Author's response to reviews

Title: Comparison of Outpatient Health Care Utilization among Returning Women and Men Veterans from Afghanistan and Iraq

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Version: 2 Date: 24 March 2010

Author's response to reviews: see over
Dear Editors and Reviewers,

Thank you very much for the thoughtful review.

Based on the reviewers’ comments we have modified our title to reflect our definition of health care use. The title is now “Comparison of Outpatient Health Care Utilization among Returning Women and Men Veterans from Afghanistan and Iraq.”

The reviewers’ comments are in bold and our answers are indented and normal text. The content from the paper is in italics and in quotation marks. The page numbers start from title page.

In response to Dr. Maciejewski concerns, we respond as follows:

MAJOR COMPULSORY REVISIONS

# 1 Background

Even though there are few studies comparing men and women in PC use and number of visits, there is extensive non-VA literature on which covariates are most important. It would be helpful to provide context for the analysis of veterans to understand what patient factors outside VA have explained variation in PC use, which would also inform what unobserved factors in the VA analysis are likely relevant.

We have revised the background to include literature on non VA care use comparing men and women and factors predicting their use. Several studies support our use of factors like age, race, and marital status. As it was a retrospective study we could not measure some relevant factors like income, education etc that were described in the literature.

We have described this on Page 3 in Background in paragraph 2:

“We women report higher use of health care services than men. These differences may be associated with reproductive biology and gender-specific conditions, higher morbidity rates in women than in men, differences in reporting of symptoms and illnesses, and/or a greater likelihood that women seek help for prevention and illness.[5-10] In addition, socio-demographic characteristics such as age, marital status, income and education levels, and belonging to a minority group have been found to affect health care needs and utilization.[10-13] Therefore understanding the effect of gender and other underlying determinants on use of health care services is important in promoting equal access to health care.”
#2) Material and Methods

a) Please state the stop codes used to define primary care because there are several combinations that have been used in prior studies. This will clarify whether the primary care definition is inclusive or more narrow.

We thank Dr. Maciejewski for the excellent recommendation. Our definition is more inclusive and includes all outpatient stop codes. We revised our methods to clarify this in our definition for health care use. The visit has been defined as one outpatient encounter per person on a unique date and location. Based on the revision we have replaced the term primary care with outpatient health care use.

This is described on page 5 in the study in the Methods section under Outcome Variables: lines 3-5

“We defined a visit as one outpatient encounter per person on a unique date and location. Only visits after return from OEF/OIF deployment were included.”

b) Generosity of VA coverage and copayments for visits and medications are better characterized by Priority Groups (1-8), and % service connection doesn't reflect the full variation. Since primary care is the relevant outcome, Priority 7 and 8 veterans must pay PC co pays but all other Priority Groups are exempt. The current use of 30%+ service connection should be replaced by Priority Group.

Our study addresses the utilization among OEF/OIF veterans within five years following separation. All OEF/OIF veterans benefit from five years of free health care coverage and are exempt from co pays. Therefore, we have chosen not to add priority group as a predictor in our models as they are less relevant to our sample population.

We have clarified the inclusion of service-connected disability a known predictor for utilization of VA health care services in Methods section under Model Covariates on Pages 5-6: lines 6-13:

“Service-connected disability, a known predictor of use of health care services within the VA system, was defined as an injury or illness that either was incurred or aggravated by military service affecting employability or functioning.[19, 20] This is based on the proportion of the disability assigned by Veterans Benefits Administration .[3, 22, 23] Persons with more than 30% service connected disability are entitled to maximum benefits, including Veterans’ retirement pay, disability severance pay, separation incentive payments, and increased amounts of VA compensation.[21, 22] Therefore, we dichotomized this variable to individuals with ≥30% and <30% service connected disability, including no disability.”
Related to this point #2, I expected to see a comparison of demographic and other factors between men and women to understand whether female PC users were different than male PC users in age, gender, race, marital status, etc. This would inform whether interaction terms are needed in the logistic and negative binomial regressions to provide a clearer picture of gender differences. I expected to see 4 comparisons in Table 1:

a) users vs. non-users (essentially Table 1 as is)
b) female users vs. female non-users
c) male users vs. male non-users
d) female users vs. male users

We have added additional tables and included the recommended interaction terms in the regression models. Table 2 compares female users vs. male users (Table 2 as is). Table 3 compares female users and female nonusers and Table 4 compares male users vs. male non-users. On the basis of the association between the independent variables and the outcomes of interest by utilization status (Table 1), gender (Table 2) female users and non users (Table 3) and male users and non users (Table 4) helped to generate the interaction terms needed in the logistic and negative binomial regressions providing clearer picture of gender differences. As none of the interaction terms were significant in both the models they were not included in the tables.

