Reviewer's report

Title: Oral rehydration versus intravenous therapy for treating dehydration due to gastroenteritis in children: a meta-analysis of randomised controlled trials

Version: 1 Date: 14 November 2003

Reviewer: Ronald L Koretz

Reviewer's report:

General

1. Many of the risk differences were called “insignificant” but the confidence intervals only touched (or barely crossed) 0. Given the conclusion that the authors made, it would seem to me that there was a true difference between the two therapies favoring IVT, but that the magnitude of that difference was not clinically relevant, since it does not seem to make sense to treat 25 children with IVT to prevent having to treat one failure. (However, this is a resource issue and not one of relative therapeutic efficacy.)

2. Technical comments about the study design and execution:

   a. The authors chose to include quasirandomized trials. Were any of these studies in that category? Since such studies do tend to overestimate treatment effects, if any were present, they should do a sensitivity analysis with those studies excluded.

   b. The data were not independently extracted by two observers; rather, a second observer simply checked the first. This is a less rigorous method, as it leaves the second reviewer more susceptible to the biases of the first.

   c. Why did the authors choose to exclude studies in cholera patients? Isn’t the question the same (although the volumes may be larger)?

   d. Two of the trials had a 2:1 ratio of ORT to IVT (El-Mougi 1994 and Santosham 1982). Why was this the case? Were two different ORT groups included? If so, is there something of importance that the reader should know about?

Discretionary Revisions (which the author can choose to ignore)

1. Since the authors did break reference 24 into two separate trials (with good reason), there were really 15 trials considered.

Minor Compulsory Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. In the Results section, Treatment Failure, lines 2-3, I did not understand how the ORT/IVT failure rates of 0.5%/1.3% were derived. Simply adding up the failures in figure 2, I calculated that there were 21/777 (2.7%) in the ORT groups and 7/189 (1.0%) in the IVT groups.

2. In line 6, paragraph 1 of the Discussion, the authors note “appropriate” children. What is (are) the feature(s) of “appropriate”? Such terminology does not help clinicians in the decision making
process.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. The authors appeared to throw out a “statistical outlier” for no other reason except the results did not agree with the others. It is to be appreciated that a body of literature exists that criticizes the process of meta-analysis because the outcomes do not agree with the results of a single large trial. The outlier was a relatively large trial; perhaps it more closely reflects the truth. Rather than dismissing this study out of hand, an effort should be made to identify reason(s) why this result was different (an evaluation of heterogeneity). The authors suggest that this was because it was the only trial that included neonates. To be fair, each of the other 14 trials should be carefully assessed to detect what factor(s) is (are) unique to that trial as well, if the authors are going to exclude a trial because of the inclusion of neonates (not because of its outlier status). In other words, if the authors are going to engage in post-hoc sensitivity analysis, make it a complete one.

2. The Gremse 1995 trial does seem to be methodologically substantially different from the others, since the only patients included were those who had failed oral therapy. (This seems to me to be a more important difference than that represented by the inclusion of neonates and emphasizes the point above.)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

None