

Anonymous referee comments on

“An evaluation of the regional distribution and wet deposition of secondary inorganic aerosols and their gaseous precursors in IFS-COMPO cycle 49R1”

by J.E. Williams et al.

This publication presents the recent developments in IFS-COMPO about secondary inorganic aerosol formation representation through EQSAM4Clim module and wet deposition. After quickly presenting model configuration and used observational data, authors presents a validation of secondary inorganic aerosols and their precursors surface concentrations. Last part is about wet deposition of SIA compared to observations.

General comments:

This study is very similar to the Remy et al., 2024, with the first part that only focuses on the performances of SIA modelling in IFS-COMPO. The novelty that is the wet deposition treatment is barely processed. I would recommend to be more specific about the changes in the code. A detailed description of the wet deposition scheme along with explanation about the choices and the expected results. The wet deposition analysis has to be longer than the first part that can be reduced. Also the CY48R1_NOEAC which allow to separate the effect of EQSAM4Clim from the wet deposition (and other changes?) needs to be more used to explain and analyse the impact of changes on wet deposition. Also, it would be interesting to have global maps of wet deposition even if there are no measurement at global scale.

The global structuration of the text need to be revised and an in-depth proofreading needs to be done before submitting. It is frightening to see as many wrong figure and table references in a submitted paper. Also please read the GMD author guidelines and try to follow them:

<https://www.geoscientific-model-development.net/submission.html>

Concerning the figures, their quality is overall poor, especially in the Appendix. I would recommend to do an effort to increase their quality and readability. Also the partitioning between the paper and appendix should be questioned. Figure A1 for example is used several times and is worth to appear in the main part of the paper.

As you are not using the “real” CY49R1, but modified version of CY48R1, I would recommend to delete all reference to CY49R1 expect from the fact that the presented modifications are meant to enter in the latter.

A section is missing to explain how are computed the statistics. Are all the data gathered in one vector and the tables represent spatio-temporal statistics?

About tables presenting budget, I would recommend to check whether the conservativity is assured and to discuss the possible explanation for differences. Generally speaking you should check different number in tables. I spotted a few problems listed in the specific comments, but I didn't bother to check everything.

I would recommend a large amount of work on this paper before publication.

Specific comments:

- Line 126: "which will be operational in November 2024" : Please update this sentence as the date is now expired.
- Table 1: The Experiment IDs are not used in the text. Please remove them or add a sentence to tell that they can be used to retrieve the data on the MARS storage system.
- Line 205: Please add a reference for the direct production from hot shipping exhausts.
- Line 211: AirBase is no longer used since mid 2000's. The new system is called AQeR (Air Quality e-Reporting)
- Line 214: How did you select "rural background stations"?
- Line 271: Figure A1. Please introduce the figure before referencing it. Also Fig. A1 is used a lot. Maybe it would be interesting to move it to the main part of the paper.
- Line 272: Please introduce Table 3 before using it.
- Line 272: "For CY49R1 [...] by approximately.1-0.2 $\mu\text{g}/\text{m}^3$ " → I assume you are talking about Fig. 2 results. Please introduce it and announce it properly or move the sentence in the SO4-- paragraph.
- Line 274: Figure A1.
- Line 279: You introduce Table 3, but you used it only in the next paragraph.
- Line 306: "Statistics relate to seasonal means" → Please be more precise about this sentence. Maybe in the section 3 when introducing observational data.
- Table 2: For Europe, according to the section 3, data must be AQeR (replacing AirBase). For US it is AirNow, and China CNEMC.
- Table 2: RMSE are missing in the table.
- Table 2: Why would you omit CY49R1_NOEAC in the Table, as you put the results on Fig. 1?
- Table3: (also valid for other budget table): Maybe you could add a minus sign in front of figure representing a loss for the considered species.
- Line 312: Figure A1
- Line 316: Weekly or seasonal?
- Figure 1: For European figures SO2_surf → SO2 and EBAS → AQeR. For US figures, AirBase → AirNow.
- Line 322: Modify AirBase and CNEC.

