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

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Review of "Online coupled regional meteorology-chemistry models in Europe: current status and prospects" by Baklanov et al.

Review by Prof. David M. Schultz, University of Manchester

### **A. OVERVIEW OF THIS MANUSCRIPT**

As the title and the introduction suggest, this paper attempts to summarize the current state of online meteorology-chemistry models in Europe. Whether this paper meets that standard depends on the reader's personal bar for what constitutes success. Broadly speaking, the 39(!) authors of this manuscript did address certain aspects

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of the current state. In that sense, the paper meets some minimum standard.

On the other hand, if the reader expected a thorough, balanced, provocative, informative, and well-written review synthesizing existing knowledge and making specific, realizable recommendations for the future, then this paper fails to meet that standard. The failure of the manuscript to meet these standards is a shame because the manuscript deserves to be written better and be useful to the reader.

The manuscript appears to be designed after a similar paper published just last year (Kukkonen et al. 2012, ACP, "A review of operational, regional-scale, chemical weather forecasting models in Europe", <<http://www.atmos-chem-phys.net/12/1/2012/acp-12-1-2012.html>>). This article was written by 25 authors and was better organized, flowed better, and was more useful to the readers, even in its original (unreviewed) ACPD form. [Disclosure: I was the third author on this article, so I know how much time and effort we put into ensuring that our article was the best that we could make it.] Some of the content in Baklanov et al. was explored in Kukkonen et al., and the literature was more thoroughly addressed at that time. For example, compare the following sections in Kukkonen et al. to their equivalents in Baklanov et al. to see how much more thorough the Kukkonen et al. sections are: wet and dry deposition (Kukkonen et al.'s section 4.5 and Table 7) and model evaluation (Kukkonen et al.'s section 5 and Table 10). Thus, why is Baklanov et al. necessary, especially when it appears so recently after Kukkonen et al. (2012)?

## B. RECOMMENDATION

There are many flaws with this manuscript, some fatal to the success of the paper, others quite minor and annoying to careful readers. The biggest flaws concern the overall picture of this manuscript.

1. Who is the intended audience?
2. What is the scope and purpose of this manuscript?

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The next biggest concern is the implementation of the manuscript. Even if the audience, scope and purpose were clear, the text is poorly organized, inconsistent, unclear, and sometimes grammatically incorrect.

Because a fundamental restructuring and re-writing AT THE HIGHEST LEVELS are necessary to make this paper reach a publishable state, I have not identified minor revisions individually except to illustrate the type of problems that exist within the manuscript. Instead, I focus on the fatal flaws and the major revisions required to bring this manuscript closer to being published.

Based on my reading of the manuscript and my specific comments below, I am recommending rejection of this manuscript. This decision was not reached lightly and was a result of five principal points.

First, extensive revisions are necessary because of the severity of problems with this manuscript. On this point alone, the manuscript deserves to be rejected.

Second, these problems persist in this manuscript DESPITE 39 AUTHORS PRESUMABLY HAVING READ THIS MANUSCRIPT in its entirety and having approved the manuscript in its present state. If 39 authors were responsible for producing this manuscript, then I would have expected that problems this severe would have been identified and fixed, which clearly they have not been. Thus, I must conclude that these 39 authors have abdicated their responsibility to ensuring the submission of a high-quality manuscript. If authors wish to receive credit for being on the author list, then they must also take responsibility for ensuring the highest quality submission possible. It is not the reviewers' responsibility for cleaning up submitted manuscripts.

Third, if the authors desire to revise and resubmit this manuscript, then they should not be placed under time constraints required by the journal in receiving a resubmitted manuscript. Instead, substantial planning, re-thinking, re-design and writing will be needed to make this manuscript publishable. The authors should take their time and not be rushed.

Fourth, getting revisions and feedback from all 39 authors will take time and require several rounds of effort. For this reason as well, I do not think the revised manuscript should be rushed.

