Author's response to reviews

Title: The influence of active coping and perceived stress on health disparities in a multi-ethnic low income sample

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Author's response to reviews: see over
Dear Editorial Team:

My colleagues and I are delighted to resubmit our manuscript, entitled: “The Influence of Active Coping and Perceived stress on health disparities in a multi-ethnic low income sample” ms #1138791797157296, to BioMed Public Health. We appreciate the comments of the reviewers and believe the manuscript is strongly improved. We have addressed all the comments made by the reviewers and they are outlined below:

Major Issues

Reviewer One

1) It would be helpful if the authors more clearly stated the gaps in the current literature and how this study addresses these gaps. One example might be that the current study extends the JHH to other minority populations (e.g., Hispanic populations) or the inclusion of oral health and not just general health was addressed in the present study.

Information more clearly stating the gaps in the literature has been provided in the Introduction (See pg. 6 of the revised ms.).

2) In the introduction and throughout the paper, when referring to avoidant coping, the authors use the term “John Henry Active Coping Scale (JHAC)”. Since the authors are testing the John Henry Hypothesis (JHH) referring to active coping at JHAC can be somewhat confusing for the reader. Is the JHAC assessing active coping or a construct slightly different than active coping? If it is simply a measure of active coping, the authors should replace their references to the measure in the introduction, results, and discussion with the term “active coping”. If JHAC is measuring a construct other than active coping then further information needs to be provided to explain how it is different.

To clarify this issue we have replaced references to the JHAC in the introduction, results and discussion to active coping.

3) The general approach to data analysis appears to be appropriate. However, the authors indicate that they first examined the 3 way interaction and then tested lower order interactions and main effects. This is generally the opposite of how these types of analyses are performed. First, main effects are entered into the equation, then lower order to higher order interactions. If the interactions are significant, the main effects are not interpreted, rather the significant interaction effects are interpreted.
The reviewer makes an excellent point. We have clarified the presentation of the analysis to demonstrate a more typical presentation of ordering of analysis.

Reviewer Two

“The main problem in this paper is that all information is self-reported and Information is cross-sectional.”

We agree with the reviewer that this is an important point. While it is impossible to change the nature of the study design we have added the limitation of the cross-sectional nature of the data to the manuscript. We have also provided a recommendation that future research focus on longitudinal study (See pgs. 18-19 of the revised ms.) In addition, we added information about how other unmeasured variables such as neuroticism or depressive symptoms could also relate to self-rated health and be potentially responsible for observed relationships in the data. We thank the reviewer for reminding us of these important constructs.

While self-report data always has some problems associated with it for psychosocial variables it is difficult to avoid. We were specifically interested in perceptions of stress, and discrimination. For many of the variables chosen in the study self-report information is a strength as it captures participants’ unique perceptions of their situation. In addition we chose scales and measures that have good reliability and validity whenever possible (e.g. self reported health has been strongly related to physiological measures of health; The Perceived Stress Scale has had extensive psychometric work done showing its validity and reliability). Therefore, we believe that even with these limitations that having multiple measures of important psychological and social correlates of health in such a large and ethnically diverse population provides and important contribution to the research literature. Our study results can help inform future research that employs more objective measures in a longitudinal design.

“The topics and variables of this paper (SES disparities, ethnic comparisons, John Henryism, health effects of stress and coping) could have created opportunity for more focused hypothesis.”

The reviewer makes an excellent point and we regret that we did not more explicitly state our hypothesis in the original ms. We have added more detailed hypothesis to the introduction (See pg. 6 of the revised manuscript.)

Minor Issues
Reviewer One

1) “The authors briefly discuss a relation between chronic stress/health/coping strategies at the top of page 5. Then they go on to discuss the John Henry Hypothesis and sight a couple of studies that have examined this hypothesis among Blacks. It would be nice if the authors could include a little more detail on these studies since this hypothesis is the focus of the analyses.”
More information regarding these studies has been added to this paragraph (See pg. 5 of the revised ms.).

2) In the Participants section at the top of page 7, the authors indicate that each interview lasted 27 minute and a mean yield of .6 interviews were completed each hour. I think that the authors can remove the mean yield and just provide the reader with how long the interviews lasted.

The mean yield of interviews has been removed (See pg. 7 of revised Ms).

3) “In the Measures section, the authors need to indicate the scaling of the self-rated health question (e.g., 1=Excellent?) and if higher scores on the PSS and JHAC are indicative of more stress/more active coping.”

This information has been added to clarify self-rated health (pg. 8 or the revised ms), Perceived Discrimination, (pg. 9 of the revised ms.) perceived Stress and JHAC (pg. 10 of the revised ms.)