We have discussed these findings in methods and results.

On Pages 6 in Methods section under Data analysis: lines12-16:

“We also included interaction terms between gender and other covariates of interest to explore the extent to which significant predictors may moderate any detected gender differences. A significant interaction (P < 0.05) would indicate that the association of gender with the outcome varied at levels of the other variable.”

On Page 7-8 in Results: lines 5-10 of paragraph 2

“Women non-users were more likely to be non-white (24% vs. 12%, p=0.001), have private health care insurance (46% vs. 37%, p=0.02) and have ≥30% service connected disability (17% vs. 13%, p=0.03) (Table 3). Men using care differed significantly from non-users in mean age (34 vs. 33 years, p<0.001), having private health care insurance (48% vs. 32%, p=0.01) and having a >30% service connected disability (17% vs. 7%, p=0.01) (Table 4).”

On Page 8 in Results of initiation of care analysis: lines 6-9:

“For the logistic regression interaction model, the following interaction terms were included: sex*race, sex*marital status, sex*covered by health insurance, sex* service connected disability. No interaction terms were significant so they were omitted from the final model.”

On Page 9 in Results of rate of care analysis: lines 8-10:

“The interaction model investigated the interactions between sex and age, marital status,
race, and service branch, but none of these interaction terms was significant and they were omitted.”

MINOR ESSENTIAL REVISIONS

#4 Was the original sample the authors started with 1620 or were some observations lost in the data creation process? The paper leaves the impression that the authors ended with the same sample size as they started with, and that seems unlikely. Please explain if there was sample loss, each reason and the specific loss.

We have restated how we derived our analytical sample and explained the loss to follow up.

This is on Page 4-5 in Methods section under Sample heading:

Lines 1-3:

“Our initial sample consisted of 1820 OEF/OIF Veterans who enrolled for care at the local VAMC or at one of the six associated community-based outpatient clinics (CBOCS) between September 1, 2002, and October 30, 2006.”

Lines 8-10:

“By restricting the sample, we also sought to control for the effect of distance on utilization intensity.[16, 17] Using this criterion, we excluded 200 Veterans, making our final sample size 1620 Veterans.”

#5 The higher odds of VA primary care if privately insured seems counter-intuitive. It would seem like private insurance would lead veterans to use PC in VA less not more.

We are in agreement on the general point that having a private health insurance should be associated with an increased likelihood of using health services outside the VA and decreased intensity of use within the VA. This is shown in our results where veterans having private health insurance had higher initiation (OR = 1.44; 95% CI: 1.15, 1.86), but compared to those without insurance were unlikely to continue to utilize outpatient primary care at the VA (IRR = 0.77; 95% CI: 0.67, 0.88). Therefore, this is in part an explanation of why the privately insured were more likely to not continue to use services because they have coverage.

#6 The age cut-points seem odd, so please justify why these cut-points were chosen. Consider entering age as a single continuous variable.

We reviewed the age cut off points and as suggested we have included it as a continuous variable.

This is described on Page 5 in Methods section under Model Covariates: line 5:

“Age was included as a continuous variable.”
#7 Discussion

#7(a) Page 9: the authors state that "issues discussed here represent valid contributions to current health policy research" but never say what they are, except that there are few published studies. That is a research contribution but not sure what the policy contribution is. Please clarify.

We thank Dr. Dr. Maciejewski for the outstanding point, and agree that it is a research contribution. Our study is a preliminary study represents contribution to health service research rather than policy. Further studies are required on types of services utilized over time and factors influencing satisfaction are required so that steps can be taken to facilitate the health care delivered at VA facilities and inform policy decisions. In the revision we have modified our statement.

This is described on Page 9 under discussion: lines 1-10 of paragraph 2:

“Although this analysis is reflective of only those Veterans who sought health care at a specific VA facility and may not reflect OEF/OIF Veterans in other VA networks or those using services outside the VA, the issues discussed here represent contributions to current health services research and suggest general recommendations to improve health care utilization by women Veterans. Our preliminary data suggest the need to provide high quality, gender-specific care. Few published peer-reviewed studies to date have examined the association between gender and health care service utilization rates among Veterans. Further analysis on types of services utilized over time and factors influencing satisfaction are required.”

