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Interactive comment on “Model calculated global, regional and megacity premature mortality due to air pollution” *by* J. Lelieveld et al.

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We thank reviewer#1 for the comments. Please find below our replies.

The health impact functions we applied are not new, indeed being the same as in Anenberg et al. (2010), because these functions are based on the most comprehensive epidemiological cohort studies available and are widely acknowledged as being the most representative. Furthermore, we do not see the problem of using our particular numerical model as it provides more geographical details than previous work, and also refers to a different period. Some overlap with previous work is actually quite useful as the comparison in section 5 reveals rather large discrepancies between studies. In section 5 we also discuss differences between our work and Cohen et al. (2005) and Anenberg et al. (2010). We believe it is important to not to merely rely on the Global

Burden of Disease (GBD) report, which does not provide country level information other than the percentage contribution to DALYs by ambient PM pollution. Especially since the outdoor air pollution impacts component of the GBD study has not yet been published we do not agree that the GBD report limits the novelty of our work.

We nevertheless agree that it is appropriate to consider the recent GBD study in our manuscript, and in the revised manuscript we will address it in our comparison with previous work (section 5), including the articles by Lim et al. (2012) and Brauer et al. (2012). The latter is mentioned as the basis for the GBD study although it does not address health impacts. In our revised manuscript we will add similar results as presented by Brauer et al (2012) for comparison (e.g. the fractions of global population exposed to particular levels of air pollution). The global PM2.5 data of Brauer et al. (2012) are combined from observations and models, notably the TM5 chemistry-transport model, while the ozone data rely solely on TM5 (see also Lim et al., 2012), hence having similar limitations as our study. Although we do not refute that Brauer et al. (2012) present innovative and state-of-the-art results, we do not accept the implication that our results are not state-of-the-art.

The global results presented by Lim et al. (2012) indicate 3.2 million deaths due to particulate matter pollution in 2010 (our study indicates 2.2 million in 2005) and about 0.15 million due to ozone pollution (our study indicates nearly 0.8 million in 2005). The relatively low number for ozone by GBD seems inconsistent with the results of earlier work that applied the concentration response factor (CRF) of Jerrett et al (2009). We suspect that another CRF was used, e.g. by Bell et al. (2004), yielding much lower mortality by ozone, mostly because only short-term health effects are accounted for, whereas Jerrett et al. (2009) also include long-term effects. Unfortunately, the use of epidemiological parameters such as CRFs has not been documented by Lim et al. (2012), which impedes the comparison. The difference between GBD and our study for PM2.5 is largely explained by the fact that we do not include natural PM2.5 whereas GBD does. Our preliminary results for desert dust, for example, suggest a premature mortality of 0.65 million per year. If this number is added to our results (2.2 million/year) the discrepancy with GBD reduces to about 10%, which in turn can be accounted for by the growth of aerosol pollution in Asia between 2005 and 2010. We believe it is important to uncover and discuss these discrepancies between different studies, which can also help putting the GBD results into perspective.

As suggested by the reviewer, we checked the country level mortality results from the GBD study (http://www.healthmetricsandevaluation.org/gbd/visualizations/country), presenting causes of premature death. Even though ambient PM air pollution is listed as one of the main risk factors of disease, we could not find any data that can becompared to our results.

The country rankings are based on country specific baseline mortality rates when available. For the other countries regional baseline data are used. The regions (WHO strata) are given in Table A2. Even though the use of country level data throughout would be preferable (unfortunately not available), the regional distinction is quite relevant as it clusters countries with similar mortality characteristics. Please note that other studies (including GBD) work with the same data sets, being the most recent ones provided by WHO. We will more clearly indicate caveats in the revised manuscript, as suggested.

Although our model resolution is substantially higher than previous work, we agree that a higher resolution than 1.1 degrees would be even better. Although Brauer et al. (2012) used the TM5 model for ozone (with regional nests for Europe, N-America and Asia at 1° resolution, the rest of the globe is at 6°x4°; the emissions are at 1°), they compiled a dataset at 0.1° resolution for PM2.5 by using an urban subgrid parameterization based on bilinear interpolation. Similarly, global satellite datasets of MODIS and MISR have been refined to 0.1° resolution. Since satellite data have limitations (fixed time overpass, cloud issues etc.) and need to be converted from column-integrated radiances to surface PM2.5, models are used to derive surface level data and interpolate to higher resolution. Although it is commendable that these datasets are stated to

represent 0.1° resolution, we prefer to apply a high-resolution version of our chemistry-general circulation model, consistently representing all pollutants and meteorological processes with a time step of 12 minutes (TM5 applies 3-6 hourly mean meteorology), and use the satellite data and other observations for independent evaluation (for which we provide several references). In the revised manuscript we will provide additional comparison of our model results with MODIS retrieved PM2.5 and other data. If the datasets of Brauer et al. (2012) could be made available, we would be happy to perform an inter-comparison with our model results.

Anenberg et al. (2012) presented sensitivity calculations based on different CRF assumptions (affecting the slope of CRF). Similarly, we will examine the influence of the shape of the CRF in our discussion of uncertainties in the revised manuscript, as suggested. The most important issue is the selected CRF to ozone pollution, while for PM2.5 this appears to be less controversial.

It is correct that there is a potential mismatch between the cardiopulmonary and the cardiovascular mortality rates. This has been discussed in detail and shown to cause small discrepancies. For example, the resulting RRs are well within the range used in other studies that focus on cardiovascular disease (p. 7749). This inconsistency is tolerable as it avoids another discrepancy (e.g. in studies that use CRFs for cardiopulmonary disease) where respiratory disease by ozone likely includes respiratory disease by PM2.5 (also included in PM2.5.cardiopulmonary mortality). This leads to double counting of premature mortality by respiratory disease. Jerrett et al. (2009) suggested that the effects of ozone are confounded by the presence of PM2.5. Since respiratory ailment is a small component of cardiopulmonary disease the effect is small, but could indeed contribute to (though not explain) the discrepancy between our results for PM2.5 and the much higher PM2.5 mortalities by Anenberg et al (2010). We will discuss this in the revised manuscript, as suggested.

The baseline mortality rates refer to the population of 30 years and older, as indicated in table 1.

We are somewhat surprised by the request to provide more details about our model as it is rather well documented and the results extensively evaluated, for which we provide references. As stated, dust and sea salt are included in our model but not considered in the calculation of premature mortality rates due to PM2.5. We are nevertheless happy to provide additional details in the revised manuscript. We can also discuss differences with additional papers, e.g. Fann et al. (2011) on the USA national air pollution impacts. As mentioned above, we will provide additional information about the model simulated PM2.5 compared to observations.

We agree with most of the minor comments, which will be addressed in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 7737, 2013.