Reviewer's report

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

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Reviewer: trudy van der Weijden

Reviewer's report:

General
1. This is a large-scale evaluation of an implementation strategy that was set out to solve the relevant problem of thiazides underuse.
2. It is a sympathetic gesture to name all the members of the implementation group. A high number of authors, 16 in total (!), meet the criteria for authorship.
3. The quasi-experimental character of the trial makes the results vulnerable for selection bias, meaning that the comparability between intervention and control group may be hampered (patients assigned to GSM panels (the intervention arm) are older and sicker (p.13 + table 1), the performance for hypertension was better at baseline already in the intervention arm (p. 17). Next to this there have been contemporaneous hypertension quality improvement projects in the control arm (p.14). The test of difference between pre-post intervention change is correct, because baseline differences are taken into consideration.
4. Typically, the proportion of patients with achieved blood pressure goals is higher than the proportion of patients with prescriptions for thiazides, in both trial arms. Is this explained by other than thiazide prescriptions?
5. It is strong that the total number of person hours devoted to the study intervention was counted and is reported (p 14).
6. The high number of measurements, pre-intervention, during intervention and post-intervention is another strong aspect.

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

7. p.9: the problem is stated: "despite extensive press, the implementation of ALLHAT findings into routine clinical practice has been insubstantial". Crucial information is missing here; WHY was it insubstantial? The implementation intervention is not focused on certain barriers for change, it is a broad intervention (due to lack of insight in barriers?). Rogers diffusion model was used (relative advantage, compatibility, complexity, trialability, observability). But it is questionable whether this was sufficient. All factors in the model seem to refer to reasons for professionals to change while the real barriers might be more organisational or patient related. I do not understand the sentence "We did not explicitly address compatibility because cost-effective care is a cultural cornerstone in the VA medical care system".
8. p.12/13: I miss a clear definition of patients that were included in this study to build the denominator for the outcome. I found information at the bottom of p 14: patients with the diagnosis hypertension were identified, then patients with a possible indication for non-thiazide first-line agents were excluded. How was the target population defined exactly?
9. The potential bias stemming from the non-randomised design must be more discussed. P.22: "the direction of the potential bias caused by differences in features of professionals is harder to predict." I would put this stronger: it can be argued that the difference in professionals might have caused the difference in change between groups instead of the intervention.

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

10. abstract, p.8: "At the end of the implementation period 42% of intervention patients were taking thiazides vs 30% of controls". Do you not mean: ... were prescribed thiazides.
11. discussion: the assumption is that all hypertensives have been diagnosed (no underdiagnosis) and that no one has been incorrectly diagnosed (no overdiagnosis).
12. p. 20: the sentence "our intervention appears to have led to better blood pressure control rates, regardless of medication regimen" is not clear to me. Do you mean that you have found a higher proportion of thiazide-prescriptions, but that you have not measured co-interventions on hypertension with other drug regimens?
13. The printed version of the figures can hardly be read.
14. p 21; “The percentage of patients who achieved goal blood pressures was 51%, only slightly lower than what was achieved in the ALLHAT.” This might be overinterpretation. Were the ALLHAT patients comparable with the patients from this study? Or where these patients suffering from more severe hypertension? Part of the goal achievement can be already prescribed to regression-to-the-mean phenomenon.

Discretionary Revisions (which the author can choose to ignore)
15. The paper is rather lengthy, some information seems unnecessary:
   - p. 9; the info on the ALLHAT
   - p.9: What is the value of the last sentence?
   - p.10; the info on VA
   - p.12; info on GMS staff physicians
   - p. 23; info on research initiative
   - table 2 can be left out
16. (I am not a statistician) Suggestion: Multivariate analysis with baseline performance as independent, and patient characteristics in the model (now no adjustment has been made for baseline differences p 15). Multilevel analysis necessary? Nearly 22,000 patient records have been analysed. It appears that no random sample has been chosen, but all patients have been analysed. No statistics necessary? Was such a large sample necessary? High risk of finding significant but clinically irrelevant differences? Advantage of such large data sets: possibility of subgroup-analysis for different ethnic groups.

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

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

Quality of written English: Acceptable

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