Fifth, if the problems in this manuscript were limited to one or two sections, I could recommend revisions instead of rejection. Because of the large number and pervasive problems with this manuscript – as well as not knowing the purpose of this manuscript and its intended audience – I cannot make specific recommendations for how the authors should revise this manuscript. The authors need to decide this for themselves. As such, the restructuring of the manuscript will make this an entirely new manuscript, and the review process should be started anew.

### C. ADVICE TO THE AUTHORS

Writing a review article is not an easy task. To write a successful review article, the authors need to consider five points.

1. First, the authors should have a clear vision for who the audience will be. Who is the intended audience for this manuscript? In some places, the level of information and terminology expected to be known by the reader is quite high (perhaps only numerical modellers would understand the text). In other places, the material is quite elementary. In Kukkonen et al. (2012), our vision for the article was for it to serve as an introduction for graduate students starting to run chemical forecasting models explaining how they work. As such, we ensured that content was cited and that the language and knowledge is consistent with a student's level of understanding.

2. Second, the goals of the manuscript should be clear, concise, and achievable. The goals of the manuscript are stated on p. 12547 in one sentence: "summarises the current status of modeling practices. . . ." This statement is too vague and is unachievable. More specifics are needed. More focus is required in the scope of the manuscript. What would be useful in this manuscript is a sense of how important processes are quantitatively, thus demonstrating why coupling is necessary or not necessary.

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3. Third, the scope of the manuscript should be clearly stated and adhered to. On p. 12547, the scope of the paper is briefly stated: mesoscale models, but not climate simulations. Yet, in several locations in the manuscript, this scope is violated when high-resolution or climate-scale simulations (p. 12559, p. 12609), or even ocean models (p. 12586), are referred to. In comparison, a whole section of Kukkonen et al. (2012, section 1.2) is devoted to providing specifics on the goals and the scope of that article.

4. Fourth, the different sections written by different authors need to be homogenized for content, depth, and language, keeping in mind the above three points. Some material is duplicated across the manuscript. One of the authors who is a native English speaker needs to finalize the manuscript by correcting grammar and improving readability before submission. When necessary, acronyms should be introduced once and kept consistent throughout the manuscript. When writing Kukkonen et al. (2012), we placed the onus on the three lead authors for ensuring consistency across the manuscript. Correct grammar throughout the manuscript was ensured by the third author, a native English speaker, who made a final edit of the manuscript before submission.

5. Fifth, the organization of the manuscript is lacking. The order of the content needs to be reconsidered. Some aspects seem to be out of order, as discussed later in this review. An order that makes more sense needs to be implemented.

#### D. GENERAL COMMENTS ON THE MANUSCRIPT

1. Some statements are so obvious that they do not need saying. Examples include: p. 12572, lines 12-15. p. 12576, lines 6-8. p. 12577, lines 19-21.

2. Some text is simply grammatically incorrect and unreadable. I have listed only some of the problems here: p. 12558, lines 11-14. p. 12570, lines 22-24. p. 12571, lines 23-24. p. 12576, lines 10-13. p. 12578, lines 17-18. p. 12583, lines 19-20. p. 12591, lines 25-28. p. 12593, lines 11-12. p. 12594, lines 23-24. p. 12625, lines 20-22.

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3. The authors should use commas and hyphens properly. The text is difficult to read because many of these forms of punctuation are omitted. One particularly egregious example is p. 12577, lines 14-15. Moreover, "online-coupling" is sometimes incorrectly hyphenated (e.g., p. 12573, line 24). These errors should be fixed throughout the manuscript.

4. Language is not clearly defined and lacks consistency across the manuscript.

a. "Coupled": In some places, "coupling" seems to refer exclusively to online coupling and offline models are not referred to as coupled. In other places, online and offline are both referred to as coupled. Another problem is that a more detailed description of online and offline models are presented much later in the paper (section 5.2) when this material really deserves to be presented earlier. What do "online integrated and online accessible" mean (p. 12595, line 10)? I don't recall these terms being defined.

b. In some places in the manuscript, off-line/on-line is hyphenated and in other places offline/online is not.

c. p. 12590, line 9: What does it mean to be "functionally related"? Is this a scientific term with specific meaning? Please define or reword.

