Author’s response to reviews

Title: Effects of primary care clinician beliefs and perceived organizational facilitators on the delivery of preventive care to individuals with mental illnesses

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Version: 2 Date: 23 Aug 2017

Author’s response to reviews:

Dear Dr. Burton,

Thank you for your secondary review of this paper; we are reassured of the quality of the review process having received additional feedback. We have attempted to address your concerns in the manuscript and believe the edits have improved the clarity and transparency of the paper. Below you will find our point-by-point response to your review; we are also submitting a copy of the unpublished main outcomes manuscript, as you requested. The main outcomes paper has now been accepted and is in press.

Thank you,

Bobbi Jo Yarborough

1. The biggest issue is that we have no way here of knowing whether these care gaps apply to all patients, not just those with Severe Mental Illness (SMI). Of concern is that reference 9, from the authors is unpublished and while it appears to answer that question, without some form of comparison it is difficult to tell. The intro seems to suggest that the authors work finds for both sides.

For the editor’s use only, as requested, we now include with our resubmission, a copy of our main outcomes paper. The main outcomes paper is now in press at the American Journal of
Preventive Medicine. In it we describe our comparison of care gap rates among groups with and without mental illness diagnoses. We found that groups with mental illnesses received preventive services at rates similar to (or better) than patients without diagnosed mental illness.

2. Introductions refers to SMI but patient selection appears to include SMI + Anxiety + MDD – please clarify.

We specifically chose to cite Dr. Walker’s paper in the introduction because it showed that all-cause mortality was significantly elevated not just among groups typically labeled SMI (psychotic disorders- bipolar disorder and schizophrenias) but also among mood disorder and anxiety groups. We have rewritten the introduction to remove the emphasis on SMI populations and more appropriately set up the paper for the broader set of diagnostic groups included in our analyses. Page 3 lines 55-58

3. You give an indication of the population covered by KPNW but not by CHCs. I think if you are going to compare them, then you need to say up front that you think they are different. Indeed, it would be good to know whether you hypothesised a difference a priori, because otherwise much of the results section could be post-hoc speculation.

We have revised the Methods section to include data that characterizes the CHCs. We have also clarified that the two settings were chosen deliberately because of their differences in patient and organizational characteristics. While we compare the two settings descriptively in the second paragraph of the results, we intentionally do not make statistical comparisons between the care gap rates in each setting. Because we expected differing care delivery models across the settings to result in differences in care gap rates (and because of measurement differences across the two settings), we analyzed the settings separately. We now make our hypothesis about the effects of organizational characteristics and our analysis approach clear in the description of the statistical analyses (in the Methods section). We also make clear that we did not have a priori hypotheses about how clinician characteristics would influence care gap rates. Page 5 lines 102-105, 110-112; Page 9 206-217

4. L 120 – instances (does this include same diagnosis on multiple occasions or multiple diagnoses?)
We required two instances of the same diagnosis on multiple dates to reduce the risk of including anyone with a single diagnosis on a single date that may have been an error. We have revised the text to make this clearer. Page 6 line 129-130

5. L259: so 4% variance is individual physician (but what about clustering in group practices where resources / policies may be shared)

There could be group level effects but these could not be disentangled from physician effects. We have added this as a possible limitation of the study. Page 16 364-367

6. L259: … and how much variance was explained by patient characteristics and how much was unexplained?

The analysis presented here partitions the variance in patient gap scores into the between-physician variance and that which is within-physicians, much like an ANOVA. We can say from these analyses that 96% of the variability in patient care gap scores occurs at the patient level. This includes both true individual differences and measurement error. We have edited the text to make this clearer. Page 13 279-280

7. L272 did practices know their care-gap rates (or at least how they performed relative to peers) beforehand? Couldn’t this be post-hoc attribution?

Practices and clinicians were not aware of their care gap rates or how they performed relative to peers; we have clarified this in the text. Page 9 lines 201-202

8. Table 1 – I think there needs to be some interpretive commentary here, or in the text, that addresses the difference between KPNW and CHCs in gap scores measurement techniques. Is 15 in one comparable to 25 in the other, or much better?
In response to #3, we have clarified the differences in measurement of care gap rates in the two settings that make comparisons of care gap rates across the settings inappropriate. Page 9 lines 206-216

9. Table 2 need to explain HEDIS (not even sure it is in the main text). Why is the significant effect of goals in CHCs not highlighted?

We have removed the term “HEDIS” from Table 2 and replaced that text with text that is consistent with how we describe clinician screening-related performance goals in the text. Page 23

Thank you for bringing to our attention that we missed highlighting the significant effect of goals in the CHCs. This was an oversight and we have corrected it. Page 23 line 423

10. Table 2 – can you translate these coefficients into something clinically meaningful? If a -0.06 difference in care gap is from 15 to 14.94 then does it really matter? What is that equivalent to in terms of % patients at risk getting a smoking cessation programme or whatever?

We have provided additional interpretation of the regression coefficients to more specifically state the magnitude of the effect expressed as a percentage. The interpretation of a coefficient of -.06 would be a 6% reduction. For items on a Likert-type scale from 1-5 (e.g. likelihood completing screenings absent symptoms), each point increase on the scale would also be interpreted as a gap score decrease on average of 6% (for a coefficient of -.06). The difference between a clinician with a score of 1 on a measure versus a 5 on the likelihood scale would be associated with a 24% decrease in care-gap score. This would be a clinically meaningful difference in screenings. Page 13 lines 284-300

11. More discussion needed about separating CHCs and KPNW. If lumped together then the “case managers have no effect” argument – which because of its position as the final statement is potentially more influential than it should be would disappear. Perhaps different things work at different stages of evolution of structured care?
We hope that by providing the rationale for analyzing CHC and KPNW data separately we have addressed this concern. Page 9 lines 206-216