

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

**D. M. Schultz (Referee)**

david.schultz@manchester.ac.uk

Received and published: 22 May 2013

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**

**Thank you for reviewing our manuscript and providing valuable comments. All your comments have been considered when revising our paper. We have thoroughly revised the manuscript to address many constructive comments from you and other reviewers. We believe that our revised manuscript has been significantly improved in terms of scientific/technical aspects, organization, clarity, presentation, and readability. However, we respectfully disagree with you on some comments, as we will point out below. Please find all our replies in red colour, following your remarks which we copied and kept in black.**

**Please see below our point-by-point replies to the specific comments.**

As the title and the introduction suggest, this paper attempts to summarize the current status of online meteorology-chemistry models in Europe. Whether this paper meets that standard depends on the reader's personal opinion on what constitutes success. Broadly speaking, the 39(!) authors of this manuscript did address many aspects 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 comprehensive review, synthesizing existing knowledge and making specific, realizable recommendations for the future, then this paper may fall short in meeting 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)?

**We respectfully disagree with you. Our paper and the paper by Kukkonen et al. (2012) review different types of models (see the models' list). While Kukkonen et al. considered off-line CTMs, we focus on online coupled meteorology-chemistry models. Jakko Kukkonen in his own comment to**

ACPD (C2796) pointed out that his review and our review are complementary, which clearly indicated some misunderstanding that you may have regarding our paper. It is in regard to online meteorology-chemistry models that we feel the need for a new review paper. Our paper is the first review of European online models and it is a synthesis of outcome of the COST ES1004 activities during the past two years.

Of course, there are some similarities in the structure of models' review, like any research paper might have: Introduction, Methods, Results, etc. The reason why some sections like those on deposition and emissions are much less comprehensive in our paper than in Kukkonen et al. is that we tried to focus on those aspects that are particularly relevant for online coupled models, i.e., where the online-coupling may have advantages as compared to the offline approach.

Several of your comments on the paper are not about scientific aspects of the paper, but about the writing style, paper structure, and technical aspects. The revised version should be more clearly following British English. We believe that the scientific value and technical quality of the paper are adequate for publication in ACP and that there is a great need for this review paper that is focusing on online integrated meteorology-chemistry models used in Europe. For this type of models developed or used in Europe, no other review paper exists.

## 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?

The main purpose of the paper is to summarise the current status of model developments and practices towards online coupled modelling of meteorology and chemistry with a specific focus on European models, to survey the potentially relevant interactions between atmospheric dynamics (meteorology) and atmospheric composition (AQ, climate), and to describe how feedback processes are treated in these models. The paper also highlights the key scientific issues and emerging challenges that have to be addressed in the four main application fields of online integrated meteorology chemistry models, i.e., in chemical weather forecasting, air quality studies, numerical weather prediction, and climate research. Therefore, the intended readership includes developers and users of online coupled models for the above mentioned communities. Our review paper also addresses scientists with a potential interest in adopting online models for their future work which need orientation with respect to current trends and which need an overview on the status and availability of this new generation of atmospheric models. The paper is, however, not targeting on undergraduate students as readers.

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.

We agree about the lack of uniformity and that the level of detail is different in different sections. 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, which was why the level of details varies from section to section, which partially contribute to the "inconsistencies".

Nevertheless, we have thoroughly revised the manuscript to address those issues regarding the implementation of the manuscript, i.e., some sections or subsections of the paper have been rewritten

and reorganized. All confusing statements have been either clarified or removed. Inconsistencies and grammatical errors have been corrected. We believe that our revised manuscript has been significantly improved in terms of scientific/technical aspects, organization, clarity, presentation, and readability.

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.

We respectfully disagree with your suggested rejection. Given the high scientific values and unique contribution of the paper to several modelling communities, our manuscript deserves publications, as indicated by all other reviewers. However, we have considered your comments seriously and have thoroughly revised the entire manuscript by rewriting, revising, and reorganizing it. We believe the technical quality of the revised manuscript is consistent with other ACP publications.

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.