5. Finally, the article is poorly written.

a. Each paragraph should focus on a single topic and be started with a topic sentence. The concluding sentence of the paragraph usually contains the material within the paragraph requiring emphasis.

b. The text lacks transition and coherence. Without coherent text, the reader is left trying to understand why they are receiving this information. Please read Gopen and Swan (1990, "The Science of Scientific Writing"): <http://www.americanscientist.org/issues/pub/the-science-of-scientific-writing>.

c. For more on writing coherent and flowing text, the authors should read chapter 8 of the book *Eloquent Science: A Practical Guide to Becoming a Bet-*

ter Writer, Speaker, and Atmospheric Scientist, available for free download at <<http://eloquentscience.com/2009/11/chapter-8-constructing-effective-paragraphs>>. If your academic library subscribes to Springer ebooks, you may also have access to the full book here: <<http://link.springer.com/book/10.1007/978-1-935704-03-4/page/1>>.

## E. AUTHORSHIP

Given 39 coauthors of this manuscript and the low quality of the manuscript, readers will ask whether all the coauthors are legitimate coauthors. If they are not, they should just be listed in the acknowledgements. The best definition that I have found for authorship comes from the International Committee of Medical Journal Editors <[http://www.icmje.org/ethical\\_1author.html](http://www.icmje.org/ethical_1author.html)>.

"Authorship credit should be based on 1) substantial contributions to conception and design, acquisition of data, or analysis and interpretation of data; 2) drafting the article or revising it critically for important intellectual content; and 3) final approval of the version to be published. Authors should meet conditions 1, 2, and 3. . . . All persons designated as authors should qualify for authorship, and all those who qualify should be listed."

Have all authors met all three criteria to qualify for authorship?

## F. MAJOR COMMENTS ON SECTIONS

1. The abstract of this manuscript does not have the components of a typical abstract. What is the purpose of an abstract? The abstract summarizes or "abstracts" the entire manuscript, so that the reader receives a digest of the purpose, data and methods, results, and principal conclusions of the manuscript. Introductory material should be limited, as it does not contribute to that purpose of summarizing the manuscript. Consider the abstract of this manuscript. The first two paragraphs are introductory material. The first sentence of the third paragraph is a description of COST ES1004. Only the last sentence really discusses the content of this manuscript.

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In contrast, consider the abstract of Kukkonen et al. (2012), which bears a much closer resemblance to an ideal abstract.

"Numerical models that combine weather forecasting and atmospheric chemistry are here referred to as chemical weather forecasting models. Eighteen operational chemical weather forecasting models on regional and continental scales in Europe are described and compared in this article. Topics discussed in this article include how weather forecasting and atmospheric chemistry models are integrated into chemical weather forecasting systems, how physical processes are incorporated into the models through parameterization schemes, how the model architecture affects the predicted variables, and how air chemistry and aerosol processes are formulated. In addition, we discuss sensitivity analysis and evaluation of the models, user operational requirements, such as model availability and documentation, and output availability and dissemination. In this manner, this article allows for the evaluation of the relative strengths and weaknesses of the various modelling systems and modelling approaches. Finally, this article highlights the most prominent gaps of knowledge for chemical weather forecasting models and suggests potential priorities for future research directions, for the following selected focus areas: emission inventories, the integration of numerical weather prediction and atmospheric chemical transport models, boundary conditions and nesting of models, data assimilation of the various chemical species, improved understanding and parameterization of physical processes, better evaluation of models against data and the construction of model ensembles."

2. In the Introduction, the authors need to say where this manuscript fits in relation to the other literature reviews that have been written. There is one sentence on p. 12551 that states what that previous work is, but that text doesn't say what the content of the earlier papers are or how these papers relate to the present manuscript. Specifically, Zhang (2008), Baklanov et al. (2010b, cited in Kukkonen et al.), and Kukkonen et al. (2012) need to be included and discussed. (What is "Grell and Baklanov et al. (2011)"? Please follow proper referencing format.) These articles (and possibly others) should

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be used as a starting point to say what knowledge has been written down and how this paper distinguishes itself from previous work.

