Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2022-295-RC1, 2022

Comment on acp-2022-295

Michael Diamond (Referee)

Referee comment on "Robust evidence for reversal of the trend in aerosol effective climate forcing" by Johannes Quaas et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-295-RC1, 2022

The authors lay out a series of trends in anthropogenic aerosol and precursor emissions, column aerosol burdens, aerosol-influenced cloud properties, and top-of-atmosphere radiation that together provide consistent evidence of a reversal in the aerosol radiative forcing trend from more negative values over the twentieth century to less negative (positive trend) over the twenty-first century. This is mainly driven by trends in North America, Europe, and eastern Asia (especially since ~2010) and is somewhat offset by trends in south Asia. The manuscript is a useful review of the trends and related literature and is particularly helpful in putting everything together in one place (e.g., Table 1 and the Supplemental Figure). I believe that some further reporting of the regional breakdowns and of absolute (in addition to relative) changes would strengthen the paper. I recommend prompt publication of a suitably revised manuscript. -MD

General comments

A. Relative versus absolute trends. I understand why the authors chose to report all trends (except for radiative fluxes) in relative, rather than absolute, units. Unfortunately, this choice would make it a bit difficult for someone not already familiar with the spatial pattern of aerosol burden to see the bigger picture. For instance, in Figure 2, one might think the global average trend is of opposite sign between MODIS and MISR just based on the maps shown, although if you were to take the global average, my impression is that both would show a decrease in AOD(f). Perhaps an additional supplemental figure, like the one already included but with absolute units, would be helpful?

B. Regional breakdown. Table 1 has a nice breakdown of the increasing versus

decreasing areas, although I would be interested in seeing a finer regional breakdown (i.e., North America, Europe, east Asia, south Asia, all other). Waterfall plots showing the global change between 2000 and 2019 and the components related to each region for some key variables (e.g., AOD, CDNC, rsutcs) could be really nice, although even just another table or an expansion of Table 1 would suffice.

C. Global results. More generally, I think it would be worth reporting globally-averaged values for each variable of interest. It is clear that the authors believe the global trends are positive (decreasing magnitude of ERFaer; e.g., Figure 5). This is also clearly implied by the title. It seems clear that the decreasing aerosol regions dominate in the global average over the increasing region(s), so why not just show this directly?

Specific comments:

- Line 15: ERFari also includes semi-direct effects.
- Line 16: If you want a classic reference for ARI as well, I'd recommend Chýlek & Coakley (1974).
- Lines 22-25: As written, this would imply the world has only warmed ~0.5 K since the pre-industrial, when the true value is closer to 1 K. Instead of just citing CO2 perhaps it'd be better to cite the value for all well-mixed GHGs (sum of ~1.5 K), or state that the aerosol forcing essentially offsets the non-CO2 well-mixed GHG forcing.
- Lines 45-47: This sentence could be simplified or broken up. Also, isn't the claim global, not just regional?
- Line 117: Is significance tested using a t-test? Do you account for temporal autocorrelation?
- Lines 107-109: Could you provide some more discussion of the differences between the MISR and MODIS trends? Even some statistically significant pixels have opposite trend signs between (a) and (c). Are there differences in the retrievals and their relative strengths/weaknesses or in what conditions retrievals are possible that could help explain this?
- Lines 112-113: If you subset the MODIS and MISR trends to the same period as PMAp, do things look more consistent?
- Lines 129-130: Especially for LWP, bidirectional changes in response to Nd are now widely acknowledged, so I'm not really sure what the "expected" changes should be in this case.
- Lines 132-133: Similarly, different senses of change in macrophysical cloud properties are possible for different cloud regimes or under different meteorological conditions in the same regime (e.g., Zhang et al., 2022), so it really isn't clear that should be one "expected" change.
- Line 144: I was a bit surprised by the Gryspeerdt et al. 2016 reference here, as the main point of that paper in my reading is how misleading such correlation analyses can be without the proper statistical controls.
- Lines 158-164: How were model variants treated? Is only one used per model, or do you average all variants for each model, etc.?
- Lines 172-173: I'm confused about what the IPCC assessment is referring to here.
- Line 174: The emulator ensemble is not introduced.
- Figure 4: It might be worth having another figure (perhaps in the supplement) showing each model individually, and perhaps the radiation fields directly (rsutcs, rsut, rsut+rlut) instead of ERF, for a more apples-to-apples comparison with the CERES

record.

- Figure 4: It also may be worth looking at variants versus ensemble average for models like NorESM with several variants to explore how much of the noisiness is due to internal variability.
- Figure 4 caption: The gray shading note is for the wrong figure.
- Figure 4: The labels for CERES (rsutcs, etc.) are clear to those familiar with climate modeling but aren't obvious otherwise. Please introduce the labels or use another descriptor.
- Line 221: More explanation of the Smith et al. (2021a) method would be helpful here, and below for Alright et al. (2021) as well.
- Line 222: What is the range quoted? I'm guessing 5-95% confidence?
- Figure 5: Please explain the colors on the x labels. I think I figured it out after staring at it for a bit, but it would be much easier on readers if the information were in the caption.
- Line 240: Zhou et al. (2021) would also be appropriate to reference here.
- Line 245: No strong trends in volcanic aerosol, or eruptions, etc.?
- Lines 245-248: Not only wildfires are relevant here but also agricultural burning, especially in Africa. Andela et al. show that burned area has actually been decreasing on average due to human activities, although there isn't a one-to-one correspondence between burned area and smoke emissions.
- Table 1: See general comment above, at minimum I would add an "all else" column. I also think it would be helpful to have some indication of how things look in absolute, not relative, units, as spatially averaging the percentage changes doesn't necessarily lead to meaningful values given the differences in the absolute amount of aerosol, etc., involved. For the reported values, are you averaging the percentage values from the maps in the figures in space, or taking the absolute values and calculating the percentage trend for the full region?
- Table 2: I believe this table is never introduced?

References:

Andela, N., Morton, D., Giglio, L., Chen, Y., van der Werf, G. R., Kasibhatla, P. S., DeFries, R. S., Collatz, G. J., Hantsson, S., Kloster, S., Bachelet, D., Forrest, M., Lasslop, G., Li, F., Mangeon, S., Melton, J. R., Yue, C., and Randerson, J. T.: A human-driven decline in global burned area, Science, 356, 1356-1362, 2017.

Chýlek, P. and Coakley, J. A.: Aerosols and Climate, Science, 183, 75-77, 1974.

Zhang, J., Zhou, X., Goren, T., and Feingold, G.: Albedo susceptibility of northeastern Pacific stratocumulus: the role of covarying meteorological conditions, Atmos. Chem. Phys., 22, 861-880, 10.5194/acp-22-861-2022, 2022.

Zhou, X., Zhang, J., and Feingold, G.: On the Importance of Sea Surface Temperature for Aerosolâ□□Induced Brightening of Marine Clouds and Implications for Cloud Feedback in a Future Warmer Climate, Geophysical Research Letters, 48, e2021GL095896, 10.1029/2021gl095896, 2021.