We agree that reviewers are not responsible for cleaning up submitted manuscripts. However, we do not at all agree with your statement on the co-authors and their contributions, which are unfortunately not factual. All authors are indeed involved in writing the text, but they contribute to different parts, since they have a large variety of expertise. The manuscript is written by a large number of authors with specific responsibilities for certain sections, tables, figures, etc., which on one hand is a strength of the manuscript as it demonstrates close collaboration of the community and ensures expert knowledge, but on the other hand is challenging with respect to a uniform writing structure and style.

We have involved all authors not only in the paper writing, but also in the revision of the paper and in the replies to the reviewers' comments. Therefore, the authorship remains the same.

In sum, we've tried to improve the organization, presentation, clarity, and readability of the text substantially, and we believe that the technical quality of the revised manuscript is consistent with ACP publications.

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.

As indicated, the paper has been rewritten and revised substantially. The revision process indeed took longer than regular papers, given its length and comments by 11 reviewers. To accomplish the revision, we indeed requested one month extension from the ACP editor. The revised manuscript is prepared for re-submission for the second-round review.

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.

We specified more clearly the objective of this manuscript and its intended audience in the revised version and followed your recommendations to which we agree. However, we do not agree that a complete restructuring of the manuscript is necessary. In addition, none of the other 10 referees suggested this and they all recognized the values of our paper and suggested an acceptance for publications after minor-to-moderate revisions.

### 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.

See our above answer to B1.

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.

We would have liked to have had quantifications here, however, that is currently not possible, since this quantification is current science and no general results are available now. Nonetheless, we believe that we achieved to summarise "...the current status of modelling practices..." by this paper.

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.

We have removed the reference to the model COSMO-M7 and the study by Zubler et al. (2011) in Section 6 as this model is targeting climatic time scales. However, in several sections where relevant for specific feedback processes, we discuss and compare specifics of model realisations for climate time scales.

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.

We agree that some sections can be improved with respect to grammar and style and we did that and also removed duplications.

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.

We somewhat reorganised the text, but kept the overall structure for the reasons stated above (see B – second).

#### 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.

We agree on the second point and removed the corresponding sentence, but not on the first and third one. Lines 19-21 on p. 12577, for example, explain why the section on emissions focuses on those processes particularly relevant for online models.

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.

p. 12558, lines 11-14. The sentence has been reformulated.

p. 12570, lines 22-24 The sentence has been reformulated.

p. 12571, lines 23-24 The sentence has been reformulated.

p. 12576, lines 10-13: The paragraph has been rewritten.

p. 12578, lines 17-18. The sentence has been reformulated.

p. 12583, lines 19-20. The sentence has been reformulated.

p. 12591, lines 25-28: The text of the final version was erroneously mistyped. The section has been corrected.

p.12593, lines 11-12: The sentence has been rewritten.

p.12594, lines 23-24: The sentence has been rewritten.

p. 12625, lines 20-22. The sentence has been reformulated.

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.

p. 12577, lines 14-15 has been rephrased, while incorrect hyphenation has been fixed throughout section 4.6 and elsewhere.

"Online-coupling" is now written consistently throughout the paper as online coupling.

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.

We made the definitions more clearly now and used them throughout the text: online coupled models include both online integrated models and online access models. Online integrated models calculate meteorology and chemistry on the same grid in one model using one main time step for integration. Online access models use independent meteorology and chemistry models that might even be on different grids, but exchange data on regular and frequent bases in both directions. The frequency needs to be the higher the smaller the time scale of the relevant processes. In contrast to online access models, offline models do not exchange data but merely provide, e.g., meteorology information to the chemistry model. This is now already mentioned in the introduction.

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.

Online integrated and online access models are already defined on page 12552, line 4-9. We rewrote this and introduced the words already in the introduction and in more detail in Chapter 3. Also, we kept the same name throughout the text.

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

We ensured that online/offline is used throughout the text.

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.

