Our validation studies suggest that measurement error is not a major problem with the data on alcohol consumption. Furthermore, as none of the potential confounders other than cigarette smoking had any consistent effect on the odds ratios these other factors are unlikely to be important confounders despite inevitable measurement bias. The hypertension variable included in the analyses related to drug treatment, not the actual blood pressure, and was included because people being treated for hypertension may have changed their alcohol consumption after the hypertension was diagnosed. In this study controlling for antihypertensive treatment had no consistent effect on the odds ratios. There is a strong social class association with cigarette smoking in New Zealand,⁴ and this, not selection bias, explains most of the social class association with coronary heart disease in our study.

Professor A G Shaper and colleagues suggest that changes in alcohol consumption may have occurred before the period we specifically examined. This is irrelevant because we categorised current non-drinkers on the basis of lifetime drinking behaviour. The reliability of case-control studies depends on the design and conduct of the specific study. The validation studies we conducted were more extensive than those in most previous reports. Furthermore, despite the potential susceptibility of case-control studies to many biases they have the advantage of assessing both exposure and confounders close to the time of the outcome of interest and therefore may provide more accurate information than cohort studies entailing assessment of exposure often years before a disease event occurs.

The issue of "more radical" exclusion criteria for prevalent cardiovascular disease is also dealt with in our paper. We report additional analyses restricted to people without current self reported medical problems, which show the same results. It would be inappropriate to evaluate the data presented in tables II and III statistically as these are raw figures. The statement that a complete lack of a dose response is biologically implausible is not only a weak argument but also false. We suggest a biologically plausible mechanism for the relation observed, although the study did not specifically examine biological mechanisms. As most of the participants drinking less than four units a week drank more than once a week the combination of a small high density lipoprotein effect and a haemostatic effect could account for the protective association observed in this group.

Finally, our findings are consistent with those of most cohort studies that have examined this issue.^{1,3} The extremely high proportion of former drinkers in the non-drinking group in the British regional heart study conducted by Shaper *et al*⁴ suggests that their findings cannot be generalised. The weight of evidence suggests that the migration theory proposed by Shaper *et al* is wrong.

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Creutzfeldt-Jakob disease and blood transfusion

SIR,—Since 1986 people who have received pituitary extracts have been excluded from donating blood on the basis of a "theoretically low" risk of transmitting Creutzfeldt-Jakob disease.¹ A similar low risk would apply to blood donations made by immigrants from Asia and Africa if they were incubating the disease after acquiring it from sheep brain rabies vaccine.

In Asia and Africa vast numbers of people continue to receive sheep brain rabies vaccine as a therapeutic agent after exposure to a rabid animal, even though scrapie has been established in sheep flocks for a considerable time. In the mountain sheep of the Kumaon foothills in the Himalayas, for example, scrapie was established almost four decades ago and 1-10% of sheep flocks were afflicted by the disease.² In the Himalayan foothills themselves the Central Research Institute continues to produce four million to five million doses of sheep brain vaccine annually. Transmission of abnormal prion protein in sheep brain vaccine might have occurred in some of the 14 documented cases of Creutzfeldt-Jakob disease in different regions in India.^{3,4}

Before such immigrants donate blood they should be asked if they have been exposed to a rabid animal and immunised with sheep brain rabies vaccine. In addition, an indirect assessment should be possible through assay of rabies specific antibodies or the persisting cell mediated immunity during exposure of lymphocytes to rabies antigen.

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Controlled trials in single subjects

SIR,—Dr Terje Johannessen presents a persuasive, yet in places contradictory, argument in favour of controlled trials in individual patients (the so called *n* of 1 trials).¹ Certainly, a beneficial effect of treatment found in a conventional "*n* of many" trial is only an average effect, and there is nothing to suggest that all patients did (or will) benefit. Dr Johannessen attempts to show us the attributes of *n* of 1 trials, rightly extolling all their virtues for the care of individual patients, and then suggests that a well controlled series of such trials may lead to conclusions that can be "generalised to the target population."

