Author's response to reviews


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Author's response to reviews: see over
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Prof. Alan Lopez

Editor-in-Chief

PHM

Dear Prof. Lopez,

Please find attached our response to reviewers for our manuscript “Prevalence of Physical activity and obesity in US counties 2001-2011: A Road Map for Action”. We found the reviewers’ comments very helpful in strengthening our manuscript. I have included a revised version of the manuscript and detailed responses to the reviewers’ comments. We have also conducted further analyses to address the issues raised by the reviewers. Moreover, we have added a web appendix table with life expectancy for every county for all the years in our study.

On behalf of my co-authors, I would like to thank you and your reviewers.

Thank you again for your willingness to consider our manuscript.

Regards,

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**PA paper reviews:**

Below are comments from the handling editor of this paper. He would like you to consider addressing the following comments in a revised manuscript.

1. **Could you be more specific about the primary aims of the study?** At first glance, it appears as if you might be interested in the correlation/association between changes in physical activity and changes in obesity. However, this gets only 1 sentence of mention in the Results section (bottom of page 9).

**Response:** In this paper, we have two distinct aims, first, we want to document for the first time decade long trends in obesity and physical activity at the county level. This descriptive task is extremely important to help communities recognize where they are making progress or not on two major health risk factors. Second, we want to quantify the relationship between changes in physical activity and changes in obesity at the population level. Policy-makers in the US have assumed that there is a very strong relationship between changes in these risks, and we want to describe the nature of this relationship over the last decade.

2. **Furthermore, there does not appear to be a consideration of covariates that might be confounding and/or masking the association – such as population rates of smoking, alcohol intake, diabetes, use of anti-hypertensive medications, renal dysfunction, caloric intake, food environment (e.g., presence of fresh produce markets/food deserts, availability of junk food), public health fitness programs, access to medical care (including rates of insurance coverage, Medicare spending rates and variations as might be ascertained through the Dartmouth Atlas), and recreational facilities.** I find the failure to consider estimated smoking prevalence concerning, since smoking would be expected to decrease BMI and decrease physical activity.

**Response:** In our paper, we characterize the observed relationship at the population level in change in physical activity and change in obesity. While many factors may confound the observed levels of obesity and PA, confounders for change in PA and obesity would be a smaller subset. A confounder for this change on change relationship would have to be something that is changing substantially overtime and could be causally related to both PA and obesity. The reviewer correctly points out that changes in tobacco consumption could be a confounder: decreases in tobacco would increase obesity and potentially increase PA leading to an attenuation of the PA-obesity relationship. The other confounders mentioned by the reviewer fall into two groups. One group including fresh produce markets, food desserts, availability of junk food, public health fitness programs all operate directly on PA or obesity as such they are not really confounders but contextual factors that help understand why caloric intake and energy expenditure may diverge. They are part of the reason the relationship between PA and obesity may be attenuated. The second set access to medical care, renal dysfunction, anti-hypertensives, we simply do not understand how change in these could influence change in obesity or change in PA.

To address the important questions raised by this reviewer and one below on the potential for confounding in assessment of the population level relationship between PA and obesity, we have
elaborated the discussion. We have also added to the results a regression analysis of the change in obesity on the change in a range of factors that we are able to track at the county level. These factors include change in physical activity, change in smoking, poverty, unemployment, number of doctors per 100,000, percent rural and baseline levels of obesity. In this regression analysis, we find the effect of a change in physical activity at the county level remains very close to the simple univariate relationship. We have also elaborated in the discussion the potential effects of measurement error on these relationships.

3. **Could you be more specific about use of NHANES data? Does it adequately account for self-reporting bias present in BRFSS? Do NHANES subjects report their heights and weights over the phone like BRFSS subjects? Or in person? One might imagine less self-reporting bias with in-person reports. Also, do NHANES subjects know that their heights and weights will be checked objectively?**

**Response:** We totally agree with the reviewer. In NHANES, respondents tend to over-report their height and under report their weight but at lower levels than those seen in BRFSS. The reason is that the NHANES respondents know that they will be measured and the interviewer is looking at them. For this reason we compare measured BMI from the examination portion of the NHANES directly to self-reported BMI from the BRFSS, not to self-reported BMI from the NHANES. Since we can’t match up individuals across the two surveys (and, indeed, most individuals wouldn’t have been interviewed in both surveys) we instead match up the mean BMI for each age and sex group in each survey cycle. We thank the reviewer for pointing out that this was not clear and have clarified this in the relevant parts of the methods section.

