in the first 12 months,1* a comparison of the curves beginning after year 1 may be con-
Founded by variables related to survival. A number of presumably high-risk patients in
the beta-blocker group were saved in the first year, most probably resulting in a patient population
on that treatment which was, on average, sicker than that on placebo after one year. Thus, a
benefit of the treatment probably beyond one year could be consistent with continued benefit. In addition, we do not know what would happen if the treatment were stopped at one year. Rose2 has posed the question, “Do beta-blockers merely keep the wolf from the door, and when protection is withdrawn, does he return?” We tend to conclude from the absolute difference in the number of deaths between the beta-blocker and placebo groups at the end of a trial how many lives had been prolonged. The results from the Norwegian timolol trial could imply that 54 deaths (152 minus 98) were delayed by an average of at least 17 months. Another interpretation could be that more than 150 patients were protected by timolol for an average of six months. We have no way of knowing with certainty over what period of time during the entire patient follow-up protection is provided—that is, when the door is closed on the wolf. For these reasons, it seems prudent to recommend beta-blocker treatment for survivors of a heart attack for at least three years, the duration of the trial1 which followed patients for the longest time.

CURT FURBERG

LAWRENCE FRIEDMAN

JEFFREY CUTTER

National Heart, Lung, and Blood Institute,
Bethesda, Maryland 20205, USA


Letting intrauterine devices lie

Str—We write to congratulate Dr Mary Pollock on her leading article (7 August, p 395) and to compare my experience with hers. My hospital clinic for inserting intrauterine devices started in 1963 and during the next decade a wide range of devices was explored. The popular depth of ‘fit’ was used and a device was removed for no good reason, the commonest indication being the onset of intermenstrual bleeding. Admittedly there was an occasional spontaneous fracture of a Lippes loop but removal, examination, including no suggestion that pelvic infection increased with duration of use, and the only device with which late pregnancy (over four years of use) was recorded was the Birnberg bow. I would not recommend the ‘fit’ and additional intrauterine device. I differ from Dr Pollock in that I see no reason for an annual medical examination. I believe that there is no complication which might occur after the first 12 months of which the patient would not herself be aware. She should not be afraid of the symptoms of pregnancy when they arise. I also believe that with an apparent menopause after the age of 48 the device should be removed after six months. To leave it for two years increases the risks of endometriosis and a difficult removal.

Dr Pollock does not discuss the vexed question of the copper intrauterine device. The argument for its routine removal (“changing”) is based on its alleged decreasing effectiveness, either from reduction in the available copper or from deposition of copper on its surface. In my own experience the rate of pregnancy with copper intrauterine devices parallels that with the Lippes loop and I have seen no pregnancy in the fourth, fifth, or sixth years of use, although I admit that my numbers have not been sufficiently large for a statistical evaluation. I shall be interested in the experience of others.

W G MILLS

Birmingham Maternity Hospital,
Birmingham B15 2TG

Str—In her leading article (7 August, p 395), Dr Mary Pollock considers the symptom-free woman who has been using an inert intra-
uterine device for many years and concludes that the patient should continue as she is, “Provided that she agrees to return for routine examinations including examination for cervical smears at yearly intervals.” The suggestion of yearly attendance for cervical smears clearly differs from the recent recommenda-
tions of the Department of Health and Social Security committee on gynaecological cytology.1 The committee was set up partly in order to prevent a large amount of unnecessary screening. The report certainly does not recommend yearly screening for cervical cancer in a woman such as this. The only explanation must be action of actinomyces-like organisms, which, as Dr Pollock mentions, are rare, and the value of treating asymptomatic infections is unclear.

A J ETTON-LEWIS

Charing Cross Hospital Medical School,
London WC2R 6EP


The health visitor and prevention

Str—Dr R P Snaith makes a rather arrogant assumption that health visitors are uncon-
cerned with psychiatric disorders on the grounds that one failed to mention postnatal depression in an article written within a limited number of words, an anecdotal experience, and a statement by a group of research workers (14 August, p 512).

All professions, of course, have members with particular strengths and weaknesses and could relate anecdotes. It was a health visitor who diagnosed Esther Randtzen’s postnatal depression, and I have painful memories of trying to support a young mother with severe puerperal psychosy with not being able to obtain help from the hospital psychiatrist. I have no knowledge of another profession besides my own that has the expertise to enable me to help one lone individual, however, condemns a whole profession because some of its members fail some of the people some of the time.

Dr Snaith clearly wanted to obtain publicity for the Marcé Society by his letter. It is un-
lucky that he included another profession to do so. Despite this, however, I am sure that health visitors, who are always...