3. Given the types of information that the authors are including in their manuscript, the authors failed to discuss other topics in this manuscript of similar importance. These topics include: \* hydrostatic versus nonhydrostatic models \* convective parameterization versus convection-permitting models (One question that needs to be addressed is how the chemistry is handled in such parameterized models.)

4. The title does not adequately describe the manuscript. This problem with the title possibly relates to the authors lacking a clear vision for the scope and goals of the manuscript. In particular, I am concerned that "current status and prospects" does not adequately represent the contents of the manuscript. If that were the case, then why the tedious discussion of model characteristics? Why is the description of the individual models so downplayed? If this manuscript is meant to address "prospects", why are there so few specific recommendations made?

5. The sections of text discussing the models in Appendix A have variable depth and content. For example, compare the difference in depth of discussion between A6 and A7. This text should be made more homogeneous across all models. Standard content should be found within each model's description.

6. Section 2. I don't understand the point of this section. If this section is supposed to provide some overview to the reader, this section is not useful for the reasons explained below.

a. There is no framework for understanding these feedbacks. They are just introduced in the first substantive section of the manuscript. Why the reader is receiving this information at this time is unclear. Better motivation is needed.

b. I was especially confused by the content on pp. 12548-12549. Is this some kind of outline? This structure of the text is inappropriate.

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c. How are these items linked together? I don't understand these chains of interactions. They are not sufficiently explained.

d. Transition between points is lacking.

e. Why is the material on COST introduced here? How is it related to the subsection title of "Survey of potential direct and feedback processes"? In Kukkonen et al. (2012), how that article related to the European scene was much more effectively addressed, not simply just the a description of the COST ES0602 action, but the whole European scene.

7. Table 3: There are numerous problems with this table and its accompanying text.

a. Why are results from climate models discussed here? This content was explicitly excluded by the scope of the paper. Either the scope should be changed or this aspect to the table should be deleted.

b. The text destroys the justification for the table when it says that "most people have key expertise in only one or two of the models/model categories". How appropriate is an expert survey where most of those responding are not experts on all aspects to which they are responding? Therefore, this survey cannot be representative of the community. This content should be deleted from the manuscript.

c. Just because the community does not identify a process as important does not mean that it is unimportant. Yet, this weakness of this survey is not addressed in the text. This is another serious flaw with this content.

d. The mean scores in this table cannot be computed in this way (Jamieson 2004).

Jamieson, S. (2004). Likert scales: how to (ab) use them. Medical Education, 38(12), 1217-1218.  
<<http://xa.yimg.com/kq/groups/18751725/128169439/name/1LikertScales.pdf>>

e. "Not included" should be given a value of zero, not 1.

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8. Sections 3.1 and 3.2. Why is this text in bullet-point format? How do these bullet points relate to one another? Why did the authors not write a complete and coherent paragraph? This outline format to the manuscript is unacceptable. One-sentence paragraphs should be avoided (line 10, p. 12552). An incomplete manuscript such as this should not have been submitted to the journal in this state.

9. Unfortunately, the incomplete states of sections 3.1 and 3.2 cause further problems later in the paper. This paper hinges upon a careful distinction between online and offline models, as this point will be returned to again and again later in the paper. As such, it is essential that the reader be clear on the distinction and the nuances distinguishing the two. The failure of this section to adequately and clearly define these terms to the reader causes confusion for the reader later in the manuscript when such background information is assumed to be clearly understood.

10. Section 4. This section is where the lack of definition of who the audience is hits the manuscript hardest. For example, on p. 12554, who is the audience for these descriptions? If early-career scientists, then not enough definition is provided (e.g., what are Eulerian versus semi-Lagrangian?). If practicing scientists, then a list of possible convective schemes (p. 12554, lines 15-16) or lists of chemical mechanisms (p. 12557, lines 19-21) is ineffective and lacks depth. For dynamics of NWP models and physical parameterizations, no reference is made to textbooks on NWP such as those by Warner and Stensrud.