4. **Please be aware of your use of causal language, especially the conclusion that “increased physical activity has no major impact on obesity levels...”, as this is a correlative epidemiological investigation, not a randomized cluster trial.**

**Response:** We totally agree and we have toned down our language throughout the text.
Bruce Neal:

Comments to authors

This is a nicely written paper with a clear message. It certainly warrants publication. The methods are strong and the limitations clearly identified. Appropriate steps have been taken to understand the extent to which systematic or random errors might account for the results. It is my judgment that the right conclusions have been drawn and that the findings are highly significant. One thing I would like to see is a much stronger conclusion drawn in terms of the enormous responsibility this places on the food industry and those in government that have the power to control the food industry. It is only in this regard that the paper falls short. Food and beverage companies are divesting themselves of responsibility for the US obesity crisis and are using physical activity programs as a smokescreen to distract from the massive adverse effects their products are having on waistlines. This powerful piece of research has much more to say in that regard than is actually stated. If this cannot be addressed in the paper perhaps there can be an accompanying editorial that picks up on this point because it is vital that it comes across, loud and clear.

Response: We agree with the reviewer and have added more on this in our conclusion. We also welcome the idea of an editorial with our manuscript and we hope that the reviewer would do so.

I have a few more specific comments on the paper:

Methods

There is a note that the NHANES response rate needs adding for other years. This should be done.

Response: We totally agree and have added these in the text.

There also appears to be unnecessary repetition about response rates in the methods section.

Response: We thank the reviewer and have corrected this oversight.

Results

I would like to know how big the differences were between the findings of the four models. Does the selection of variables included change the qualitative conclusions for any outcome? Or are the differences just quantitative in nature. And if the latter are the differences large or small? It seems unusual for this group of researchers not to provide more insight into this aspect of the work.

Response: The estimated level of obesity or physical activity for a given county varies between the four models. However, the large scale regional and temporal trends are robust to the choice of model. Similarly, we find weak correlations between change in physical activity and change in obesity with all models. the strength of our analysis is that we have a validation procedure which allows us to empirically evaluate the relative predictive performance of the several models we consider. Thus all estimates reported are based on the model with the best predictive power.
Discussion

Its rates of IHD, not rats

Response: We thank the reviewer and have made the correction

Is it right to blame Americans? The “personal choice” perspective implied is a pretty old fashioned way of thinking about causation in obesity. It’s almost certainly much more about food environments that, while protecting the populace from acute illness caused by spoiled food, subject the population to perpetual long-term food poisoning that causes chronic diet-related ill health on an industrial scale. It’s the transnational food corporations, the advertising agencies and the weak regulatory environment that the American government oversees that is responsible. These are now the chief vectors of disease in the US.

Response: We totally agree and we have made the statement more balanced. We however, feel that there is a key role of individuals within a given socio-cultural context in fighting the obesity epidemic. Put another way, to respond to the food environment requires both efforts to change that environment and more informed and empowered consumers who are motivated to change food intake.

As per the comment in the results section, it would also be helpful to have a bit more of a description about what was the thinking behind the inclusion of each set of variables in the different models that were fitted. And how the findings varied for each model, for each outcome and what this might mean for the robustness of the estimates obtained.

Response: We have added a description of why we chose each county-level covariate to the methods section. We chose this set of covariates based on an exhaustive covariate selection procedure wherein we fit all possible models given a particular set of covariates (and subject to certain constraints: e.g. that all race variables be included or all excluded) and chose the set of covariates to include in our small area models based on which model performed best on the Akaike Information criterion. In both the case of obesity and PA, the final set of covariates was actually also the initial set of covariates—the models including all of the covariates we considered were ranked the highest. In terms of the small area models, we did not test different small area models with different sets of county-level covariates. However, if you compare the performance of the Full and Covariate models (which include these county-level covariates) to the Naïve and Geospatial models (which do not), there is a significant increase in predictive power and model performance as evidenced by a noticeable decrease in the root mean squared error and mean relative error and a noticeable increase in the concordance correlation in the cross-validation procedures we have carried out on these models and described in the methods section.
Chaoyang Li:

