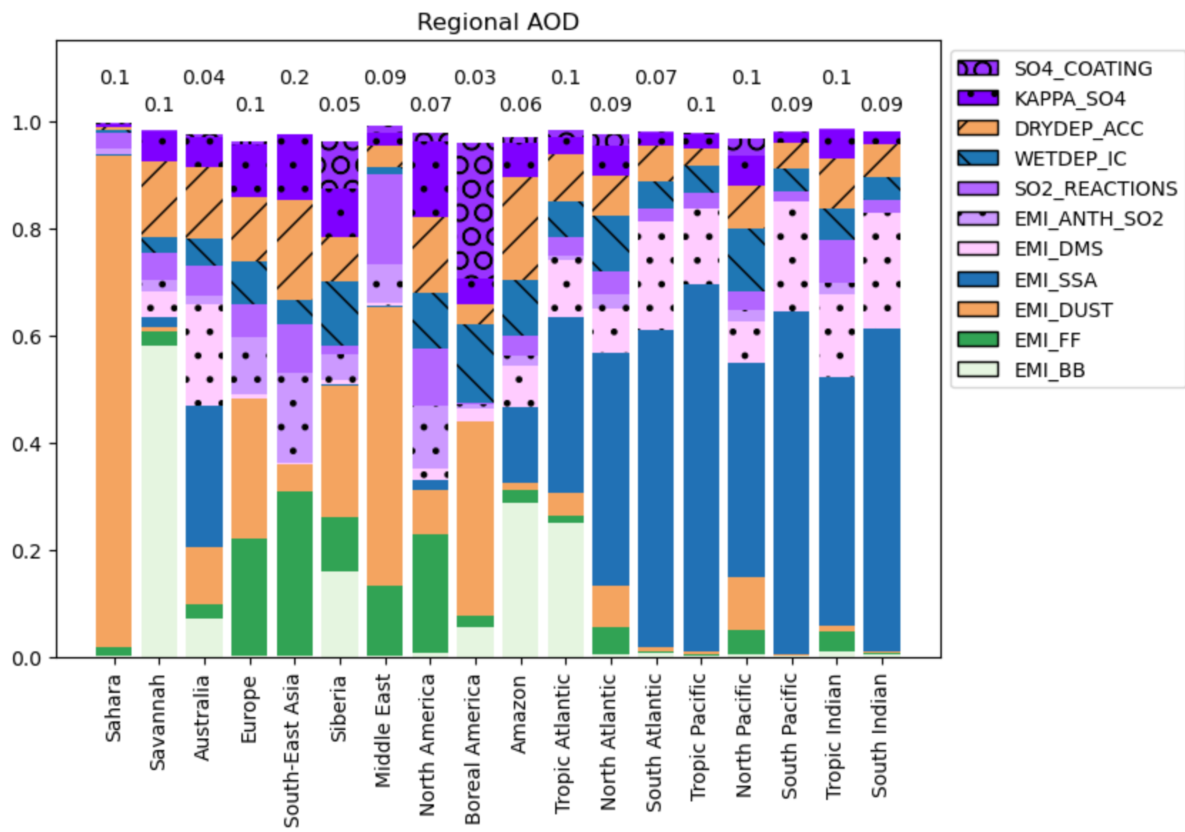
We thank the reviewer for taking the time to carefully read our paper and provide helpful comments. Below we adress the reviewer's points one by one.

1. *It is not mentioned how the fractional variance (shown in Fig. 4) is calculated for AOD and, more importantly, for the temporal autocorrelation at a lag of 6 days (ρ_6). It would also be useful to show how much variance (in absolute terms) is explained collectively by all of the contributing components for AOD, perhaps in a supplementary figure.*

   The fractional variance was calculated by combining the total variance in AOD or rho6 across one million parameter combinations (let's call it Σ^2) and the variance if only a single parameter i is allowed to vary (and all others have their default value, let's call it σ_i^2. This can be calculated using the emulator). The fractional variance is $\sigma_i^2/_{\Sigma^2}$

   We will clarify this in the final version. We think it is a good idea to add a measure of the total (absolute) variation to the figures but feel that the variance itself is less suited than the 5-95% percentile range of the variable itself (which may be compared to AEROCOM model errors and observational uncertainty). The figure would look like this (quantile range at top of each bar):



2. *Fig 4a: The legend for EMI_DMS is not accurate*.

