about the 14 others except that some, if not most, of them had suffered a penetrating injury. Most centres today would perform emergency computed tomography for all such patients at their first attendance. This does not seem to have been the case in the study, since the authors claim that "none of the 20 patients who underwent computed tomography for a second time showed measurable change from the findings at the first attendance," which leads us to believe that most of the patients who were operated on had computed tomography for the first time at the time they reattended. One could thus conclude that a vault fracture is an indication for immediate computed tomography and neurosurgical evaluation rather than a predictor of computed tomographic abnormalities and surgery for patients who reattend (as the authors conclude).

Secondly, Voss and colleagues state that their data "support the continued use of selective skull radiography to identify those at high risk for intracranial complications." While we accept this statement in principle, we think that it is important to define better the criteria that emergency doctors should follow in requiring skull x ray films. Surely the data presented do not disprove Masters *et al*'s recommendations that skull radiography is unnecessary in patients who present for the first time after a head injury that did not result in loss of consciousness, amnesia, or other neurological abnormalities and that these patients should be sent home with an advice sheet.³ To prevent an epidemic of unnecessary skull radiography it would be helpful to know whether any such patient was among those who underwent neurosurgery on reattendance in the authors' study.

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1 Voss M, Knottenbelt JD, Peden MM. Patients who reattend after head injury: a high risk group. *BMJ* 1995;311:1395-8. [With commentary by G Murray.] (25 November.)

2 Masters SJ, McClean PM, Arcarese JS, Brown RF, Campbell JA, Freed HA, *et al*. Skull x ray examinations after head trauma. *N Engl J Med* 1987;316:84-91.

Authors' reply

EDITOR,—We agree with Daniele Coen and colleagues that ideally all patients with a skull fracture or a penetrating injury should undergo computed tomography and neurological evaluation. This, however, is impossible where head injury is common or where computed tomography and neurosurgeons are scarce. If we had offered computed tomography to all our patients with penetrating wounds to the head or skull fractures who were classed as having minor head injury we would have obtained at least 10 times the number of scans, with the same yield. Generally, most of these patients go home and their problem resolves spontaneously. The purpose of our paper was not to repeat the many studies on outcome in patients after their first attendance with head injury but to try to formulate risk factors for the 2% who reattend on the basis of the advice sheet on head injuries.

We agree with Masters *et al* that skull radiography is probably unnecessary in most patients with blunt head trauma due to acceleration or deceleration who have no positive features such as loss of consciousness. This probably also applies to patients who are going to have computed tomography before observation in hospital. We believe that a skull fracture indicates that a non-trivial force has been applied to the head. This

may be associated with intracranial bleeding, and patients merit at least careful observation even if neurologically normal. Not all patients who develop intracranial complications after head injury have an initial brain injury, and early computed tomography is unlikely to be contributory in this group.

Skull radiography is particularly valuable for departments that do not have unlimited access to computed tomography: even after losing consciousness, patients who are neurologically normal and do not have a skull fracture can usually safely be sent home immediately to the care of a responsible person, thus releasing hospital beds. If such patients reattend, however, it makes sense to perform computed tomography if features indicate that a complication has developed, irrespective of the initial presentation. Another reason for skull radiography is to detect intracranial air, a foreign body, or a slot fracture from a penetrating injury: these are important even if the patient is neurologically normal. Two of the five patients with cerebral abscess, two of the eight with extradural haematoma, and three of those with subdural haematoma who had neurosurgery were neurologically normal at their first attendance; all those who reattended were neurologically normal on discharge from their first admission.

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Costs of prevention

Place impossible political burden on local authorities

EDITOR,—I submit that John Cairns does not go far enough in his editorial on the costs of prevention.¹ He suggests that prevention is not necessarily better or less expensive than cure. I would say that it is impossible to compare prevention and cure because the outcome measures are different. Cure is measured in terms of recovery from illness or of saving life, while prevention can be assessed only in terms of extended years of useful life, with the intended effects of intervention being explanatory variables rather than prime outcome measures.

The effects of prevention on the curative services may be immediately beneficial, but thereafter, by extending life, they are at best neutral and more likely to add to the burden. Furthermore, unless the risk is high, individual preventive measures give a low return and so are far more expensive than cure. At present, the public tacitly accepts that prevention is more expensive than cure. In his Richard Dimbleby lecture on BBC television in 1978 the late Lord Rothschild pointed out that expenditure on preventing death is related to the perceived rather than the real risk. It may be morally correct for society to decide that it is prepared to spend 10 times as much on preventing a death in a road accident (£10⁵) than is generally spent on cure (£10⁴). One question whether this is a rational moral decision when the sum per death probably multiplies 100-fold in the sequence road accident, railway accident, fire, and radiation. These differences put an impossible political burden on local authorities if they have to apportion one budget.

The problem must be solved centrally by a separate government allocation for health promotion. It might be possible for the same authority to coordinate services at a local level and for health promotion and treatment services to commission specific items from each other. Subject to this, the budgets must be held entirely separately and no virement allowed.

