Author’s response to reviews

Title: Associations Between Provider Communication and Personal Recovery Outcomes

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Author’s response to reviews:

Ana Claudia de Ornelas Maia (Reviewer 1)

1) I'd say just to change those terms to not be repetitive in the text: Introduction: lines 50, 51 "A systematic review synthesizing these descriptions of personal…change for "Study about these descriptions….

We have reviewed the manuscript and incorporated edits to reduce repetitive text (Introduction, p.3, line 22).

2) Introduction, Discussion and conclusion - I would make a more objective and scientific description Just. The paragraphs are great and this makes reading tiresome for such clear information.

The manuscript has been edited throughout to provide greater clarity including the Introduction, Discussion and Conclusion. Revisions are documented in track changes edits.

Jerome Kroll, MD (Reviewer 2)
1) What types of 'probable' mental illnesses were represented in the population that participated in the California Well-Being Survey (2014) from which this research sample was drawn. I raise first the question of how broadly applicable or generalizable are the findings based upon studying this population. Can we generalize these results to a population characterized as SPMI (serious and persistent mentally ill)? Is it a population of 'walking wounded' who are mostly not SPMI? Did the California survey include the homeless, the incarcerated, and those presently in mental hospitals or wards? I am not implying that the California study should have or could have included the assumedly more ill populations, but, as I indicated, how applicable are the premises and working models of the questionnaires to working with SPMI?

We agree with Dr. Kroll that the manuscript can benefit from more clearly stating which populations the study’s findings can be generalized to. In the Participants and Procedures section (p. 6, lines 11-12), we provide information on the breakdown of the 2014 California Well-Being Survey (CWBS) respondents by K6 scores. Nearly half of 2014 CWBS respondents (47.2%) reported levels of severe distress (K6 scores of 13 or greater) which is indicative of serious mental illness. Levels of severe distress on the K6 are indicative of a serious mental illness (Kessler et al., 2010). A little more than half of 2014 CWBS respondents (52.8%) exhibited mild to moderate levels of psychological distress which has been associated with functional impairment and mental health treatment need (Prochaska, Sung, Max, Shi, & Ong, 2013).

In addition, within the Discussion section (p. 14, lines 6-11), we now note that the study sample excluded certain populations. The following excerpt has been added:

“It is important to note that our sample excluded certain segments of the population (e.g., homeless, incarcerated, hospitalized) in which the nature and severity of mental illness could significantly differ. Nonetheless, our analyses did control for levels of psychological distress accounting for potential differences in mental illness severity among study participants obtaining services across different mental health care settings.”

Kessler, Green, Gruber, Sampson, Bromet, et al. (2010). Screening for serious mental illness if the general population with the K6 screening scale: results from the WHO World Mental Health survey initiative. Int J Methods Psychiatr Res, 19, 4-22.

2) Were patients referred to MH professionals rather than to general medical doctors because they were not as seriously ill - perhaps referred for counseling rather than medication, which sometimes can be read as an indicator of seriousness of illness? If so, then the two populations studied (those seen by MH and those seen by medical doctors) may not have been comparable. If so, the population seen by MH may have been able to form a closer working relationship with the provider and thereby was able to have a more positive treatment experience which was reflected in higher scores given in the 'show respect' item.

We agree with Dr. Kroll that the severity of mental illness could influence the relationship with providers. Our analyses account for this possibility by including level of psychological distress as a control variable, which we note in our response above to Comment #1. In addition, to clarify, participants who obtained services from a mental health professional could have also received medication treatment given that mental health professionals included psychiatrists.

3) As someone who sees a more acute and more disturbed population of patients, I sometimes wonder what is meant by the universal approbation for providing care that is responsive to patient preference. Almost every manic patient with whom I have worked wants, and often demands, reduction in anti-manic medications. I provide much respect and long discussions about the nature of the illness, the philosophic questions of what is an illness, discussions about benefits and outcomes and side effects of medications, and the likely consequences of going off medication. When I acquiesce to the patient's preference, the patient more likely than not has a relapse and ends up being brought to the hospital by family, group home staff or police. I sometimes worry about my score on the 'shows respect' tickbox. Lest I just seem like a crank discussing my issues rather than those raised by the authors here, I would like to see the authors discuss the applicability of the principles of patient-centered care (such as patient preference), which are considered a hallmark of quality care, to broader domains of patient populations. If higher approval ratings under the item 'shows respect' depend upon the mental health professional agreeing with the patient to lower medications with subsequent relapse, then the rating item is moving in the opposite direction of patient recovery.