Surely this returns us to the problem for which *n* of 1 trials are supposed to be superior to conventional trials: how will a series of trials in which different treatments emerge as optimal for different patients give us any better information on how to treat the next patient than that on average (or in general) this treatment did better than that treatment? Given that no more solid a statement can be made concerning the next patient by using a series of *n* of 1 trials, we must accept, as Dr John A Lewis points out, that it must be more efficient to plan that series in a well coordinated way.²

Another contentious issue with *n* of 1 trials is how many constitute a series. We are all concerned to have predefined plans and agreed sample sizes. Even interim analyses and sequential trials, though allowing for repeated looks at the data, do so in a

controlled manner. *N* of 1 trials (should) have prespecified numbers of randomisation periods, thus maintaining the same element of control. So when, after a series of *n* of 1 trials, will we be prepared to look at them all and make decisions on policy? I am concerned that in most cases it may be when a statistically significant benefit of treatment has been shown, without any regard for the inevitable repeated looks at the data that may have (informally) taken place.

Blinding in clinical trials is a continuous requirement that goes well beyond the treatment of an individual patient. No one would suggest "unblinding" in each case at the end of the allotted period of treatment, for this would probably influence the recruitment of future patients. Unblinding should take place only at the end of the trial.^{3,5} The essential link between samples and populations is a weak issue in clinical trials. The criteria for inclusion and exclusion define the population; the sample of that population that is recruited is rather closer to a haphazard than a random sample. If results for each patient are disclosed as they become available this will surely affect which patients get recruited so that not only will we be missing the random selection but we will not even know to which population the results apply.

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Investigations: getting from guidelines to protocols

SIR,—The impetus for the protocols mentioned in the editorial by Dr D Jenkins seems to be the large variations in clinical practice observed in the United States.¹ A 40-fold variation between British doctors and their North American counterparts in the use of tests for the investigation of hypertensive patients² suggests that caution must be exercised in extrapolating the use of health care resources from the United States to the United Kingdom. Laboratory usage is just one aspect of wide differences in medical behaviour for which there is no single explanation.

Availability of resources may partly account for these variations. In our health care service laboratory services account for less than 4% of the total hospital expenditure; in contrast, investigations account for 25% of patient bills in American reimbursement schemes.³ Protocols, therefore, rather than curtailing laboratory demand and expenditure may increase it.

Protocols may also waste health care resources.^{4,6} One possible reason for this is that protocols are designed for the "average patient" and therefore with complicated patients the need for additional resources will be easily justified, but it is unlikely that an uncomplicated patient will consume fewer resources than those laid down by the protocol.

Protocols have been usefully deployed in the management of a few common medical conditions or the use of certain investigations.⁷ It is hard, however, to envisage written protocols encompassing all medical illness (there are 10000-20000 diagnostic and operation codes) or all tests (over 100 clinical chemistry tests in our laboratory).

Externally imposed protocols are likely to be unpopular and could lead to emotive statements

such as "You mean to say, doctor, that you think this test might help my child, but it's against the law even if I want to pay for it out of my own pocket." Locally agreed protocols are more likely to be effective, but as Dr Jenkins points out these may be based on anecdotal evidence and personal experience.

Apart from difficulties in drawing up protocols, other problems with protocols which will need to be addressed are their implementation (perhaps information technology will fulfill its promise in this respect), the long term assessment of their effectiveness, monitoring adherence, and the continuous medical effort that will be required to update them. Under whose responsibility will these fall?

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Childhood diabetes and material deprivation

SIR,—Mr Y J Crow and colleagues reported that the incidence of diabetes mellitus in children was higher in the deprived areas of northern England.¹ We have examined the relation between incidence and deprivation in Scotland (1977-83) using a measure developed by Carstairs and Morris² and taking account of urban-rural differences in incidence.^{3,4} Our results will be published elsewhere,⁵ but we have reanalysed the data to present them in the same format as table II in the paper by Crow and colleagues.

Our results are contrary to the findings of the Newcastle team—the incidence of diabetes in Scottish children has decreased in the most deprived areas. However, this finding was confined to the urban areas, and there was little relation with deprivation in rural areas. One explanation which we have postulated is that Carstairs and Morris's score underestimates deprivation in rural areas.⁶

There were only minor methodological differences between the studies. The deprivation score we used has three factors in common with the Townsend index (unemployment, car ownership, and overcrowding) but the fourth factor in Carstairs and Morris's score is low social class, whereas Townsend's fourth factor is home ownership. We used postcode sector as the geographical unit whereas Mr Crow and colleagues used enumeration

district. It is unlikely that these differences can explain the contrasting findings. Both studies were large, and both have validated the completeness of ascertainment.