We reformulated the whole paragraph into the following:

In the atmosphere chemical tracers are often functionally related in the sense that the mixing ratio of a chemical species depends (non-linearly) on that of one or more others. As an example, Plumb (2007) points out the surprisingly compact non-linear relationship between N<sub>2</sub>O and NO<sub>y</sub>, which is observed in the lower stratosphere, and which is important for the O<sub>3</sub> chemistry there. Lauritzen and Thuburn (2011) pointed out that transport schemes used in atmospheric modelling should respect such functional relations and not disrupt them in unrealistic ways. The implications of not maintaining functional relationships will generally lead to the introduction of artificial chemical reactions.

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.

In every chapter, we included in the beginning the topic to come. The summary of the main points is included in Chapter 7, instead of summarising at the end of each chapter. This avoids a duplication of information and also provides a synthesis of our comprehensive reviews in a concise manner in Chapter 7.

- 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 Better 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>.

The references are very useful. We benefit from them in this paper revision and also our future paper writing.

## 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?

Please see our answer to B 1; the authorship remains the same.

## 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.

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."

We rewrote the abstract to make the purpose, used data and methods, results, and principal conclusions of the manuscript more clear:

"Online coupled mesoscale meteorology and chemistry models have undergone a rapid evolution in recent years. Although mainly developed by the air quality modelling community, these models are also of interest for numerical weather prediction and regional climate modelling as they can consider not only the effects of meteorology on air quality, but also the potentially important effects of atmospheric composition on meteorology. Two ways of online coupling are distinguished: online integrated and online access coupling. Online integrated models calculate meteorology and chemistry on the same grid in one model using one main time step for integration. Online access models use independent meteorology and chemistry modules that might even have different grids, but exchange data mutually in two ways on a regular and frequent basis. This article offers a comprehensive review on the current European research status of modelling practices towards online coupled modelling of meteorology with atmospheric chemistry. Eighteen regional online coupled models developed or used in Europe are described and compared. Topics discussed include a survey of the potentially relevant processes for interactions of atmospheric physics and composition; a brief overview of European model developments and existing online mesoscale models; an analysis on how feedback processes are treated in these models; numerical issues of coupled models; and case studies and model evaluation methods. Finally, this article highlights selected scientific issues and emerging challenges that require proper consideration to improve reliability and usability of these models for three scientific communities: air quality, meteorology modelling including numerical weather prediction, and climate modelling. This review will be of particular interest to model developers and users in all three fields as it presents a synthesis of scientific progress and provides recommendations for future research directions and priorities in the development, application, and evaluation of online coupled models."

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

We added in the introduction the contents of the previously published papers and highlighted in which direction the current paper gives new insights.



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.)

We deliberately left out some aspects, which are not of more relevance in online models compared to offline models (e.g., hydrostatic versus non-hydrostatic models). This as well as not touching the way convection is parameterised also minimises the overlapping with the paper by Kukkonen et al. (2012).

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?

We did not want to overload the main part of the paper with detailed descriptions of single models, therefore left those to the appendix. However, the characteristics of models (more general) are relevant, thus they take some space in the main part of the paper. The processes described are not all implemented in models these days but we describe some of the prospects. However, we agree that our recommendations were not clear enough and we therefore highlighted them more in the conclusions chapter 7.

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.

We would very much have liked to include the same content and depth for each single model description. However, this depends on the information that we received from all modelling groups involved. We thus had to decide to either remove some models from the information on models at all or keep some inconsistency. We decided for the last to document the variety of models and focused on having all tables in the main part well filled.

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.

We gave more motivation for these feedbacks.

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.

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.

For a, b, c, d: We are sorry for your confusion. We rephrased the text, using full sentences, to make the feedbacks better understandable.

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.

We moved the section on COST into the introduction to introduce the COST action in the very beginning and only refer to it later.

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

Thanks for providing this useful comment. Table 3 and its accompanying text have been modified based on those comments (see below for details).

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.

The online coupled meteorology and chemistry models are used in three major application areas: meteorology calculations and especially NWP, AQ, and climate studies. Obviously, due to different time scales important interactions differ from one model application category to another, as described in Section 2. This paper focuses on NWP and AQ models, however, including all the three model categories and identifying the important interactions for each model category is very useful for the scientific community as a whole: some processes are currently not well known and joint efforts of all three communities would help to come to a faster progress in science.

