General remarks on the reviews for minor revisions of the manuscript egusphere-2024-224

We thank both referees for reviewing and supporting the revisions we made on the first version of the manuscript. We acknowledge that the representation of aerosol composition in MACv2-SP, as well as the 'built-in' effects of the parametrization, have to be clarified throughout the manuscript. Below, we respond point by point to the comments left by Referee #2, and provide a list of relevant changes in the manuscript.

Anonymous Referee #2

I thank the authors for considering my comments, and especially for clarifying how aerosol absorption is represented in MACv2-SP. I have read section 4 of Stevens et al. (2017), which confirms that changes in radiative efficiency due to changes in aerosol composition in the different regional plumes are not represented in MACV2-SP. That choice was made despite evidence of there having been changes in radiative efficiency due to changes in aerosol composition. Stevens et al. (2017) resolves that contradiction by saying "experiments to judge the magnitude of [aerosol composition] changes are warranted" (last paragraph of their Section 4).
Reply: Thank you very much for reading in detail section 4 of Stevens et al. [2017] and providing valuable external view on our work. Changes in aerosol composition are indeed not represented in MACv2-SP more than the fractions of SO_2 and NH_3. Stevens et al. [2017] suggest that the 'brightening of aerosols' from the reduction in BC fraction prior to 1970 (Figure 10) should results in an increase in efficiency, while the opposite trend after 1970 should imply a decrease in efficiency. The evolution of aerosol efficiency over time may be significantly influenced by changes in aerosol composition. However, these changes typically occur alongside variations in aerosol emissions, potentially limiting their impact on global efficiency. The increase in efficiency after 1980 in our findings appears to contradict with the observations mentioned above. We address this inconsistency in greater detail below, where we discuss temporal changes in aerosol composition and their implications for our results.

That built-in behaviour of MACv2-SP has clear implications for the present study. That lack of representation of changes driven by aerosol composition may let other drivers emerge, like cloud masking. So this MACv2-SP behaviour must be clearly documented in the paper: in the abstract, in the last paragraph of section 2.1, and in section 3.5, and in the conclusion, which could suggest a similar analysis in more complex aerosol-climate models.
Reply: We agree that the design choices made in MACv2-SP have implications for the results of our study. In this revised version, we further discuss the impact of not including changes in aerosol compositions within each plume.

Regarding cloud masking, our study focused on the observed trend in the direct effect of aerosols. We did not investigate the trend in the indirect effect, thus cloud masking falls outside the scope of this research. Nevertheless, our methodology provides relevant insights: By employing the PRP method, we effectively separated the direct and indirect effects of aerosols. This approach isolates the forcing from the direct effect, independently of any masking effects due to aerosol indirect effects. Notably, we found that the total aerosol forcing equals the sum of the isolated direct and indirect effects, indicating that the indirect effect does not mask the direct effect through enhanced cloud masking. It would be indeed valuable to investigate such effects in more complex aerosol-climate models, provided these models can effectively dissociate direct and indirect effects similarly to our study with the PRP method.

There is also a point where the present paper seems to contradict Stevens et al. (2017). The authors write that in the revised section 2.1 that the plume scale factor applied to the reference year (2005) depends on SO2, NH3, and BC emissions. But Eq. 17 and Table 5 of Stevens et al. (2017) clearly say that only SO2 and NH3 are considered, not BC. The mention of BC in their Table 6 is only informative, although it suggests that accounting for BC would have been a good thing. As written in that paper, "Other factors, which do not correlate with regional NH3 and SO2 emissions" are not represented. So a decorrelation between BC and SO2 emissions (and the subsequent change in radiative forcing efficiency) is not possible in MACv2-SP. I am not sure to what extent that matters for this study – it depends on whether the decorrelation has happened in reality, especially in Southeast Asia. But the description of the method should make clear that BC is not involved in the scaling factors, and the implications of that choice should be discussed in sections 3.5 and the conclusion.
Reply: Thank you for noticing this contradiction. We clarified in our Method section that BC is not explicitly considered in the MACv2-SP. This is particularly relevant for South Asia, as this region has been driving the trend in total direct effect in recent decades (our results, Quaas et al. [2022], etc.). Inventories suggest that South Asia has maintained a consistent aerosol composition since 1970, with the BC fraction relative to 2005 levels remaining stable (approximately 1, as shown in Table 6, Stevens et al. [2017]). This consistency implies that aerosol efficiency in South Asia has likely remained stable over the past few decades. Therefore, including changes in aerosol composition over time, such as the BC fraction, would not significantly impact our results and would not influence the overall discrepancy between total direct effect and global emissions. Even though the aerosol efficiencies in European and North American regions have decreased since 1980, their contribution to the total efficiency is relatively small compared to that of Asian regions. Furthermore, emerging biomass burning plumes in recent decades have exhibited lower Single Scattering Albedo (SSA) values, representing an increase in absorbing aerosols and a resulting decrease in total efficiency. However, since the relative emissions from these biomass burning plumes are small, they do not counterbalance the increase in total efficiency driven by Asian plumes.

List of changes in the manuscript

We included the discussion on aerosol composition in the relevant sections as suggested by Referee #2:

- We specified in the abstract that MACv2-SP employs a constant regional direct effect efficiency.
- We clarified in Section 2.1 that BC is not included in the MACv2-SP aerosol composition for time-varying aerosol forcing and that changes in aerosol composition over time is not included. Furtheremore, we added that some of these global changes in aerosol composition are implied through the distinction between industrial and biomass burning plumes. We then emphasized that we consider these limitations in our analysis.
- In the Result section, 3.5, we discuss the implications of excluding changes in aerosol composition on our findings, as discussed in the answer to Referee #2.
- Finally, we summarized this discussion in the conclusion and suggest similar study in more explicit aerosol-climate models.

References

Bjorn Stevens, Stephanie Fiedler, Stefan Kinne, Karsten Peters, Sebastian Rast, Jobst Müsse, Steven J. Smith, and Thorsten Mauritsen. MACv2-SP: a parameterization of anthropogenic aerosol optical properties and an associated Twomey effect for use in CMIP6. *Geoscientific Model Development*, 2017. doi: 10.5194/gmd-10-433-2017.

Johannes Quaas, Hailing Jia, Chris Smith, Anna Lea Albright, Wenche Aas, Nicolas Bellouin, Olivier Boucher, Marie Doutriaux-Boucher, Piers M. Forster, Daniel Grosvenor, Stuart Jenkins, Zbigniew Klimont, Norman G. Loeb, Xiaoyan Ma, Vaishali Naik, Fabien Paulot, Philip Stier, Martin Wild, Gunnar Myhre, and Michael Schulz. Robust evidence for reversal of the trend in aerosol effective climate forcing. *Atmospheric Chemistry and Physics*, 22(18):12221–12239, September 2022. ISSN 1680-7324. doi: 10.5194/acp-22-12221-2022.