#7(b) Page 11: the authors state that "the removal of insurance barriers to health care is necessary" but this is an overly strong statement given the single site and lack of measurement of non-VA primary care use funded by Medicaid, Medicare or private insurance. I recommend dropping this statement.

We have dropped the statement.

In response to Dr. Drapeau concerns, we respond as follows:

MAJOR COMPULSORY REVISIONS

#1 Table 2 provides interesting data on the socio-demographic and military profile of women and men who use health care services. These data show that women users tend to be younger than men users, that they are more likely to be single and non white and that they are less likely to be in Marine Corps. Given that the objective of the study was “to examine gender differences in utilization of outpatient primary care health services”, one wonders why interactions between gender and these variables (age; marital status; “race”; service branch) were not investigated in the regression models and why these findings are not commented in the discussion.

We have rerun the models with the interaction terms and described above in the answers to reviewer 1’s question no #3.
Although the discussion is well structured, it could be more thorough.

(a) For instance, the authors argue that the steps taken by the VA to improve the delivery of gender specific health care services may have contributed to increase the use of health services by women Veterans. An additional contributing factor, not discussed by the authors, is that women who were in Afghanistan and Iraq may have been more exposed to traumatic events or may have experienced a higher level of service-connected disability than those serving in other countries or in previous conflicts and thus they may have a higher need for health services.

We have added this to the discussion on Pages 11: lines16-20 of Paragraph 3:

“An additional factor contributing to the increased use of health services by women Veterans may have been that, compared with previous conflicts, more OEF/OIF women had been exposed to combat and thus had higher levels of service-connected disability.[32] Our results confirm this finding, as female users had higher levels of service-connected disabilities than male users.”

(b) The authors attribute the much higher number of medical “visits” observed in their study (roughly 16 per year) compared to that observed by Maciejewski et al (3.3 per year) to the free five-year health care coverage following separation. Was the sample in Maciejewski et al.’s study made up of Veterans from Afghanistan and Iraq? If not, it might be an indication that Veterans from Afghanistan and Iraq have a higher need for services. Was the definition of health services similar in both studies? Maciejewski et al.’s study was published recently (2007). Was the five-year health care program implemented at that time? Does this program cover only health services offered by VA?

Thank you for the excellent observation. Indeed the definition of health services in Maciejewski et al.’s study was different than our outpatient use criteria. We have clarified this in our methods section and to reflect the change we have replaced the term primary care with outpatient care.

We agree with the reviewer and as suggested we also included interaction between gender and private health insurance but it was not significant. Therefore, we concur it cannot be inferred from this study that “removal of insurance barriers to health care is necessary, but not sufficient to eliminate gender disparities in health care use”.

We have removed this from the discussion.
to eliminate gender disparities in health care use”. Hence, we have dropped this statement from the discussion.

We have dropped this from the discussion (Ans #7(b) of reviewer #1)

MINOR ESSENTIAL REVISIONS
#3 The authors report that “Among the 1620 Veterans who enrolled for care 46% utilized care” (page 6). This is somewhat confusing. Does it imply that 54% of the Veterans who enrolled for care did not receive it? Or that they receive care that was not health-related? Data on Table 1 are misleading in that the percents shown under the “Utilizers” and “Non Utilizers” columns are not calculated from the number of respondents in these categories. For example, 54% corresponds to 54% of women instead of 54% of “Utilizers”. By the way, the standard terms are “users” and “non users” (instead of “utilizers” and “non utilizers”). The percent of non white is 24% in women and 12% in men (ref. Table 2).

We have clarified in the results that of the 1620 people who enrolled in the VA 54% (874/1620) did not visit the outpatient care.

This is described on Page 6 in Results section: line 4 of Paragraph 1:

“During the observation period, 54% (874/1620) of the study population did not have any visits to the local VAMC for outpatient health care.”

We have corrected the terms to “Users” and “Non users”, and also recalculated the percentages from the number of respondents in these categories in Table 1

We have corrected the percentages of non white 24% in women and 12% in men (ref Table 2).

Thank You for your constructive comments. We hope that we have sufficiently addressed the editors and reviewer’s concerns. Please let us know if we can do more to make the paper worthy of your standards.

Sincerely,
Mona Duggal and Cynthia Brandt (for the authors)