and one shows all forms of alcohol to be harmful. Of 10 cohort studies, only one shows a consistent significant benefit for all forms of alcohol. Ecological studies are least prone to confounding by sick non-drinkers and are the least supportive of the theory that alcohol may protect against heart disease.

We question the wisdom of publishing these papers without giving any form of public health advice. They will be perceived by the public as giving a signal to consume more alcohol.

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Authors' reply

EDITOR,—The proposal that alcohol has a protective effect on the risk of ischaemic heart disease no longer has the charm of novelty. Frontline research should now attempt to identify those people who benefit from drinking alcohol and those who do not.

We investigated the interplay between alcohol consumption, serum low density lipoprotein cholesterol concentration, and risk of ischaemic heart disease. The strength of the association between alcohol and risk was highly dependent on the low density lipoprotein cholesterol concentration: the higher the concentration the stronger the inverse association. There was no association among men with a low low density lipoprotein cholesterol concentration (defined as those with concentrations in the lowest fifth, < 3.63 mmol/l).

For obvious reasons, studies on alcohol and risk must be natural experiments. Accordingly, it is not possible to conclude whether associations are causal. Suggestive of causality are a set of criteria accounted for in our study, such as temporality, strength of association, and biological plausibility.^{1,2}

The possibility of bias also has to be considered. Non-drinkers were older than drinkers. In regression analyses we took into account the possibility of a non-linear relation between age and risk. Including age as a continuous variable, a dichotomous variable, or several five year age group dummy variables had no influence on the results. Non-drinking was not simply a proxy for old age, suggesting increased risk. A bias caused by "sick non-starters" seems unlikely. We excluded all men with overt cardiovascular disease at the baseline, and relevant electrocardiographic changes were equally distributed among drinkers and abstainers. We discussed other potentially biasing factors in our paper and a previous one.³

The *BMJ* is not a health magazine for the public. In our paper we did not attempt to deviate from the hypothesis advanced. We did not discuss the well known potential risks and side effects of alcohol consumption since public health advice was not the issue. In a liberal society with a free press it is generally not possible to prevent the public from misinterpreting scientific results.

In his editorial Ian R White claims that our hypothesis has been tested before.⁴ No previous study has analysed the interaction of alcohol, low

density lipoprotein cholesterol concentration, and risk, presenting data on the association between alcohol and risk stratified by different levels of lipid concentrations. Another claim is that our findings could be the result of chance. That is possible. We agree that the results of one epidemiological study can never stand alone.

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Review of interventions should help to reduce inequalities in health

EDITOR,—Graham C M Watt¹ accuses us of being perverse in our review of interventions that health services could use to help reduce inequalities in health.² We made clear, as he does, that socioeconomic factors are likely to have more influence on variations in health status than are health services. That does not mean, however, that aspects of health services are not influential in widening inequalities or that the resources of the NHS cannot be used to help reduce them.

Our review and its subsequent extensions³ identified over 100 studies evaluating ways in which health services alone or in collaboration with other agencies could make some positive contribution in this area. For example, a rigorous study of intensive control of blood pressure showed that it was effective in reducing social class differences in mortality,⁴ and a variety of health, social, and nutritional interventions aimed at the general population or targeted at low income groups were shown to improve children's health.

Watt's other criticism—that "evidence based medicine from randomised controlled trials is least likely to emerge from areas of social deprivation"—is unclear. If he means that trials of treatments do not usually include deprived populations and so their results cannot be applied, this would imply denying these populations access to treatments of generally proved effectiveness, so increasing inequality. If he means that such trials are not appropriate for assessing methods for reducing inequalities in health, we would counter that a number of health care and social interventions have been evaluated with randomised controlled trials, which have provided important and reliable evidence that can be used by policymakers. For example, a trial of negative income tax showed a large and significant effect on birth weight.⁵ Our reviews, however, did not confine themselves to randomised controlled trials but systematically identified relevant experimental and quasiexperimental studies and summarised the results of major observational studies.^{2,3}

Watt makes the unstartling observation that we need to tackle social deprivation. If we concentrate solely on macro policy there is the danger that NHS decision makers will be let off the hook and social justice allowed to slip off their agenda. Our review provides a useful sum-

mary of potentially effective interventions that may fall within the scope of the NHS. While this approach might not have such political cachet as that adopted by Watt, we think that disadvantaged populations have a right to benefit from the application of the results.

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Time for a ban on landmines

Workload resulting from landmine injuries is huge burden on hospitals

EDITOR,—One aspect of the epidemic of antipersonnel landmines¹ that is often overlooked is the implication for the indigenous surgical services of the constant flow of injuries caused by the mines.² The countries affected are the ones least able to help themselves, with surgical provisions already stretched to the limit. Few figures exist on the epidemiology of landmine injuries and the workload these impose on local services, as most of the data have been gleaned from Red Cross hospitals dealing solely with war injuries.³

Last year, while working in Angola, I conducted an audit of the surgical admissions to Kuito General Hospital. Of 795 admissions over 10 months, 211 were due to war-type injuries inflicted on civilians. Landmine injuries accounted for 82 of these. Most of the rest were gunshot wounds. War injuries, whether caused by landmines or high velocity rounds, use a disproportionately large amount of surgical time and resources in comparison with civilian-type trauma.⁴ Each injury frequently requires two or three operations. The patients with war injuries who were admitted accounted for 42% of the general and orthopaedic operations carried out during the study period (268/643). The consequences of such an influx of patients for a British hospital are almost unthinkable.

Such a large burden of work detracts from the ability of local surgeons to care for patients with non-traumatic conditions. In addition, the resulting large population of young people with amputated limbs becomes an economic burden on the society as a whole: a one legged man cannot feed his family, and a one legged woman cannot look after her children. In Kuito an average of six people a month lose their legs because of landmine injuries. Such an incidence would tax the resources of a district general hospital in Britain; the effect on Kuito is disastrous.

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