4) “On the JHAC, the authors reference “scales” of the measure in the 2nd sentence of the last paragraph on page 9. Are there subscale and if so how many and what do they assess?”

We regret the confusion that was created by using the term scales. The JHAC is a single scale and was calculated as such. We have removed the word scales and replaced with scale (See pg. 10 of the revised ms).

5) “The authors indicate in the Discussion section on page 14 that SES was significantly associated with general health for whites and Hispanics but the relation was “mediated” by stress. The authors did not conduct mediational analyses, rather the relation appears to have been moderated by stress ….

The reviewer makes a good point. The term mediated has been changed to moderated.

**Reviewer Two**

**Some of the citations should have been updated more especially stress and health citations (7 to 10)**

We updated these citations paying particular attention to stress and health citations. (See pg. 5 of the revised ms.).

*There should be some indication of response rate. Although I am not expert in US telephone based sampling methods I believe there are some standards to report how many respondents finally participated to study from certain selected sample.*
The reviewer raises a valid point that has become the topic of a great deal of study and research in survey research. Several guidelines and standards have been discussed and we based the original text on standards agreed upon by American Association for Public Opinion Research (AAPA), The Council of American Research Organization (CASRO) as well recommendations and typical reporting from The Behavior Risk Factor Surveillance Survey (BRFSS). CASRO when discussing response rate states “Often response rates in survey research are calculated by simply dividing the number of completed interviews by the number of individuals who were selected to participate in the research. However, this method is too simplistic and does not do justice to the complexity of research design, sampling process and practical difficulties of contacting and assessing potential survey participants.”

In light of current survey research guidelines we believe that the explanation provided in the text provides sufficient information to satisfy standards and guidelines on reporting response rates in phone survey research. However, if the reviewer wishes we have prepared a table that further details the dispositions based on AAPOR and CASRO guidelines. We feel this table may be redundant with the text but want to be comprehensive in answering an important critique about response rate and potential bias. (See Table One in Revised MS). Depending on guidelines used there are numerous ways to report response rate, cooperation rate, refusal rate and we feel that picking any one rate would be misleading and potentially confusing to readers. However, in interest of being complete we provide here several CASRO and AAPOR estimates of response rate, cooperation rate and contact rate for our sample. We have concerns of including any one estimate and feel that the table now provides the pertinent information to evaluate response rates.

*** CASRO Response Rates ***
- simple [unknowns eligible]: 0.1871
- simple [unknowns not eligible]: 0.4016
- CASRO [unknowns divided]: 0.1871
- e for CASRO [proportion eligible]: 1

*** AAPOR Cooperation Rates ***
- Cooperation Rate 1 = 0.4016
- Cooperation Rate 2 = 0.4016
- Cooperation Rate 3 = 0.4269
- Cooperation Rate 4 = 0.4269

*** AAPOR Refusal Rates ***
- Refusal Rate 1 = 0.2512
- Refusal Rate 2 = 0.2512
- Refusal Rate 3 = 0.5391

*** AAPOR Contact Rates ***
- Contact Rate 1 = 0.4659
- Contact Rate 2 = 0.4659

“I did not find information about age range or mean age of the sample. Age should
have been used as a covariate in the analyses."
The information about age has been added to the Results section (See pg. 12 of the revised ms) and to table two. In addition, age has been added as a covariate in the analyses and parameter estimates have been adjusted accordingly.

“Possible problems with statistical power in interaction testing should have been addressed in the limitations of the study or methods section.”

This important point has been added to the discussion section (See pg. 16 of the revised ms.).

“It is not clear is the subjective SES scale is continuous or dichotomous in tables 2,3 and 4”.

This information was added as note to each table to clarify that the variable was dichotomous.

Number of items in discrimination scale is missing. Cronbach alphas with this sample should have been reported in methods section for continuous scales.

The number of items on the discrimination scale has been added to pg. 9 of the revised ms. and Cronbach alpha’s were conducted and added to pg. 12 of the manuscript.

“In table 1 it would have been easy to add p-values for differences between ethnic groups. Number of subjects in the analyses should have shown somewhere in other tables as well.”

P values for difference between ethnic groups have been added to Table Two. In addition number of subjects in the analyses has been added to the other tables.

“Relating to subjective information and cross-sectional design there should be some discussion how much psychological factors like neuroticism or depressive symptoms could cause the association between self rated health indicators and perceived stress.”

This is an important issue to address. We have specifically added the information that due to study design other important variables may have been overlooked and may be causal in nature (See pg. 18 of the revised ms.)

We thank you very much for your time with this manuscript. We feel it has been greatly improved by incorporating the comments and critiques of the reviewers.

Sincerely,

Jennifer M. Watson, PhD.