Author's response to reviews

Title: Efficacy of Helicobacter pylori eradication therapies: a single centre observational study

Authors:

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The manuscript has been revised in the light of the referees' helpful comments. No great structural changes have been made but the specific changes made in response to the referees are noted below.

Referee 1

1. The discussion has been edited and altered slightly. There are only 2 paragraphs specifically addressing the problems of PPI-C-N (page 16 para 3, page 19 para 2). It is mentioned in the 1st paragraph of the discussion and also in the conclusions. I do not regard this as overly repetitive. The two referees disagree on the prominence to be given to the problems associated with 1st line PPI-C-N therapy. Referee 1 suggests less emphasis and referee 2 more. Overall the emphasis has not been changed but a short note on the potential confounding effects of retrospective studies has been added (p14, para 3). I believe sufficient emphasis has been placed on the avoidance of PPI-C-N as can reasonably be drawn from this study.

2. The choices for penicillin-sensitive patients have been outlined on page 19.

3. Naturally I am unaware of the data awaiting publication by the author regarding 2nd line therapy after PPI-C-N failures. Clearly this is an important issue which has merited a controlled trial. As things stand there are no published data on which to base such 2nd line therapy. Page 17, para 2 has been amended to reflect the need for published randomised trials in this setting. It is gratifying that the referee's trial shows comparable results but as it has not been published, I do not feel the data can be commented upon.

4. Page 18, para 2, says there is no logical choice for 3rd line therapy after both C- and N-. This is still true as any logical choices in the situation will avoid C and N and more data are needed.

5. The point on cost-efficacy is valid, however these meta-analyses of antibiotic dose in PPI-based regimens are not strictly relevant here. I am not aware of any data at all concerning different antibiotic doses with RBC. Therefore it does not seem appropriate to discuss these cost-efficacy points within the current study. The need for cost-effectiveness studies has been addressed on page 20.

6. The parenthases on page 7 are correct.

7. Issues surrounding possible bias have been addressed (page 14-15). There was no difference in effectiveness of different regimens when prescribed by different groups (see page 15)

Referee 2

1. As this was not a randomised trial neither intention to treat or per-protocol analysis are really appropriate. No attempt to assess compliance was made so strict per-protocol analysis was not made. Patients were analysed according to overall efficacy and page 8 has been amended accordingly.

2. Further comment on the possible confounding variables have been included as well as comments on the role of studies such as this compared to randomised trials have been included (page 14-15). There
was no difference in results for different diagnosis or setting of care, noted on page 9. The problems
with analysis by prescriber are noted on page 10, para 1. I have no other specific explanation, except for
those already included as to the superiority of RBC- vs PPI therapy. This may reflect a difference
between closely monitored trials and clinical practice by generalists. The small difference in efficacy
may only be manifest outside clinical trials when compliance and other patient or doctor-related factors
come into play. This is noted on page 15.
3. The data does support the contention by de Boer (BMJ 200)) and this is noted (page 17, para 1). As
discussed above there is disagreement between the referees' concerning the emphasis to be put on the
failure of PPI-C-N. This important point has been noted already, in abstract, results, discussion and
conclusions and I feel the emphasis is correct for the study.