Author's response to reviews

Title: Chronic benzodiazepine use for insomnia in patients 60 years of age and older: discordance of patient and physician perceptions

Authors:

Leevin Mah (leevin.mah@utoronto.ca)
Ross E.G. Upshur (rupshur@idirect.com)

Version: 3 Date: 7 May 2002

PDF covering letter
May 1, 2002

Dr. Claire Colett, PhD
Assistant Editor
BioMed Central

Re: Manuscript “Chronic benzodiazepine use for insomnia in the elderly: a survey of patient and physician perceptions”

Dear Dr. Colett:

Thank you for your consideration of our manuscript entitled “Chronic benzodiazepine use for insomnia in the elderly: a survey of patient and physician perceptions:” We would also like to thank reviewers Chris Butler and Fiona Stephenson for their thorough reading and constructive comments. This letter will summarize the changes that we have made in response to these comments, and we hope that therefore this manuscript is now acceptable for publication.

With respect to Dr. Butler’s comments, there is a lack of clarity as the advice on publication is “accept after discretionary revisions”, but there are both discretionary and compulsory revisions requested. We have tried to respond to all requests for revision. I will start with Dr Butler’s discretionary revisions.

Comment No. 1

Dr. Butler suggests that we change the use of chronic to long term and we have made these changes from chronic to long term, and the exact time frame, short term becomes long term, in our definition, after one year. He suggested that we insert in the abstract the numbers of each category of clinician. We have declined to do this as it is stated clearly in the body of the text and we do not think it adds to the abstract to have this information for the time being. We have changed it from being considered an observational study to a cross-sectional study. We have also accepted his suggestion to reconsider the use of the term “elderly”. We have now changed it to older patients or people age 60 and over, so this now coheres to the actual age group of the patient population. We have made it clear that we are attempting to quantify the discordance between patient and practitioner views, rather than simply identify this discordance. We have also taken up the suggestion to use the terminology “renewed a prescription for benzodiazapine”. We changed Table 1 as per his suggestions as well. We have related our findings to the broader issues of discordance, particularly the work of Nicky Britten. We have also added a section discussing shared and collaborative decision making, and cited a recent paper by Elwyn and Edwards, and thank Professor Butler for bringing this to our attention. In terms of
his compulsory revisions, we have included information on how the survey instruments items were pre-tested. This was more for clarity and face validity rather than for any construct validity. We have more fully indicated that we believe that there is a clinical importance to the results that we have found, and this is now tied more closely to the issues with respect to concordance and shared decision making. This is now found in the discussion.

Dr. Butler asks us about the evidence for tachyphylaxis and adverse effects of benzodiazepine withdrawal. A literature search on tachyphylaxis and benzodiazepines in Medline found no citations. The issue of benzodiazepine withdrawal has been addressed recently in a series of papers in a clinical pharmacology journal, but they were all for anxiety and obsessive compulsive disorder in benzodiazapine use, not for insomnia in the elderly. Therefore, the physician’s warnings remain based on the studies we cited in the introduction to the study. There are, in fact, no prospective cohort studies. Most of the studies are retrospective using administrative databases. These are cited in the introduction. We have taken up his suggestion to add into the limitations about patients fear of withdrawal. I think this is a good point and thank him again for suggesting it.

We have also addressed the issue of why we chose a quantitative survey and not a qualitative study. The reason is that we set out to identify and quantify the discordance. One cannot presume that a discordance did exist between patient/physician perceptions without first characterizing this. That is why we suggest in our conclusion that the results of our study do indicate a discordance and that this should be followed up with further qualitative studies to explore these reasons with physicians and patients. He also asks for how long the respondents were taking benzodiazepines. We agree this is a crucial piece of information and regret our excluding it. This is now part of the inclusion criteria in the methods section. We have also polished up the language as much as possible and given it a thorough revision. We hope this tightens up the language and makes the flow of the sentences better.

Comment No. 2

With respect to Fiona Stephenson, she has two discretionary revisions. The first point is a long one in which she asks, would encouraging discussion mean in practice the imposition of physician opinion over the patient’s. We think this is not the case and again, following Professor Butler’s good suggestion, we have now linked the discordance to the need for shared decision making and moving towards concordance, which indicates equal partnership and management decisions between patient and physician. We hope this meets this requirement.

In terms of limitations, she states that the study does not explore the reason for the discrepancy between the patients but they have not considered the effect of this discrepancy. We beg to differ with Professor Stephenson on this point, as on page 8 and
In the discussion, we discuss some of the possible reasons for the discrepancy. It must be stated that the deeper reasons for the discrepancy between patient and physician discordance remain unknown at this time. However, we need to proceed in a stepwise fashion, first identifying whether there is a discrepancy and then moving on through the use of qualitative methodology to further explore the reasons for this, and that is what we have recommended in our conclusions. Again, we thank the peer reviewers for their thoughtful and perceptive comments. They have greatly added to the paper and I believe, strengthened it. We hope that these revisions meet your needs satisfactorily and look forward to hearing from you with respect to your decision whether to accept this manuscript or not.

Sincerely yours,

Ross E.G. Upshur, BA (Hons), MA, MD, MSc, FRCPC and Leevin Mah MD, CCFP

Department of Family and Community Medicine, Primary Care Research Unit
Sunnybrook and Women’s College Health Sciences Centre
University of Toronto