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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Potential impact of a US climate policy and air quality regulations on future air quality and climate change" *by* Y. H. Lee et al.

Anonymous Referee #3

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The manuscript "Potential impact of a US climate policy and air quality regulations on future air quality and climate change" presents a study which evaluates changes in both mortality rates and radiative effects caused by atmospheric gaseous and particulate matter. The manuscript fits well in the scope of Atmospheric Chemistry and Physics. The conclusions of the study are scientifically sound and I can recommend publishing the manuscript after the following minor issues and technical corrections have been addressed:

- Abstract, Line 15: "a strong positive radiative forcing" is overstating the global value of 0.04 Wm^{-2}
- Page 31387, Lines 27-28: This sentence is ambiguous. Is the limitation the lack

of chemical compounds or that Akhtar et al. 2013 study only the direct effect?

- Page 31389, Lines 9-10: I am not familiar with these regulations. They should be given with a reference or a brief explanation.
- Page 31390, Line 17 vs Figure 1: The emission scenario name c50nq is inconsistent in Fig 1 (c50noaq)
- Page 31392, Lines 14-16: Vehkamaki et al. (2002) parameterization is known to underestimate or produce negligible nucleation rates in the boundary layer. Are there some issues in the sulfuric acid concentrations in the model or why is this parametization used with reduced sulfuric acid concentrations?
- Section 3 Model descriptions: The radiation calculation of ModelE2-OMA has not been explained.
- Page 31392, Line 28: Which "optical properties"?
- Page 31394, Line 12: The acronym SICE has not been explained.
- Methods regarding the calculation of mortality rates require more discussion and clarification in Section 3.2. It has been briefly mentioned that naturally emitted sea-salt and dust aerosol have been neglected in $PM_{2.5}$ values because they are highly varied. In my opinion, this requires more justification than variability since they would contribute to a significant amount of $PM_{2.5}$. I would expect that in some areas this increase would make the mortality rates much less sensitive to changes in $PM_{2.5}$. For example, Anenberg et al., 2012 have justified this exclusion by weaker toxicity of sea-salt and dust. On the other hand, Giannadaki et al., 2014 have studied the PM2.5 dependent mortality rates for dust. (Giannadaki, D., Pozzer, A., and Lelieveld, J.: Modeled global effects of airborne desert dust on air quality and premature mortality, Atmos. Chem. Phys., 14, 957-968, doi:10.5194/acp-14-957-2014, 2014.)

- Page 31396, Lines 4-5: I don't follow the logic why linear data suggests that $CRF_{base,PM}$ is the most appropriate for the US.
- Page 31403, Lines 19-20: What is considered as mild? Impact on what?
- Page 31404, Lines 4-8: Have you diagnosed the nitrate burden? Is the change in burden opposite to surface PM levels?
- Page 31406, Line 16 (+ where this comment applies): I don't recommend talking about dis-benefits when it comes to climate effects since the effects caused by regional warming can be considered both positive and negative depending on the point of view.
- Please check the grammar and spelling throughout the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 31385, 2015.