“Exploring the Uncertainty Associated with Satellite-Based Estimates of Premature Mortality due to Exposure to Fine Particulate Matter” by Ford and Heald.

This study estimated the premature mortality in US and China by using satellite data and GEOS-Chem model simulations, and quantified the uncertainties of the results caused by different methods and dataset used to derive. The study is useful to constrain the estimated health effect due to increased concentrations of fine particulate matter with satellite-based observations.

I have a few major concerns and some specific comments as below. Firstly, the relationship η, which links PM2.5 and AOD, is derived from the GEOS-Chem simulation in this study, although the authors have conducted a couple of sensitivity experiments to understand how much difference would be caused due to the uncertainty in η, I am curious that how these would be different from the real η if directly linking the surface PM2.5 and satellite AOD. Secondly, the relative risk (RR in the paper), which is a key factor to determine the premature mortality due to exposure to PM2.5, differs significantly because the pathophysiological mechanisms are currently unclear. The authors assessed the uncertainty of the estimated mortality rate by using different PM2.5 concentration response function. I wonder is it possible to give us a “better choice” for the study region such as US and China? Finally, the authors have conducted a few sensitivity experiments to test how different factors impact their estimations, which is a good attempt to improve our understanding. The disadvantage is lacking of the detailed explanations and discussions on theses sensitivity results.

Specific comments:

p.25333, at top of the page, it is difficult to see here how PM2.5 contributes to health from the equations Eq.1 and 2; please add an equation to describe the link between PM2.5 and RR, if possible.

p.25333 last paragraph, You use crude death rates, instead respiratory disease, to determine baseline mortalities, which will overestimate the burden of death due to air pollution. Can you find and use the death rates from non-accidental death? In

China, it is even cruder as population rather than death rate is used to estimate. Can the authors estimate the biases caused by this?

Fig.2. the text is too small to see, I suggest the authors to make this figure bigger.

Table 2: table caption, "… in Eq. (8)…", should be Eq.(7).

p.25339, line 5-20, You removed the satellite observations with high AOD (>2.0), can you explain how do you decide this threshold? since AOD could be very high (over 2.0) in some cases, e.g. heavy pollution?

Fig.5. How do you compute the values shown in Fig.5? Which field in Eqs corresponds to the results shown here? can you clarify that if the results are P in Eq. (2), or others?

p.25344, bottom paragraph. It would be good to give a plot to show the AERONET sites used in the comparisons in both US and China. The quantitative comparison of AOD between satellite and AERONET is not shown in a plot and/or table, and not even given in the text. Please include these comparisons.

p.25347 and Fig.7: As I can see the NMB is apparently largest in Southeastern China from the experiment vertical profile, but there are no explanation in the text. For the test Relative humidity, there are positive NMB in Southeastern and Northeaster China, but negative NMB in western and Central China. The necessary explanations and discussions are needed in this sensitivity tests.

Figure 8, figure caption "… in Table 3", should be Table 4.

Figure.9, I suggest to move the Figure 9 and associated text into section 4, rather than last section, i.e. section 5.