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1 Cairns J. The costs of prevention. *BMJ* 1995;311:1520. (9 December.)

Prevention encompasses more than screening

EDITOR,—John Cairns's editorial entitled "The costs of prevention" should more accurately have been titled "The costs of screening."¹ Cairns perpetuates the widespread misconception that prevention consists only of screening programmes. Prevention encompasses much more than screening, as so well discussed by Rose.²⁻⁴

Mass strategies such as the fluoridation of water, immunisation, nutrition programmes, environmental protection, a reduction in cigarette smoking, and legislation on the use of seat belts and drink-driving are also part of prevention. Certainly, all preventive programmes have costs and risks as well as potential benefits, which need to be examined critically. The balance of potential costs and benefits for mass strategies may, however, be quite different from the balance for screening strategies (which are targeted at identifying high risk individuals), as Rose has also pointed out; Cairns does not consider these at all.

This title and the subtitle ("Not necessarily better than cure") in a journal such as the *BMJ* may be misconstrued as applying to all preventive programmes, but the argument in the editorial is made for only one, relatively limited, aspect of prevention—screening; other aspects are ignored.

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Measurement of prostate specific antigen as screening test for prostate cancer

False positive results cast doubt on conclusion

EDITOR,—Carol Parkes and colleagues' main conclusion that 60-74 year old men with a prostate specific antigen concentration ≥ 12 times the normal median value have about a 50% chance of developing clinical prostate cancer within three years seems speculative and unsupported by the data presented.¹ We are told that over the whole age range of the 49 000 men studied 16 developed prostate cancer within three years of a blood sample being obtained, of whom 13 would have been detected if ≥ 12 times the normal median concentration of prostate specific antigen was used as the cut off value. With this cut off 0.5% of the men—that is, about 245 (on the assumption that serum samples were available for most subjects)

—would have yielded false positive results, so someone with a positive result on screening would have a 1 in 19 chance of developing clinical prostate cancer within three years. The authors do not mention this but instead invoke calculations based on population based data (not from this study) to argue that looking at men aged 60-74 would give the much more impressive specificity quoted in their conclusion.

We must speculate why we are not told how many of the men aged 60-74 who were studied had ≥ 12 times the normal median concentration of prostate specific antigen but did not get prostate cancer. The investigators must know these data, and omitting them severely damages the credibility of the conclusion.

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1 Parkes C, Wald NJ, Murphy P, George L, Watt HC, Kirby R, *et al.* Prospective observational study to assess value of prostate specific antigen as screening test for prostate cancer. *BMJ* 1995;311:1340-3. (18 November.)

Paper gives misleadingly optimistic view

EDITOR,—Fifteen per cent of men aged 55-69 have prostate cancer; tumours are detected in 5% by rectal examination, measurement of prostate specific antigen, and transrectal ultrasonography plus biopsy. Only 1%, however, develop clinical cancer. How did Carol Parkes and colleagues find that measurement of prostate specific antigen is a good predictor?¹

They achieved their results (85% sensitivity, 0.5% false positivity) by using prostate specific antigen concentrations of ≥ 12 multiples of the median and by concentrating on men presenting within three years of blood sampling. Tumours presenting after a short time are advanced and incurable: figure 2 in the paper therefore provides a deceptively optimistic view of the utility of this test. Prostate cancer progresses slowly, and men presenting after more than six years, who are likely to benefit from curative treatment, are missed when high prostate specific antigen concentrations are used for screening (their figure 1). Within six years 45 tumours were detected, of which 18 were predicted by the prostate specific antigen concentration; within 10 years 101 tumours presented, but only 22 were predicted. Thus, of the 56 tumours presenting between six and 10 years, only four (sensitivity 7%) were predicted by a prostate specific antigen concentration ≥ 12 multiples of the median (table III). Data from men presenting beyond 10 years (164 men) are not given; the sensitivity must have been worse. These data do not support the contention that measurement of prostate specific antigen is "good enough to justify a trial of screening": even if it were, screening should not be performed until trials have shown radical treatment to be better than watchful waiting.

Table III shows that the sensitivity for detection of tumours presenting between six and 10 years was 7% (4/56) for > 12 multiples of the median, 16% (9/56) for > 8 multiples of the median, and 27% (15/56) for > 4 multiples of the median; increased sensitivity has the price of an increased number of false positive results. The variance among centres in median control prostate specific antigen concentrations (0.47-0.75 ng/ml) is worrying. Given the retrospective nature of data collection on cancers, tumours must have been missed, and the assumption that all cancers that presented clinically were important is false because some would have been unsuspected cancers detected after transurethral prostatectomy (many of which are low stage).

By concentrating on men with high prostate specific antigen concentrations presenting soon

after testing, the authors have promoted a misleadingly optimistic view of prostate specific antigen testing.

