

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-570

Anonymous Referee #2

Referee comment on "The role of anthropogenic aerosols in the anomalous cooling from 1960 to 1990 in the CMIP6 Earth system models" by Jie Zhang et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-570-RC2>, 2021

Zhang et al. investigate the role of aerosol forcing in CMIP6 models for a period in which temperatures were too low compared to observations. The study is important and diligently performed. It is of large interest to the readers of Atmos Chem Phys and mostly well written.

I have only two major comments, but a number of specific ones.

Major remarks

It would be necessary to list the aerosol effective radiative forcing for each model, as defined from the fixed-SST simulations. This could be in Table 1 or 3; it would be useful to discuss this in comparison to the transient diagnostics.

The term A in Eq. 1 is wrong ($A = 1$ in general). This has consequences for the analysis in Section 5.

Specific remarks

l22 The term "aerosol-forcing-sensitivity" is not a standard term and I don't understand it at this point. Either the abstract needs to define it or manage without this new term.

l45 It might be wise to make clear that greenhouse gases accumulate, aerosols do not at time scales longer than a week.

l48 The authors need to clarify where the end point of this sentence is. In the most recent years, anthropogenic aerosol emissions clearly decreased.

l50 But Chinese ones reached a peak already in 2011 and declined sharply since then.

l89 should read "van Noije"

l90 Mauritsen et al. (2019) is not the appropriate reference for MPI-ESM-1-2-HAM

l153 this term "aerosol-forcing-sensitivity" is introduced here the first time and needs to be defined precisely.

l156 The authors need to provide a reference for this statement, or demonstrate by other means that it applies. It is not obvious, since the organic aerosol emissions often occur in very different places and are only partly linked to the fossil fuel burning that generates most anthropogenic SO₂.

l164 drop "is"

l167 And of course natural variability

l169 It would be useful to clarify at which scale the data are aggregated. Are these

monthly or annual means? global or regional means? If little aggregation, can the change in sulfate load not become very small?

I170 Forcing or effective forcing?

I177 It would be very useful to disentangle the two. The aerosol effective forcing is readily defined by the fixed-SST simulations designed for this purpose. The authors could investigate this in comparison to the same period in the runs they investigate here. Another option would be to make use of the DAMIP simulation with varying aerosol.

I183 To which extent is it possible to include the feedbacks in this distinction? Are the feedbacks mainly in the ACI term in this definition?

I184 This is an approximation that makes several mistakes. It neglects the (regionally very important) absorption above clouds, and it also neglects the adjustments to aerosol-radiation interactions.

I186 Lifetime is only measurable in terms of horizontal and/or cloud albedo.

I200 This seems to be wrong. Instead of A , one should use OSR_{cld_hist} in Eq. 1.

I208 This is not true, firstly because a lifetime effect may also involve changes to cloud albedo, and second because this lifetime effect is (by far) not the only influence on cloud extent (as the authors immediately acknowledge).

I214 Presumably, "increases in cloud droplet number concentration"? In fact, this is the key impact of aerosols on clouds (Twomey effect, radiative forcing due to aerosol-cloud interactions). But adjustments of cloud water path are also included.

I223 Maybe it is noteworthy that, e.g., Chen et al. (Nature Geosci 2014) or Christensen et al. (ACP 2017)

I226 In fact, the decomposition is so far off "first" and "second indirect effects" (and, by the way, the terms are obsolete since AR5) that it is better to drop this paragraph.

I241 Anomalies with respect to which time period average?

I242 The terms "MMM" and "MME" seem strange to me. Often, MMM is multi-model mean, but here it seems it is, in contrary, the single-model ensemble mean. What is a "multi-member ensemble"? is that not tautological? Is the term "MMM" necessary at all? Why not simply "for each model, the ensemble mean is shown"? What does the acronym "MME" stand for?

I255 What distinguishes the "first" member from any member?

I292 "Sensitive to initial condition" seems strange wording for an influence of internal variability.

I320 A colour scale that evenly is distributed in positive and negative directions should be chosen. Or else this discrepancy should be pointed to in the caption.

I370 This is only true at a very superficial glance. There is not intimate link evident.

I374 This correspondence is hardly evident.

I389 why "relative"?

I389 The more immediate reason is the higher incoming solar flux at lower latitudes.

I399 Perhaps this should be more cautiously "approximately linearly". See debate of Stevens (2015, doi: 10.1175/JCLI-D-14-00656.1), Kretzschmar et al. (2017, doi:10.1175/JCLI-D-16-0668.1), Booth et al. (2018, doi:10.1175/JCLI-D-17-0369.1).

I402 What is "this nonlinearity"? Before, linear relationships were described.

I479 Why "generally"? It is of course a coarse approximation only.

I519 The cloud albedo term may also carry a substantial contribution by adjustments, namely via the adjustments of liquid water path.

I529 This is not quite true. Delta reff is also influenced by changes in liquid water path. The forcing can be identified when investigating droplet number concentrations.