Author's response to reviews

Title: An examination of the temporal and geographical patterns of psychiatric emergency service use by multiple visits patients as a means for their early detection

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Author's response to reviews: see over
To the Editorial Staff,

BMC – Psychiatry,


Thank you for reviewing my manuscript entitled ‘An examination of the temporal and geographical patterns of psychiatric emergency service use by multiple visits patients as a means for their early detection’ as well as for giving me the opportunity to respond to the reviewer’s comments and suggestions. Overall, the reviewers made many comments and queries that required extensive modifications to the text and, at least in one instance, required the addition of data not present in the first manuscript.

Overall, the queries by both reviewers have, in my opinion, substantially enhanced the quality of the manuscript, that I am returning to you today. I will begin with the major points and comments made by the two reviewers.

**Dr Bruffaerts:**

Major point 1 - Only the number of visits were studied in this study. Other patterns of use should be included, such as outpatient visits or parameters that might adversely affect continuity of care, as these might also contribute to the ‘multiple visit’/ revolving door phenomenon.

Response; Upon reading this comment and re-reading the manuscript it is obvious that Dr Bruffaerts makes quite a valid point. I have attempted to address this issue in several ways. The first is to modify the title of the manuscript. It is now clearly indicated that we are studying ‘temporal’ and ‘geographical’ patterns of emergency service use, not any kind of pattern. The introduction has also been modified to better reflect this, as well as the discussion and the conclusion. The modified introduction also abodes the major point made by Dr Curran, about the absence of preamble to the ‘multi-center’ trial part of this study. This is now more fully detailed and explained.

Second, I have re-introduced the concept touched upon in my Psychiatric Services article that, at least at the very high end of the frequent user spectrum, continuity of care may be neither cause nor solution. I have introduced this point by restating data in the methods section (page 8, first paragraph) that heavy PES users were already under multidisciplinary outpatient care. These multidisciplinary teams were responsible for a patient’s outpatient and inpatient care. They were also responsible for the liaison with community and government resources. This is restated in the conclusion section. In addition, in the methods section on page 7, second paragraph, there is a brief mention that each PES in Montreal possesses a ‘sector’ (catchment area) whereby, depending on a citizen’s address, he/she must seek acute care at a specific PES in a specific hospital only. This situation I believe is similar to that in Belgium. It is designed to promote continuity of care.
As an aside, these teams included the gamut of psychiatric and psychiatrically oriented professionals (psychiatrist, psychologist, social worker, nurse clinician, residents and students). They were also often in contact with community and government agencies with regards to a particular patient. They were also responsible for inpatient care when hospitalization was required. I directed the 8-bed observation unit at the main PES site from the early 90s until 2000-2001, during my stay at the University of Montreal. To my recollection it was not infrequent that these teams requested (and obtained) the right to distribute a patient’s monthly government social assistance allowance in order to maintain patient contact.

Major point 2-
The validity and reliability of the assessments made in the PES.
Response; There is now a complete paragraph dedicated to this subject in the methods section (Page 7, last paragraph), fully detailing the procedures used in order to minimize diagnostic uncertainty.

Major point 3-
The question of triage in the Er.
Response; There is now a complete paragraph dedicated to this subject in the methods section (Page 7, second paragraph), fully detailing the procedures used in order to minimize diagnostic uncertainty.

Major point 4-
Clinical and policy implications.
Response; The conclusion section has been rewritten with this comment in mind. In addition, I have added a paragraph in the results section (page 13, first paragraph) in order to increase this manuscript’s clinical pertinence. This paragraph deals with the subject of the ‘time to the first cluster’. I have gone back to the database and, for a selected group of multiple visit patients, calculated the time (in days) to the completion of the first cluster. The time was substantially shorter that the total time each patient took to complete all visits, dovetailing with the concept of early detection. This is also mentioned in the abstract and in the discussion sections.

I would have liked to include all multiple visit patients per visit count in this analysis but this would have required writing many algorithms to traverse the 55 thousand record database, each algorithm having to be checked for validity. With the review timeline this was not realistic. Using three examples at the frequent user extremes (patients making 4 visits, those with 10 and those with 40 visits or more) essentially accomplished the same goal.

Major point 5-
More information required with regards the statistical procedures.
Response; The statistics section is now more detailed and elaborate. It was not explicit as most demographic, social and socio economic data (per
multiple visit group) was detailed elsewhere. Pertinent differences (in relation to being in either of the multiple visit groups) were detailed in another publication.

Minor point 1-
The meaning of the shading in table 1.
Response; This has been corrected as per the reviewer’s suggestion, both in the text (results section) and in the legend to the table. NB, The shading had to go as per BMC Psychiatry’s requirements. Shading was replaced by formatting the cell (not nearly as elegant though).

Minor point 2-
Language corrections.
Response; The manuscript has been extensively rewritten as well as revised by an English speaking colleague

Dr Curran:

Major point 1-
The multi-center design of the trail has to be more fully explained.
Response; The rationale is now more fully explained in the background section and in the methods section. In addition, as stated above in my response to Dr Bruffaerts major point 1, the title of the manuscript has been changed to be more accurately reflect the data.

Major point 2-
Question about the division of multiple visit patients into visit clusters as being helpful. Figure 4 is difficult to understand. In addition, figures are confusing in general. Also, there is a query about the pertinence of figure 4 and of the data it holds as being in synch with the other data. A query is that this data might be deleted from the article.
Response; This point actually encompasses several major issues. The first, about the division into visit clusters. This is a crucial point that touches just about every article on frequent users in the past 20 years. Fundamentally, I agree with the reviewer. My solution is by no means the best one. This particular point, in fact, this subject area, is in dire need of standardization. What this article provides, using selected cut off points (others could have been used although none provide greater diagnostic separation) is a continuum between visit frequency and diagnostic severity, which is essentially are very simple idea. In this light I have arranged both table 1 and 4. In table I, I have collapsed the 2 to 3 visit group into the single visit group as both were used as reference points to the two multiple visit groups. This required recalculating OR for the collapsed group. Table 1 is now more streamlined and I have not calculated OR in columns.
were the total was 2% or less as this would have been quite useless. The same can be said for table 4 (hopefully) which I have further rearranged esthetically.

As to keeping this data I feel that they add to the article. I have, throughout the revised manuscript, made an effort to better integrate this data, going from title to conclusion. In addition, in line the point about division into visit clusters, these data illustrate the point that subgroups exist within these divisions (for instance, the repeat cluster group contained within the 11 or more visit group or the subgroup that visits different PESs). These subgroups are not strictly based upon visit frequency, although in a general sense there is a correlation. The identification of such subgroups might ultimately render strict frequency based cut off points less relevant.

Major point 3- The discussion needs relevance and should be more specific. A better integration between data and pertinence should be made.

Response; Part of the discussion and all of the conclusion section has been rewritten with clinical and policy pertinence in mind. I have also tried to increase the synergy between data and clinical pertinence throughout the other sections of the manuscript during the rewrite.

Major point 4- Missing OR values in table 1.

Response; The OR values are in the legend to the figure as the meaning of the shaded areas within the individual cells. The definition is slightly altered from the previous version of the manuscript. NB, shading had to go due to BMC-Psychiatry’s formatting requirement. It was replaced by formatting the cell.

Minor point 1- Meaning of shaded area.

Response; This has now been addressed.

In the hope that this revision will meet with your satisfaction,

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