

Interactive comment on “Online coupled regional meteorology-chemistry models in Europe: current status and prospects” by A. Baklanov et al.

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General comments.

This is a very important, impressive and useful paper which will be a benchmark for the coming years on the subject of on-line coupled regional meteo-chem models. I would like to congratulate the authors with this major achievement.

Thank you for reviewing our manuscript and providing valuable comments. We have incorporated all your comments and suggestions in the revised manuscript. Please see below our point-by-point replies to the specific comments. Please find all our replies in red colour, following your remarks, which we copied and kept in black.

Specific comments.

- Although it is implicitly clear that the subject concerns the troposphere, I would like to suggest to make this explicit in the Introduction. There are also on-line models for the troposphere and stratosphere.

In the introduction, we added the sentence “This is particularly true for regional-scale models, which are the focus of this review, whereas in global-scale and in particular in stratospheric modeling, the development and availability of online coupled models is more advanced.”

- Although the size of the paper is already impressive, I would have liked to see more text and information under 4.7.2, Wet and dry deposition. The important subject of which land use/land cover data base to use is not mentioned (large impact on dry deposition). Wet depositions is a very important sink for aerosols, and its treatment in the models is associated with many uncertain aspects, of which rain, where and how much, might be the most important

We agree about the importance of the deposition processes. However the referee 1 requested to shorten this section even more, because these processes are not specifically considered in online models only. So, we modified the text, concentrating more on the aspects of advantages of the online modeling approach related to the deposition processes. The important subject of which land use/land cover data base to use is now included. Furthermore, the importance of land use/land cover data sets and parameterizations is now also acknowledged in the conclusions section, as this point was made by several referees.

- Under 6.3, Model evaluation, I would have liked to see, for example on page 12614, text concerning the inherent problem that by evaluating for example calculated PM 2.5 concentrations of an online model with observations, it is nearly impossible to determine by which process the calculated concentrations are influenced, by emissions, or by the feedback through clouds, etc. In my opinion the methodology by which results of an online model can be evaluated is still missing. Maybe the way forward would be detailed evaluation of processes in the model, with also using ratios of calculated concentrations for evaluation,

for example EC/PM 2.5, and NO₂/PM 2.5 .

We certainly agree that comparing a quantity like modelled PM_{2.5} concentrations with measurements does not reveal much information about the processes involved. Ratios as suggested by the reviewer would help. Another way would be using a process analysis tool to distinguish the contribution from various processes to the concentrations. Such tools exist for offline models to determine the contribution from emissions, transport, chemistry, etc. It would help substantially to use such tools for online models to distinguish the effects of feedbacks. We added a sentence in section 6.3 regarding this.

Another option is to examine, if certain spatial-temporal features embedded in the observations are modelled well. This entails evaluation of the spatial-temporal correlation structures among certain variables in observations against those in model outputs. Note that commonly used model evaluation methods, namely, mean bias, RMSE, etc. are not very useful here to reveal strengths (and weaknesses) in a model.

Evaluating whether including a feedback or not improves model performance is a very difficult task due to the chaotic nature of weather, and either evaluation against long-term measurements or using ensemble modelling approaches are necessary to distinguish signals from noises. This aspect has been elaborated a bit further in the revised manuscript.

Under 7. Conclusions and recommendations, my impression is that here and there aspects are mentioned that have not been addressed in the paper before, like under 7.2.1 Emissions, the aspect of emissions from ships and aviation. Maybe these items could be listed explicitly under "important items not addressed before.

The conclusions have been rewritten and were made more consistent with 4.7.1 and 4.7.2, where shipping emissions and volcanic emissions are now mentioned.

- Under 7.2.1 meteorology-dependent emissions are mentioned. I miss the meteorology-dependent anthropogenic emissions like house-hold heating

This has been added.

- Most of the tables are very useful and informative. I do however have a problem with table 1 and 2, on page 12695. Here the words : modulates, controls, determines, affect, influences are used. This gives the impression that a kind of grading of strength of impact is behind these words, controls is stronger than influences. However, the use of these words is not consistently in the tables,. I would suggest to have a careful look to the use of these words, and make it consistent.

We carefully checked the use of the different words and made their use consistent.

Technical corrections

- page 12545, line 6. OCMC is used without clarification

OCMC is the acronym for "online coupled meteorology chemistry", which has been removed in the revised manuscript.

- page 12546, line 20 stated "definitely improves". I would suggest is expected to improve

The suggested change was made.

-page 12548, line 20 and 24. I have problems to understand the sentence "mechanisms of altered meteorology impacts on meteorology, and altered chemistry impacts on chemistry. Could it be something like: by which altered meteorology has an impact on other meteorological phenomena.

The suggested change was made.

- page 12551, line 13 gives online access models, but the definition comes later, under 3.2.
This term was defined when it is first mentioned in the revised manuscript (see definitions in introduction and its extension in section 3).

- page 12555, line 6, please add nitrate, ammonia. I also could not find a reference to table 5 somewhere in the text.

The suggested changes were made.

- page 12559, line 2 add a in generate a code and line 3 , a numerical solver

The suggested changes were made.

- page 12560, line 13, why is and evaporation between brackets?

The suggested change was made.

- page 12583, the text of line 13-18 is already on page 12549 . And line 25 gives the word exacerbate, do you mean increase?

The repetition has been removed. We use 'increase' instead of 'exacerbate'.

- page 12589, line 12, at large instead of a large

The suggested change was made.

- page 12591, line 26, "and to be developed" should be removed

The suggested change was made.

- page 12596, line 22, better background than clean air

The suggested change was made.

- page 12597, line 3 and 20, background instead of clean

The suggested change was made.

-page 12600, line 21, reference to table 4 should be table 5

The sentence refers to Table 4, where the IFS-MOZART system is presented. The reference is then maintained to Table 4.

-page 12614, line 26 stated : not employ much data assimilation to allow for model internal feedbacks: I do not really understand the sentence

This statement means that no nudging or data assimilation techniques should be employed when studying feedbacks. To avoid confusion, the last part of the sentence on the aspect of data assimilation was removed.

-page 12618, line 1.

What is meant exactly by drag interaction?

The 'drag interaction' is linked with turbulent fluxes at the surface. This is rewritten correspondingly in the revised version.

-page 12627, line 22, COT is not defined

COT (= cloud optical thickness) is explained in the "abbreviations list". We now define this acronym in section 6.2, where it is used for the first time.

-page 12703, table 7. Under approach the word mixed is used, it means model and sectional? And also mass only is used, the text gives bulk

Probably the “mixed” term is not the best for this case for the Table 7. The M7 aerosol model uses the pseudo-modal approach with mixed modes for soluble/insoluble components. So, for simplification we can write ‘pseudo-modal’ here and describe the specifics in the text with the corresponding reference on Vignati et al. (2004).