

Response to Referee #3

We thank all referees for their constructive comments and suggestions. We respond to the specific comments below in blue color, repeating the reviewer's comments in italics for reference.

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}*

Response) This phrase was intended to use for the US, not for the globe. So we have modified to the following:

"leading to a strong positive radiative forcing (RF) **over the US** by both aerosols direct and indirect forcing"

- *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?*

Response) Both.

"A major limitation on the climate impact estimates in Akhtar et al. (2013) is that they only use direct radiative forcing of sulfate, black carbon and organic carbon aerosols: **no direct forcing by gas pollutants and no indirect effects**"

- *Page 31389, Lines 9-10: I am not familiar with these regulations. They should be given with a reference or a brief explanation.*

Response) We have provided the reference.

"(see Akhtar et al. 2013; the details of each regulation can be found in <https://www3.epa.gov/air/oarregul.html>)"

- *Page 31390, Line 17 vs Figure 1: The emission scenario name c50nq is inconsistent in Fig 1 (c50noaq)*

Response) Thank you for catching the error. We have changed from c50noaq to c50nq in Fig. 1.

- *Page 31392, Lines 14-16: Vehkamäki 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 parameterization used with reduced sulfuric acid concentrations?*

Response) We agree with the reviewer that Vehkamäki et al. (2002) nucleation scheme is known to underpredict nucleation rates. However, ModelE2-TOMAS with the nucleation scheme overpredicts CN3 ($D_p < 3 \text{ nm}$) and CN5 ($D_p < 5 \text{ nm}$) in the upper troposphere. By

reducing sulfuric acid concentrations, the model CN3 and CN5 compares well with observations. Further, more elaborate, work is required to find the exact cause, but this might be related to simulated sulfur dioxide vertical distribution. In Lee et al., (2014), we found that ModelE2-TOMAS and ModelE2-OMA predicts too much sulfur dioxide lifted into the upper/free troposphere over the Pacific Ocean, possibly due to overly-strong convective transport, which can be oxidized to form sulfuric acid and then contribute to nucleation.

- *Section 3 Model descriptions: The radiation calculation of ModelE2-OMA has not been explained.*

Response) The first paragraph in Section 3 includes the description of ModelE2 radiation calculation. ModelE2-OMA is the default aerosol module for ModelE2

“The radiation scheme accounts for size-dependent scattering properties of clouds and aerosols based on Mie scattering (Hansen et al., 1983) and non-spherical light scattering of cirrus and dust particles based on T-matrix theory (Mishchenko et al., 1996).”

- *Page 31392, Line 28: Which “optical properties”?*

Response) We found that our original sentence sounds redundant and confusing, so we have modified the sentence.

OLD: “In ModelE2-TOMAS, size-resolved AOD is computed using a volume-averaged refractive index, and optical properties are based on Mie theory.”

NEW: “In ModelE2-TOMAS, size-resolved AOD is computed using a volume-averaged refractive index, based on Mie theory.”

- *Page 31394, Line 12: The acronym SICE has not been explained.*

Response) Thanks for pointing this. In Section 3.1, we have defined what SICE stands for: “... monthly mean sea surface temperatures (SST) and sea ice (**SICE**) coverage averaged from 2001 to 2010 ...”

- *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 PM_{2.5} 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.)*

Response) Thank you for the good point. We have modified to address the reviewer’s suggestion.

OLD: “Since sea-salt and dust aerosols are mostly naturally emitted and highly varied due to wind-dependence of their emissions, the health impact of a policy-driven measure is obtained without them.”

NEW: "Since 1) sea-salt and dust aerosols are mostly naturally emitted and highly varied due to wind-dependence of their emissions and 2) the toxicity of sea-salt and dust particles is weaker than anthropogenic aerosols (Anenberg et al., 2012), the health impact of a policy-driven measure is obtained without them."

• 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.*

Response) The CRF_{base,pm} uses a log-linear CRF slope; the CRF_{high,pm}, a log CRF slope ; the CRF_{low,pm}, a power-law CRF slope. Here we meant that the CRF_{base,pm} might be the most appropriate for the US, as the CRF slope using the US data is a log-linear. We have clarified it in the revised manuscript (New part is in bold).

"Epidemiological studies indicate that the CRF slope derived from U.S. data is **log-linear** over the concentration range from low to ~40 $\mu\text{g m}^{-3}$ [Krewski et al., 2009; Laden et al., 2006]. This suggests that the CRF_{base,pm} (**i.e., log-linear CRF**) might be most appropriate for the US."

• Page 31403, Lines 19-20: *What is considered as mild? Impact on what?*

Response) We have clarified the sentence.

Old: "We find the US air quality regulations have a moderate impact over the Atlantic Ocean and the Pacific Ocean nearby California, roughly $0.1\sim 0.5 \text{ W m}^{-2}$ in 2030, and a mild impact throughout the NH"

NEW: "We find the US air quality regulations have a moderate impact **on radiative forcing** over the Atlantic Ocean and the Pacific Ocean nearby California, roughly $0.1\sim 0.5 \text{ W m}^{-2}$ in 2030, and a mild impact throughout the NH, **roughly $\pm (0.01\sim 0.05) \text{ W m}^{-2}$.**"

• Page 31404, Lines 4-8: *Have you diagnosed the nitrate burden? Is the change in burden opposite to surface PM levels?*

Response) As stated in Section 3, nitrate is computed in ModelE2-OMA (but not in ModelE2-TOMAS). Some regions in the US show opposite changes between burden and surface 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.*

Response) We understand the reviewer's concern in some degree, but we prefer to keep "climate dis-benefit" and "climate benefit" in our manuscript. Since the climate policies aim to reduce GHGs to mitigate global warming, if it results in positive radiative forcings, more stringent climate policy is required to counteract that positive forcing. In this sense, we think it is reasonable to use "climate dis-benefit".

• Please check the grammar and spelling throughout the manuscript.

Response) Yes we did.