11. Much of section 4 was without structure and organization, particularly section 4.2. As I was reading along, I had no context for why I was being presented information. What is the motivation for the authors to tell the readers this information? There was no transition between concepts.

12. This problem with the lack of transition and coherence becomes even more severe in section 4.3. Almost each paragraph is given its own subsection heading (e.g., 4.3.2, 4.3.3). No linkage is made between the different sections. Transition is nonexistent. A

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more coherent structure needs to be given to this section. The structure of this section should be consistent with the other sections of the paper.

13. Sections 4.3.4 and 4.3.5. These sections are poorly written. After reading them, the reader does not have any better understanding of what internal and external mixtures are. Moreover, section 4.3.4 contains no references. Why? Each section should be thoroughly cited if this is to be a state-of-the-science review. Expecting references for each statement is a minimum requirement when writing a review article. Am I to believe that all 39 authors read through this content and approved it? Again, this example illustrates the lack of oversight and raises significant questions about whether all the coauthors meet the requirements for being listed as coauthors.

14. Section 4.3.6. I keep looking for an overarching framework for the aerosol modelling text. I do not feel that the authors have done an adequate job of explaining the forest to the reader before describing the individual trees.

15. Section 4.3.7. This entire section has one reference. The section is poorly written. Consider the first sentence, which I assume is meant to be some kind of definition of what ageing is. The reader is not told what a "mixing state" is. Vague statements such as "The ageing of mineral dust particles changes their capability to act as IN or CCN." are not developed. How is that? What evidence is there? No discussion of the various ways that ageing is handled in models is presented, despite being "an important process that should not be neglected." (Vaguely worded.)

16. Section 4.3.8. This topic is hugely important in chemical weather, yet only one small paragraph discusses it.

17. Section 4.3.9. This section lacks coherence. The second sentence is redundant with a similar sentence elsewhere in the manuscript (p. 12560, lines 5-7).

18. Section 4.3.10. After reading this section on TTD, I still don't have any understanding of why particles should be transported against the temperature gradient. Why

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should they be transported down the temperature gradient in the first place? This section doesn't say how TTD is modelled. Again, this section illustrates the lack of identity to the readership. Who is this information for? If it is for early-career scientists, the text does not take the time to explain these concepts to the reader, even at an introductory level.

19. Section 4.4.1. This section lacks coherence. One-sentence paragraphs should be avoided.

20. Section 4.4.2. What does "processes taken into account" mean? Please write a more specific and meaningful title for this section. Also, the manuscript now exhibits a fourth level of subsection headings with "Condensation/deposition of water vapor", etc. Is this level really needed? Can the manuscript be reorganized and rewritten to avoid this level of structuring?

21. The one-paragraph section on "Formation of precipitation" is inadequate. Citing only three microphysical schemes, including Kessler and Lin et al. (which are not widely used anymore), provides an out-of-date and biased view of the field. I thought that this review was supposed to represent the state of the science?

22. Section 4.5.2.

a. The second paragraph seems out of place. Radiation is handled elsewhere in the manuscript.

b. The paragraph beginning with "In the STRACO cloud scheme" is poorly organized. It reads as if it is a list of model characteristics rather than a coherent discussion of the relevant microphysics.

c. This section, in particular that paragraph, has a different feel than many other parts of the manuscript where fewer direct comparisons are made between different models. Although I appreciate the model intercomparison, the rest of the manuscript should address their topics in similar ways. Again, this inconsistency should be smoothed out

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among the different sections.

d. The paragraph that is split across pp. 12571-12572 needs to be more clearly written.

23. Section 4.6. This entire section spanning four pages contains only a handful of references. Again, the audience for this material is unknown. The content is so simple that it is not consistent with other sections of the manuscript, which are pitched at a much higher level. For example, p. 12574, line 24 talks about a "strong effect on the interaction", yet this is not quantified or cited. Yet, despite this concern, some terms are not defined (internal and external mixtures, semidirect versus direct versus indirect).

24. Section 4.6.3 lacks coherence. The paragraph spanning pp. 12576-12577 reads like a list. Better synthesis of this content is necessary to engage the reader.

