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Comment on acp-2021-86

Anonymous Referee #2

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-RC1, 2021

Review of Tarin-Carrasco et al., *Reducing future air pollution-related premature mortality over Europe by mitigating emissions: assessing an 80% renewable energies scenario*. Submitted to ACPD.

General: Tarin-Carrasco et al. use WRF-Chem to study emissions scenarios and subsequent impacts on human-health throughout Europe. Included in their analysis are "present" conditions, a future climate forcing scenario, and a renewable energy adoption scenario. Within each scenario, the effect of projected changes to the population's age distribution are explored. The authors report that the current health burden attributable to PM exposure in Europe is high and will increase in the future (albeit with slight benefits following a stringent renewable energy strategy), almost exclusively driven by an aging population.

Overall, I believe this manuscript requires significant modification before it can be considered for publication in ACP. I outline my comments below and attempt to provide guidance, where possible.

Specific/Major Comments:

-The REN80 scenario is framed as an "80% renewable energies scenario." However, this scenario only considers emissions from the energy sector. Meanwhile, mobile, residential, and industrial emissions are left alone. This seems to be an important oversight since these other sectors have clear impacts on the trajectory of the energy sector. For example, vehicular electrification. If adoption of electric vehicles expands, as is expected, this will have a large effect on the energy sector. If residential heaters using wood combustion are replaced with electric heaters, that too would have a massive impact on primary PM emissions. It seems the title of the manuscript does not quite match the contents. Rather, the scenarios analyzed here are assessing what the PM impacts might be if the energy sector was 80% powered by renewables during the 1991-2010 period; not a future where Europe is 80% powered by renewables.

In addition, what's notable to me is that we are today (i.e. 2021) closer to the "Future" time slice (only 10 years away from 2031) than the "Present" time slice (11 years away from 2010). With the benefit of hindsight in this situation, how is this 80% renewable

energies scenario fairing? What is the 2020 contributions of renewables vs. nuclear vs oil vs coal vs gas vs other fuels? Is the REN80 scenario too optimistic? Seem about right? Too pessimistic?

-Why are the older RCP scenarios used and not the newer SSP scenarios? Why is the RCP 8.5 scenario used when it is such an unlikely scenario (Hausfather and Peters, 2020)? If one of the more "likely" scenarios were used, perhaps much of the reported climate penalty in the Iberian Peninsula would diminish. Also, please indicate in Table 3 which year of ACCMIP emissions were used for each case. I believe the FUT-P2010/50 simulations used present day emissions from ACCMIP, but it isn't clear to me.

-Health impact assessments associated with air pollution exposure traditionally report results in terms of premature deaths. Premature deaths are a tangible number that is easy to understand (unlike YLLs and DALYs, in my opinion). However, issues can arise when attempting to communicate health burdens attributable to air pollution exposure. This is because changes in air pollution exposure is not the only variable affecting these health impact calculations. For example, this manuscript presents a scenario in which PM decreases throughout Europe, often in places > 1 ug/m3, except for some slight increases in the Iberian Peninsula and SW France (Fig. 4). However, calculated health impacts nearly double (895k to 1480k in Present and REN80-P2050 scenarios, respectively). Unless someone is familiar with the underlying concentration-response functions and how they are applied, this is likely very confusing. I believe the authors have an opportunity to (and should) update the presentation of their results in a way that can help the community more broadly communicate the health burden associated with air pollution exposure.

In Apte et al., 2018, the health burden associated with long-term PM exposure is reported in terms of decrements of life expectancy. What is advantageous of this health burden presentation is that the results are largely driven by differences in exposure. When health impacts associated with various scenarios (especially over long periods of time and into the future) are presented in terms of premature deaths, the results can be heavily skewed by changes in the size of the underlying population and age structure. When health impacts associated with various scenarios are normalized (e.g. # premature deaths per 100,000 habitants), the effects of changes to the population size are eliminated but the results can again be heavily skewed by changes in the age structure of the population. Table 4 illustrates both examples quite well. I believe health impacts reported in terms of decrements of life expectancy can mitigate the influence of non-air pollution exposure effects. I believe the Global Burden of Disease reports life expectancy per-country, which can help facilitate these calculations. Without such corrections, I believe many would generate misguided conclusions about the benefits of reduced air pollution exposure.