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.

Even so those participating in the survey are not experts on all aspects of the modelling, the survey remains of value since the "Don't know" was frequently used to illustrate this non-expert knowledge. The frequent use of the "Don't know" increased the trust in the survey results.

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.

This limitation has been added in the text.

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>

We agree that the averages are somewhat questionable and argue mainly with the frequencies of single answers. We also point out in the text that the distinction of the categories is subjective. Nonetheless, we think the outcome of the questionnaire is of value.

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

The mean score 2 (ranks the “adequacy of the representation of the interaction in models”) has been modified and set now to ‘not included = 0’.

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.

The text is no longer in itemisation style but a coherent paragraph. The manuscript was, however, not incomplete, just in another style.

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.

The definitions are more clearly given now (see previous comments) and now used consistently throughout the text.

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.

Indeed we were thinking about this problem when we wrote section 4.1. Admittedly, the section does not provide enough detail to be suitable as a textbook (or even a section thereof) for early-career scientists. If we had aimed for that, the section would have become far too long to be included in a journal paper. At the same time, experts and practicing scientists are not likely to find the information they look for in the text itself. With that goal in mind, the length of the section would have exceeded reasonable limits, too. Thus, one alternative would be to leave out the section completely. However, we’d like to argue that the section is needed, as the meteorological models are obviously a substantial part of online coupled meteorology chemistry models. Thus we decided to rather keep the section, but to make sure that the literature we are referring to provides updated information for further reading and that key terms, such as convection, advection, diffusion, etc. are briefly mentioned. An important part of the section is Table 5, where updated references for further reading are provided. As such, we believe that the section is helpful for readers with knowledge in modelling of the atmosphere. When searching the internet for harmonized information about mesoscale meteorological models we felt that it is indeed very hard to find such overview information. In this respect, we find this section particularly helpful.

We have modified the first paragraph of the section to make its purpose more clear.

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.

The section 4.2 has been revised correspondingly.

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 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.

We included additional text linking the different parts of Section 4. Specifically for section 4.3: We reduced the number of subsections, which makes section 4.3 more coherent.

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.

These sections are merged into the new section 4.3.3 and we added references where necessary. The definitions of internal and external mixture are clearly explained.

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.

We now describe the forest in an introduction to section 4.3 and thereafter the 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.)

We improved this section (now section 4.3.3).

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

This section (now section 4.3.4) has been slightly extended. A comprehensive overview on the mechanisms currently applied in air quality models including those used in Europe is given by Gong et al. (2011).

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).

This section has been removed.

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 should they be transported down the temperature gradient in the first place? This section doesn't say how TTD is modeled. 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.

The description of TTD, theory and modelling, is matter of a several pages of mathematical derivations. We shortly describe the outcomes and give references to the full theory and applications in atmospheric modelling instead.

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

The text has been revised.

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?

The header has been changed, the lower level of subsections has been kept to separate the different processes. A rewriting would be possible, but we assume it is easier for the reader to find the processes he or she is interested in, if we keep the subsection titles.

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?

The schemes mentioned here neglect any chemical reactions but are simple bulk schemes. The more advanced schemes are described in Section 4.5.

22. Section 4.5.2.

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

The paragraph has been removed, and the whole section 4.5.2 has been rewritten.

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.

The whole section 4.5.2 has been rewritten.

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 among the different sections.

The whole section 4.5.2 has been rewritten.

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

The whole section 4.5.2 has been rewritten.

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).

pp. 12574: four references have been added for supporting this statement on the effect of mixing state and composition of PM on cloud optical properties. Most of the references on specific models and modules are contained in Table 8; they are not repeated in the text. Definitions of internal and external mixtures were provided in Section 4.3; we have chosen not to repeat them in the context of this section, although it does make section 4.6 less self-contained.

