Atmos. Chem. Phys. Discuss., 15, C8696–C8700, 2015
www.atmos-chem-phys-discuss.net/15/C8696/2015/

Atmospheric Chemistry and Physics Discussions
Open Access

ACPD
15, C8696–C8700, 2015

Interactive Comment

Interactive comment on "Exploring the uncertainty associated with satellite-based estimates of premature mortality due to exposure to fine particulate matter" *by* B. Ford and C. L. Heald

Anonymous Referee #3

Received and published: 29 October 2015

The authors present an interesting paper in which they estimate the health burden of PM2.5 in the US and China, compare those estimates with previous studies, and then explore uncertainties in the calculation due to satellite estimates of PM2.5, health function parameters, etc. The paper is unusual in its detailed treatment of atmospheric science and satellite retrievals, as well as concentration-response functions within a single paper. To my mind, this is both a strength of the paper – as different uncertainties are addressed within a single paper – and a weakenss, as the discussion ranges over a wide body of literature and can be hard to follow at times.

Overall, my sense is that the paper is a worthwhile addition to the existing literature.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

However, I feel that the presentation of the complex discussion can be improved and I have some general questions or concerns about the approach:

1) It seems that the main points of the paper are summarized in Figure 9. Differences in health burden are presented when exposure estimates are driven by the model vs. two satellite estimates, and then uncertainty analysis is performed on 3 parameters individually. Given that the uncertainty due to individual parameters has been estimated by the authors, I am surprised that they did not take the next step to estimate an overall uncertainty given uncertainty in those parameters individually. Also, is the uncertainty in CRF in Figure 9 a simple uncertainty given the confidence intervals from a single study, or does it somehow account for uncertainty as illustrated in Figure 8 or Table 4?

2) The goal as stated p. 25354 is "to explore how mortality burden estimates are made and how choices within this methodology can explain some of [the discrepancies among previous studies]." The authors have succeeded in estimating how different modeling choices or parameters contribute to the overall uncertainty. But as there are many differences among many different studies, I don't know that this paper helps to clarify those differences in results – or it certainly does not explain why a particular study is high or low vs. others. The results shown are not surprising given the current literature, and since previous studies have often included analysis and discussion comparing their results with others, I'm not sure that the authors add something new here. The results are interesting and seem to add to the literature, but I'd encourage the authors to think harder about what is new and present that more clearly.

3) Related to #2, despite the complexity of the paper and its extensive discussion, I thought the bottom-line messages were rather few. The authors should consider reorganizing in places to reduce repetition, and/or removing excessive discussion.

More specific modeling questions & concerns:

1) I might be wrong, but I'm not aware that anyone uses a linear function as described in equation 3.

ACPD
15, C8696–C8700, 2015

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

2) They assume the C0 to be zero. I don't think that there are other studies that use zero as C0, and I am concerned that it requires a significant leap of faith to assume that the same concentration-response relationships hold at concentrations below which we have observations. If the authors keep this assumption, they should do more to discuss and justify this choice.

More specific comments: - The title focuses on satellite-based estimates, but model estimates are also used here, and uncertainty in concentration-response factors are also a major focus of the analysis.

- Given the interest in models, satellites, and ground observations in the paper, I am surprised the authors didn't mention "data fusion" types of approaches such as Brauer et al (EST, 2012), who did data fusion to underlie the Lim et al. global burden estimates. Do data fusion studies reduce these uncertainties? The question may be beyond the scope of the paper, but I thought it deserved at least a little qualitative discussion.

p. 25331, l. 8-19 – The discussion here seems to mix up estimates of concentration to drive epidemiological studies vs. concentrations to drive risk assessments. This also seems to be confused a few times later in the paper. I would think that using concentrations to drive risk assessments would be the main purpose here. The last sentence of this paragraph I don't think is true – many epidemiological studies consider health effects for whole populations using monitors as imperfect estimates of concentration (and not as estimates of personal exposure).

p. 25332, l. 8 – "both" is ambiguous here since you've just talked about monitoring, satellites, and models.

p. 25334 top – it's not clear to me whether one value is used for the whole US or if different values are used in different states. If the first, then why is it important to start with state level data and use gridded population?

p. 25336 – The authors are correct that different studies use the terms linear and loglinear in different ways. But the discussion here doesn't quite clarify how the authors are using these terms.

p. 25338 top – what is the source for emissions for the rest of the world?

p. 25340 bottom shows model performance compared to IMPROVE and AQS. In contrast, p. 25341 top discusses AERONET AOD, but presents no model performance evaluation.

p. 25341, l. 23 – what does "initial fraction" mean?

p. 25346, l. 12-15 – This discusses how compensating errors may be hidden by NMB. For that reason, it is common to also present Normalized Mean Error.

p. 25352 bottom – this long discussion of low thresholds might seem more appropriate to present in methods (there is some discussion there) or in a discussion sector.

p. 25354 – Is this the first time Figure 9 is referenced? I find it a little strange to present a new figure in the conclusions section.

p.25355, l. 14 – "correctly applying response functions is a determining factor" Are the authors claiming that some previous studies have done this incorrectly? I would think that there may be better or worse choices to make, but that authors may have reasons for choosing the approach that they do. I also wouldn't call these "epidemiological approaches" because these are risk assessment studies, not epidemiology.

p. 25355, l. 17 – "using only populated places" I don't understand what this means. There should be no health effects in unpopulated places.

Table 1 – I'm surprised that uncertainty is shown for only one study – certainly at least some of the other studies also estimated uncertainty. Also, Punger & West 2013 estimated US PM2.5 burden. Zheng et al. isn't listed in the references.

Figure 2 is pretty small and difficult to read. Is it true that all of these studies are chronic PM2.5?

ACPD
15, C8696–C8700, 2015

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

Figure 8 & 9 – units should be “deaths per year”

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 25329, 2015.

ACPD
15, C8696–C8700, 2015