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- 1 Parkes C, Wald NJ, Murphy P, George L, Watt HC, Kirby R, *et al.* Prospective observational study to assess value of prostate specific antigen as screening test for prostate cancer. *BMJ* 1995;311:1340-3. (18 November.)
- 2 Catalona WJ, Richie JP, Ahmann FR, Hudson MA, Scardino PT, Flanigan RC, *et al.* Comparison of digital rectal examination and serum prostate specific antigen in the early detection of prostate cancer: results of a multicentre clinical trial of 6630 men. *J Urol* 1994;151:1283-90.

Trials of treatment are needed before trials of screening

EDITOR,—Considerable uncertainty still exists in the urological community about the importance and the optimal management of localised carcinoma of the prostate. The options are watchful waiting, radiotherapy, and radical prostatectomy. The benefits of radical prostatectomy have not been proved.¹ There is an associated mortality, and complications include impotence and incontinence. Radiotherapy offers no advantage over watchful waiting.² Screening is not benign and is associated with psychological morbidity.³ Is it therefore ethical to instigate a trial of screening, as suggested by Carol Parkes and colleagues,⁴ when the optimal management of this disease is uncertain?

Systematic reviews of the available evidence comparing various modalities of treatment with conservative management have found the data inadequate for valid comparisons.⁵ If the basic tenet of screening is to be fulfilled there must be an effective treatment that can reduce mortality related to the disease. Otherwise the use of screening may merely increase lead and length time bias. Randomised studies comparing the most promising treatment with conservative management are required before a trial of screening is undertaken.

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- 1 Graverson PH, Nielson KT, Gasser TC, Corle DK, Madsen PO. Radical prostatectomy versus expectant treatment in stage I and II prostate cancer. A fifteen year follow up. *Urology* 1990;36:493-8.
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- 4 Parkes C, Wald NJ, Murphy P, George L, Watt HC, Kirby R, *et al.* Prospective observational study to assess value of prostate specific antigen as screening for prostate cancer. *BMJ* 1995;311:1340-3. (18 November.)
- 5 Middleton RG, Thompson IM, Austenfeld MS, Cooner WH, Correa RJ, Gibbons RP, *et al.* Prostate cancer clinical guidelines: panel summary report on the management of clinically localized prostate cancer. *J Urol* 1995;154:2144-8.

Measurement presents technical difficulties

EDITOR,—In their evaluation of measurement of the prostate specific antigen concentration as a screening test for prostatic cancer Carol Parkes and colleagues advocate the use of values expressed as multiples of the median value in a healthy

population matched for age.¹ Currently, laboratories report actual numerical values for prostate specific antigen.

The authors do not discuss the technical difficulties in measuring the low prostate specific antigen concentrations seen in healthy men, which affect the reliability of median values. The authors used the Tandem-R prostate specific antigen assay (Hybridtech), in which the two lowest standards used for calibration are zero and 2.0 ng/ml. All their median values in normal men were < 2.0 ng/ml and therefore fall in the range of the assay that has greatest imprecision. Variability arises because of counting error and a greater proportional contribution of misclassification errors at low concentrations.

In addition, the use of different standard curve fitting techniques can lead to considerable differences between laboratories. By applying two different curve fits to a single dataset we could alter the apparent prostate specific antigen result from 0.56 to 0.67 ng/ml. Such small apparent differences become important when medians are multiplied by large numbers to produce cut off values (for example, the 12 multiples of the median recommended by Parkes and colleagues as a clinical cut off would, in our example, give values of 6.7 and 8.0 ng/ml, respectively). In addition, prostate specific antigen concentrations were reported to two decimal places, which implies a detection limit of 0.01 ng/ml. Since the manufacturers of the kit quote a detection limit of 0.1 ng/ml and since between batch imprecision of the assay in our hands is $> 3\%$ at values < 1.0 ng/ml, we suggest that it is inappropriate to report results to two significant figures.

The principle that Parkes and colleagues use is currently used in biochemical screening for Down's syndrome with assays of human chorionic gonadotrophin and α fetoprotein. For prostate specific antigen, however, the authors imply characteristics of the assay that do not reflect its actual performance. If the median data are flawed then the advantage of an approach in which concentrations are expressed as multiples of those medians should be questioned.

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Authors' reply

EDITOR,—Paul Warren suggests that we should estimate the predictive value of measurement of prostate specific antigen direct from our data. The predictive value of a positive result depends on the prevalence of the disease. Our population included a high proportion of men under 60 (median age of those with the disease 57), so an estimate of the positive predictive value in our subjects would be lower than one based on older men, in whom the disease is more common. If screening were shown to be effective then measurement of prostate specific antigen would be best offered to men over 60. That is why we based our estimates on men aged 60-74, applying the detection and false positive rates to the prevalence of the disease in this age group.

D E Neal and F C Hamdy suggest that the prediction of cancer that would present clinically over the next three years is of no value. There is no evidence of this, and that is what a trial of screening and treatment needs to determine. The result should not be prejudged. An important and surprising observation was that, of 22 cancers that