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

Anonymous Referee #6

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This manuscript presents a review of the current status of online models with specific focus on models developed in and applied over Europe. An article of this nature would be of interest to the ACP readership as it attempts to present a synthesis of scientific progress in the development, application, and evaluation of online coupled meteorology-atmospheric chemistry models, a modeling paradigm that has received significant attention over the past decade. Assembling a multiple-author comprehensive review such as this is a challenging undertaking and the authors should be commended for their effort. However, in its current form many of those challenges are evident in the structure and organization of the manuscript as reflected in the differing level of detail, writing styles, and general flow amongst the various manuscript sections. I believe that with some effort these can be easily addressed to develop a valuable contribution to the scientific literature.

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

The following comments and suggestions are offered which may help improve the usefulness of the manuscript:

1. While the manuscript could be considered to be comprehensive, in its current form I feel it is too lengthy to be impactful. Though the process descriptions are largely accurate, one could argue that these are not necessarily unique to online models and similar details can be found in existing literature. Additionally, the disproportionate level of detail among these descriptions could leave potential reader wondering if the more detailed sections have special relevance to online models (which is not the case).
We agree that the level of detail is different in different sections and removed detail in the revised version at some points. More specifically, the different subjects touched upon in our manuscript have different levels of scientific understanding, and are of different relevance for online-coupled models. This is one of the reasons why the level of detail varies. Another reason is, of course, the different authorship of different sections. The choice of different authors for different sections had to be made as none of us has expertise in all the different aspects of online-coupled models. However, we have tried to shorten and harmonize the sections more and to improve grammar and style to the extent possible.

a. As an example, section 4.2 presents an excellent and detailed overview of chemical mechanisms (their structural basis and formulation, relative level of detail etc.) employed in current models, but does not necessarily speak to implications/needs for online models – what aspects of chemical mechanisms and/or representation of atmospheric chemistry need to be considered for coupled/online models that are not considered for traditional offline models?
Yes, we consider the chemical mechanisms used in online models as a very important issue, so decided to describe/overview them. We mostly touched the mechanisms realized in the considered 18 online models, however, we agree that they are also used in many offline models. This section has been slightly modified correspondingly.

b. Similarly, the extensive discussion on treatment of aerosols instead of providing an overview of the approximation/assumptions in representing the hydrophyllic/ hydrophobic constituents and size distribution of airborne aerosols could instead be focused on implications for optical/radiative properties of the aerosols and consequent impacts on the modeled feedbacks.
These sections have been modified and extended with additional references.

c. Much of the description on emissions while comprehensive is also applicable to any air pollution modeling system. Perhaps the discussion could only focus on meteorologically modulated emissions with some indication of the relative benefits associated with their estimation in coupled systems as opposed to traditional approaches.
We Agree. These sections have been modified more focusing on meteorologically modulated emissions and their effects. Additionally we also significantly modified the conclusions Section 7.2.1 on "Emissions and Depositions".

d. The discussions in section 4.6 and 4.8 are more germane to online coupled atmospheric models and can be expanded to include the pertinent process connections or current limitations as they relate to online coupled systems.
Section 4.6 has been restructured, expanded and correspondingly rewritten. Section 4.8 has been substantially rewritten and shortened following recommendations from several reviewers.

e. A careful review could also help reduce some redundancy in this section – for example much of the information in section 4.3.9 is also provided in section 4.3.6. While I realize that some of the detail in section 4 may be necessary to satisfy the COST action task (as stated in the abstract), the detailed background information not specific to online systems could be deleted or moved to an appendix. I would strongly recommend reducing the length of Section 4 to focus only on processes and modeling methods as they pertain to online/coupled modeling. It would help reduce the manuscript length significantly and provide a clear picture of how process treatments differ (or should differ) in offline and online approaches.
Section 4 has been substantially rewritten and restructured, partly shortened. Some redundancies in this section, keeping the main focus on only the processes important for online coupled modeling, have been removed. However, Section 4 is a key section for the paper, so its shortening has been done only where it is possible.

2. Section 2 is intended to provide the rationale and requirements for online atmospheric models and consequently is an important component of this manuscript. As currently structured, the flow is somewhat broken, and I would urge the authors to consider restructuring the write-up to more clearly emphasize the needs for these systems.
Section 2 has been restructured and modified following this and other referee's comments.

The expert survey results in Table 3 though largely subjective (as also suggested in the discussion to be affected by individual opinions), contain some interesting information that could probably be expanded on in the discussion in this section. Categorizing the importance of the various interactions across the three major application areas of these models is very useful. As indicated at several places in the manuscript discussion, many of these interactions are not mutually exclusive. It would make the results of this survey even more effective if the authors can identify the commonalities in the needs and scientific gaps across the three application areas. It appears to me that with some effort the authors should be able to provide some guidance on the critical knowledge and/or model process representation gaps that could help address the needs of all three application areas. Such a discussion would be of interest to developers and users of these evolving online coupled atmospheric models.

Thanks for these useful comments. Actually, the difference and commonalities among three model categories have been stated in the discussion text on Page 12550 lines 8-13: “These results show that the perceived most important interactions differ from one model category to another. In general, most of the meteorology and chemistry interactions are more important for CWF models than NWP and climate models, and those interactions are represented better in CWF models than in NWP and climate models (see averaged scores in Table 3).”
Based on the survey results, we also provided recommendations for the model developers in the following paragraph on Page 12550 lines 14-18: “Therefore, primary attention needs to be given to interactions with high rank of importance (score1) together with low score in the model representation (score2), such as “improvement of aerosol indirect effects” for both NWP and climate models, “changes in liquid water affect wet scavenging and atmospheric composition” and “improvement of wind speed – dust/sea-salt interactions” for CWF models.”
In order to provide more comprehensive information, recommendations on how to improve aerosol indirect effects for NWP and climate models have been explained.

3. Page 12583, line 10: OCMC should be spelled out.
We removed this shortening.

4. Page 12584, line 5: it would be useful to explain why online access models are limited in representing chain effects. The brief description in section 3 suggests that major distinction between online access and online integrated models could potentially only be the way data is shared between the “chemistry” and “dynamics” calculations – it is not apparent how this would limit the representation of chain effects. I can see that there may be issues related to consistency if common processes are represented differently (in some cases that may be by design).
The main reasons for such a limited ability are now better explained in the text.

5. Page 12584, lines 6-11: the paragraph as written is awkward and does not say much – should be rewritten or deleted.
This paragraph has been shortened and rewritten.

6. Page 12585, lines 10-20: It would be useful to include some discussion on the relevant time scales for representing the impacts of pollution on crops, the subsequent carbon cycling and how these may be represented for typical simulation time periods of regional systems.
The CO_2 impact is instantaneous on the leave stomata while the ozone causes damages to the plant during the whole growing season. Both effects accumulate year after year accelerating and decelerating, respectively, the plant growth. To address the reviewer’s comment, the following statement on the impact in regional models has been added in the revised version:
“If the time scale of the simulation is up to a year the short-time effects of CO_2 and ozone can be taken into account.”

7. Page 12586, first paragraph: the discussion does not appear to fully capture all the models listed in Table 4 that also appear to have the stated degree in complexity in representing chain reactions. The paragraph could be deleted without loss of any significant information that can already be gleaned from the tables and model descriptions´in the appendix.
The discussions have been removed as suggested.

8. I am glad to see that the authors have devoted some effort in Section 5.1 to highlight a fundamental but often ignored aspect of representing pollutant transport. It is however important to note that the issues associated with wind mass consistency and its manifestation as artificial first order source/sinks terms in the numerical solution of tracer advection are not new as implied in the discussion and can in fact be traced to several early studies with Eulerian systems and diagnostic wind fields (e.g., Kitada et al., Atmos. Environ., 1983; Mathur and Peters, Atmos. Environ., 1990). Approaches to achieve this consistency have also been suggested by Odman and Russell (2000; http://people.ce.gatech.edu/_odman/23itm.pdf) and Byun (1999) using wind and density fields from prognostic formulations. For completeness, it may be useful to present the discussion in this context with the appropriate citations.

Thank you to the reviewer for pointing this out.
On page 12587-12588 we reformulate to the following “Since Rasch and Williamson … consistency property (e.g., Jöckel et al., 2001).” into: “It is also important to mention the so-called wind-mass inconsistency problem, which turns out not to be trivially resolved in online models (e.g., Jöckel et al., 2001).“
The section 5.1.3 has been modified/extended in response to the comments from reviewer #6.

9. Mass consistency, conservation, and spurious un-mixing are inter-related issues in the numerical representation of tracer advection. Additionally, a conservative, monotonic, and positive definite advection scheme alone does not guarantee mass consistency if the 3D wind and density fields do not strictly satisfy continuity – the discussion in section 5.1.3 should be modified to clarify these aspects.
The section 5.1.3 has been modified accordingly.

