Author's response to reviews

Title: The short-term association of selected components of fine particulate matter and mortality in the Denver Aerosol Sources and Health (DASH) study

Authors:

Sun-Young Kim (puha0@uw.edu)
Steven J Dutton (steven.dutton@epa.gov)
Lianne Sheppard (sheppard@u.washington.edu)
Michael P Hannigan (hannigan@colorado.edu)
Shelly L Miller (Shelly.Miller@Colorado.edu)
Jana B Milford (Jana.Milford@Colorado.EDU)
Jennifer L Peel (Jennifer.Peel@ColoState.EDU)
Sverre Vedral (svedal@uw.edu)

Version: 3
Date: 16 May 2015

Author's response to reviews: see over
May, 15, 2015

Dear Editor,

We are resubmitting our revised manuscript after taking into account the suggestions of reviewers. All our revisions were made for reviewers’ additional suggestions. The title of the manuscript was changed to reflect a suggestion of a reviewer.

The manuscript is original work, has not been previously published whole or in part, and is not under consideration for publication elsewhere. All the authors have read and agreed with the responses to the comments and the revision of the manuscript.

Thank you for your consideration of this revised manuscript.

Sincerely yours,
Sun-Young Kim
Research Assistant Professor
Institute of Health and Environment
Seoul National University
Seoul, 151-742, Korea
phone: +82-2-880-2759
email: puha0@uw.edu
Reviewer's report 1

COMMENTS TO AUTHORS

I appreciate the willingness the authors showed in rethinking part of their work in accordance with the reviewer’s comments. However, I have a few additional observations about the changes the authors have made in their revised version.

We appreciate additional comments and provide our responses below.

Major compulsory revisions

1. My second point has to do with the authors’ attribution of OC to the primary aerosol fraction. Now, although, in their revised version, they recognize the advisability of addressing this hypothesis in some future source-apportionment analysis using much richer PM components, they nevertheless conclude that there is a stronger association of mortality with “EC and OC than with secondary inorganic aerosol”, suggesting that EC and OC are essentially primary aerosol. I think this conclusion should be avoided, since it is not definitively supported.

We revised our conclusion to clarify that the associations of EC and OC are stronger than secondary inorganic aerosols, sulfate and nitrate. Use of “secondary inorganic aerosols” should not be taken to imply anything about secondary organic aerosols, such as secondary contribution to OC. The revised text reads (page 15, line 18 in the change-tracked manuscript):

“Associations were stronger for EC and OC than for sulfate and nitrate, secondary inorganic aerosols, especially in those with cancer as an underlying cause of death.”

Discretionary revisions

1. The first of my previous comments was about the lack of analysis for metals. The manuscript already mentioned this as a limitation and attributed their not having analyzed them to the short period about which data were available; it seems instead that the scarcity of analyses of metals is itself the problem. I suggest that the authors change the title, replacing “components” with “PM2.5, EC and OC”.

We changed the title to clarify our use of the restricted numbers of components. We added ‘four’ before ‘components’ instead of addressing specific components in order to avoid a long title. Below is the revised title.

“The short-term association of selected components of fine particulate matter and mortality in the Denver Aerosol Sources and Health (DASH) study”

Also, we revised the text to clarify our aim of our analysis focusing on these four components in the Introduction. The revised text is (page 5, line 23; page 6, line 1):
"Here we report findings from the DASH study on associations of daily concentrations of PM$_{2.5}$ and four PM$_{2.5}$ components (EC, OC, sulfate and nitrate) with daily mortality attributed to the most common causes of death in the 5-county Denver metropolitan area over the 5 years of the study."

2. The sentence with which the authors justify the contradictory correlation of PM$_{2.5}$ with nitrate (very high) and with NO$_2$ (very low) is really unclear and difficult to accept, as it assumes there are some reactions of NO$_2$ which form nitrates in winter, and that this is what causes an increase in PM$_{2.5}$ concentration. I would suggest that the authors simply mention the unusually low correlation or, if they wish to make assumptions, that they describe them more clearly.

We deleted our hypothesis for reactions between PM$_{2.5}$, nitrate, and NO$_2$ and stressed the low correlation. The revised text reads (page 9, lines 13-16):

"There was the high correlation of PM$_{2.5}$ with nitrate, but distinctively low correlation with NO$_2$, along with the negative correlation with temperature."

3. In addition, the change the authors have made in order to justify their contrasting results for nitrates (low effects), as compared with those found by Ostro (clear effects), is a bit contorted. It would be more useful simply to underline the contrast, without adding any improbable-sounding hypothesis.

We deleted our explanation for different population characteristics and statistical methods which turned out to be relatively less likely as hypotheses for different findings from Ostro et al based on our sensitivity analyses.

4. Finally, my last major comment involved the results regarding cancer mortality, which usually shows limited increases for short-term exposure, in contrast with the results given in the manuscript. I suggest that the authors, in this case too, only underline the contrast. The hypothesis of a PM$_{2.5}$ contribution to indoor levels cannot justify the difference from other studies carried out in countries with hot climates, where opening the windows could be thought to be a more frequent behavior.

There are actually very few time series studies in which findings on cancer deaths have been reported. In light of our finding, we feel that it is appropriate to pose plausible explanations in the Discussion. We believe that viewing those dying of cancer as a frail and susceptible population, coupled with the observation that temporal contrasts of outdoor PM$_{2.5}$ are paralleled by those indoors, provides a plausible explanation for our findings regarding short-term ambient air pollution exposure association with cancer deaths. Thus, we prefer our current description without further revisions.
Reviewer's report 2

Minor Essential Revisions

In reference to my previous comment 1), in the paper by Dai et al (2014) the authors do find a significant association with cardiovascular mortality (note that they report percent changes, thus results are significant if the intervals do not cross zero). Thus, the text should be changed.

We appreciated this comment. The revised text reads (page 11, lines 17-19 in the change-tracked manuscript):

“Another meta-analysis study in 57 U.S. cities, however, found a short-term association with cardiovascular mortality but stronger association with total mortality [21]. As opposed to single-city time-series studies of PM$_{2.5}$, a few recent time-series studies focusing on PM$_{2.5}$ components found higher effect estimates for cardiovascular mortality than total mortality.”

The results of cancer and non-cancer mortality shown in Figure A1 are interesting in my opinion should be included as supplementary material instead of "data not shown".

We added Figure A1 as a supplemental figure.

In reference to my previous comment 4), I still think power could be an issue. In the new sentence, the authors compare their study with references 1 and 3, but they should also compare it to Dai et al. (ref 21), which contains 1.5 million deaths from cardiovascular causes. Many of the significant associations they found are around a RR of 1.01 (percent change = 1.0), and the present study often fails to declare similar associations as significant.

We added the possible impact of lack of power based on Dai et al. to the Discussion (page 12, lines 6-7).

“While these two studies had generally larger numbers of daily deaths for cardiovascular diseases than ours, suggesting that power may have been better, the daily sampling of PM$_{2.5}$ components for 5 years actually resulted in comparable or better power in the DASH study compared to those studies which used regulatory monitoring data measured every 3rd or 6th day for 4 and 3 years, respectively. It is possible that there is still insufficient power in single city studies, however, in light of the evidence of associations for PM$_{2.5}$ from multi-city studies [21]. … Future studies expanded to other areas and multi-city settings may allow for better assessment of the association of PM2.5 component with cause-specific mortality.”