities and relieving distress, disability, and disease. Not only do they adduce no evidence to support their assertion that public health skills are "presently redundant" in the measurement of outcomes but they later state that public health has an accepted role in evaluating the benefit of health services.

Like Whitty and Jones, we are concerned about the mergers of health authorities, impact of fundholding, effects of relatively diminishing resources, and serious lack of reliable information on activity and effectiveness of health services. Our particular skills in advocacy and analytical interpretation of data and the ability to take an overview of the population will help to meet the challenges within the NHS as well as in a wider local context.

Now is an exciting time to work in public health, as the quantity and quality of our new recruits attest. Our work is reflective and analytical, and we are more prone than other specialties to question our role. In that case we should take care to give the positive aspects as wide publicity as the negative ones.

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- 1 Whitty P, Jones I. Public health heresy: a challenge to the purchasing orthodoxy. *BMJ* 1992;304:1039-41. (18 April.)
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Vertebral fractures

SIR,—Cyrus Cooper and L Joseph Melton's editorial brings together several of the current thoughts on spinal deformity.¹ We disagree, however, with the conclusion that the "clinically

manifest burden [of vertebral fractures] is considerable." The burden of a vertebral fracture must be the continued pain, disability, or deformity that arises plus the prospect of further fractures in the future.

Any quantification of fractures should closely relate structural deformation to symptoms. The authors barely touched on the problem of grading deformities let alone relating them to symptoms. The plethora of grading systems indicates the problem. No system rises appreciably above all others in its ability to relate to symptoms.

In a study that we carried out in Nottingham subjective assessment was no worse than any of three grading systems that were compared by cross correlations of deformity, and none of the grading systems related significantly to pain or disability (unpublished findings). The study also showed that even the measurements of vertebral dimensions that the grading systems use are open to subjective interpretation.

We are surprised that Cooper and Melton do not quote a study by Cooper himself with colleagues.² This cross sectional epidemiological study of vertebral fractures showed, like ours, that there was no significant difference in the prevalence of back pain between groups with and without fractures. A study by Ross *et al* did show a correlation of back pain with limitation of activity and vertebral fractures but only for those in whom lateral radiography showed a loss of 35% of the area of at least one vertebra.³ This was the most severe grade and is above the grades 1 and 2 that Cooper and Melton quote. Ross *et al* found no correlation of fractures of Cooper and Melton's grades 1 and 2 with back pain unless there were multiple deformations.

So do we need grading at all? Why not define vertebral fractures in population studies as occurring above some level of deformity—say, a 35% loss of area?

From Cooper and Melton's figures for the frequency of vertebral fractures in the United Kingdom, we would expect 114 patients a year to be admitted with painful vertebral fractures in Nottingham. Hospital Activity Analysis data, however, show that only 18 patients were admitted with pain. Though we acknowledge that coding problems and underreporting occur with Hospital Activity Analysis data, we suggest (based on admissions to hospital) that the clinical problem is probably smaller than the authors suggest.

The subtitle of the editorial asked how large the silent epidemic is. Do we have a silent epidemic or not? Until a relevant definition of a vertebral fracture is found further research work will be hampered.

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Routine weighing during antenatal visits

SIR,—The tone of the editorial on routine weighing during antenatal visits was disappointing.¹ The authors seem to overlook the fact that routine antenatal care is no more than a screening process. Thus the tests applied during antenatal care should

satisfy the normal requirements for screening tests: each test should be cheap, easy to apply, reliably reproduced, sensitive, and specific. Although routine weighing satisfies the first two criteria, none of the quoted references suggests that it comes anywhere near to satisfying the remainder. If we are to make progress in detecting poor fetal growth we must begin to apply scientific principles to antenatal care. No longer is it sufficient to answer the question "Why should it not be done?" The important question is "Why should this be done?"

Dawes and Grudzinskas have shown that routine repeated measurement of maternal weight in all patients adds nothing to the reliable prediction of babies who are small for gestational age.² Further studies have confirmed this finding and indicated why the observation lacks sensitivity and specificity.^{3,4} If serial measurement of weight had never been a part of antenatal care there would not be any reason to introduce it now. This is not to imply, however, that measurement of maternal weight during pregnancy is never appropriate, merely that routine measurements are a waste of resources and may be misleading.

In the same issue as the editorial M G Dawes and colleagues highlight the confusion surrounding the perceived reasons for routine weighing in antenatal clinics.⁵ Those who currently apply this test gave 40 different reasons for doing so. Clearly there is no consensus, even among practising health care workers, over its application let alone its interpretation.

Routine serial measurement of maternal weight during pregnancy is not a valuable procedure and should be abandoned.

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- 1 Dimperio DL, Fentzen BH, Cruz A. Routine weighing during antenatal visits. *BMJ* 1992;304:460. (22 February.)
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SIR,—Diane L Dimperio and colleagues conclude that serial antenatal weighing should continue as a screening test for preterm delivery, low birth weight, and pre-eclampsia¹ but provide no evidence that it fulfils the criteria for a successful screening test.² Certainly the conditions they wish to identify are common and important enough for screening to be worth while, but there is no evidence of a silent period in the development of any of these conditions during which treatment is beneficial, with the possible exception of treatment with low dose aspirin for pre-eclampsia. Pre-eclampsia is better screened by measuring urine protein excretion or blood pressure, or both, than by serial weighing.

