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Geoscientific Model Development Discussions

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 #2

Received and published: 6 August 2019

The paper describes the latest developments of the AER-IFS global aerosol model development by ECMWF in the framework of the Copernicus Atmosphere Monitoring Service. The model description is very clear and comprehensive and accompanied by various detailed evaluation experiments. Therefore, I support publication in GMD once the following minor points are addressed.

The code is however not available publicly, which is usually an issue for GMD publication which I leave up to the editors to decide. I note that I did not request access to the code to perform the review.

In the introduction and perspective, I am surprised that the added value of implement-

ing coupled aerosol model to improve the weather forecasts is not mentioned in the context of ECMWF activities. This may be due to the low priority given to aerosol-cloud interactions (P3L29), but more explanation should be given in the text on this choice to understand if the impact is thought to be marginal, or if the work is planned in the future.

My only general concern regards the lack of context about state of the art in global aerosol modelling, as it would be useful to explain in more details where the AER-IFS lies in comparison with other major similar models.

Specific comments:

P1L17: these mortality numbers are only for ambient air pollution (excluding indoor air).

P2L14: a link to the ICAP-MME service should be provided as a web search did not allow me to reach those alternative global aerosol products.

P3L29: why mentioning only the impact of aerosols on surface temperature as photolysis rates are also sensitive as acknowledged P5L5.

P4L13: what would be the implication of including stratospheric chemistry for AER-IFS given the importance of heterogeneous processes in the stratosphere?

P4L30: is SO2 or rather sulphate aerosol production rate (as stated P5L9) provided by IFS to AER-IFS when run in coupled mode?

P5L1-8: why not mentioning here the new CAMS-GLO-AP emissions that should be included in the global forecast soon (if not already)?

P6L8-19: the scaling factor for biomass burning emissions seems very ad-hoc. How is it handled for instance by regional models? What are the perspectives to represent explicitly condensation to cope with this issue?

P5L19: suggest adding SOA in the title of the section. Given the importance of SOA

acknowledged P7L16. More information on the state of the art on SOA modelling in global aerosol models should be provided.

P9L26: typo: M86 instead of G86

P10L3: quantitative scores (RMSE, correlation and bias) should also be provided as it seems that the new formulation sometimes leads to high overestimations of low AOD levels.

P13L5: the figure legend says total emission are presented whereas only super-coarse are mentioned in the text.

P21L3: where was it shown that differences where small? Is it only true locally or just on average?

P24: use the same unit in the table & figure

P26L13: I find it confusing to present here ageing, while hygroscopic growth is presented in 3.6

P28L7: what is referred to as CAMS mineral dust model? AER-IFS?

P28L15-24: the notations are not defined (SS1, SS2, etc.)

P29 Table 9: what is referred to as CAMS model? AER-IFS?

P29L11: confirm if the upgrade to 137 levels has been completed by now

P30 Table 10: what is referred to as IFS versions? AER-IFS?

P31L9: typo “wityh”

P32L24: typo: “Unites”

P37L10: the sentence is inconsistent. Fire and dusts are mixed, and my understanding is that Europe was discussed in that paragraph.

P43L1: whereas coupling improves AOD over Europe, it is not the case for PM and

should be reminded in the conclusion.

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