It would be useful if Mr Crow and colleagues could assign urban or rural codes to each enumeration district to see if the relation with deprivation was consistent in urban and rural areas.

Other studies that have examined the relation between incidence and social class have produced conflicting results. One small British study supported the findings in Scotland, but another produced equivocal results.^{6,7} A recent Swedish study reported findings in keeping with those of the Newcastle group.⁸

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Deprivation indices

SIR,—There has been much discussion recently on the relative merits of different deprivation indices. Three such indices—the underprivileged area score,¹ the Townsend index,² and the Carstairs index³—are similar in that they are all based on a combination of several socioeconomic variables, taken from the 1981 census, which are (variously) standardised and transformed and weighted. The underprivileged area score was developed as a measure of the potential workload or pressure on the services of general practitioners; the Townsend and Carstairs indices were developed as measures of material deprivation. The Townsend and Carstairs indices each include four variables related to material deprivation; the underprivileged area score includes similar variables plus four others (elderly living alone, single parent families, mobility, and ethnic groups) that general practitioners, in a national survey, thought were also relevant to their workload or pressure on their services.

The Townsend and Carstairs indices seem to correlate more strongly with standardised mortality ratios than the underprivileged area score,⁴ which correlates more strongly with infant mortality.⁵ For the electoral wards of England and Wales all three indices intercorrelate strongly

(correlation coefficients of about 0.85): about three quarters of wards that are more than one standard deviation above the mean of the underprivileged area score and the Townsend index are common to both indices.

We consider it to be important that areas of deprivation and of increased workload or pressure on the services of general practitioners should be identified to help concentrate limited resources for health (and other) services in these areas. That is why we are concerned to point to the common conclusions that emerge from the use of all three indices. As statisticians are never likely to agree among themselves on the structure and derivation of the perfect index endless discussion of the differences among the indices is unlikely to be productive. These measures may be refined further when additional data become available on a uniform basis. In the mean time more effort should be put into using the indices for the purposes for which they were developed and less into dissecting out the fairly small differences among them.

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Psychological therapy in the NHS

SIR,—The issue of who should provide psychotherapy or counselling in its various forms¹ is less urgent at present than the issue of who should provide the resources for it.

Training in communication skills, which would include counselling, should be part of the training of every doctor because such skills are an essential element of everyday practice. That is a long step from suggesting that doctors should be the main providers of counselling.

The behavioural psychotherapies have been developed as clinical interventions largely by doctors, but, regrettably, most practitioners have little or no training in them. There is a powerful argument for ensuring that all doctors have some education in behavioural methods and that some, such as general practitioners and psychiatrists, have more specific training. Again, this does not imply that doctors should be the main providers.

The dynamic psychotherapies have always been practised by a small minority of practitioners, and it would be unreasonable to expect more people to provide them even if the evidence of efficacy was more persuasive. Cognitive therapy, which some regard as common sense enriched by behavioural method, is of proved value. Other talking therapies are perceived by many as effective and helpful. Knowledge of some or all of these techniques is of value to medical practitioners if for no other reason than that it increases their understanding of themselves and of the people with whom they work.

Age and sex standardised incidence of childhood diabetes in Scotland (1977-83) by categories of deprivation index

Category	Population aged under 19 (000)		No of cases		Incidence per 100 000 (95% confidence interval)
	Boys	Girls	Boys	Girls	
1 (most deprived)	122.3	117.9	139	129	15.8 (13.9 to 17.7)
2	122.0	116.9	159	141	17.9 (15.8 to 19.9)
3	123.0	118.2	202	173	22.2 (20.0 to 24.5)
4	122.1	115.1	199	199	24.0 (21.7 to 26.4)
5	123.5	116.8	220	173	23.4 (21.1 to 25.8)
6 (least developed)	123.2	116.9	203	188	23.3 (21.0 to 25.6)