Author's response to reviews

Title: Access to the US Department of Veterans Affairs Health System: Self-reported Barriers to Care among Returnees of Operations Enduring and Iraqi Freedom

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Version: 3 Date: 16 November 2013

Author's response to reviews: see over
Dear Editor,

Thank you for providing reviewer and editorial comments regarding our study. We appreciate the opportunity to provide responses to these thoughtful critiques. Our specific point-by-point responses to the concerns follow.

Reviewer concerns:
1. On p. 13, barriers to VA Care should be reported as listed in Table 1.
   We agree the barriers should be reported as listed in Table 1. While we had previously separated the perceived stigma items, we have now listed the barriers serially in the order in which the items appear in Table 1.

2. Table 2 is somewhat confusing. It shows "the final variables associated with self-reported exclusive use of VA care" (p. 14, para. 3). However, as the column heading is e.g. "any barrier", it seems rather to show the association between e.g. socio-demographic variables and the "presence of any reported barrier" (cf. p. 14, para. 2). This needs to be clarified.
   On pg 11 we clarified in our analysis method that we intended to examine the extent to which barriers to VA care influence actual use of VA health care, we next conducted a series of logistic regressions in which we examined the unique contribution of care system and other factors to VA use.
   Linked to this, Table 2 shows columns for each regression model: any barrier; distance or location barrier; and wait time barrier. The title of the table is “Summary of multivariable analyses of barriers to exclusive VA care among OEF-OIF veterans.” We have simplified the table and clarified by adding a row above the name of each model and footnotes to Table 2 that clarify each model is testing a different main effect.

3. The comments about differences in barriers in the 2 groups should not be made when there is no evidence at all that the differences are not likely to be due to chance. This does not mean you need to adhere to a strict cut off of the traditional p=.05 if there are sizable differences in estimates which would be very important if they were real and the test for a difference has a reasonably small p value. In this case you should report the situation. However the differences that are commented on such as wait times, where the paper states that participants in the OEF-OIF group were more likely to report barriers in wait times, are only small with absolutely no evidence that they are likely to not be due to chance.
   They should therefore not be commented on as being different. Obviously if you take 2 samples you will get a different estimate. The chance of 2 estimates being identical is very small but when the study has produced no indication that the populations from which they are drawn are likely to differ on the characteristic it is misleading to make comment. Adding comment that it is not statistically significant as has been done in some parts of the paper is not adequate as the assumption would be that there is weak evidence of a real difference if you are commenting on it, but it simply did not reach the critical level you have chosen to use. This is not true in most places in this paper.
   We agree and have changed the sentence saying there were minor differences in some barriers we tested and none were statistically significant.
4. It does seem quite remarkable that not even an error of an estimate of the 8 variables in common included in the models including any barrier and distance barrier differed. Although it is not surprising they are similar, to be identical would be remarkable – are you sure there has not been a cut and paste problem? The significance annotation for married is different in the 2 models, perhaps indicating that it is against the wrong estimate.
We thank the reviewer for this observation. We have rechecked all values and significance levels and made minor changes to values in the Distance Barrier model in Table 2 and in the Additional File 2, Table 2, Model 4. These changes do not modify the findings but we are very happy to have these be accurate.

5. It is a little confusing to list different variables associated with the same outcome from the 3 models differing only in 1 explanatory variable. In fact the estimates do not differ much at all. It would be better to decide your main analysis and comment on the variables which appeared to be associated in this model and then simply comment that this was similar in the other 2 models. You are including the details in the table so people can see the detail if they wish.
We agree that our modeling which builds sequential sets of variables is confusing.

We have struggled with a way to show the full results which we considered important because it will allow readers with different theoretical viewpoints to examine models with variables that tend to be of interest to health services researchers. Because of this and reviewer comments, we include now only our final model with a comment that progressive additions of different variables are available in the additional files.

We have simplified and clarified Table 2 to make it clear the model building did not make any interpretational differences across the models, but for readers who are interested in the details, we have uploaded the 3 Additional Files. We still think these tables will be useful. We have clarified the language in the table and made consistent what those final models are in the methods and the results and they do not need to be in the interpretation anymore.

6. Discussion – remove the 4th sentence about differences – see above – the evidence is too weak to make comment, even giving the qualification of not statistically different.
We agree this is a very modest difference and we have removed the sentence as suggested.

7. P16 last sentence in 1st full paragraph, cannot say that the analysis of this cross sectional study suggests that if access barriers were removed OEF-OIF returnees might use VA services exclusively. This sentence should be deleted.
These results are based on a cohort and it is possible that people moved closer and further away from VA services based on initial care. We think wait times are more a part of the system itself and would have been in place. However, because our interviews are cross-sectional we are urging additional caution in interpreting these and have refined the sentence, although the results are consistent with what others have found. These cross sectional results are similar to access barriers to care that others have noted. This is a cohort and these data are based on the baseline first interviews and we look forward to having follow-up data at a future point in time.

8. P14 in paragraph above the instruction to insert Table 2- logistic regression (not logistical).
We thank the reviewer for this observation and we have made the change indicated.

9. P15 top line- you are reporting odds ratios, not relative risks so they had 7 times greater odds, not risk.
We thank the reviewer for this refinement and we have made the change indicated.

10. P16 1st complete paragraph- change ‘chances’ to ‘odds’ (3 places).
We thank the reviewer for this refinement and we have made the change indicated.
11. Additional table 3 typo in the error for ‘other non working’ in model 4.
   We thank the reviewer for this observation and we have corrected the typo.

Editorial concerns:
1. Please also ensure that your revised manuscript conforms to the journal style.
   Finally, we have revised the manuscript to conform to the journal style by removing capital letters in the
   title which were not proper nouns, and assuring the reference list and files are formatted correctly.

   Thank you again for your consideration of this article.

Sincerely,
Christine Elnitsky