

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2021-264

Anonymous Referee #1

Referee comment on "Description and evaluation of the tropospheric aerosol scheme in the Integrated Forecasting System (IFS-AER, cycle 47R1) of ECMWF" by Samuel Rémy et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-264-RC1>, 2021

This manuscript presents a description of the changes carried out between C47R1 and C57R1 in the aerosol module of IFS. My main concern is about writing quality. In its current state, the manuscript is very difficult to read and not very well structured. Many information is not provided in the manuscript as often only the changes made in the model are described with differences with parameterization used operationally and not making the paper confusing. For examples, almost no indications are given on how the formation of secondary aerosol was taken into account in the model. The hypotheses of the model are often not clearly explained. The manuscript is more a technical note designed for people working with the IFS model and not a scientific paper for the whole scientific community. In my opinion, the manuscript should not be accepted in its current state. I strongly advise the authors to make significant revisions of the manuscript, to present well the parameterizations by explaining their physicochemical significance and the assumptions behind these parameterizations. The authors should write a standalone manuscript with all the information necessary to understand the model.

Major comments:

No explanation is given in the choice of the parameters in Table 2. What is the basis for these parameters? I think there are wrong values given. For some lines, several r_{mod} are given with no explanation or on the opposite not enough values for DUST or salt lines where there is several modes of aerosols. Are the parameters corresponding to a volume distribution or a number distribution? The values of r_{mod} seem to be very low (often around 10 nm, as if the aerosol would all be nanoparticles in the atmosphere). It does not seem realistic as it is not in the accumulation mode as it should be. In that case, all the processes calculated with the aerosol diameter are probably not well represented in the model. However, later in the text, it is explained that the deposition rates are not

computed with the aerosol size. The density of organic aerosol seems very high.

Almost no precision is given in the representation of phenomena. Many times, the authors refer to the previous article, without giving a short description of what the model does and what are the assumption. I think the paper should be entirely restructured as it is often not clear what is done and what is the rationale behind the parameterizations. Here are a few examples :

- The description of deposition rate arrives late in the text (section 5) whereas global budget including deposition are given quite early. It would help to provide all the information of the model earlier.
- How was the partitioning of nitric acid and ammonia represented? Has a thermodynamic module been used? How is the formation of coarse nitrate represented in the model? In the current
- Section 2.2. What are the heterogeneous reactions included in the model?
- SOA emissions scaled to CO? While I personally think that such approaches are not reliable, I understand that some models use this kind of approach for simplification purposes. It should be explained how these emissions are determined and what kind of chemistry it implicitly accounts for. What are the consequences of using this kind of simplified parameterizations? Are there SOA/CO ratio specific of emissions sectors (I don't understand how a single ratio could be used)?
- Section 5.2.1. I don't understand how the Di parameter was selected. Should the evaporation of droplets lead to an "evaporation" (probably not the good term as I don't see how non-volatile dusts or BC can evaporate) of particles from the droplet? In reality, it would probably stay in the droplet unless the evaporation of the droplet is complete or has a low settling velocity.
- I don't understand the basis behind the equation in section 5.2.2 with some not defined parameters.
- Section 6.2: Before comparing to AeroCom, it would be useful to provide information on the exercise. It is very difficult to understand what is done without having basic information on the exercise. What are the conditions of the simulations? When comparing the flux of emissions for Sea Salts, are the emissions corrected by the factor 4.3? Are all the emissions computed for the same conditions of humidity or are they corrected the same way? I understand the idea of the AeroCom exercise to compare the budgets between the model to see how much results differ between models. I however fail to see the interest of evaluating the representation of the budget from IFS-AER by comparison to the median of models. All models could fail to represent one process, in that case the median of models would be wrong.
- Comparison to observations: why did you keep traffic stations in the analysis. At the resolution of the simulations, it induces a very large bias. All the comparison should be redone without including traffic stations (or removed from the paper).
- The aqueous-phase sulfur chemistry seems to be lacking from the model (as only the gas-phase chemistry of CB05 is mentioned) while it is generally the main oxidation process of SO₂ and of sulfate formation. I don't see how the sulfate aerosol formation could seem trustworthy. The fact that IFS-AER overestimates sulfate concentrations without accounting for the aqueous-phase chemistry seem to indicate that the representation of some phenomena are not accurate.

Minor comments:

"The mass mixing ratios of these two are passed from IFS-CB05 to IFS-AER, used in the nitrate and ammonium production schemes, and updated in return by those schemes." Are IFS-CB05 and IFS-AER, two separate models. In that case, it would be necessary to have a scheme. Otherwise, I suggest to change or remove this, sentence as I think the authors just want to say that the concentrations given the gas-phase chemical mechanism are used as inputs for the aerosol module.

P1,L5. "The parameterizations of sources and sinks that have been updated since cycle 45R1 are described" While correct, the sentence is a bit confusing as "are described" refer to 'the parameterizations' at the beginning and not 'since cycle 45R1'

P1, L9: if you use the IFS acronym it is probably better to say "of IFS" rather than "of the IFS"

P1, L10-11: "components that are not used operationally will be clearly flagged." Should I understand that is not the case currently. In that case, should this sentence be highlighted in the abstract?

P1, L12: a wide range **of**

P1, L13: What is meant by an increase in skill?

P2, L15: Not clear what is meant by imbalances

L22-26 : This paragraph with many sentences beginning by "Section ..." could be improved

P2, L28: I was not sure what was meant by "bulk-bin scheme ». It should be explained. I don't think that it the good expression. It seems to be a model approach with a single mode and not a "bin" scheme that represent for me a sectional approach

P7, L5: What does the "implementation of a cap" mean?

Title of Section 4: Primary aerosol sources ?

P23,L1: remove as in "longer than as simulated"

P23,L11: Not sure what is meant by ""the very short lifetime ... is dominant".

P30, L19: a bias of 2-5 $\mu\text{g}/\text{m}^3$ over Europe does not seem low. Later it says that the bias is negative where the number provided is positive. Is it an underestimation or overestimation