- Line 322: For China, is it weekly or seasonal?
- Line 326: “along with associated biases”. Add “not shown” or modify Fig. 2.
- Figure 2: The format of Fig. 1 is very interesting, why not use the same for Fig. 2 and have the three area and the biases on the same figure?
- Figure 2: Change EBAS to EMEP in the title.
- Line 341: “The low MB for SO₂ [...] is too fast”. I don’t understand this sentence. What rate of oxidation for which species? Please be more precise.
- Line 354: Are you referring to direct production from hot shipping exhausts?
- Line 370: Please add that you plotted surface concentrations.
- Figure 3: Why not plotting also CY49R1_NOEAC
- Line 388: I don’t understand the reference to Fig. 8-10.
- Line 392: EBAS → EMEP
- Line 401: Figure A3.
- Line 401: “The corresponding [...] in Table 5.”. This sentence is not properly places as you don’t use Table 5 for a while.
- Line 403: “Tich?” → “Tichy”
- Figure 4: EBAS → EMEP in the title
- Line 423: Introduce Table 5 here.
- Line 434: “Although maximum [...] during winter.” Do you have an explanation for this phenomena?
- Line 439: Figure A5 → Fig. A4
- Line 441: “This suggests [...] of HNO₃” → Is this a remark true at global scale?
 - Figure 4 also seems to show that the emissions of NH₃ are too strong in Europe when considering NH₃+NH₄. What can you say about this?
 - Also, in the US what are the implication of the ammonium aerosol formation together with sulfate that are overestimated according to Fig. A2?
- Line 455: Table A1
- Line 456: Figure A3?
- Line 456: “Differences between [...] burden in Table 4.” You are comparing surface field with 3D diagnostic. This is not necessarily relevant, especially when it comes to SIA.
- Line 459: -0.26 µg/m³
- Line 479: I didn’t see a proper discussion section.
- Line 484: Table 5
- Line 486: Figure A3

- Line 488: Figure A5
- Line 503: Table 6
- Line 514: Figure A6?
- Line 519: Figure A6
- Line 520: You introduce Fig. A 7 6 now, after referencing it twice.
- Line 521: I supposed you mean relative difference in percentage.
- Line 523: Please add a reference for the sentence about direct HNO₃ emissions.
- Line 536: I thought you didn't have direct shipping emissions?
- Line 545: please keep using ppb all along the paragraph.
- Figure 6: EBAS → EMEP in the title.
- Line 557: "The evolution [...] bottom panel" I don't see any bias plotted.
- Line 559: Fig. A7
- Line 569: Table 8 is not introduced
- Line 578: Fig. A7
- Table 7: relative difference for coarse NO₃- in CY49R1 is false.
- Line 597: I would rather write 0.2-2 µg/m³
- Line 612: please add "surface" concentrations
- Line 645: "corresponding" → "comparison with observations", or something approaching.
- Line 649: "following figures" → what figures?
- Line 664: Figure 8
- Line 664: Figure 8 should be introduced in the next paragraph as you use it.
- Line 665: "To allow [...] 1000 mgS/m²/year" You should remove this sentence as it doesn't reflect the figures content.
- Line 682: "by around 10 %"
- Line 694: I suppose it is Table 4.
- Line 699: temporal → spatial
- Line 702: Do you have volcanic passive degassing emissions in IFS-COMPO? Please discuss also this aspect.
- Line 705: As Fig. A1 is quite difficult to analyse due to its poor quality, we have to believe you.
- Line 744: I don't see a measuring point in Iowa.
- Line 747: 0.77 → 0.72, or a high correlation is achieved in CY48R1.
- Figure 8, 9 and 10: Labels are unreadable for Asia

- Line 715, 760 and 799: Table 10
- Line 780: Figure 13?
- Line 788: I don't understand the sentence about the fully coupled forecasting system. Please explain.
- Line 790: "values range from 50-800 mgN/m2/year with the highest value (>2000 mgN/m2/year)"
If the values range from 50 to >2000, please write 50 to 2xxx.
- Line 806: "with the next operational IFS version (CY49R1)". No you didn't use CY49R, you used CY48R1 with aerosol evolution submitted for CY49R1.
- Line 816: I would not be so direct. Indeed some aspects are better represented using EQSAM4Clim, but other don't. Please be more specific about this part of the conclusion.
- Line 854: Please modify seasonal with monthly
- Figure A4: how many stations were used?
- Figure A5: The observations are missing in the legend. Also use CY4xxx as reference instead of Experiment IDs.
- Table A1: relative difference for CY49R1 bias and RMSE should be negative.
- Line 896: As for figure A1

Author contribution:

I don't understand VH contribution as you did not use the "complete" CY49R1 cycle, but only a modified version of the CY48R1.

Code and Data Availability:

I Guess there is a missing statement for the simulation data availability. Maybe there are available through MARS.

Acknowledgements

Please modify AirBase with AQeR and add CNEMC.