Author's response to reviews

Title: Individual and neighborhood-level socioeconomic characteristics in relation to smoking prevalence among black and white adults in the southeastern United States: a cross-sectional study

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Author's response to reviews: see over
Dear editor:

Thank you for the opportunity to respond to the comments of the three reviewers for our manuscript entitled “Individual and neighborhood-level socioeconomic characteristics in relation to smoking prevalence among black and white adults in the southeastern United States: a cross-sectional study” (BMC Public Health Manuscript #1652565545591746). We are very appreciative of the helpful suggestions provided by the reviewers and have revised our manuscript accordingly. Please see below for a point-by-point response to each reviewer comment followed by the location of the revision in the updated manuscript.

We look forward to hearing from you soon.

Sincerely,
Sarah S. Cohen, PhD

Response to reviewer comments

Reviewer #1

Reviewer’s report:
This manuscript describes an interesting study in which the authors describe the associations between current cigarette smoking prevalence and individual-level and neighborhood-level characteristics in a large group of black and white adults.

Compulsory revisions
1. I wasn't sure why the authors presented all the results stratified by sex and race. This would seem to substantially limit any comparisons between males and females, and black and white, which I would have thought would be of major interest. Indeed, in the discussion (page 9, first paragraph) the authors suggest that the associations varied to some extent by race and gender. This would suggest interactions were included in the models, however the stratified approach precluded that option. Also, the authors’ analytical approach mean that comparisons of the strata-specific prevalence ratios (as has been done on page 7, third paragraph) can only be descriptive, and not tested statistically.

RESPONSE: Based on differences observed along race and gender lines in the existing literature, we planned a priori to examine race and gender-stratified models. This decision was further supported by statistical testing of interaction terms between race as well as gender and the neighborhood summary score in our dataset. We have now included the results of these statistical tests in the methods section [Revision: p. 6, lines 23-26]

2. While the authors acknowledge in the results (page 6, first paragraph) that household income and educational attainment were generally low among both blacks and whites, this is not mentioned later in terms of how the results of this study can be generalized to the broader community. Comments such as those made in the last paragraph on page 9 would seem to be more appropriate if derived from a population-based cohort, rather than the current study cohort.
RESPONSE: Thank you for noting this oversight. We have now expanded the limitations section of the discussion to explicitly mention issues related to generalizability given the design of the SCCS [Revision: p. 13, lines 9-15]

3. Given the stated study objectives would seem to be an ideal candidate for the standard multilevel modeling approach, it would help if the authors provided more justification for the specific analytical approach that they used.

RESPONSE: We had the same thoughts as we began this analysis. We initially used multilevel models to estimate odds ratios for smoking with a block-group level random-intercept was specified to account for the nesting of individuals within block groups. We used maximum pseudo-likelihood procedures within the SAS GLIMMIX procedure for our calculations. However, we ultimately decided that the prevalence of smoking was too high to use the odds ratios as our primary measure of association and that the calculation of prevalence ratios was more appropriate. This decision meant that we needed to use log-binomial models which unfortunately led to convergence problems within the existing statistical software packages for this type of model due to the very large sample size in our study. We do believe that the statistical approach we ultimately used (marginal Poisson models with GEE) is appropriate for the analysis. Of note is that we observed similar results when we used multilevel modeling techniques to estimate odds ratios for smoking and in the analyses presented here with prevalence ratios (although as expected based on the high prevalence of smoking, the prevalence ratios were closer to the null value than the odds ratios overall). We have re-written the section on the statistical modeling to clarify our modeling approach for all readers [Revision: p. 6, lines 6-23]

Minor Essential Revisions:
4. First paragraph (page 3) – “recent work has begun to examine….“ Should include some references.

RESPONSE: Additional references including studies in both the United States, Great Britain, and Australia have been added [Revision: p. 3, line 6]

5. First paragraph (page 3, line 4). For clarity and consistency, “….increased smoking across..” should be “....increased smoking prevalence across...”.