Even if there were effective interventions none of the papers quoted by the authors give data in a form such that the sensitivity and specificity of the test can be derived. They all simply describe weak correlations between low weight gain and various poor outcomes. Only Dawes and Grudzinskas have described the test characteristics of low weight gain (below the 10th centile) for predicting small for gestational age babies (sensitivity 19%, specificity 87%) and of high weight gain (above the 90th centile) for predicting high blood pressure (sensitivity 26%, specificity 80%).³ This performance is much worse than that of other variables measured in screening tests, such as fundal height, ultrasonographic measurements, blood pressure,

and urinary variables. Dimperio and colleagues provide nothing in their editorial to contradict Dawes and Grudzinskas's conclusions that routine weighing fulfils only two screening criteria (it is cheap and acceptable) and that it should stop.³

It is profoundly depressing that at a time when obstetricians are attempting to rationalise their management of pregnant women⁴ the editorial should respond to this excellent scientific evaluation of a common screening procedure in such an unscientific way.

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- 1 Dimperio DL, Frentzen BH, Cruz A. Routine weighing during antenatal visits. *BMJ* 1992;304:460. (22 February.)
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AUTHOR'S REPLY,—Though one objective of antenatal care is to screen for problems, another is to provide teaching and anticipatory guidance that, if followed, result in optimum outcomes for both the mother and the newborn infant. Active promotion of healthy behaviours, such as consumption of a diet that provides adequate but not excessive energy, should be part of routine prenatal care. Methods of estimating the energy requirement of a pregnant woman exist but are time consuming, do not take into account individual variation, and have not been correlated with the outcome of pregnancy. A simple and more effective method of assessing energy sufficiency during pregnancy is weight gain. A comprehensive analysis of scientific data has resulted in guidelines for weight gain that are consistent with desirable outcomes for mothers and newborn infants.¹

As both inadequate and excessive gains in weight are associated with undesirable outcomes of pregnancy they should be avoided. Weight gain in the second half of pregnancy is especially correlated with fetal growth,² and thus assessments of weight after the initial booking continue to be essential for good care. Those providing care should routinely monitor weight gain to reinforce a positive pattern or intervene if the pattern is becoming abnormal. Women with poor weight gain should be assessed to determine why their energy intake is insufficient for their requirements. Unusually high weight gain may, but does not necessarily, reflect excess energy intake. When it occurs, assessment should determine whether it is a result of excess energy intake, abnormal fluid retention, or multiple pregnancy.

The routine of weighing the patient and subsequent counselling has two additional benefits: it relieves patients' anxiety about weight gain and introduces the topic of the overall nutritional adequacy of the diet. Promotion of good nutrition should be an important component of antenatal care. Women who receive intervention with emphasis on achieving an optimum weight gain and nutritional adequacy have improved outcomes compared with those who do not.³ Routine weighing of all women as part of antenatal care is valuable and should not be discarded.

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Manipulative therapy and physiotherapy for persistent back and neck complaints

SIR,—Bart W Koes and colleagues have shown the benefits of manipulative therapy compared with physiotherapy,¹ supporting the findings of an earlier trial conducted by the Medical Research Council.²

Firstly, however, contrary to the statement in their paper, it is not possible to differentiate patients with disc herniation from those with other causes of back pain on the basis of a non-specific complaint and physical examination. Radiological investigations such as computed tomography, myelography, and magnetic resonance imaging are usually required to achieve this distinction.

Secondly, intervertebral discs start to degenerate in early adulthood, becoming symptomatic after fragmentation, with herniation through an intact annulus or impingement on the spinal canal.³ As manipulative therapy entails small movements of high velocity, applying sudden stresses to chronically degenerating discs may precipitate protrusion of a disc. Manipulating the spines of patients with back pain of undiagnosed aetiology is not without risk, and known complications, although rare, range from injury to the cervical cord⁴ to brain stem infarction.⁵ Our experience includes two cases of compression of the cauda equina after chiropractic manipulation⁶ in which the diagnosis was delayed, resulting in long term disability. As a result we endorse calls for further trials to elucidate the role of spinal manipulation in the management of low back pain and for a review of chiropractic training in the United Kingdom.^{7,8}

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- 1 Koes BW, Bouter LM, van Mameren H, Essers AHM, Versteegen GMJR, Hofhuizen DM, et al. Randomised clinical trial of manipulative therapy and physiotherapy for persistent back and neck complaints: results of one year follow up. *BMJ* 1992;304:601-5. (7 March.)
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SIR,—I am concerned about the inappropriate selection of patients and treatment in the study by Bart W Koes and colleagues.¹

The introduction mentions that, in the patients selected, no underlying disease could be established and the causes of the complaints remained unknown. Why? Were the assessors lacking the