

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-86

Anonymous Referee #1

Referee comment on "Reducing future air-pollution-related premature mortality over Europe by mitigating emissions from the energy sector: assessing an 80% renewable energies scenario" by Patricia Tarín-Carrasco et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-86-RC2>, 2021

The authors have investigated premature mortality changes due to PM_{2.5} over Europe under present and future conditions. The authors used WRF-Chem to predict PM_{2.5} under the present and RCP8.5 scenario, and calculated premature death using the GEMM method. It is an interesting paper that separates different causes of premature death including climate changes, emission changes due to mitigation to renewable energy, and population changes. This paper is within the scope of ACP but I have a few concerns that must be addressed and believe that a major revision is needed before it can be acceptable for publication in ACP.

Major comments

1. The authors said their results are in agreement with the results of Andersson et al. (2009) and Crippa et al. (2019) without giving any numbers of previous studies (line 211). The authors estimated premature deaths due to PM_{2.5} over Europe to be 895,000, which is a lot higher than 546,000 of Andersson et al. (2009) and 210,000 of Crippa et al. (2019). As referee #2 pointed out, this value is also higher than 647,000 of Burnett et al. (2018) with the same GEMM method. Values from those three previous studies are even below 725,000 which is the lower end value of this study in terms of 95% confidence interval. The authors should give the reason or justify their premature death calculations.

2. Following up on the comment #1, the reason could be due to the old aerosol scheme (GOCART) used in this study, which was developed 20 years ago (Table SM1). Both inorganic and organic aerosol formations are very simplified in this GOCART scheme. For example, it doesn't have nitrate, aerosol thermodynamic module, anthropogenic SOA, etc. (Chin et al., 2002). Biogenic SOA is included in the GOCART scheme but simply 10% of biogenic VOCs is assumed to be SOA without any consideration about volatility, gas-particle partitioning, and further oxidation. The author said this model configuration has been validated in detail in previous studies by citing Tarin-Carrasoco et al. (2019), Jerez

et al. (2020), and Palacios-Pena et al. (2020). However, I was not able to find the PM2.5 evaluation of their model configuration in the cited studies. Only Palacios-Pena et al. (2020) reported some evaluation against AERONET AOD at 670 nm. The model overestimated the observed AOD over Europe (Figure 2 in Palacios-Pena et al. (2020)), and I speculate the surface PM2.5 might also be overestimated similarly in the model, which made higher premature deaths of this study compared to previous studies (comment #1). However, premature deaths are calculated by PM2.5, not AOD. The authors should evaluate their model against the observed surface PM2.5, especially when using the 20-years old aerosol scheme.

3. It is unclear whether the authors included biomass burning emissions in the simulation. I think biomass burning contributes relatively a small fraction of PM2.5 mass over Europe (~10-20%), but it can be an important source especially when PM2.5 changes are less than 1.0 $\mu\text{g m}^{-3}$ (Figure 4). Because RR in eq (1) has a non-linear response to PM2.5 concentration, the correct representation of biomass burning can be also important to accurately calculate premature death.

4. The authors did a good job in calculating emissions for 80% renewable energy scenario (REN80). However, in reality, there will be reductions in emissions from other sectors as well, similar to what we observed from 1990 - 2017 trends over Europe (<https://www.eea.europa.eu/data-and-maps/indicators/main-anthropogenic-air-pollutant-emissions/assessment-6>). One example could be vehicle emissions, emission factors of recent vehicles (EURO-4 or EURO-5) were significantly decreased compared to old vehicles (EURO-1 or EURO-2) (Figure 4 in Huang et al., 2018). This means that baseline PM2.5 will be different in the future. Not only differences in PM2.5 but also baseline PM2.5 is important in calculating changes in premature death, as the RR function is non-linear. For example, PM2.5 changes of 15 \rightarrow 10 $\mu\text{g m}^{-3}$ would give different premature death changes compared to PM2.5 changes of 10 $\mu\text{g m}^{-3}$ \rightarrow 5 $\mu\text{g m}^{-3}$.

5. In section 3.3, the authors explain the main reason for PM2.5 changes is precipitation and wet scavenging. This is also conclusion #1 of their paper. This is an important finding that climate can cancel/enhance PM2.5 changes in a specific region of Europe. However, no figures were presented in the manuscript. Please consider a more detailed discussion on changes in precipitation and resulting PM2.5 with figures (wet deposition fields would be best) if possible. And does the cloud also affects PM2.5 through changes in photochemistry? or only precipitation is important?

Minor comments

6. Section 2.1. Chemistry models typically simulate dry aerosol mass, but I think aerosol liquid water should also be included in mortality calculation. How did the authors calculate aerosol liquid water in addition to dry aerosol mass?

7. Table 2 nicely shows emissions from the energy sector for two scenarios. Please consider adding anthropogenic emissions from other sectors in another column, so that readers can see the effects of REN80 on total anthropogenic emissions over Europe. The Sum of all other sectors could be OK, although the emission from each sector would be

preferred.

8. Line 207: What are the natural sources the authors referring to? Does it biogenic VOCs? biomass burning? dust? or sea salt?

9. Line 215: Please provide the quantitative numbers from the previous study.

10. Line 245: Can authors provide RR function shape to show sensitivities of different disease categories to changes in PM_{2.5}?

11. Figures 7 and 10: In some panels (a, c, d, e), I can't see differences due to the color scale. It looks like the color scale is adjusted for panel f). Please consider adding a colorbar under each panel with a different color scale.

References

Huang, C., Hu, Q., Lou, S., Tian, J., Wang, R., Xu, C., An, J., Ren, H., Ma, D., Quan, Y., Zhang, Y. and Li, L.: Ammonia Emission Measurements for Light-Duty Gasoline Vehicles in China and Implications for Emission Modeling, *Environ. Sci. Technol.*, 52(19), 11223–11231, 2018.