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Interactive comment on "Description and evaluation of the tropospheric aerosol scheme in the Integrated Forecasting System (IFS-AER, cycle 45R1) of ECMWF" *by* Samuel Rémy et al.

Anonymous Referee #1

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General comments

This work describes the aerosol module IFS-AER used within the Integrated Forecasting System (IFS) from ECMWF, with changes described up until cycle 45R1. Since this model is used as part of the Copernicus Atmospheric Monitoring Service (CAMS) to forecast and reanalyze atmospheric composition, the model description and evaluation is of clear importance to the scientific community. The paper provides details behind the parameterizations used in the aerosol module, and compares model results to observations of particulate matter and aerosol optical depth (AOD). The improvements added in cycle 45R1 lead to better agreement with the observations compared

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with the model version from cycle 40R2. The paper presents a thorough description of the IFS-AER model, and would be suitable for publication in GMD. However, I do have some comments regarding the clarity of the manuscript and the comparison with the observations. Specifically, the motivation behind the code revisions/additions that were chosen could be more clearly discussed in the introduction of the paper. As well, the authors should try to identify which modification was responsible for the improvement between cycles 40R2 and 45R1. These changes would make it easier for the reader to follow the logic of the paper.

Specific comments

P1L13: The code cycle 40R2 was not mentioned before in the abstract, and therefore it is unclear why the version 45R1 is compared to 40R2, and not 32R2.

P5L1: When running coupled with IFS-CB05, SO_2 no longer is a prognostic variable in IFS-AER. Why there are more prognostic variables in the coupled version?

Why are certain code changes not included in operational cycle? It is clear to me that certain options would be computationally expensive (for example: P4L13-stratospheric chemistry; P4L29 - coupling with IFS-CB05) but I do not understand why others are non-operational (e.g. P5L17 - height of emissions for biomass burning and SO_2 from volcanoes, especially considering statement on P6L20-24). Perhaps a general statement could be added for why certain code segments are operational or non-operational.

P6L7: “remarkably consistent between the three datasets”: I would say two datasets, since only the anthropogenic emission inventories are compared.

P6L10-14: In cycle 45R1, do the scaling factors come from Kaiser et al. (2012) or Rémy et al. (2017)?

P7L21 and Figure 1: Are the units μg m^{-2} s^{-1} correct? Figure caption reads kg m^{-2} s^{-1}, and also the there are higher values shown than 0.25.

P10L3: It is mentioned that G14 is closer to the AOD observations, but this is not immediately clear to me from Figure 3, where for some months (e.g. May-June, Oct-Nov) it seems like M86 is closer to the AOD observations. Can you back this statement up with any quantitative measures (e.g overall bias?)

P13L8: “Maps of probability of occurrence of observed AOD by MODIS dust AOD above different thresholds”- this phrase is not clear to me.

P14L1: I do not understand how and why the anthropogenic SO_2 emissions are divided into these categories. Does “high”/“low” refer to altitude or the magnitude of emissions?

P14L28: What is the rationale behind the change in SO_2 lifetimes in the later cycles?

P21L7: In Table 6 the deposition velocities of sulfate aerosol also differ between continents/oceans

P23L20-21: This pattern of ZH01 showing lower deposition velocities for fine particles and higher deposition velocities for coarse particles is not clear from Table 7. For example, fine mode sea salt is indeed slower in ZH01, but fine mode dust is faster.

Why is the particle radius not considered in the wet deposition scheme? Aerosol radius can play a role in both in-cloud and below-cloud removal processes (e.g. Seinfeld and Pandis, 2006, Chapter 20). I am curious why a radius dependence was included for dry deposition but not for wet deposition.

P28L15: Is there a reference/derivation for the coefficients in the PM formulae? It can play an important role in the model evaluation, as mentioned in P39L3-5.

