Reviewer's report

Title: A quasi-experimental test of an intervention to increase the use of thiazide-based treatment regimens for people with hypertension

Version: 1 Date: 14 July 2006

Reviewer: Martin Lee

Reviewer's report:

General

I will comment strictly on the statistical methods used. The use of cusum charts is a novel and interesting approach to the evaluation of their time series data. However, there are a few issues concerning this approach that I feel need to be addressed before I can be sure of the validity of their findings. First, given the apparent multiple evaluations of the data through the monthly determination of the cusum or the proportions, isn't there a problem with Type 1 error rates in this analysis? I realize in quality control applications where these techniques are typically used that this is not really a concern because of the use made of the charts. However, there are inferential determinations being made here for which this might an important consideration. This same comment would appear to apply to the change-point analysis. (Parenthetically, an appropriately chosen hierarchical Bayesian model would get around this problem.)

It seems that a fundamental flaw of such an approach is an inability to handle baseline covariates reflecting differences in the study groups. The authors choose to ignore these differences, but do point out that they would provide greater support for the implementation group. Because of the relatively small numerical intervention effects seen, it would be incumbent on the authors to consider this adjusted analysis in order to buttress their results. They might consider a repeated measures analysis with covariate adjustment.

The authors argue that clustering should not be in issue in at least one of the study groups, but I would like to see an evaluation of their data that actually supports their approach which ignores this potential effect.

Finally, the comparison of proportions pre and post-intervention seems oversimplistic given the time-series nature of the data. An examination of Figures 5 and 6 do not suggest a particular noticeable difference in the patterns of the outcome (blood pressure goal) post-intervention. I would be interested to see whether a time-dependent analysis demonstrates any significant difference. In this context, the large sample sizes here probably guarantee such findings anyway. The authors do argue that the small differences seen are clinically important, but were there any effect size/power calculations performed pre-study? I think that such a discussion belongs in the Methods section.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

While the results of the study are, at face value, compelling, I would like to see responses to the questions raised above before a final decision on the paper can be made.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

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Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

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