25. Section 4.8. This entire section is problematic.

a. The introductory material is vague. The nine effects are just listed, not defined or integrated into the text. What is the point of providing information that the reader has no context for?

b. The material in the middle of p. 12583 is redundant with material earlier in the manuscript.

c. The authors have not given the reader a clear sense of what this chain effect means.

d. Moreover, the relationships are not linear and progress from one to the other on the list. Such abstractions would seem to be quite artificial. For example, temperature gradients are not the only cause (effect?) of turbulence. This content is simply unintelligible to the reader.

e. Also, statements are made that online modeling is "very limited in its ability to produce chain effects". What does this mean? Can all these chain effects be generalized in this manner? No citations are provided for this statement.

f. Grammatical errors and lack of clarity in the writing are common.

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- g. The section lacks quantification and references.
- h. This section throws out a bunch of processes without any assessment of how well models simulate them or how quantitatively important they are.
- i. This whole section lacks coherence and clear understanding for the reader. It should be deleted.
26. Section 5. The title of this section is vague ("Numerical and computation aspects"). The material in this section expands upon material in section 4.1, making the reader wonder why this material is not better organized. The information on advection schemes and mass continuity seems to be of basic importance to modeling. Why does this material appear so late in the manuscript?
27. Section 5.1.1-5.1.5. Much of this material seems to be quite specialized. Who is the intended audience? A high level of understanding of the architecture of numerical models seems necessary to understand this material. This material seems to repeat the problems with section 4.3 in that almost each paragraph is its own section.
28. Section 5.1.4. In the second paragraph, describing why these problems occur would be helpful for the reader (if they are deemed important enough to include in a revised manuscript).
29. Section 5.3. This material, especially in the first paragraph, is elementary and obvious. What are "good habits"? Why is the switch to first person ("we" and "our") occurring now in this section? The text should have a consistent language and tone throughout.
30. Section 5.4. This is another section with numerous problems.
- a. Some of this section is quite elementary (e.g., p. 12595, lines 12-16).
- b. Much of the section lacks references (p. 12595, lines 20-25; p. 12596, lines 4-6 and lines 11-18).

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- c. What does "reordered and relumped" mean? Could you use more scientifically precise language, please?
- d. The paragraph about Bangert et al. seems out of place. Why select just this one study to discuss in detail?
- e. Vogel et al. is only provided one sentence, and it is unclear what the reader is supposed to take away from it.
- f. I also don't understand the point of these remaining examples. No useful or practical information is provided to the reader.
- g. There is little discussion of the relative merits of various approaches.
- h. There is little discussion of what a model user needs to do when the meteorological and chemistry initial analysis and lateral boundary conditions are inconsistent because they derive from different models.
31. Section 5.5. The first sentence is poorly worded. How long is a "long time"? How does "meteorological modelling" differ from "weather forecasting"? Why does IC have to be introduced as an abbreviation? It is annoying to the reader to have to remember what IC stands for. Spell it out. The quality of the English language needs improvement in this section. Again, terms are used that all readers may not understand. What is an "adjoint"? What are "input functions"? Readers want to know.
32. Section 5.5.2. The reader is left hanging at the end of this section, waiting for resolution of these issues. "Methodological and technical challenges" is vague.
33. Section 5.5.3. Are all these studies using online coupled models? They should be as that is the point of this manuscript. "One can mention" needs to be reworded to sound more natural.
34. Section 6.2. What criteria determine which models make it into this study? Section 2.1 of Kukkonen et al. (2012) provided criteria that determined which models were

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discussed and which weren't. This paper should be more explicit in this regard. How complete is this review? How were the models selected for inclusion in Appendix A?

35. Section 7. The second paragraph of this section talks about the review, but I didn't sense much in the way of model intercomparison. The lists of models is tedious. The last paragraph of section 7 is bland and needs to be more powerful.

36. Section 7.1.1. This section calls for better "coordination and integration". More specifics are needed about exactly what kinds of coordination and integration are desired. Otherwise, your call is an empty desire that will not be realized. The authors should provide specific recommendations. The recommendations should also be realistically achieved.

37. Section 7.1.2.

a. Specifics of how the online coupling occurs is lacking in this section.

b. More organization would help this section, as well.

c. "ABL structure. . .should be improved." All model users agree that ABL structure in models needs improvement, but how? Offering bland statements is not helpful. More specifics and quantification of how bad the schemes are is needed in this manuscript if you wish to offer these specific recommendations.

38. Section 7.1.3. The last paragraph is redundant.

39. Section 7.1.4. This section lacks specifics, especially given the huge section on data assimilation in the manuscript. The manuscript should be consistent in tone and depth from one section to another.

40. Section 7.1.5. This section advocates that "long simulations are needed to demonstrate the benefits of online coupling", but isn't that what this paper already argues strongly for? For example, on p. 12623, the authors say, "the online approach will certainly improve forecast capabilities". So, which is it? Are the benefits demonstrated

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or are they not? The paper needs to strike a consistent argument throughout. Again, the lead author or authors should ensure consistency in message throughout all the sections of the manuscript.

41. The bullet points of Sections 7.2.1–7.2.4 are inappropriate in this manuscript. The manuscript should not have been submitted to the journal in this incomplete state.

42. The manuscript has no natural conclusion section. The manuscript ends abruptly, leaving the reader hanging.

43. Table 6. The large number of "NA"s is unacceptable. The authors have the responsibility to find out this information and fill the table in completely. Otherwise, the authors do the readers a disservice. Yes, it may not be easy, but that information exists somewhere. It is the authors' job to track that information down. Also, spell out the column titles.

44. Tables B1 and B2 seem redundant with Tables 1 and 2. I don't understand why they exist.

45. Figure 1. I have tried and tried, but I don't understand the relationship between the different features in this figure. It needs to be better explained or redrafted to be more clear. As it is now, I don't find this figure informative. Kukkonen et al. (2012, their Figure 1) has a much more clearer figure illustrating the difference between online and offline models.

46. Figure 7 is confusing. It needs to be explained better.

## G. SPECIFIC COMMENTS

These comments are typical of some of the issues that occur in this manuscript. This is only a partial list; it is not comprehensive. The authors should be responsible for taking these comments and fixing them, but also generalizing these concerns to the whole manuscript. Not doing so will result in an unacceptable manuscript.

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1. p. 12553, line 20: No one I am aware of refers to the physics as the "core". The dynamical core serves as the basis for the model; physical parameterizations or packages are attached to the dynamical core. As such, there can be no "physics core". Delete this phrase.
2. It is unnecessary to use "see" in front of figures or references (e.g., p. 12560, line 15). Delete throughout.
3. p. 12560, lines 5-7: I don't understand this sentence. Would an example help explain what the authors mean?
4. p. 12561, lines 20-22: I think this statement is too general to be true. Have the authors evaluated all possible schemes? I am sure that the relationship between the number of bins and model accuracy is not as straightforward as the authors attest. Moreover, only two citations are provided. This statement needs to be eliminated or better defended.
5. p. 12566, lines 25-26: Houze (1994) should be Houze (1993). Straka has a textbook focused on the modelling of cloud microphysics and should be cited here. <<http://www.cambridge.org/gb/knowledge/isbn/item2327528>>
6. p. 12569, line 17-20: This paragraph has no citations. The text is vague and lacks precise meaning.
7. p. 12570, lines 28-29, and p. 12572, lines 17-18: No references for these statements are provided.
8. p. 12572, line 6: "thence" is not commonly used in English and should be rewritten.
9. p. 12572, line 24: "biological particles" is vague. Please be more precise.
10. p. 12573, lines 1-3 and 8-9: These statements are generalizations that are only true in some situations. Furthermore, no references are presented to support these statements.