Table 9: Are these listed size distribution parameters used also for the dry deposition/sedimentation schemes? For sea salt and dust the size bin limits were mentioned previously in the text, but for OM/BC/Sulfate/Nitrate this is is the first mention of the assumed size distribution. Perhaps the assumed size distributions for these particulate species can also be mentioned in Section 2 or 3, when the different particle species are introduced.

Section 6 and Table 10 clarified some confusion that I experienced earlier in the paper, since they introduce the discussed code versions and highlight updates in the new CY45R1 code. I wonder whether it would be better to move this section and table earlier in the paper, since the reader would then be able to refer to this table when the code versions are mentioned.

P31L10-11: I don't fully understand the logic why this would affect the wet/dry deposition ratio of BC compared to sulfate. It could also be related to the choice of deposition parametrizations and the size distributions of particle types.

Model evaluation metrics: The RMSE and bias metrics will be biased towards detecting deviations between the model and observations when AOD/PM are high, since AOD and surface PM vary by a few orders of magnitude. The model's skill at matching lower AOD/PM locations could be overlooked using RMSE/bias. Did you consider using normalized metrics for the model evaluation, for example mean fractional bias? I think normalization would anyways help with comparing the bias and RMSE between regions in Table 12, since the mean AOD/PM also varies between regions.

P40L1-5: What is the reason for the improvement in model skill between CY45R1 and CY40R2? Can you point to a change in Table 10 that is responsible?

P40L6-8: Does the seasonal cycle of the North American bias in PM2.5 provide evidence for the hypothesis that the SOA emissions are responsible? Since this bias is larger for May-Nov?

P43L1: This statement could be made more accurate, to say that the coupling improves the *error* of the forecasts, especially over Europe. The *bias* seems to be worse everywhere except Europe and Africa in the coupled version.

P43L8-9: The final two concluding sentences could be made stronger, since these ideas were not discussed very much in the model evaluation. Perhaps these future upgrades could be linked to deficiencies that were found in the results?

Technical corrections

P2L10: space missing before (NCEP)

P4L18: “the” missing before prognostic variable

P4L26: Two components are considered of organic matter and black carbon, hydrophilic and hydrophobic fractions, with . . .

P7L29: fluxes of sensible

P9L19: two schemes

P9L20-21: is also shown to compare

Figure 5: Caption should specify that super-coarse dust is shown.

P14L23: CY is used before the cycle name for the first time. This should be made consistent throughout the paper.

P15L1: a diurnal cycle and a simple dependency on temperature

P15L23: “important decrease”- significant decrease -> but also could specify where - in North America and Europe SO_2 emissions have decreased strongly, in East Asia they have increased since 2000 (e.g. Hoesly et al., 2018)

P16L11: the equation numbering restarts from 1 here. Reactions should be numbered R1, R2, etc.

P17L24: fraction of airborne calcite in coarse

P23L10: the first comma should be a period?

P23L19: provides

P23L30: ZH01, notably

P23L31: will be addressed

P25Eq38: j-1 should be in superscript in numerator

Table 11 caption: in parentheses, not in hypens

P31L9: with

P32L5: combines

Figure 10 caption: should read (top) / (bottom) not (left) / (right)

P33L5: acronym RMSE should be defined the first time it is mentioned

Figure 12: titles of plot refer to global sites, but the caption and text refer to North American sites

P38L4: by a higher contribution from nitrate and ammonium

References

Hoesly, R. M., Smith, S. J., Feng, L., Klimont, Z., Janssens-Maenhout, G., Pitkanen, T., Seibert, J. J., Vu, L., Andres, R. J., and Bolt, R. M.: Historical (1750–2014) anthropogenic emissions of reactive gases and aerosols from the Community Emissions Data System (CEDS), Geoscientific Model Development, 11, 369–408, 2018.

Seinfeld, J. H. and Pandis, S.N: Wet deposition, in: Atmospheric chemistry and physics: from air pollution to climate change, John Wiley Sons, 932–979, 2006.

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