As suggested by Dr. Kroll, we now note and cite articles that have expressed concerns about patient experience surveys reflecting providers’ acquiescence of patient preferences. In the Discussion section, p.14, lines 14-18, the following excerpt has been added:
“Further, some have expressed concerns that consumer experience surveys may tap into providers’ compliance with patient expectations or preferences even when they are contraindicated (e.g., stopping medication) (50, 51), highlighting the potentially complex relationship between consumer experiences, the delivery of evidence-based care, and personal recovery.”

4) I note that there are data on race/ethnicity/language and age and gender demographics (Table 1), which has a footnote that [computer] models included controls for age, gender, race/ethnicity and psychological distress. I assume that no mention is made in the text means that no significant findings were uncovered. However, I would like to see a brief discussion about this, especially since so much attention is paid to cultural sensitivity issues that one might expect differences to show up if the data were available. For example, were the MH professionals or medical doctors same ethnicity or gender as the subjects of the CHIS (2013) or the CWBS (2014)?

As suggested by Dr. Kroll, we have included the estimates for the control variables in Table 2 as well as a description of the results. The following excerpt has been added to the Results section (p.11, lines 1-4):

"With respect to covariates, higher psychological distress and male gender were associated with worse outcomes across all five personal recovery domains. African Americans and Latino Spanish interview participants experienced better personal recovery outcomes, whereas age was not associated with any personal recovery outcomes."

Unfortunately, we are unable to provide information on the race or gender congruence between provider and participant given that neither the CHIS 2013 or CWBS 2014 collected such information.

5) If allowable, could there be one or two brief narrative histories derived from the telephone interviews that might flesh out the type of responses some subjects volunteered during the telephone interviews that might illustrate the types of responses given in response to the formatted questions.
Unfortunately, we are unable to provide brief narrative histories of participants given that this type of information was not part of our data collection. We administered structured telephone interviews that consisted of Likert-type questions drawn from prior population-based studies. Any spontaneous disclosures participants might have made were not recorded by data collectors.

Graham Meadows (Reviewer 3)

Introduction:

1) The introduction is generally clear but I'd suggest the authors could improve the statement of aims and hypotheses. This presently sits on page 5 and is not separately headed. Implicitly there are hypotheses suggested in this section but they are not explicitly stated. I'd suggest - assuming these were in some way articulated - that these would be better presented as a small number of specified hypotheses especially since the analytic framework and reporting is suitable for hypothesis testing. If the investigation is to be presented as fully hypothesis-free then this should be justified.

As suggested by Dr. Meadows, we have revised the introduction to more clearly state the study aims and hypotheses. On p.5, lines 5-12, the following revised excerpt has been incorporated:

“Using the 2014 California Well-being Survey, a cross-sectional population surveillance survey of California adults with probable mental illness, this study aims to: (1) empirically test whether personal recovery outcomes are related to consumer experiences of two aspects of provider communication – showing respect and explaining things in an understandable way; and (2) assess whether the effects of provider communication differ depending on whether care is provided by general medical doctors versus mental health professionals. Although consumer experiences have been linked to health outcomes (28), it is unclear whether a similar relationship will be found with personal recovery outcomes given the dearth of research in this area. Individuals with a diagnosable mental disorder are more likely to obtain care from general medical providers than psychiatrists or other mental health specialists (29, 30). Given that care from general medical doctors likely consists of psychotropic prescription medication, whereas mental health professionals may be more likely to provide psychotherapy-based treatment, we hypothesize that the impact of provider communication may be greater for mental health professionals.”

