Reviewer’s report

Title: Prevalence of commonly prescribed medications potentially contributing to urinary symptoms in a cohort of older patients seeking care for incontinence

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Reviewer: Qiushi Feng

Reviewer’s report:

The authors explored the prevalence of medication potentially related to Lower Urinary Tract Symptoms (LUTS) in a clinic group of incontinence patients. Due to emerging evidence for the association between the use of some certain medications and LUTS, the study provides valuable estimations about how the LUTS-related medications are used among incontinence patients. Before I recommend for publication, however, the paper still has some main problems for major compulsory revisions, and I hope the authors to address them in their revision.

1) I think the authors did a good job to highlight the importance of examining the potential links between certain medications and LUTS in later life; however the relevance of the current paper to the major theme as mentioned is not fully justified in the manuscript. For instance, the authors’ main criticism on the current literature, i.e. “Few data derive from clinical trials (line 88)”, seems not directly related to the current research design, since the study itself is also not a clinical trial. Moreover, although the authors rightly pointed out the limitations of the population-based research designs, to merely use a group of incontinent patients without any reference group, i.e. LUTS-free old adults, is methodologically limited as well in exploring the link between use of certain medications and LUTS. Therefore, I am expecting to see more discussions about how recruiting a group of incontinence elders in a clinical setting is methodologically necessary or advantaged to explore the potential link between medication and LUTS? Last, the authors had set two additional aims in the paper. One goal is to examine the medication class and severity/type of urinary incontinence, which is different from onset/execration of LUTS as the focus on the introduction. Nevertheless, I did not see any discussions about the mechanism through which the medication class specifically affect severity/type of urinary incontinence.

2) The data and method applied in this study are sometimes problematic. In modeling the medication class against types of urinary symptoms/incontinence, the author did not specify whether the models had been adjusted or not. More important, the authors took the level of severity as a stratifying variable, as was not fully justified in either the introduction or the analytical strategy. Why was severity level not considered as a dependent variable as had been suggested in the introduction? What is the rationale to stratify those patients by severity level and then examine the association between medical class and LUTS? Moreover, in the part of measurement, the authors indeed mentioned they collected the
frequency, daily dose, and treatment duration for each prescribed medication; however there were no attempts in the paper to describe these important variables and even to incorporate them into the models. Some details about the research design and measurements are not clear as well. For example, how many clinics are involved in the study? how many participants rejected to participate? how many participants were excluded due to missing data? 
and are the self-reported comorbidities based on previous medical diagnosis or not? Last, as high as 91% patients in the dataset was female. Considering the age range of the group was not too old, I doubt the female gender was overrepresented in the sample, and would like to ask the authors to explain possible reason and discuss about the potential bias due to this problem.

3) Some results presented in the study were not impressive, and their implications were not fully discussed. For example, polypharmacy was found out as the major factor related to the use of LUTS-related medications; however this variable is highly associated with multimorbidity as the authors had realized (Line 310-311) and in practice, it is hard to consider it as a valid “predictor” for the use of LUTS-related medication. Therefore I did not see the paper provided valid “predictors” as the one main pursuit in this study. Moreover, the results for the association between use of LUTS-related medication and the severity/type of urinary incontinence was not reported in any tables, since most of the results are not significant. And the authors seemed to consider that the association was “subtle”, and the current methods are not “sufficiently sophisticated” enough to capture. I would like to encourage the authors to be more specific in discussing these points. At least I hope the authors could clearly point out how current study may contribute to the field by presenting similar results with large epidemiological studies. For the only significant association found in the study, i.e. the use of benzodiazepines and mixed incontinence in patients with moderate to severe incontinence, the authors’ comments were also sketchy, and more efforts should be made to improve. In fact, I will suggest dropping the part about medication discontinuation which is not so related to the topic, and focusing more on the above issues.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests