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Atmospheric Chemistry and Physics
Discussions

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 valu-

able contribution to the scientific literature. 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).

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?

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.

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.

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.

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.

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. 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.

3. Page 12583, line 10: OCMC should be spelled out.

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).

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

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.

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.

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 oder 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.

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.

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?

11. Page 12591: "conservative flux form for conserved variables" is awkward and should be reworded.

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.

13. Page 12592, line 20: MCCM should be spelt out at the first instance.

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.

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.

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.

17. Page 12595, line 14: "have also to be known right at the beginning" is awkward – please reword.

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.

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?

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.

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?

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.

d) Page 12601, line 1-3: this discussion is confusing – it is not clear what the "direct online configuration" is?

e) Page 12601, line 21-22: which is the easiest CDA technique to implement?– the discussion appears to be incomplete.

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.

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?

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 12541, 2013.