This is an excellent study, in which the researchers examined the county level changes in the prevalence of physical activity and obesity using data from the Behavioral Risk Factor Surveillance System (BRFSS). One of the major strengths is that they used measured weight and height data from the National Health and Nutrition Examination Survey to adjust BRFSS data for self-reporting bias in height and weight. This paper is well written. The methods are appropriate and well described.

Response: We thank the reviewer.

There are no suggestions for major compulsory revisions. The following are suggestions for minor essential revisions or discretionary revisions:

Abstract:

1. Please consider changing “a state based random-digit telephone survey that covered all United States counties” to “a state based random-digit telephone survey” or “a state based random-digit telephone survey that covered the majority of United States counties.”

Response: We have made the change.

2. Please consider changing “a nationally representative sample” to “a nationally representative sample of the U.S. civilian non-institutionalized population.”

Response: We agree and have made the change.

3. The conclusion, “but a reduction in caloric intake is needed to curb the obesity epidemic and its burden” does not seem to be well supported by the results of this study. A modification on this sentence is recommended.

The relatively weak correlation between changes in physical activity and changes in obesity at the very least suggest that either increases in physical activity lead to some increases in caloric intake or other factors including confounders such that the observed relationship overtime is not strong. In either case, promotion of physical activity alone given the experience of the last decade may not be enough to reverse the obesity epidemic. We have reworded the sentence as suggested to reflect this nuance.

Methods:

4. Please consider modifying the sentence “The NHANES is a nationally representative cross-sectional survey of self-reported health and an extensive array of biomarkers” to summarize the NHANES survey and its contents more accurately and precisely. Please refer the following website: http://www.cdc.gov/nchs/nhanes/about_nhanes.htm#intro

Response: We agree and have made the changes.

5. It may be more informative to assess the change in average body mass index (BMI), in addition to obesity prevalence.
Response: We thank the reviewer but we felt that obesity prevalence would be more appropriate for our message. More importantly, we directly estimate the prevalence of obesity in our models and not BMI. Estimating BMI would require a different modeling strategy; one that is worthy of pursuing in future research.

Results:

   6. It might be more informative to report the change in average BMI over time among persons who had normal weight (BMI<25 kg/m2), who were overweight (BMI 25-29 kg/m2), or who were obese (BMI≥30 kg/m2) in addition to obesity prevalence. Please consider adding this information to Figure 6.

Response: The dependent variable in our mixed effects regression model is obesity status, not BMI. This type of analysis would, as noted above, require a different modeling strategy and is beyond the scope of this paper.

Discussion:

   7. Since this is an ecological study, two additional limitations may be discussed. First, ecological design may not enable ones to a causal inference for the observed associations. Second, ecological fallacy may exist; thus it may explain in part the discrepancy in the association of physical activity and obesity between population data and individual data.

Response: We have added two additional limitations to our discussion: that an area-level analysis may not be appropriate for drawing conclusions about the individual level and also that our study reports on correlations which may not be causal in nature.
Colin Bell:

- Minor Essential Revisions. The author can be trusted to make these. For example, missing labels on figures, the wrong use of a term, spelling mistakes.

Response: We thank the reviewer and we have made these corrections.

1. Abstract conclusion: Be specific about the positive health impacts of physical activity

Response: We agree and have added this to our abstract about the benefits of obesity from previous studies.

2. Methods paragraph 5: There appears to be an over-adjustment for race in the covariate model which includes both race/ethnic group and racial composition of county. The authors should explain what the county racial composition adds to the model.

Response: We thank the author and have clarified this in our text. We have included both individual level and county-level race covariates to account for the fact that there may be both direct effects of race (i.e. the effect of being a particular race) as well as contextual effects (i.e. the effect of living in a county with a given racial composition)

3. Table 1 and 2 were not supplied with the article

Response: We apologize and we have included them in our revisions.