   This is an unfortunate matplotlib issue that the main author has not been able to fix yet. We think the legend is correct but the boxes are too small to sometimes show any hatching or stippling.

3. *Lines 252-253: It is surprising to see sea salt and DMS contributing substantially to the AOD variance over a predominantly continental region such as Australia. Could the authors comment on the physical reasons for this pattern?*

   Mean AOD (~ 0.06) over Australia (in this model) is rather low allowing big influences from natural sources. The uncertainty of the normalised parameters that scale fossil fuel, biomass burning, dust, seasalt and DMS emissions in the PPE are quite similar. The dust and biomass burning emissions are very seasonal unlike seasalt emissions, so their annual effect will be reduced. Our analysis is not very different from the one in Bhatti et al. ACP 2026 although absolute numbers differ (it concerns the same PPE). We are currently trying to understand this discrepancy which is likely due to different definitions.

4. *Lines 247-249: The statement, "The first thing to notice is that the same processes are important for AOD and ρ_6 variance, although their relative contributions can be quite different" appears to contradict the discussion that follows, where the dominant contributors to AOD variance and ρ_6 variance differ in most regions. This statement seems more appropriate for the UKESM ensemble results shown in Figure 6.*

   We can see how this might be confusing. The authors made an (unspoken) distinction between 'important' and 'dominant' contributions. We will clarify this in the final paper.

5. *Figure 8: It is not clear how the different lag values given in the caption should be interpreted, since this figure compares AOD rather than autocorrelation. In addition, the mean values reported in the legend of each panel appear to be significantly lower than suggested by the scatter points. Please verify these values. The same issue with mean values appears to be present in Figures 9 and 11.*

   There are mean AOD values at different lags because the autocorrelation calculation requires the calculating of means over different subsets of the timeseries (see μ in Eq. 4). We could have provided just one (e.g. at lag zero) but decided to be consistent between figures. We will add some explanation to the text. The mean values in the legends are correct; they refer to mean differences between satellite and AERONET.

6. *Figure 8: Please state explicitly in the figure caption that the panels show comparisons for different satellite products. It would also be helpful to mention that the satellite products are based on 1° × 1° averages located close to or overlapping the AERONET sites. Also, please provide a list of the AERONET stations used in these comparisons in the supplementary.*

   We can add that information to the caption. We are not sure about the usefullness of a list of AERONET sites. The sites actually used have been identified in Sect 2.1

7. *The description of the AERONET sampling used to compute the autocorrelations shown in Figure 9 is missing. Were only the collocated AERONET observations corresponding to the satellite overpass times used (i.e., approximately one observation per day)? If so, the AERONET-derived autocorrelation coefficients may depend on the satellite overpass time and could therefore differ among the various panels of Figure 9. Please clarify.*

   The description is not missing but can be found in Sect. 2.5. The reviewer is correct that only AERONET data near overpass times are used and that this implies that the AERONET dataset used for evaluation will differ by satellite. It is possible to use a single dataset for all satellites at a substantial reduction in total data count (and increased statistical noise). For a discussion of this in the context of AOD evaluation, please see Schutgens et al. *ACP* 2020.