The knowledge of basic concepts (e.g., internal/external mixing, direct and semi-direct effects) is assumed as pre-knowledge for the target audience of this paper, thus no definitions are necessary. However we provide some of them now.

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.

pp. 12576-12577: Section 4.6.3 has been revised towards a more consistent and structured presentation of the various radiation modules.

25. Section 4.8. This entire section is problematic.

The section has been shortened and rewritten to address the below issues of confusing statements, grammatical errors, inconsistencies, etc.

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?

Those nine effects have been removed in the revised manuscript.

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

The redundant material has been removed.

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

The "chain effect" has been clarified along with several examples in Section 2.

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.

This content has been removed in this section. A more clear description of this chain effect is provided in Section 2.

The reviewer is correct that it is difficult to identify the cause and effect of the complex chain effect in an absolute sense. In this example, the chain feedback starts with temperature gradients that affect turbulence mixing, which in turn affects surface concentrations and boundary layer outflow/inflow. This does not rule out possibilities of other factors that can also influence turbulence. The nonlinearity and interwoven relationships through multiple processes among the multiple variables attest the needs to use online coupled models to accurately simulate these processes.

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.

This statement refers to online access model, rather than online integrated models. The limited ability of online access model is caused by possible inconsistencies of meteorology and chemistry as a result of the limited frequency of exchange data in both directions between meteorological and chemical model (e.g., such exchanges may not happen during each time step and on the same grid, leading to inevitable inconsistencies). To address the review comments, the reasons for such a limitation have been provided in the revised version.

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

Those errors have been corrected and some confusing statements have been clarified throughout this section.

g. The section lacks quantification and references.

Additional references have been included.

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.

This section characterizes the representation of interacting mechanisms in online models, thus it offers useful information to the readers. To address the reviewer's comments, we have modified this section to streamline it for improved readability.

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?

Section 4 deals with processes, in Section 5 we focus on numerical and technical characteristics of the online models used in Europe. The authors believe that the numerics section makes more sense to be presented after the description of the scientific characteristics about atmospheric physics and chemistry, and not mix it within it. We agree with the reviewer about the importance of this section, and in this sense the section has been revised and improved following the recommendations of all the reviewers' comments.

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.

The readers of this paper should be model developers and users, who wish to understand why certain aspects of their results might look strange. Sections 5.1.1, 5.1.2, and 5.1.3 have been rewritten.

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).

The paragraph is re-formulated.

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.

The section has been rewritten for better consistency with the rest of the paper. We agree with the referee that much of what is mentioned in the first part of this section might be "obvious and elementary" (or should be). However, it is also our experience that it is also often forgotten when new code is developed. Since models are rarely developed from scratch, but generally "evolving" with more and more variables and processes being added, some of the choices (coding wise) which were the most efficient for the previous versions of the model might not be any more with the changes it has undergone. Therefore, the section is meant to remind the reader to reconsider, if the current implementation is still the optimal or if it needs to be adapted to the new (and more complex) model.

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).

The text has been slightly changed also in accordance to ref #6.

b. Much of the section lacks references (p. 12595, lines 20-25; p. 12596, lines 4-6 and lines 11-18).

References have been added where needed.

c. What does "reordered and relumped" mean? Could you use more scientifically precise language, please?

Text changed avoiding these words, which are typically used in atmospheric chemistry modelling. We replaced it by words more known by the wider community.

d. The paragraph about Bangert et al. seems out of place. Why select just this one study to discuss in detail?

The paragraph has been removed.

e. Vogel et al. is only provided one sentence, and it is unclear what the reader is supposed to take away from it.

The paragraph has been removed.

f. I also don't understand the point of these remaining examples. No useful or practical information is provided to the reader.

The examples have been removed.

g. There is little discussion of the relative merits of various approaches.

We added a few sentences and linked the approaches used to Section 5.2.

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.

We do not think that it is possible to provide a general guidance for model users if the chemistry and initial boundary values are derived from different models. As there is a large variation of scales to be nested and chemical composition available in the different models, the actual approach strongly depends on the purposes of the application.

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.

