Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-949-RC1, 2019
© Author(s) 2019. This work is distributed under
the Creative Commons Attribution 4.0 License.

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol radiative effects with MACv2" *by* Stefan Kinne

Anonymous Referee #2

Received and published: 15 March 2019

The author describes the application of the MACv2 global aerosol climatology to calculating direct and indirect aerosol radiative effects and forcing, decomposed by aerosol composition, in both the LW and SW. Maps and time series of the different effects are presented. The author estimates a total aerosol forcing of -1Wm-2 with a possible range of -0.7 – -1.6 Wm-2. They also show a relatively constant forcing over the last decades despite a large regional shift due to shifting emissions.

Overall the paper is well structured but a number of issues need rectifying before I would consider it suitable for publication in ACP:

- The paper describes the forcing responses of this particular setup comprehensively, and would be well suited for GMD. In order for it to meet the scope of ACP however I would suggest much more discussion around how these estimates are an improvement on existing e.g. AeroCom estimates, particularly in the context of the many other

studies and efforts in this vain, both modelling and observational.

- In particular, the paper aims to provide an estimate of the aerosol forcing, but ignoring cloud adjustments seems a significant omission and requires much stronger justification, particularly when discussing potential uncertainties. For example, on P2L9-10 the author states that feedbacks (adjustments) can be considered secondary, though the provided citation makes no such assertion – they just assume it for their purposes.

- The first indirect (Twomey) effect is accounted for, but the fittings used to extract the sensitivity from satellite retrievals is non-standard and needs much more justification and discussion. Why was this functional form chosen, how were the parameters chosen, and what are their uncertainties?

- The color scales used for the plots are very difficult to interpret and at times even use the same color for different values. Worse, these non-monotonic colormaps can also distort the impression of the values being plotted (see e.g. https://matplotlib.org/users/colormaps.html and references therein) and should be replaced.

- Also, the manuscript can be difficult to read at times and would benefit from editing help from someone with full professional proficiency in English.

I also have a number of other, more minor, suggested changes:

- P1 L10: '...major aerosol indirect effect. . .' should be 'first aerosol indirect effect'

- P1 L12: 'locally' -> 'local'

- P2L13-14: The author claims that this setup avoids 'time-consuming aerosol processing' though the fields used to create the MACv2 climatology are derived from just these costly models. This should be clarified.

- P2L19: Suggest 'before impact results' be replaced with 'before forcing results'

- P3L13: The use of the AeroCom phase 1 models is introduced here, but later on

some phase 2 values are also used and should be introduced as well.

- P3L20: Shouldn't all the terms in the scaling equation related to AODf? Currently some relate to AOD.

- P3L20: IPCC5 -> CMIP5

- P6L21: The description of double radiative calls is confusing, please consider rephrasing. The author should also probably cite Ghan 2013.

- P7L5: The assertion that feedbacks are on the order of 10% requires a citation. The author should also acknowledge that there is considerable uncertainty around this.

- P8 Fig5: Perhaps space out the maps a bit more vertically so that it's clearer which mean values apply to which maps. The 'anth all_sky' toa probably doesn't need scaling either, it's misleading when glancing at the data. Perhaps put the TOA and SURF plots on different scales? It should also be labelled as 'all_toa_a' for consistency.

- P9L3-4: The assumption of ignoring potential anthropogenic course mode aerosol should be discussed in the introduction.

- P10 Fig7: These fields appear to be presented at a degraded resolution compared to the previous ones, are they not calculated on the same grid?

- P10L23-25: The assumed anthropogenic dust contribution should be introduced in the introduction section. It's not clear when this is included and when it isn't.

- Fig 8.: It's very hard to compare these maps as they are all on different scales. There is also a lot of information to try and absorb. Could the all-sky plots be removed since the discussion focusses on the different effects of coarse mode aerosol on the solar and IR bands?

- Figure 9: The .1 scalings should be written 0.1 to make them clearer. It's also impossible to compare the magnitudes of cooling and warming using these color scales, a usual blue->red scale would be much clearer.

- P12L26: 'tome' -> 'time'

- P13L6: "also have to considered" -> "also have to be considered"

- Table 2: The coarse mode dust is used here again, it needs introducing and the introductions of anthropogenic aerosol only contributing to AODf needs clarifying

- P17L7: The assumption of constant LWC affects the size of their droplets, and hence their reflectivity, not the other way around. The sentence should be altered to make this clearer. Something like: ". . .that the resulting water cloud droplets are more numerous and smaller, assuming no change to the cloud liquid water content (LWC). With smaller drop sizes the solar reflection. . ."

- P17L11: ". . .then smaller. . ." -> ". . .the smaller. . ."

- P17L33: This sentence doesn't seem to make sense: "However, there is reliance, that regional associations will provide the needed link.". Please rephrase.

- P18L3: "to extract" -> "the extraction of"

- P18L4: "meant that" -> "means that"

- P19L9-11: There are many reasons why the satellite retrieved response could be biased low as well, and recent work has even suggested very large sensitivities (Rosenfeld et al. 2019). The most that can be said is that they're different for reasons still to be fully understood.

- P19L12-13: Why is a single sensitivity applied globally? The 1x1 degree sensitivities are available and are presumably very different locally. Or is the sensitivity calculated globally? In which case large errors are likely to be present (Grandey and Stier 2010). This section needs more discussion and justification.

- P20L14-16: Given these uncertainties it could presumably also be underestimated then?

- P25L4: continued -> continues

- P26L6: “As alternate background ICAP” -> “As an alternate background, ICAP”

- P26L12-14: This isn’t clear. Does the ICAP ensemble include AODf and anthropogenic AOD? How does it calculate them? From which models? Does if assume the same Lamarque emissions as MACv2?

- P28L2-5: This is a confusing sentence, consider re-phrasing

- P28L9-10: The uncertainty due to the parameterisation of the Twomey effect is probably very large due to the large uncertainties in measuring AODf and CDNC individually, confounding (e.g. meteorological) factors, and the (weak) causal relationship between AOD and CDNC. There are also likely to be large uncertainties due to the choice of parameters used to fit these distributions since the error bars are large and small AODf values are not measured. Both of these should be discussed and estimated. Potentially large liquid cloud adjustments are also ignored and should be discussed.

- P32L35: matches -> matched

- P33L15: larger -> large

References:

Ghan, S. J.: Technical Note: Estimating aerosol effects on cloud radiative forcing, Atmos. Chem. Phys., 13, 9971-9974, https://doi.org/10.5194/acp-13-9971-2013, 2013

Daniel Rosenfeld, Yannian Zhu, Minghuai Wang, Youtong Zheng, Tom Goren, Shaocai Yu: Aerosol-driven droplet concentrations dominate coverage and water of oceanic low-level clouds, Science 2019

Grandey, B. S. and Stier, P.: A critical look at spatial scale choices in satellite-based aerosol indirect effect studies, Atmos. Chem. Phys., 10, 11459-11470, https://doi.org/10.5194/acp-10-11459-2010, 2010.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-949, 2019.