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<th>Relative risks are inflated in published literature [2]</th>
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Abandoning treatments that you have used for years is difficult

**Editor—**Why is it so difficult to put research findings into practice, especially when a traditional treatment is shown to be of little value? Christopher Del Mar and colleagues report their meta-analysis of antibiotic treatment for children with acute otitis media.1 Three days after a general practitioner colleague brought her family to see him in an evening surgery, her 10 month old daughter had had four infections in total; the first two she had diagnosed as acute otitis media and treated with erythromycin, the third was a minor gastroenteritis, and the last was thought to be another ear infection worthy of antibiotic treatment. By the fourth day of the illness, however, when the child was brought to my surgery, a rash had developed, particularly cluing to the viral nature of the infection. The reason for the consultation was that the child’s father, having just moved from the United States, where “putting tubes in is routine,” was concerned that the child might need myotomy to prevent further ear infections and deafness. I pointed out that the evidence for benefit from this operation was weak and heard myself saying to the parents, “I think you’d do better just treating the child by simply treating each infection as it arises.” Then I remembered the paper I had just read, clearly showing that antibiotic treatment conferred no benefit in terms of the control of symptoms; and a doubling of the chance of vomiting, diarrhoea, or rashes. The mother had also read the paper but, like me, had assumed that we would go on treating her child with antibiotics for any acute ear infection. Why? While I avidly take up new treatments with proved benefits, such as the eradication of Helicobacter pylori or antiretroviral treatment in AIDS, when years of practice are overtaken and shown to be of little value it is all but impossible to switch to doing nothing. My perception of what my patients have come to expect should play a major part.

If we could remove the obstruction to implementing research which shows that a treatment can be safely abandoned, then we could reduce unnecessary side effects and consultations and save money for more effective treatments as well. It seems a crucial step. A radical suggestion would be to exclude from the NHS any treatments proved to have no benefit; patients could still have them if they wished, but it would seem reasonable to ask them to foot the bill for expensive placebo.

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**Authors’ reply**

**Editor—**As the Cochrane version of our review of antibiotics and otitis media will be continuously improved and updated, we are pleased to receive comment.1 In answer to Helle Krogh Johansen and Peter C Gatzchie, we identified eight (not six) trials of antibiotics versus no antibiotic (as shown in our Table 1). Only six of these studies reported clinically relevant outcomes. Rosenfield et al’s meta-analysis identified four trials of antibiotics versus no antibiotics and 29 comparing different antibiotics.2 Because we identified twice as many studies for the principal question and our principal outcomes were patient centred (rather than microbiological), we believed that an update was required. Only seven (not 29) trials of antibiotics versus no antibiotics showed, by 24 hours is present in only 40% of patients. By two to seven days pain is present in only 14% of patients. Because antibiotics afford no benefit at 24 hours, we calculated the number needed to treat for pain at two to seven days. We apologise for the approximation involved in using the odds ratio to estimate the number needed to treat (equals 17). The latest version of the Review Manager software (RevMan v5) allows two better ways of calculating the number needed to treat. The relative risk of 64% means a 36% less chance of having pain at two to seven days if antibiotics were used initially. Since 14.4% of children still have pain, the number needed to treat is 1/(0.14 x 0.56) = 20. Alternatively, directly estimating the risk difference in the meta-analysis gives a risk ratio of 0.038, with a corresponding number needed to treat of 1/0.038 = 26. The authors correctly point out a transposition error in our figure 1—the subtotal for deafness at three months was carried over from the first month. The smooth figure should read 38/182 (treatment) and 49/188 (control); the summary odds ratios indicated in figure 1 are correct. The authors also point out that most of the information about vomiting, diarrhoea, or rashes comes from the study by Burke et al.3 Unfortunately, only three of the eight trials report important side effects: clinical trials should report adverse as well as beneficial outcomes. Perhaps Jonathan Efron and Rhys T Nguye are right to be concerned about severe infections and the developing world. We could not distinguish different outcomes between severe and milder cases. However, a policy of proportionate antibiotic prescribing has not led to disaster in the Netherlands.4 We sympathise with Ian Hill-Smith’s dilemma, although we do not suggest that antibiotics are useless; their benefits are just rather modest. Accordingly, their use should be discretionary rather than either prohibited or mandatory.

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**Relative risks are inflated in published literature**

**Editor—**Relative risks are often reported incorrectly in medical journals. In a paper in the BMJ, Jian-Min Yuan and colleagues describe a relative risk of 3.72 for the relation between cancer of the upper aerodigestive tract and heavy drinking as a "3.7-fold increased risk"; this description is incorrect. They also describe a relative risk of 1.50 for total mortality and heavy drinking as a "50% excess risk," this description is correct. They also write that "beer drinking was associated with a significant 1.7-fold excess in risk of death from stroke;" the excess is 70%.

In an earlier paper by the same authors in JAMA there were similar problems? A relative risk of 1.6 was correctly described as a "60% greater risk," while a relative risk of 2.5 was incorrectly described as a "2.5-fold increased risk". The paper also stated that "rates in Shanghai Chinese were 2-fold to 8-fold higher than in Los Angeles whites," but the rates in Shanghai were actually two to eight times those in Los Angeles whites. Such problems were not confined to the interpretation of relative risks. The sentence "In China, the yearly per capita consump- tion of cigarettes has increased 3-fold between the 1950s and 1987, from about 500 to 1748," is incorrect; the increase is actually twofold or 200%. Pero et al, in an accompanying editorial, stated "that heart attack mortality is five times lower, and that stroke mortality is five times higher." This was a problem because the authors were referring to the ratio of 5.3 (366/69) and 1:42 (48/210) respectively.

I have found similar problems in reports by screw American, British, and Chinese authors (in alphabetical order). This problem is important when relative risks or differences of two measures are described. We have to be cautious about the confusing meaning of the suffix "fold"; n-fold is equal to n times, and is equal to n x 100%. Therefore, a relative risk of 3.5 is 2.5-fold, or 2.5 times, or a 250% increase or excess in risk, not 3.5-fold or a 350% increase or excess. I won- der how long a "36% wireless" of such inadvertent inflation of relative risks in the literature.

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