-The present-day health impact numbers reported here are noticeably high: 895,000 for Europe. For example, Burnett et al. 2018 report 647,000 premature deaths for Europe. Does the WRF-Chem set-up used here have a high PM bias? I'm curious, why not use "observed" PM from one of the various satellite products and apply the change in PM calculated from the CTM to one of those datasets? Essentially, use the CTM to calculate the sensitivity of each scenario and apply to "observations."

-The differences in endpoint are discussed throughout the manuscript, including how the proportions change over time (e.g. much of Sections 3.5.1, 3.4, and Fig. 6). However, I believe the same baseline mortality rates (y0 in Eqn. 1) are used in both "present" and "future" simulations. It seems that these comparisons are difficult to make under those assumptions.

Technical/General Comments:

- "Worldwide air quality has worsened in the last decades as a consequence of increased anthropogenic emissions, in particular from the sector of power generation." Worsening air quality has certainly been the case in some locations, but far from all. For example, air quality in China, home to nearly ~20% of the global population, has dropped precipitously in the last decade.
- An 80% renewables adoption only yields a reduction in PD of 4%. That is a surprising result and likely because the 80% scenario only considers the power generation source of emissions in a present-day environment (see comment above).
- Line 34: exposition > exposure
- Line 36: define "this pollutant"
- Line 37: stablished > established
- Line 41: As constructed, this sentence is really confusing. First, the respiratory impacts that are cited are for ozone, not PM. Second, why cite global numbers when that table has European numbers and that is the focus of the effort here?
- Line 44: As constructed, this 70% number is incorrect. 70% of global mortality (i.e. total mortality) is not attributable to PM. That would be something like 45-50 million premature deaths a year. Do the authors mean 70% of air quality attributed mortalities are attributable to PM? I do not know, please re-visit that citation.
- Line 57: Please revise the writing of "In Europe, despite agriculture is the sector": it is confusing as written.
- For each case considered, are the 20-years of simulation simply averaged together? This should be noted in Section 2.3.
- Line 161: The PM decrease is strictly a primary PM decrease. I suspect secondary PM to be the dominant source of PM exposure. With that said, how is this primary PM speciated and are there volatility assumptions for the organic portion?
- Table 3: Indicate which year of ACCMIP emissions were used for each case.
- Line 188: "cities" written twice.
- Line 194: Do the authors mean "decrease" in the REN80 scenario? I see a decrease in Fig. 4 over the Baltic states.
- Section 3.1, and the manuscript more broadly: It would be helpful if population-weighted concentrations were presented throughout.
- Line 212: Crippa et al 2019 seems to indicate ~260,000 for Europe, far less than 895,000.
- Something seems strange in Fig. 7. For example, Poland. It essentially has no shading in panels a)-e). However, it is full of red in panel f). Same throughout the Balkans and Eastern Europe. Is panel f) not the summation of panels a)-e), because this figure seems to say no.
- Section 3.5: Several times, "FUT-2050" was used. That should be "FUT-P2050".
- There is a lot of repetition in Sections 3.4 and 3.5.1.
- Figure 10: A "P2050" case is mentioned in the figure description. Please keep all scenario names consistent (i.e. only use Table 3 names).
- Line 308: These few sentences on lung cancer are highly speculative. I recommend deleting them.
- I recommend getting rid of the log-scales on Fig. 11. That presentation tends to exaggerate the age bins that have minor impacts (younger ages) and deflate the age bins where most impacts occur (older ages).
- Line 339: "trusting" is a strange word to use here.
- Line 340: easter > Eastern

References:

-Apte et al., Ambient PM2.5 Reduces Global and Regional Life Expectancy; ES&TL, 5, 9, 546-551, 2018.

-Hausfather and Peters, Emissions – the ‘business as usual’ story is misleading; Nature, 577, 618-620, 2020.