RESPONSE: Thank you, this clarification has been made [Revision: p. 3, line 5]

Discretionary Revisions
6. The first paragraph under statistical methods (page 5) would seem better suited to the results section.

RESPONSE: This paragraph has been moved to the beginning of the results section [Revision: p.7, lines 5-14].

Reviewer #2

Reviewer's report:
This is a well conducted multi-level analysis of the individual and community-level correlates of smoking among a large sample of relatively socio-economically disadvantaged people in the USA. The findings, that community level disadvantage has a modest positive effect on smoking rates above and beyond individual level disadvantage, are consistent with much of the previous literature in this area. The key weakness of the paper is a lack of clarity in the rationale and a limited discussion of the significance of these potentially interesting findings.

Major Compulsory Revisions

1. The background is very brief and doesn’t do a great job of situating this study within the rest of the literature. The previous studies that have examined specific effects by race and gender should be described in a bit more depth and an explanation for why examining sex/race effects across a wide age spectrum might be interesting and useful – just because it hasn’t been done doesn’t mean it’s a worthwhile gap to fill. A couple of sentences expounding why this analysis is important is necessary here.

RESPONSE: Thank you for this important comment. The introduction has been entirely re-worked to emphasize the focus of this report, namely the lack to date in the literature of valid comparisons of effects of neighborhood SES on smoking across race groups and why these comparisons will inform the ultimate public health goal of developing novel interventions to reduce smoking [Revision: Introduction, p. 3]

2. As with the introduction, the discussion is a bit limited when it comes to what this paper is contributing to the literature. The sizable gap that is filled (race/gender specific models) is not really developed in any meaningful way beyond a sentence that says, “reasons for overall differences in neighbourhood level effects on smoking remain uncertain”. I think this is a bit half-hearted – the authors need to figure out why what they’ve found here matters, either in terms of future research questions or in terms of public health implications.

RESPONSE: As in the introduction, the discussion has been revised to more clearly explain the implications of this study in terms of public health implications and future research [Revision: Discussion, p. 10, lines 24-26; p. 11, lines 1-7]

Minor essential revisions

3. The methods section is clear until the middle of page 6, when a lot of methodological steps are compressed in a few sentences. In particular: there’s no justification for why duration of residence was controlled in all models, the explanation of the random intercepts and marginal models was minimal – non-experts should at least be able to follow the broad decisions being made here.

RESPONSE: Based on this comment as well as comment #3 by Reviewer #1, we have revised the statistical methods section to clarify our adjustment for duration of residency as well as the modeling techniques employed [Revision: p.6, lines 11-26]

4. Page 10, para 2: The discussion is stronger when focussing on the results between high- and low-income groups, although the final sentence doesn’t quite justify the findings of the study where high-income people were actually more affected than low income.
RESPONSE: We have now clarified the final sentence of this paragraph to be more consistent with the message of the observed results [Revision: p. 12, lines 10-13]

5. *I think the final para on page 10 should come earlier in the discussion section, laying out how the overall findings fit into the literature before drilling down into the race and gender specific results.*

RESPONSE: Thank you for this suggestion. The paragraph beginning “Collectively, our findings....” has been moved closer to the beginning of the discussion section and now precedes the discussion of race and gender-specific results [Revision: p. 10, lines 16-23]

6. *Page 11, para 2: these issues may explain inconsistencies across studies, but no attempt has been made to explain the internal inconsistencies of the current study – in particular the lack of association between neighbourhood disadvantage and smoking rates among white women. This incongruous finding needs to be discussed somewhere rather than ignored after being presented.*

RESPONSE: Thank you for noting this oversight. Indeed, the lack of neighborhood effects among white women was notable and a paragraph has been added to the discussion discussing potential reasons for this observation. [Revision: p. 11, lines 21-25; p. 12, lines 1-2]

7. *Page 11, para 3: I think the authors should at least consider here the impacts of using a largely disadvantaged sample – smoking prevalence of around 45% overall is about twice the US general population smoking rate. There must be some concerns about the generalisability of these findings that should be discussed here.*