11. p. 12578, lines 10-12: This sentence requires references.
12. p. 12579, lines 4-5 and 7: These two sentences are redundant.
13. p. 12580, lines 4-6: Electrical discharges have been parameterized in meteorological models (e.g., Mansell et al. 2005, JGR), and NO<sub>x</sub> production has also been parameterized (e.g., Price et al. 1997, JGR; Tie et al. 2002, J. Atmos. Chem.). This information should be cited within the manuscript.
14. p. 12580, line 5: Does "it" refer to NO<sub>x</sub> or lightning?
- 15, p. 12580, lines 20 onward to p. 12581, line 21: Much of this material is missing references.
16. p. 12580, line 21: "random process" is an inappropriate description. Fire initiation locations may not be easily anticipated, but they are certainly not random.
17. p. 12581, lines 1-4: No evidence is presented in this manuscript for this statement. Support it or delete it. Similarly, why the three recommendations in lines 17-26 are most important is not supported by evidence presented in the manuscript.
18. p. 12582, lines 2 and 12: What substance is being deposited is unclear because of vague and inconsistent terminology in section 4.7.2. Initially, it is referred to as the vague "material". Later, gases and aerosols are specified. But, are they handled the same way in models? The text doesn't seem to suggest that they are. This should be more explicit.
19. p. 12582, line 11: It is unclear how dry deposition occurs indirectly through soil moisture. Moreover, no reference for this process is provided.
20. p. 12588, lines 2-5, and p. 12590, lines 4-6: These two paragraphs close with intriguing ideas, but they are not developed, leaving the reader left wanting. The paragraphs should conclude with more resolution to the problems raised.
21. p. 12590, line 8: Here is an example of how verbose text can be made more

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concise and simple. "Recently, it has been pointed out by Lauritzen and Thuburn (2011)... " can be rewritten "Lauritzen and Thuburn (2011) showed..."

22. p. 12590, line 11-12: What does this sentence mean? This sentence appears to be specialist information and is not explained for the introductory reader.

23. p. 12591, lines 17-18: This material is difficult to understand. If the models are online (two-way interaction), then there is no "driving model". Also, what is a "coupler"? No other section of the manuscript appears to use the term "coupler." These terms need to be defined or deleted and made consistent with the rest of the manuscript.

24. p. 12592, lines 13-22: This material is vague. For example, what are "significant errors? If this material is deemed important, then it must be explained to the reader.

25. p. 12593, lines 15-20: This material is elementary.

26. p. 12594, line 29: Two forms of the word "simple" are used in this sentence. The sentence could be reworded to be more concise.

27. p. 12595, lines 1-5: Needs a reference.

28. p. 12595, lines 5-7: This text is redundant with material earlier in the manuscript.

29. Section 5.4 title: Use common terms: "Initial and lateral boundary conditions" would be preferred.

30. p. 12600, line 18: What is an "integrated model"? This type of terminology should be defined in the text or made consistent with the rest of the manuscript if it is a synonym of another term.

31. p. 12601, line 21: What does "it" refer to?

32. p. 12601, Why does the abbreviation "AQ" start to appear frequently now? It hasn't been used much in the manuscript until this section. I would just spell it out throughout the manuscript. The abbreviation provides no advantage to the reader.

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33. p. 12602, lines 2-4: This sentence is redundant with text already included in this section.
34. p. 12602, lines 11-13: This sentence should appear earlier in the manuscript.
35. p. 12610, lines 2-13: This content is vague and needs more specifics.
36. p. 12613, line 9: Delete "the study of". These words are unnecessary.
37. p. 12613, lines 16-19. Please provide references for this statement.
38. p. 12613, lines 25-27: Please list references in chronological order here and throughout the manuscript.
39. pp. 12613-12614: I don't sense the large-scale organization of this section of text. The reader needs to see the forest through the trees.
40. p. 12614, lines 15-27: More specifics and revision to this text would help improve readability.
41. p. 12624, line 7: Is the abbreviation SLCF needed if it is only used in this section, and a few times at that?
42. Table 2: Does "modulates" provide the correct meaning the author intends? I don't think so.

NOTE: I have also uploaded a PDF of this review in case the text formatting has problems in HTML.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/13/C2599/2013/acpd-13-C2599-2013-supplement.pdf>

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