Analyses:
2) To this reviewer the reader could benefit from more presentation of findings as associated with provider types. At present we only have one small table of regression on outcome measures representing a highly selective presentation of the regression analyses. This begs a considerable number of questions and gives us no real indication of the size of differences between groups in practical terms. So I would suggest in the way of additional tables, either for the main paper or supplementary content:

Some key frequencies, at least of key outcomes broken down by the three provider type classifications and the dichotomous divisions of respect and explanation. While the findings where people have seen multiple providers cannot be associated with specific provider types they are of interest and I do not see benefit in not presenting them to the reader. This will make for a reasonably large table but still only essentially by my calculations a 6x5 structure with sub-content in each cell so I would not think this unfeasible with some careful table design. Some cells will be fairly small but then the underlying balance of the data set contributing to the regression will be clearer to the reader.

We have added a table of the means and standard deviations for provider shows respect by provider type as supplementary material. We added supplementary information for provider shows respect given that significant interactions were observed only for provider shows respect by provider type.

3) In regards to the regression, there could usefully be an account as to whether and how the critical assumptions for the regression model were examined and the findings of this. The regression from my reading included people who saw all provider types and so, as above, we could usefully therefore see these findings. An option might have been to exclude these from the analyses but they probably are more usefully part of the full picture and from my reading they were included - so I'd suggest the reader should see fuller information on the regression findings. The findings for instance for age and gender form important parts of the context to the findings associated with the key hypotheses so the reader will be able to make more sense of the paper for seeing these.

Reviewer 2 also suggested presenting the results of the control variables (i.e., psychological distress, age, gender, race/ethnicity), which are now provided in our revised manuscript (see response to comment #4 by Reviewer 2).
In addition, as suggested by Dr. Meadows, we have added estimates for the main and interaction effects for participants who had seen both provider types (see Table 2). There were significant main effects for having seen both provider types but no significant interaction effects. We have incorporated the following description of the main effects in the Results section (p.9 line 23 to p.10 lines1-2):

“Also, having seen both provider types was associated with greater levels of connectedness (β=0.27; p<.05) and lower levels of internalized stigma (β=−0.55; p<.01) compared to a mental health professional only.”

With respect to the critical assumptions of the regression models, VIF ranged from 1.06 to 1.47, providing no suggestion of collinearity problems. Kurtosis estimates ranged from -1.02 to 0.83 indicating that the dependent variables were well distributed. In addition, R-square estimates were added for the regression models in Table 2.

Discussion and conclusions:

4) This might have been assisted by some a-priori hypotheses as suggested above.

In places, eg p 11 'effects' and 'influential', the authors seem to succumb to the understandable temptation to ascribe causality even though the limitations section notes that this is not clearly allowable from this study design. This does not preclude possible causal attributions being discussed - perhaps more of these - but in tentative terms.

Throughout the manuscript, we have revised language which might imply causation. Changes have been tracked in the manuscript via “track changes” editing mode in Microsoft Word.

5) There might be more to explore in discussion with the fuller presentation of findings I have suggested.

As suggested by Dr. Meadows, we have added to the Discussion section (p.13, lines, 1-11) the following excerpt describing the additional results that are now presented in our revised manuscript:
“This is one of the few studies that have examined the relationship between sociodemographic characteristics and levels of psychological distress to personal recovery outcomes (7). Our finding that females and certain racial-ethnic minority groups (i.e., African Americans, Latino Spanish interview participants) exhibited better personal recovery outcomes warrant further research. Prior research suggests that the conceptualization of recovery outcomes may differ by gender and race/ethnicity (44, 45). Likewise, the negative association between psychological distress and personal recovery outcomes suggests the need to better understand how to best facilitate personal recovery outcomes among those with more severe mental illness. For instance, in a small study involving individuals with serious mental illness, interactions with professional staff in which participants felt seen and heard appeared to support personal recovery (46).”

6) There could usefully be an acknowledgement that a critical set of influences on differences between experiences of different care settings could arise from differences in nature and severity of the mental health problems under treatment.

As noted in our response to comment #1 by Reviewer 2, we now note that our analyses controlled for level of psychological distress and that our study excluded certain segments of the population with potentially different levels of mental health problem severity (e.g., homeless, incarcerated, hospitalized).