10. Page 12589, lines 5-10: it is not apparent to me why excessive damping would occur for small courant numbers – did the authors imply large courant numbers?
The excessive damping comes in play because the number of damping re-mappings (both in flux form and in cell integrated finite volume schemes) that are needed to simulate a given period becomes the higher the smaller the time step. In classical semi-Lagrangian methods this is also a well-known observation: if trajectories are correct, the advection equation is solved the more accurate the longer the time step, i.e. the larger the Courant number (also far beyond 1).

11. Page 12591: “conservative flux form for conserved variables” is awkward and should be reworded.
Yes, it is simply deleted: “for conserved variables”.

12. Page 12591, line 18-25: Two-way interactions between meteorology and chemistry can also be represented in what are classified as online access models. It is not readily apparent what is implied by “more complete integration between meteorology and chemistry” – adding some specificity would help clarify what is being implied. One could argue that based on process time scales, in most practical applications of such models, not all chemistry-transport-removal processes need be coupled/integrated at the finest time step. This should be a consideration in determining the level of integration and coupling between processes to facilitate the practical use these detailed modeling systems (timely short-term forecasts to long-term climate assessments). The suggestion that online access modeling is less effective (page 12625, line 20) may also need to be qualified.
The authors use the definition of Baklanov and Korsholm (2008) on online integrated and online access models. From the authors perspective, an online access model is the one using a coupler, that exchanges information among model processes (i.e., meteorology and chemistry) maintaining the model components separately, and not always using one main time step and might even calculate on different grids, despite 2-way exchange of information. Thus, two-way coupling considering full chains of feedback processes is not possible or at least very problematic.
We clarified this also in the manuscript.

The suggestion presented in page 12625-line 20 is a result of the experience from modellers. The specific result comes from the IFS-MOZART coupling, where the online access approach was less efficient than the online integrated approach (realized in the new online integrated C-IFS model).

The reviewer is raising a very valid point: How tight does the coupling need to be to generate a realistic representation of feedback effects? Additional text discussing this and specifically the online access approach has been included. The two-way coupling interval in online access models should be at least shorter than the time-scale of the relevant processes involved.
For aerosol direct effects, for example, we don’t see a strong need for updating the aerosol fields very frequently. For aerosol indirect effects, however, a tight integration (and hence short time steps) are

more critical. However, there is no simple and universal answer to this question. More studies for different feedback mechanisms and applications are needed.

13. Page 12592, line 20: MCCM should be spelt out at the first instance.
The model acronym has been removed from the sentence.

14. page 12592, last paragraph: The discussion on varying degrees of complexity in coupling is somewhat subjective and does not add much. The classification alone of slight, moderately, fully coupled provides no useful insight on the implied complexity– the entire paragraph can be deleted without any loss of relevant information.
The authors understand the point of view of the referee. Currently, there is no exact definition of the degree of online coupling. The authors present the approach of Zhang (2008), who firstly introduced this degree of online coupling classification. In this sense, the authors believe that the information is relevant for the reader, pointing out the different degrees of coupling that a model may have.

15. Page 12593, lines 4-10: It is not readily apparent what "full-coupling" implies and what if any specific distinction or similarity between the listed models one would glean from this discussion that cannot be found in the appendix and tables – the paragraph could be deleted or should be re-written.
Thanks for the comment. The meaning of "full-coupling" is now clarified in the revised manuscript. The authors mean by full-coupling the inclusion of direct and indirect effect of aerosol on meteorology (radiation-aerosols, clouds-aerosols), it is used synonymous with online integrated. To avoid confusion we do not use it apart from the short introduction of this concept. Table 4 presents the feedbacks implemented in the revisited models in column 5 (Feedback of pollution to meteorology).

16. Page 12593, lines 11-20: The authors are correct in pointing out that consistency between nests should be maintained – however this is easier said than done. The use of traditional two-way nesting approaches designed primarily for passive tracers, poses fundamental mass conservation challenges for non-linear reactive flow problems. Adding and then harmonizing the non-linear feedback effects simulated over the different resolution nests will pose even greater challenges – thus simply inheriting the approach from mesoscale models into the coupled systems is likely to create many consistency issues. Some discussion on these potential pitfalls with two-way nesting should be included.
The authors fully agree with the referee comment. A sentence has been added to the last paragraph of Section 5.2: "A traditional two-way nesting approach may create many consistency problems due to the non-linear reactive effects of the chemistry between nests."