4. Figures 4, 5 and 6 and tables 1 and 2 should refer to as obesity ≥ 30 kg/m² rather than > 30 kg/m². Hopefully this is just an error in the presentation and not in the analysis.

Response: We thank the reviewer, this is an error in the title not in our analyses, we have corrected the mistake.

5. Results para 3: Can the authors please quantify the largest decrease in obesity prevalence for men and women and name the relevant county?

Response: We have added this information to the text.

6. Conclusions para 1 and 2: These paragraphs appear to lay the responsibility for physical activity and healthy weight on individuals. Particularly the statement “It is possible that for a segment of population, neither the will nor the physical or mental capacity exist to engage in physical activity.” It is highly likely that the physical, financial, social and policy environment in some counties account for the variation observed and therefore society as a whole should share responsibility. Some comment along these lines is warranted.

Response: We agree and have made it more balanced. We however, feel that there is a personal responsibility as well in addition to the societal and other important factors. We have added text regarding this point and pointed out that personal role is one of them.
7. Conclusions: replace meant with mean in the following sentence Second, if caloric intake was substantially over the level required for energy balance, the reported 74 Kcal/day reduction in intake may still meant the US was not in energy balance despite higher levels of physical activity.

Response: We thank the reviewer for noticing this error and have corrected it in the text.

8. Conclusions: Final paragraph. Given that the manuscript is primarily about physical activity and obesity it would make sense to focus the conclusions on how best to strengthen physical activity and the relationship of physical activity to obesity

Response: We agree and have added language to that effect
Rachel Huxley:

- Minor Essential Revisions

1. **It would be helpful if the authors could compare these data on change in the prevalence of physical activity with NHANES data that would seem to indicate no change in physical activity between 2001-2006 (Carlson et al. 2006).** In that paper the authors discuss how differences in the surveillance systems yield different physical activity trends. In particular, and of relevance to this current report, the BRFSS reports the highest level of physical activity (compared with NHANES and NHIS) possibly because it includes more domains of physical activity than other surveillance systems. Moreover, it has been suggested that survey questions that require respondents to recall participation across multiple domains of physical activity may overestimate their physical activity level. Another difference that should be mentioned in the limitations section is that the BRFSS questionnaire is a telephone survey (unlike NHANES which was administered by questionnaire) which could give rise to any number of biases. In particular, the sampling frame of the BRFSS excludes households without landlines, and if the prevalence of landline usage declined over time then this raises concerns about non-response and generalizability.

Response: We thank the reviewer and have added a discussion on the Carlson et al. in our discussion. The main difference in both surveys is the geographic location as BRFSS is in almost all counties while NHANES in in 15 large urban cities (counties). The percentage of households with no landline telephones has not changes much in the US during that period has remained around 2%. The CDC National Center for Health Statistics tracks the telephone coverage in the US and has shown no change http://www.cdc.gov/nchs/data/nhis/earlyrelease/wireless201212.pdf. We agree that the questions are slightly different in both surveys but we are using BRFSS questions throughout. However, we have added a limitation on the differences of the 2 surveys in these aspects.

2. **The authors should be more cautious when stating in the results that “there was a poor correlation between level of physical activity and obesity in the US counties”. It is entirely possible that uncorrected measurement error in ascertainment of BMI (based as it was on self-report, that was corrected using NHANES adjustment) and physical activity could have attenuated the association. It is also possible that the data mask different patterns in rural and urban areas (the authors themselves allude to the possibility that in the Discussion that “the behaviors of residents in urban settings are different from those in rural areas”. In a recent paper from NHANES (Befort et al. 2012 Prevalence of obesity among adults from rural and urban areas of the United States: Findings from NHANES 2005-2008. The Journal of Rural Health) the prevalence of obesity was much higher (39.6%) among rural compared with urban populations (33.4%) and that there was an interaction between obesity with activity level by area such that being physically active was significantly associated with a reduced prevalence of obesity in urban but not rural areas.
Response: We totally agree about the comment and have added this as a limitation of our study in the discussion section. Changes in self-report bias over the time interval could attenuate the observed relationship between obesity and PA. We have also tested whether change in obesity is different in rural and urban areas by including a dummy variable in our regression of change in obesity on change in PA and change in other factors.