The beginning of Section 5.5.1 has been rewritten to be more precise. We replaced "IC" by "initial conditions" throughout the text. The term "adjoint" is now shortly explained, the term "input functions" has been deleted, and we now refer to "emissions" and "boundary values" explicitly.

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.



The text has been extended slightly. However, it is out of the scope of this section to provide a comprehensive discussion of all those challenges.

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.

This sentence has been combined with the first sentence.

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 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?

As explicitly stated at the beginning of Section 6, "the focus is on an overview of application studies of online coupled meteorology and chemistry models in Europe published during approximately the past ten years.". This review might not be complete; however, we believe that it covers the most important case studies within the scope of this paper.

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.

We rewrote this paragraph and removed the model name lists.

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.

This paper provides some general directions regarding coordination and integration of models and scientific efforts in section 7.2. More specific recommendations are not possible now, but are expected to be formulated towards the end of the COST action (spring 2015).

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.

The section has been entirely rewritten reflecting the critical comments.

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.

Since there is no unique solution for improving ABL calculation, but only several approaches (e.g. improved vertical resolution, improved horizontal resolution, and other closures) that do not deviate between offline meteorology and online coupled meteorology chemistry models, we removed this statement.

38. Section 7.1.3. The last paragraph is redundant.

We do not find this paragraph redundant and thus kept it (with slightly changed wording).

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.

We have mentioned the main ideas concerning data assimilation and that more details will be addressed in a specific and thus more extensive paper focusing purely on data assimilation. Being more specific, we added hybrid data assimilation techniques that combine the advantages of variational and EnKF techniques. So, we added in 7.1.4 the following:

"...optimal interpolation, variational approaches, EnKF or hybrid techniques combining the advantages of both variational and EnKF techniques are applicable."

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 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.

For 40 – 42: The conclusions' section 7 has been rewritten, the need for long-term simulations is not discussed here anymore.

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.

The original caption was somewhat misleading, because most of mechanisms are just gas phase only. The table caption has been modified to make it clear in the revised version.

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

Tables 1 and 2 are introducing the main ways/mechanisms of Meteorology's impacts on chemistry and chemical species' impacts on meteorology. The Tables B1 and B2 are more specific including main model variables needed for evaluation of different effects of meteorology on chemistry and vice versa. These tables are more technical, so that is why they are included into the Appendix B.

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.

Yes, both figures demonstrate the differences of the two approaches. We find that the present design of the figure better highlights the similarities in the input (centre column) and that the main differences lie in how this input is treated and the output is delivered. We consider that the new one in this paper is more informative especially for readers thinking of going from offline to online models.

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

Figure 7 has been removed.

## 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.

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.

The word 'core' has been removed where it was inadequate.

2. It is unnecessary to use "see" in front of figures or references (e.g., p. 12560, line 15). Delete throughout.

Corrected.

3. p. 12560, lines 5-7: I don't understand this sentence. Would an example help explain what the authors mean?

The sentence was reworded and an example was added.

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.

We restricted this statement to bin numbers usually employed in the models discussed in the paper, for larger bin numbers this is not true anymore (e.g. Weisensten et al, Atmos. Chem. Phys., 7, 2339-2355, 2007).

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>>

Corrected and reference added.

6. p. 12569, line 17-20: This paragraph has no citations. The text is vague and lacks precise meaning.

The whole section was shortened and two references that illustrate the effect are added.

7. p. 12570, lines 28-29, and p. 12572, lines 17-18: No references for these statements are provided.

The text has been rewritten.

8. p. 12572, line 6: "thence" is not commonly used in English and should be rewritten.

The text has been rewritten.

9. p. 12572, line 24: "biological particles" is vague. Please be more precise.

Added.

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.

The text has been rewritten..

11. p. 12578, lines 10-12: This sentence requires references.

Reference have been added

12. p. 12579, lines 4-5 and 7: These two sentences are redundant.

The text has been rephrased.

