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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The influence of model spatial resolution on simulated ozone and fine particulate matter: implications for health impact assessments" *by* Sara Fenech et al.

Anonymous Referee #2

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The manuscript by Fenech et al. considers the impact of model resolution (140 km vs 50 km) on the attributable fraction of premature mortality to O3 and PM2.5 in Europe. This question of model resolution influences on such health effects estimates has been raised previously in a few other targeted studies but has yet to be evaluated in Europe at these scales. The authors find that the impact of resolution is spatially variable, and significant. Hence, this study is of value of the community for better understanding health impact assessments in Europe, and contributes more broadly to a body of work that helps us understand the mechanisms governing scale dependencies. The manuscript is clearly organized and easy to read. There are through some areas where the analysis could be more focused, and I have some concerns related

to model performance at the two different resolutions, and how that translates into a potential recommendation for future research into health impacts. These aspects and others are described in detail below; addressing them will constitute minor revisions, after which this paper will be suitable for publication in ACP.

Major comments:

156: I understand the authors motivation here, to isolate the impact of model resolution from the impact of resolving differences in baseline mortalities. However, I disagree with their approach. But computing country-level AF and country-level baseline mortalities, the authors neglect any impact on mortality estimates that may come from sub-national variability in AF and baseline mortalities. It seems to me that a better (?) approach would be to map the O3 and PM2.5 concentrations from both the coarse and fine simulations to the same fine-scale resolution of the available population and baseline mortality rate information. This way they would have a consistent comparison that isolates the impact of the air quality model resolution, but their final estimates of mortality would be more accurate and more sensitive to differences in the air quality model resolutions. I'd suggest they at least consider this approach, which is just a post-processing step and doesn't involve any more model simulations, to see if it makes a significant difference, or explain why it isn't the recommended approach.

When presenting the AF results, it would be interesting to know if the differences between the two scales of analysis are greater than the error bars in the AF estimates stemming from the uncertainty in the concentration response parameter (beta). In other words, when are the model-dependent differences significant, compared to the health-data uncertainties? See papers by Thompson et al. in this regard.

It seems somewhat problematic, in terms of drawing conclusions, that the fine-scale simulated concentrations are, in many seasons, a poorer match to the observations than the coarse scale simulations, for both O3 and PM2.5. I strongly insist that the authors should present the statistical evaluation of biases in O3 during the warm season and annual ave PM2.5 in the main text, not the supplemental, as these are the scales most relevant to the focus of this work (health impacts). This is rather critical information that the reader shouldn't have to dig for.

Overall, given these biases, would the authors recommend using the fine scale model over the coarse scale model for health impact analysis, especially for PM2.5 where the bias in the annual average concentration is higher at the finer scale? Or are there enough observations to say which is better at estimating exposure? This wasn't clear to me. I think this warrants some discussion, with conclusions in the abstract and conclusions.

Also, model bias relative to observations should be considered when discussing regional differences in modeled spatial resolution of population-weighted concentrations (section 4.2) – in other words, is one model resolution notably better in heavily populated areas? This is a critical question which I couldn't find a direct evaluation of, although all the pieces are available to make the comparison. The same comment applies to comparison of AF (section 4.3).

426 - 437: Regarding comparison to studies in the US, I think an interesting conclusion is that the differences owing to model resolution is not something that is consistent in sign, spatially (or that could thus be easily corrected for without knowing the spatial dependence). This is self consistent with their own evaluation of the variability of the difference across regions within Europe. Still, one might hypothesize about additional factors that control these differences. Did the authors consider the speciation of the PM2.5 and how this might affect the differences between coarse and fine scale simulations? For example, both Punger and West (2013) and Li et al. (2015) note that the differences are more significant for primary anthropogenic PM (e.g., BC) than secondary anthropogenic PM or primary natural PM. I contrast, Thompson 2014 noted the biggest impact of resolution going from 36 km to 4 km was for secondary PM. I didn't see PM2.5 speciation discussed anywhere in the present work.

Minor comments:

1.13: Given that it is a regional modeling study, would make more sense to phrase as "at resolutions typical of global (∼140 km) and regional (∼50km) models." Throughout, it would make more sense to me if the results were referred to as "coarse" vs "fine" rather than "global" vs "regional", all results are regional and this could be misleading to someone just glancing at the figures. Further, most regional models these days are more like 12 km scale or finer.

1.15: The differences seem a bit more modest, all less than 30% and most less than 10%. Not sure if "strong" is the right word.

28-33: Readers not familiar with AF may think these numbers are very small – the authors might wish instead (or additionally) to present the amount by which AF is changing owing to model resolution (i.e. a factor of two to three). So consider not the changes in total mortality (which is small, 5%) but instead the % changes in pollution attributed mortality. I think the authors should also state the differences in the total over the entire domain, rather than just the range across regions, even if the total benefits from some fortuitous cancelation of under and over estimates.

52-62: I'm not sure how "strong" the effects are that are being discussed in this paragraph – can the authors be more quantitative when describing previous works? The following paragraph on PM2.5 is much better in this regards.

68: The reference is Li et al. (2015). not 2016. The authors refer to the paper both ways in the text but only include an incorrect reference to 2016 in the bibliography.

78: The authors should also consider the results of Thomson and Selin (2012), which found there were some differences between O3-related premature deaths at the 36 km scale and finer (24, 12, and 4 km) scales, although these tended to lie within the range of uncertainty of the health impact estimates. Further, they should discuss the O3 health impact results from Thompson 2014, which were found to be more sensitive to resolution than PM2.5 health impacts.

Table 1: The placement of the “difference” row is confusing. It is the difference in the model simulated mean, and should be labeled as such and more clearly located directly below the row reporting the mean, not the rows reporting NMB and SD. The choice of significant figures for the difference also seems odd. For example, why the DJF mean difference is reported as 10 rather than 9.6 isn’t clear, when other numbers are resolved to the tenth of a ug/m3. Same comments apply to Table 2. Annual average PM2.5 and warm-season (April - Sept) O3 should be added to these tables, not put in the SI.

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