RESPONSE: We appreciate this comment and agree that it is important to explicitly state this limitation of the study design. We have now included a discussion of generalizability in the limitations section of the discussion [Revision: p. 13, lines 9-15]

8. *Page 11/12: The large time period between the US Census data used for the neighbourhood level data and some of the individual level data (2000 vs 2009) should be discussed as a potential limitation – neighbourhood level disadvantage may have changed significantly in some areas over the decade in question.*

RESPONSE: We agree that this is a limitation of the study and have noted it as such in our discussion of study limitations. As now indicated in the text, participants did not enroll evenly over the 2002-2009 enrollment period of the SCCS; nearly 50% of the study population was enrolled by 2004 and only 14% were enrolled in 2008-2009. [Revision: p. 13, lines 22-25]

**Reviewer #3**

**Reviewer's report:**

**Major compulsory revisions**

1. The paper was very US-centric and gave little consideration to previous work published outside the US. Ideally, we want studies that produce findings that allow us to infer from
the specific to the general: it is more important to know if and how neighborhoods influence smoking behaviour in general (not just in the Southern US) and so a greater focus on neighborhood-based studies conducted elsewhere (e.g. Europe, Asia) would help broaden the relevance of the study. A quick search of Medline using the terms ‘neighborhood’ and ‘smoking’ identified 260 studies, many of which focused on SES and the contribution of the neighborhood context to smoking behaviours.

RESPONSE: We acknowledge that the bulk of the literature that is discussed in this manuscript comes from the US. We agree with the reviewer that there have been many large and well-conducted studies across the globe; a select number of these studies have now been referenced in the introduction. We would like to note that the reason for the US focus in this manuscript is that this analysis primarily deals with comparisons by race which has rarely been done in many studies outside the US. [Revision: p. 3, lines 7-8]

2. I would like to see the authors be more critically reflective about the sample used in the study. Clearly, this isn’t a population-based representative sample as it was recruited via CHCs which provide health services primarily to low SES persons. This approach introduces a number of limitations: for example, it is not possible to calculate a response rate for the study as there was no sampling frame; we have no idea how representative the sample is of the neighborhoods where the participants lived; and recruiting primarily low SES participants increases the socioeconomic homogeneity of the sample, so the SES differences that were reported are likely to be underestimated vis-a-vis the broader population.

RESPONSE: Thank you for this comment; we agree that the SCCS design is not strictly population-based (i.e. it is not a random sample of the country or the southeast) and the recruitment structure brings with it the limitations the reviewer has noted and this should be stated explicitly. However, we do feel that the unique design of the SCCS brings great advantages. We have now expanded the discussion to more clearly state the limitation of generalizability and to balance that limitation with the advantage of increased comparability across race groups [Revision: p. 13, lines 9-15]

3. For various reasons, 7,655 (10.5%) of participants were excluded prior to analysis: based on my experience with missing and excluded data it is very unlikely that these exclusions were random, and are likely to be related to SES; hence their removal would further reduce the socioeconomic variability of the sample.

RESPONSE: This is an important point and we appreciate the attention brought by the reviewer. As noted in the manuscript, 4.1% of the subjects who were excluded were dropped because their self-reported race was other than White or Black, an exclusion made in the design phase of the study (because of our a priori interest in comparing blacks and whites) and is unlikely to be related to SES. The 2.8% of the population with missing information on smoking and/or individual-level characteristics is of greater concern in terms of non-random missingness; however, this is a small percentage of missing data on SES measures for most large-scale epidemiologic cohort studies and a small enough proportion that the non-random missingness is unlikely to have affected the observed results [See Results, p. 7, lines 5-14]
4. The issues raised in points 2 and 3 might account for the small neighborhood SES effects observed in the study.

