data from slightly fewer women being analysed in the PARIS review than in the Cochrane review. The strength of our analysis was that the use of IPD enabled us to classify individual women more accurately as having received antiplatelets for primary prevention, and by whether they were high or low risk at trial entry, rather than having to include all women from one particular trial in an analysis as was necessary in the Cochrane aggregate data review.

Two factors might have contributed to the modest differences in data for fetal and neonatal deaths between the two reviews, as shown by M Chandiramani and colleagues. First, use of IPD allowed significant rechecking of the original data, which for some trials changed the overall result for this outcome. Second, the PARIS analyses used fetal or baby death before discharge as the definition of this outcome, rather than fetal or baby death at any time, as reported in the trials included in the Cochrane review. So, although the PARIS analysis included slightly fewer women, they had slightly more fetal or neonatal deaths. The net effect is a slight shift in both the relative risk and confidence interval. Nevertheless, the relative risks from both reviews are similar, both suggesting a modest reduction (10%) in the relative risk of baby death. Although the PARIS analysis gives a slightly more conservative estimate of benefit, which does not quite achieve significance, we believe it represents a worthwhile effect, especially when coupled with a clear reduction in the risk of preterm birth which is also an important outcome for mothers (and health-care systems).

Although it will be interesting to explore the differences between results of the PARIS and Cochrane reviews further, we believe they give the same overall message and we reiterate that we believe the PARIS review is the most reliable analysis of the effect of antiplatelets in preventing pre-eclampsia and the most robust data to discuss with women when making decisions about care.

We declare that we have no conflict of interest.

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Two cochlear implants: halving the number of recipients

In their Comment (Sept 1, p 719),1 Benjamin Wei and colleagues deal with the issue of two cochlear implants and their superiority to one. Although the mentioned advantages are appreciated, in practice more important factors are involved in making the decision.

Most patients with hearing impairment live in developing countries with limited health-care budgets and where the rate of congenital deafness is higher than the 1–2 in 1000 rate of the developed world.2 The rate is estimated to be 2–3 per 1000 in Iran, yet in the past 10 years we have inserted fewer than 2000 cochlear implants. Most patients are from lower socioeconomic groups, there is no insurance coverage for cochlear implants, and the price of an implant is about US$20 000, which has not changed significantly since the mid-1990s. Luckily, almost all implants are distributed from the government sector so patients have to pay only a third of the price. However, this is still roughly equal to the gross domestic product per head.3 Although many families might describe the result as a miracle, use of two implants could only halve the number of recipients.

On the other hand, we have seen important changes in the design of the implants every 4–5 years.4 If a patient was given two implants 10 years ago, how could he be offered a newly designed hybrid implant now? Keeping in mind that, after insertion of conventional implants, we observe a gradual change in the normal structure of the cochlea, it seems unwise to hinder further therapeutic intervention for the patient with an eye to the advent of molecular and stem-cell technologies.

At present, use of two cochlear implants should be decided with more vigilance and a holistic view in most clinical settings.

We declare that we have no conflict of interest.

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