17. Page 12595, line 14: "have also to be known right at the beginning" is awkward – please reword.
The sentence has been reworded as: "In addition to the meteorological fields that have to be provided from the MetM in the offline approach, the 3-D distribution of chemical species has to be provided right at the beginning of the forecast, when online coupled models are used."

18. Pages 12596-12597: The discussion on initial and boundary conditions as currently presented while representative of approaches used in traditional offline modeling has little relevance to integrated/online/ coupled modeling. I suggest moving this section to the appendix or deleting it.
These paragraphs have been partly rewritten, shortened, citations and information relevant for online models have been added; this is also in accordance to reviewer #1.

19. Page 12598, line 25-28: the other reason that ICs have a little impact on surface level/BL air quality is due to the strong forcing from sources (emissions) and sinks (deposition) – an "exponential-decay type term" does not necessarily capture that. Also it is not obvious what "pollutants tend to be governed by their input functions" implies – what are these input functions?
The "strong forcing" is not only due to emissions and deposition, but also to atmospheric dispersion (e.g. for pollutants emitted at the surface, which may rapidly mix vertically) and chemical reactions

(e.g., rapid decay of nitrogen oxides). We have removed the term “input functions”, which was confusing and we now simply state those important forcing functions, namely emissions and boundary values.

20. The discussion on data assimilation in section 5.5 is extremely relevant to the design and applications of coupled systems. The section will however benefit from some editorial work and restructuring that help clearly identify the needs for various applications as well as areas for further development

a) One important aspect that seems to be missing in the current discussion is the interplay between assimilation and the representation of feedbacks and chain effects – can data assimilation mask the modeled feedback effects (e.g., direct/indirect aerosol radiative effects)? How does one go about deciding the strength of assimilation and which variables to assimilate so that the modeled feedbacks and chains are not artificially suppressed? Some discussion along these lines would be useful.
A more detailed discussion of the difficulties associated with CDA in online coupled models is outside the scope of the present review and will be addressed in another paper focusing solely on CDA.

b) Page 12599, line 6; the implied improvements in reaction rate constants via chemical data assimilation is not obvious– am assuming the authors are suggesting that this is possible only in well constrained inverse modeling applications?
Although CDA has been used to estimate the kinetics of SO_2 oxidation in a CTM (Barbu et al., 2009), it is unlikely to be a major application of CDA, since most major kinetic constants are obtained from laboratory experiments. We have replaced this example by “dry deposition velocities”, since those have large uncertainties and cannot be easily determined in the laboratory.

c) Page 12599, line 20-21: “more complete representation of the atmosphere albeit with other limitations” is a contradictory – the discussion in this paragraph is not clear and will benefit from rewording.
We have now specified that remote sensing provides a representation of the atmosphere that is “spatially” more complete. We also broke that sentence into two distinct sentences for clarity.

d) Page 12601, line 1-3: this discussion is confusing – it is not clear what the “direct online configuration” is?
We have now specified that this is the “future online coupled configuration” of the ECMWF model.

e) Page 12601, line 21-22: which is the easiest CDA technique to implement?– the discussion appears to be incomplete.
The first two sentences of this paragraph have been completely rewritten as a single sentence.

21. I believe the length of section 7.1 can be reduced significantly. As currently written large portions of this section include summary/recap of discussions in previous sections – this is not needed and should be removed to improve the flow of the discussion in the section. The section can thus focus, primarily on the challenges, which is what it is intended to be.
The Section 7 has been rewritten following several reviewers comments.

a) The authors point out that simulated effects of aerosols on shortwave and longwave radiation differs strongly among the models and that a recommendation on the complexity of parameterizations is not currently possible. It would be useful if an approach to understand these differences is proposed. Should this not be a recommendation for future evaluation studies as well as guidance for measurement needs? For instance would closure experiments help reduce the associated uncertainties?

Thanks for the nice comments. Yes, we completely agree, however we are not ready to give such recommendations yet. The currently initiated AQMEII, phase 2 international initiative is focusing on this issue, and we hope to give such recommendations based on the expected results of this models exercise and further analysis of WG4 of the COST Action EuMetChem.

b) One aspect that did not clearly come across in the discussion of evaluation methodologies were approaches that help assess both the simulated magnitude and directionality of the feedback effects and the various chain interactions – some discussion along these line would be useful.
Yes, we agree, but this is also expected to be analysed only on the next phase of our studies. The evaluation of feedback parameterizations (or of their effect on meteorology) is extremely difficult in practice. We do aim at developing methods during the above-mentioned COST action but feel it is too early to give helpful directions at the current stage.