RESPONSE: We find it unlikely (as discussed above in our response to comment #3) that small effects of neighborhood SES were observed due to missing data. It seems more plausible that the small effects were due to the SES distribution of the individuals. However, it should be noted that while the majority of the cohort is of low SES, there are sizeable numbers of individuals with higher SES for comparison. Because of the large sample size, between 500 and 1000 individuals in each race-sex group had individual household incomes of $50,000/year and several thousand in each group had 16+ years of education. Further, there was notable overlap of individuals in the cross-tabulations of individual and neighborhood SES. [See tables 1 and 2]

5. Page 9. The authors note that the SES associations “varied to some extent by race and gender”. Why not test this formally with some interactions? Given the large sample-size it would seem to have sufficient power to test for interactions.

RESPONSE: Thank you for this suggestion. We agree that formal testing of interactions can be valuable and we did in the course of our analysis perform statistical testing for interactions with race (as well as gender) and the neighborhood advantage summary score. Both interactions were significant and details of these tests have now been included in the manuscript. [Revision: p. 6, lines 23-26]

6. Page 11: “A major strength of this investigation.....participants of generally similar individual-level socioeconomic and geographic situation”. I disagree with this statement: socioeconomic homogeneity is a limitation, not a strength. Significant and sizeable SES differences can only be detected in a sample with inherent socioeconomic variation. Again, this might account for the limited neighborhood effects that were observed.

RESPONSE: While we understand the reviewer’s comment that socioeconomic homogeneity is a limitation if the focus is on comparing effects across a spectrum of SES, we had a different focus in this paper. The strength we are trying to emphasize is the similar socioeconomic status and geographic situation between the race groups, i.e. that the black participants and the white participants are similar across measures of SES and geography. This is important because in many existing cohorts, and particularly those that are based on nationally-representative samples, SES is unevenly distributed between race groups by simple virtue of the SES differences that exist in the United States between races. In the SCCS, the black and white participants are of similarly low SES and live in the same communities which we believe reduces the residual confounding related to SES that is inherent to many other studies of this topic. [p. 3, lines 17-26; p. 12, lines 23-26; p. 13, lines 1-6]

7. Page 12: “However, it could be the case that current neighborhood may exert influence on the continuation of smoking even if it did not influence the initiation”. A broader reading of the neighborhood and smoking literature (beyond the US) would better inform this speculation, as work has been done on this issue. See for example: Giskes K, van Lenthe FJ, Turrell G, Bruch H, Mackenbach J. A longitudinal follow-up of smokers shows those living in deprived areas are less likely to quit. Tobacco Control 2006; 15:485-488.
RESPONSE: Thank you for bringing this area of research to our attention. The paper by Giskes et al. has now been added as a reference to this section [Revision: p. 13, lines 17-19]

Minor essential revisions
1. I acknowledge that the study was approved by institutional review boards, but did the persons visiting the CHC for health care know and give consent for their information to be used as part of the study? It would be easy to clarify this with an extra sentence indicating consent from participants was obtained (in addition to the review board approvals).

RESPONSE: Thank you for this important comment. All study participants signed an informed consent form at the CHC before beginning the SCCS baseline interview. This information has now been explicitly stated in the manuscript [Revision: p.4, lines 8-9].

2. Page 6: given the sample and the recruitment method, it is not surprising that the smoking prevalence was high as participants were mostly low SES.

RESPONSE: We agree with this statement and have clarified the results to make clear that the prevalence is likely high due to the large proportion of low-income cohort members [Revision: p.7, lines 18-20]

Discretionary revisions
1. I didn’t think Figure 1 was necessary and could be deleted without any loss of impact

RESPONSE: We appreciate that much of the information in this Figure is provided in some form in the text. However, if space is not a constraint, we would prefer to keep the Figure in the manuscript as we feel that it gives readers a useful snapshot of several pieces of important information. The figure succinctly shows the overall geographic area covered by our population, the location of the health clinics across the states, the relative density of participants across the area, and also highlights the high density of study participants in the deep South, an area that has often been underrepresented in epidemiologic research [Revision: None].