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.  
**This information was added.**

14. p. 12580, line 5: Does "it" refer to NO<sub>x</sub> or lightning?  
**The text is clearer now.**

15. p. 12580, lines 20 onward to p. 12581, line 21: Much of this material is missing references.  
**Reference have been added**

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.  
**Reworded.**

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.  
**L 1-4: The text is rephrased to better understand our conclusions.**  
**l17-26: The text is rewritten and references have been added.**

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.  
**The text is rephrased.**

19. p. 12582, line 11: It is unclear how dry deposition occurs indirectly through soil moisture. Moreover, no reference for this process is provided.  
**The text is rephrased.**

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.  
**Regarding p. 12588, lines 2-5: As stated in lines 6-7 the idea is described in more detail below in the text (in this particular case in section 5.1.4)**  
**Regarding p. 12590, lines 4-6: See the above suggested add to section 5.1.3.**

21. p. 12590, line 8: Here is an example of how verbose text can be made more concise and simple. "Recently, it has been pointed out by Lauritzen and Thuburn (2011). . . ." can be rewritten "Lauritzen and Thuburn (2011) showed. . . ."  
**The text has been modified.**

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.  
**The text has been rewritten for clarity.**

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.  
**p. 12591, lines 17-18: Following the reviewer comment the sentence "...meteorological driver" has now been modified by "...meteorological model".**

The term “coupler” refers to a module that exchanges information between the meteorological routines and the chemistry routines. Such a coupler, sometimes, perform interpolations, allowing the use of different models on different projections to work together. A clarification sentence has been introduced in the revised manuscript. The concept coupler is maintained in the manuscript because it is the main component that differentiates online integrated and online access models.

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.

This section has been rewritten.

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

Following the reviewer comment, the section has been shortened and the most elementary material has been removed.

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.

The suggested change was made.

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

This section (concerning GPUs) has been extended, to include more information, and some references have been added.

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

This is not a mere repetition, but it is emphasised that as true as it is true for MPI also GUP models are not easily derived from current model codes.

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

The title has been changed to “Initial and lateral boundary values”

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.

“Integrated model” was used as synonymous of “online coupled model”; we now use only “online coupled model”.

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

This sentence and the previous one have been rewritten and combined into a single sentence (“it” referred to CDA to improve initial conditions).

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.

The abbreviation “AQ” is defined in the abstract and in the introduction; therefore, it seems appropriate to use it within the main text.

33. p. 12602, lines 2-4: This sentence is redundant with text already included in this section.

The two sentences have been rewritten into a single sentence and we refer now to previous text (“As mentioned above,...”).

34. p. 12602, lines 11-13: This sentence should appear earlier in the manuscript.

We put the sentence “Online coupled mesoscale meteorology and chemistry models have seen a rapid evolution in the past few years particularly in the United States (Zhang, 2008) and are becoming increasingly popular in Europe” in the Introduction p12545 line 28 before “Historically,...”. On page 12602 line 11 the sentences was rewritten as: “Along with the increasing interest for and spread of the online coupled meteorology and chemistry models, the need for a thorough evaluation through comparison with observation is growing”.

35. p. 12610, lines 2-13: This content is vague and needs more specifics.

It is not clear what part the reviewer refers to. On page 12610, lines 2-13 contain partly Section 6.2 and Section 6.3. But we rephrased some sentences here anyway, which might solve the issue.

36. p. 12613, line 9: Delete "the study of". These words are unnecessary.

“the study of” was deleted.

37. p. 12613, lines 16-19. Please provide references for this statement.

Citation is added.

38. p. 12613, lines 25-27: Please list references in chronological order here and throughout the manuscript.

For 37-38: all references have been listed chronologically.

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.

Reply to G39-G40:

We have reworded the text and the structure of the section to clarify the points we wish to make. We believe the section now addresses more clearly the topic of model evaluation of regional air quality models by discussing the existing framework, available results of evaluation of offline and online models, identifying challenges to be tackled in model evaluation of online coupled models.

41. p. 12624, line 7: Is the abbreviation SLCF needed if it is only used in this section, and a few times at that?

We do not use it anymore.

42. Table 2: Does "modulates" provide the correct meaning the author intends? I don't think so.

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