With regard to the comments of Dr R Ahmad and coworkers (7 February, p 372) it is interesting that their patients undergoing carpal tunnel release did not have arthropathy affecting large joints. This suggests that the carpal tunnel syndrome, though often seen in association with the arthropathy that affects large joints, may have a different pathogenesis. It is therefore perhaps not surprising that synovial iron deposits were rare in their patients. Amyloid deposits may be a factor in the carpal tunnel syndromes, and some patients with amyloidosis associated with plasma cell dyscrasias are known to develop this complication.[3]

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## Empirical evidence and authoritarian ethicists

SIR,—Dr Iain Chalmers's letter (24 January, p 247) is a perfect example of the way in which some doctors accept lower academic standards for ethical than for scientific matters.

Dr Chalmers has spent years in promoting the proper scientific study of various perinatal issues and in criticising those research projects that he considers not to have been correctly performed or interpreted. As soon as I question a couple of points about a comparison of two ways to obtain informed consent, however, he becomes sad at my attempt to "discredit" the comparison, apparently because the comparison was one of a "vanishingly small" number of studies collecting empirical evidence about the information needed for consent. He goes on, however, to mention a review of such empirical evidence recently published by *IME Bulletin*,[1] which considers 30 studies on this subject. Were Dr Chalmers to review 30 studies in one particular area of perinatal epidemiology I suspect that he might complain that the subject had been overresearched, rather than saying the number was "vanishingly small." Dr Chalmers next criticises me for not endorsing the call of the review's author for more research. One of my editorial changes to Dr King's published paper was to draw attention clearly to the problems for which more research is needed.

At the Institute of Medical Ethics we are trying to promote high academic standards in the study of medical ethics. If that requires the questioning of the validity of empirical studies then such questioning will continue. And, to revert to my criticism that prompted Dr Chalmers's letter, I do still find it pretty odd to enter patients into a trial without their knowledge or consent in order to assess the extent to which one should respect their autonomy.

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**This correspondence is now closed.—ED, *BMJ*.

## Poverty and teenage pregnancy

SIR,—Susan Williams and colleagues have shown the importance of poverty among teenage mothers in the east end of Glasgow (3 January, p 20) and conclude that social inequalities do not seem to have reduced and that inequalities in health will persist as long as such disadvantage continues. We have further evidence of this trend in the results from two national samples of births in France in 1972 (11 254 births) and 1981 (5508 births).[1]

In both 1972 and 1981 pregnant women aged under 20 were significantly underprivileged compared with older women.[2] Moreover, the social circumstances of teenagers deteriorated during these 10 years: the proportion of pregnant women aged under 20 who were not living with their child's father was 11% in 1972 and 18% in 1981, and the proportion of those who were unemployed during pregnancy increased from 42% to 72%. The relative "marginalisation" of pregnant women aged under 20 may result not only from the economic crisis but also from the unequal distribution of reliable means of birth control. In 1972 very few were available in France; by 1981 the pill was covered by national insurance and available to minors without parental consent, and termination of pregnancy was legal. Nevertheless, access to effective contraception or legal abortion remained very difficult for economically and socially underprivileged women.

During these 10 years the take up of antenatal care increased among all pregnant women, whatever their age, but in 1981 the proportion of women who had fewer than four antenatal visits remained higher among women aged under 20 than among other women. In 1981 the preterm delivery rate was 10·5% among women aged under 20, very similar to the rate observed in 1972, and 5·3% among older women (8·0% in 1972). The relative risk of preterm delivery in young women increased from 1·4 in 1972 to 2·1 in 1981.

The number of teenage pregnant women is decreasing, but the results show that they still need special support, financial help, and medical care.

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## Depression and outcome in acute myocardial infarction

SIR,—Dr P H Silverstone writes "Clearly, depression in the first 24 hours after myocardial infarction represents a considerably increased risk of early death..." (24 January, p 219). The interactions between mental state and the various manifestations of coronary artery disease are interesting but far from clear, and although Dr Silverstone shows an association between depression and impending death, it is difficult to be sure which is cause and which effect. Aware of this dilemma, Dr Silverstone gives spot measurements of enzyme activities as evidence of similarity of infarct size in the survivors and non-survivors. The mean aspartate transaminase activity in one group was given as 202 IU/l, with a standard deviation of 378, which illustrates one of the pitfalls associated with the presentation of non-Gaussian data and casts suspicion on the statistical inference drawn. Spot assays are for diagnosis and relate poorly to infarct size, and in any case survival depends on how much cardiac function remains.

Psychiatric measurements must also be used properly. The Montgomery-Asberg rating scale was not designed for use in a coronary care unit, within 24 hours of infarction, and may not be robust enough to withstand these circumstances. Surely the simplest explanation for the relation between mood and death is that the lower your cardiac output the worse you feel and the worse you do?

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AUTHOR'S REPLY,—Dr Janet and Mr Tom Treasure raise an interesting point regarding my original article—namely, whether the depression I describe was simply a manifestation of more severe disease. Unfortunately, I was limited by space and could not give the information they require. Firstly, the enzyme activities were in fact the highest reached in a series of four assays done on consecutive days. Enzyme activities have been shown to relate well to infarct size, but usually with creatine phosphokinase rather than aspartate aminotransferase.[1] At the hospital where this study was carried out assays for creatine phosphokinase were not available until the end of the study. The last 15 patients underwent creatine phosphokinase assays, which correlated with the aspartate aminotransferase and lactate dehydrogenase results.

There were no electrocardiographic indications that the infarcts were larger in the groups with depression and no changes on continuous cardiac monitoring or in the incidence of congestive failure, which correlate with early death.[2,3] The Montgomery-Asberg rating scale was designed to be sensitive to change, specifically for research purposes. Measurements were taken with this scale on the first day and on successive days until discharge. The patients' rating scale scores declined over this period, so that at seven days 30% of the surviving patients were depressed. This result is very similar to those of previous studies that have looked at depression in patients after infarction using other rating scales.[4]

In another study I looked at patients with acute first subarachnoid haemorrhage, acute gastrointestinal bleeding, and pulmonary embolism. In all of these groups the same relation was shown—namely, that patients with equivalent lesions who are depressed do far worse than those who are not, and that 40-50% of patients who are admitted acutely are depressed, as measured by the Montgomery-Asberg scale.

All studies of depression in the physically ill have the same difficulty in the use of rating scales. For example, poor appetite, difficulty in sleeping, and lethargy may well be due to the underlying physical problem. It is for this reason that the cut off for depression in this study was set higher than normal.

The answer to the question "Don't you feel worse because you are worse and therefore you do worse?" is that in a range of life threatening illnesses patients who are no worse but feel worse do worse.

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