Reviewer's report

Title: Dose and aging effect on patients reported treatment benefit switching the first Overactive Bladder therapy with Tolterodine ER to Fesoterodine: Post-hoc analysis from an observational and retrospective study

Version: 1 Date: 27 March 2012

Reviewer: Christopher Chermansky

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BMC Urology Manuscript ID# 1540717393677686

Major Compulsory Revision:
1. The authors present a study detailing a subgroup of patients from the IMPACTA study who switched from tolterodine to fesoterodine. They present their study as a "real world" study. What does that mean? I don't know an accepted definition of a real world study. Either strike out that language throughout the entire manuscript or respond to how this study differs from the 3 already published clinical trials mentioned in the Background section (references 18, 19, and 21) showing superiority of fesoterodine over tolterodine. The reader would appreciate better clarification in the Discussion section of how this study compares to the other 3 published studies.

Minor Essential Revisions:
2. In the Background section in the 2nd paragraph and in the Discussion section in the 2nd paragraph the authors state that dose escalation has not become routine in clinical practice. I don't believe that is correct. Contemporary studies support dose escalating solifenacin and fesoterodine. Please respond and provide additional references.

3. The authors need to describe the OAB-V8 score mentioned in the Abstract and Methods sections, and they need to reference the article that best describes that score.

4. In the Results section in the paragraph titled "Switching Characteristics" the authors report differences in treatment length. What was the exact p value when comparing dose and age? p<0.05 isn't specific enough. Also the authors further mention in their Discussion that this difference in treatment length is a study limitation. Yet, they refute that statement with the very next sentence by stating that is was not a cofounding factor. Thus, is it or is it not a study limitation? If not, remove that confusing language.

5. In the Results section the authors summarize their findings in Tables 3 and 4. They states that nocturia was slightly higher in the older group (p=0.051). This wasn't statistically significant and should be removed from discussion.

6. With respect to Figure 1 legend, choose better words than “patient disposition” to describe the diagram.
7. Throughout the text the English grammar is borderline poor. I see fragments separated by semicolons throughout. Please revise the manuscript to include only complete sentences that are clear and concise.

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I serve on advisory boards for both Astellas and Allergan, and I also serve on Speakers Bureaus for both Astellas and Allergan.