

***Interactive comment on* “Changes in the aerosol direct radiative forcing from 2001 to 2015: observational constraints and regional mechanisms” by Fabien Paulot et al.**

Anonymous Referee #2

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This paper presents an important contribution to the effort to understand current trends in aerosol loadings and their associated radiative forcing. The authors use a combination of recent observational datasets, the CEDS emission inventory and the atmospheric component of the GFDL model. They find overall consistency in regional trends between the methods, but also identify potential issues with the emission inventory. They are also able to subdivide the trends into contributions from individual aerosol components, so far as the accuracy of a single model can go.

Unfortunately, it is at present also a very difficult paper to read and understand. Even after multiple readings, there are several details I struggle to understand, and some

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conclusions that I can't quite grasp how the authors have arrived at. Hence, while I realize it will require some work, I strongly recommend that the authors fully revise the paper for clarity and readability. This will make the important results presented here much more accessible to the community.

Some examples and comments:

- A good illustration is the term SDRECS. While it is well defined in the paper, it is technical and not standard in similar literature. Why not simply write "change in outgoing radiation"? Most of the paper deals with shortwave under clear sky conditions, so this is implicit even from the title. The same goes for R_{utcsaf} , R_{utcs} and similar.

- One challenge, especially in the latter part of the paper (the regional trends and RF discussions), is to follow where the conclusions depend on the specific aerosol parametrizations of the GFDL model, and where they can be assumed to be more general. I would encourage the authors to add some further discussion of how model dependent the conclusions are. E.g. in the Conclusions, how general are the remarks about possible issues with the CEDS inventory? This is an important discussion for a dataset that will form the basis for much of CMIP6. A specific example: The authors conclude that "we find significant uncertainties in the CMIP6 emissions, including in the seasonality of NH_3 ". In the paper, as far as I can understand, this is documented through the following: "We conducted a sensitivity simulation using the seasonality of NH_3 column from AIRS (Warner et al., 2017) 265 to modulate NH_3 emissions. We find that this revised seasonality significantly reduces the simulated winter trend in SDRECS ($0.08 \text{ Wm}^{-2} \text{ dec}^{-1}$), improving the agreement with observations." I would expect some more discussion and documentation on this point, to make such a broad conclusion.

- A more technical example: In Figure 1, introduced on line 177, the authors show both CEDS and MEIC emissions. However, "MEIC" isn't defined or discussed until line 300, making it difficult to understand even the first figure without already having read the

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entire manuscript. Please review for clarity, with a community reader in mind.

- For the DRF discussion, it would be good to put the results in a broader context. AeroCom is mentioned; where is AM3 relative to the model mean in terms of forcing strengths? E.g. a comparison to the similar (but much less detailed) results in Myhre et al 2017, ACP (<https://www.atmos-chem-phys.net/17/2709/2017/>) would be useful.

In conclusion, I hope the authors take the time to revise the paper for clarity. As it stands, it will be difficult for the community to access - which is a shame, as the results are important and of broad interest.

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