possible that the "brief intervention" was quite similar to, if not identical with, "usual care." In addition, the doctors who provided brief intervention in both experimental groups also provided "usual care" to patients randomised to this condition, greatly increasing the chance of contamination.

The authors also do not provide any description of the procedure used to train doctors in the specific interventions or any description of the method used to determine their competence in performing the interventions. We cannot assume that the doctors actually provided the brief interventions the authors described. Because of these methodological problems, this study cannot be cited as an adequate test of the effectiveness of brief interventions.

Secondly, the authors' finding that the supported brief intervention procedure "significantly increased the success rate" also has to be considered with caution. As the authors note, the self report data which produced the significant differences among groups is tainted by the high failure rate on urinary cotinine testing in smokers who claimed to have stopped smoking. Validation of outcome is further tainted by the low compliance with urine cotinine testing in the supported brief intervention group. Additionally, since the authors did not ask about the use of nicotine gum at follow up, the meaning of a "failed" urine cotinine test is unclear. As the authors themselves conclude, without appropriate validation of self reported smoking status, smoking outcome results are subject to question.

Several previous studies have shown the effectiveness of doctors' counselling efforts,^{1,2} though carefully controlled studies that document the method of physician training and physician compliance with the counselling intervention are also needed. The United States National Cancer Institute is presently supporting a number of such studies. Their results are eagerly awaited.

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- 1 Russell MAH, Wilson C, Taylor C, Baker CD. Effect of general practitioners' advice against smoking. *Br Med J* 1979;ii:231-5.
- 2 Schwartz JL. *Review and evaluation of smoking cessation methods: the United States and Canada, 1978-1985*. Bethesda, Md: National Cancer Institute, US Department of Health and Human Services, 1987:50-9.

Cocktail stick injuries

SIR,—Retained cocktail sticks are found not only in wounds of the extremities (19-26 December, p 1658).

A previously fit 19 year old male student presented with a six week history of abdominal pain relieved by antacids, two weeks of right sided backache with malaise, and loss of appetite and weight. On examination of the abdomen there was slight right upper quadrant discomfort and the tip of the spleen was palpable. He was feverish (40°C) and had three splinter haemorrhages and a soft ejection systolic murmur. The white cell count was $9.7 \times 10^9/l$ (81% neutrophils) and the erythrocyte sedimentation rate 65 mm in the first hour. Blood cultures one and two days after admission yielded *Streptococcus milleri* group C. Echocardiography showed no abnormality. Abdominal ultrasound examination showed no hepatic or renal abnormality but an enlarged spleen, and in the absence of any other obvious focus of infection treatment for endocarditis was started, with 14

days of penicillin and gentamicin. Symptoms and fever resolved although the peripheral white cell count was $13.2 \times 10^9/l$ in the second week of treatment.

He was discharged home well taking oral amoxycillin but within five days was readmitted with increasingly severe right sided abdominal pain; at laparotomy a cocktail stick was found protruding through a hole in the second part of the duodenum into omentum. *Serratia marcescens* was isolated from the small amount of surrounding pus. The patient subsequently admitted that the offending foreign body had been ingested during New Year's Eve revelries some three months earlier—serving to remind us of the importance, in the investigation of febrile gastrointestinal illness, of the question, "And what have you been eating?"

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Embryo research

SIR,—Professor R M Winston is entitled to his opinion (12 December, p 1501) but must it masquerade as fact in a leading article? It is paradoxical that it should occur during a series in the same journal expounding the use of logic.

Firstly, we see yet again an unfounded, unscientific assumption that there is something different about the preimplantation human embryo—something not human, even undifferentiated. To support this argument he, for some reason, mentions the use of abortive, blatant contraceptives, the use of which was foisted on the public long before their real mode of action became known. He goes on to say that many patients would benefit from the detection of genetic defects. His examples—Lesch-Nyhan syndrome, Tay-Sachs disease in Ashkenazi Jews—actually leave one with the opposite impression—that is, that only a tiny minority of pregnant women would benefit and only those who had risk factors beforehand.

Next, Professor Winston justifies embryo experimentation in that it leads to the birth of healthy babies by in vitro fertilisation and, even more incredibly, might help to prevent miscarriages. Two thousand healthy babies by in vitro fertilisation is a drop in the lake of potentially healthy babies aborted, and a drop in the ocean if you add those prevented by contraceptives that prevent implantation. We are left with the idea that a decision banning embryo research imposed by a democratically elected government on unelected research workers, who are at best unrepresentative of the general population of Britain, is unjust. A strange meaning of the word "justice."

If the state banned embryo research it could at least claim to be representing a majority—not, as Professor Winston puts it, "the sincerely held views of a minority."

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AUTHOR'S REPLY—My leading article is, of course, perfectly appropriate as such because it reflects an important body of medical opinion, including that of the BMA.

Nor is it "unscientific." The contention that the preimplantation embryo is different from an implanted fetus is founded on biological facts. Before implantation it is indeed totally undiffer-

entiated with the potential only for becoming a baby. The evidence is that at least 40% (and possibly most) of human preimplantation embryos are lost by the time of the first menstrual period. The "human-ness" of the preimplantation embryo is something we can argue about, but are not sperm and eggs from our species (which are unprotected by law) equally human?

Mr Mallett, unlike most people, opposes blatant contraceptives; perhaps this explains his obviously deeply felt views. Apparently the low dosage pill and the intrauterine contraceptive device were "foisted on the public." Surveys in Hammersmith's family planning clinic confirm that few people worry about the mechanism of action of these contraceptives and that, after thorough explanation of how the device works, not one of our patients has discontinued this contraception for those reasons. Hardly "foisting," Mr Mallett.

There is now no doubt that embryo research will combat severe congenital disease. Of course, Tay-Sachs disease and Lesch-Nyhan syndrome are rare. Since when has affliction with a rare disease meant that that disease is unimportant to patients, their families, or their doctors? But Mr Mallett is typically selective in ignoring the article's discussion of common fatal diseases such as muscular dystrophy and cystic fibrosis—a defective gene carried by one in 20 of the population.

No, Mr Mallett, legislation banning this work would not represent the views of a majority. Most people in Britain support embryo research. In the past two years three independent national opinion polls—and four surveys by responsible television and radio programmes, too—have increasingly confirmed that the majority approve this research. But, more importantly, in a free democracy those suffering the appalling consequences of severe genetic disease and sad effects of infertility or repeated miscarriage should be allowed the benefit of research which they almost unanimously support and which is voluntarily accomplished solely as a result of their own treatment by in vitro fertilisation.

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Light on seasonal affective disorders?

SIR,—Since Drs Melanie Abas and Declan Murphy (12 December, p 1504) admit that the mode of action of light treatment in seasonal affective disorder is unknown and that the extent (if any) of a placebo effect needs establishing one wonders what has been achieved in this subject.

They fail to comment on the possibility of deleterious side effects of intense photic radiations. For example, the work by Lewy *et al* which they quote involved daily two hourly exposures, for possibly 28 days,¹ to a luminance level of about half the threshold intensity producing photoreceptor damage in macaques in a single 12 hour session.² The traditional safety factor of 10 was not considered.

There may be important differences in safe exposures between phakic and aphakic patients because of the protection that the normal crystalline lens may provide for the retina.³ Since the density of the macular pigment appears to be correlated, if only on the basis of a cluster, with the presence of red hair, a further undocumented variable may have been included. Even the possible difference in pupillary diameter—for example, between blue and other eyes—ought to be considered.

As the difficulties of reliably determined retinal illumination (and hence the alleged therapeutic