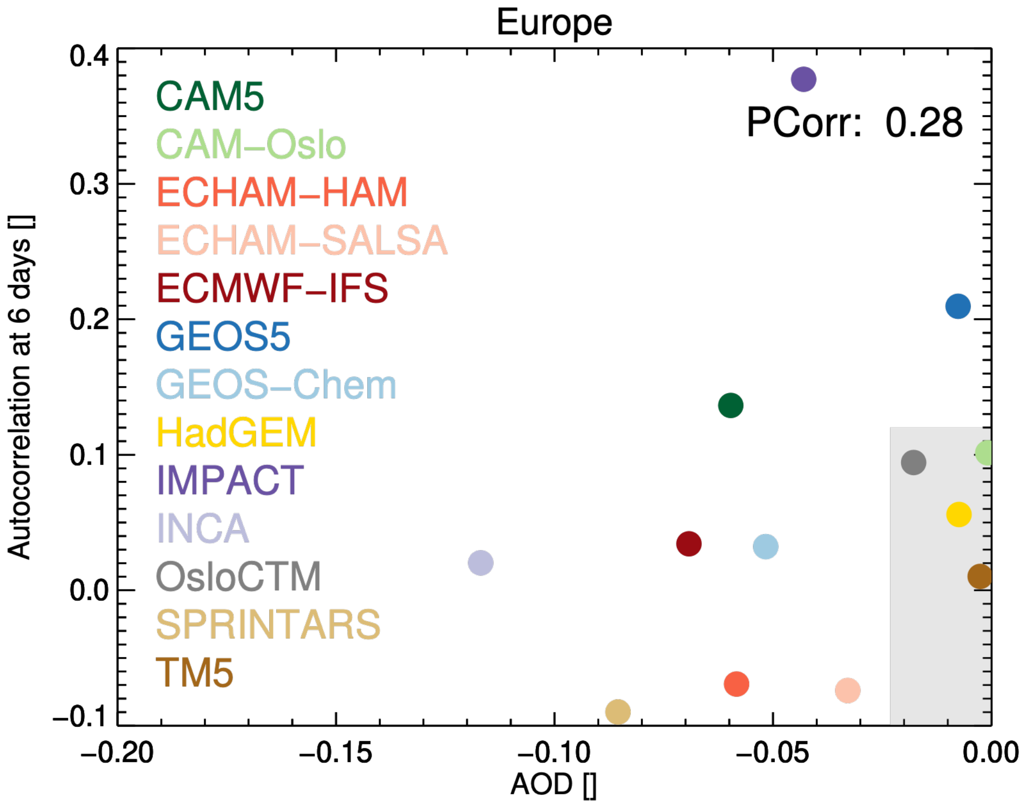
8. *Lines 349-354: Is the information discussed here presented elsewhere, for example in a supplementary figure or table? If so, please provide the appropriate reference.*

   We did not add any figures because we feel it is too much of a detail in our overall analysis. We will add some text to clarify this.

9. *Section 8: A useful addition would be a direct comparison of AOD between the models and the satellite products. Since the manuscript already discusses which processes dominate the variance in modelled AOD (Figures 4 and 6), such a comparison would help explain the differences in the estimated autocorrelations between the models and observations.*

   We agree. The reviewer was not specific about what sort of plot but we came up with the one below. It shows, per region, the error (vs POLDER-RemoTAP) of modelled AOD (horizontal axis) and ρ_6 (autocorrelation at day 6, vertical axis) for each model. The grey area shows the uncertainty range of the observations (conservatively, by showing the 3σ band, for AOD we assume $\sigma = 5\%$, based on our evaluation). The plot shows 1) how many models disagree with either one or both observations; 2) the low correlation between errors in AOD and ρ_6 (a few regions show high correlations but low correlations are standard). The correlation in errors in AOD and ρ_6 is identical to the correlation in AOD and ρ_6

themselves. This shows again that autocorrelations provide independent information from AOD. We propose to show figures for all regions in the supplement and will provide a brief discussion in the main text.



10. *Lines 440-445: Given the discussion in Section 2.6, could the authors comment more explicitly on how the inferred autocorrelation timescales relate to aerosol lifetimes? Establishing this connection would help strengthen the physical interpretation of the results.*

    Looking back at the equation for the simple box model Eq. (6) it is clear that any timescale of the solution will be set by timescales in sources and losses. The latter refers to the local lifetime. From analytical and numerical solutions we find that if the source timescales are (comparitively) very short, the decorrelation time is dominated by (actually equal to) the local lifetime. Conversely, if the local lifetime is (comparitively) very short, the decorrelation time is dominated by (actually equal to) the source timescales. If both source and loss timescales are of similar magnitude, the decorrelation time is a complex mixture of source and loss timescales (see e.g. Fig. 2 & 3 or Sect. S1).

    We will add text similar to the above to the Conclusions section.