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

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Overall I think that the paper should be published in ACP, subject to revisions which should take less than a few weeks to carry out. My comments are generally requests for clarification, as opposed to serious issues with the descriptions, as well as identifying places where the authors are repeating things said elsewhere in the paper or which are not necessarily of key relevance for the topic under review.

One key concern I have is the length of the paper, which is much longer than is usually acceptable for a journal publication (it would make a very good reference book!). I've included some suggestions on how the flow and information presentation could be improved with reduction of some sections and/or moving these to an appendix.

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

Specific comments:

Page 12546, line 14: "NWP does not require detailed chemical processes". This statement presupposes that the impact of feedbacks on NWP is small, while other parts of the paper note that long term evaluation of full feedback models is still needed (and will hopefully come to light under the AQMEII-2 project). It would be more accurate to state that "NWP in its current state does not include detailed chemical processes"
We agree that it is too early to draw final conclusions for the NWP community. We just stressed the problem, but more evaluation studies (including the phase 2 of AQMEII) will be needed. The text has been corrected correspondingly.

Page 12546, line 20-21, "For CWF ... on-line integration definitely improves AQ..." This statement has been made in advance of the review itself – the authors have not made a case for these improvements at this stage in the paper. Some references that support the statement are needed here, or a better justification for the statement.
We fully agree. Some justification and corresponding references have been included in the revised version.

Page 12547, last paragraph of section 1: At the same time, the authors acknowledge elsewhere that longer evaluation periods are needed in order to determine the impacts of on-line feedbacks on the meteorological forecast and vice-versa. This should also be mentioned here: the time periods which have been used for evaluation (up to this point, in the literature) have been short – and this may also affect the ratings of which aspects are more or less relevant. So a caveat is needed to that effect in the manuscript.
A sentence regarding this point has been included. This issue is also discussed in Section 6.

Page 12547, line 26, days to decades: consider the impact of feedbacks associated with convective cloud formation (indirect effect) via CCN growth. This may be a relatively fast process (hours rather than days for the time scale).
We agree, it is corrected to 'from hours to decades..'.

Page 1258, line 18 – Tables 1 and 2 are missing the impact of meteorology on the removal of chemicals through deposition (i.e. consider the met-dependent portions of surface resistance and deposition velocity calculations). Note also that geographical input data such as vegetation type and land use type are key inputs; not sure where this fits in to the tables. Table 2: note that the introduction of aerosols may affect wind direction as well as speed.
We added "Land surface (soil type and vegetation cover, soil moisture, leaf area) influences natural emissions (e.g. dust, BVOCs) and dry deposition and in Table 2 wind direction.

Page 12549, line 10: Connection between aerosol absorption of sunlight and cloud liquid water is unclear to me. I think there may be a few processes that are needed between the two to make that connection clear.
The description of the interaction chains is rewritten and should be better understandable now.

Page 12550, discussion on Table 3: Table 3 is interesting, but also shows biases, in that it is dependent on the expertise and background of the respondents. The authors note (line 21) that the results might be affected by individual opinions – definitely, and (line 25) that very few climate modellers were involved in the survey. There needs to be a few lines of justification regarding why a survey that is subjective in this nature should be done.
Thank you for these comments. The text has been corrected correspondingly following comments of other reviewers. 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" increases the trust in the survey results.

Page 12554, line 20-21. ABL heights – explain the importance of the ABL (as opposed to, for example, the values of diffusion constants – is the ABL a useful diagnostic of model performance or a variable required for prediction)?

The ABL height is controlling the dispersion of pollutants and therefore, is an extremely important diagnostic for ACTMs. This is especially true for online coupled models where the ABL height can also depend on aerosol concentrations and their feedbacks. The diffusion coefficients are very far from constant, highly uncertain and affected by numerical diffusion, so the ABL height is still an important characteristic for many models. Diagnostic parameterisations are not always suitable for inhomogeneous terrains, so prognostic formulations are preferable (see references included).

Page 12556, line 17-19. It may be worth referencing mechanism intercomparison papers here (e.g. Kuhn et al 1999): the level of detail in a chemical mechanism does not necessarily imply an increased level of accuracy in the simulated results; the authors state that computational constraints are the only thing preventing use of a very complex mechanism – what evidence is available that suggests the very detailed mechanisms actually do a significantly better job in the context of 3D AQ models? It would be good to have something quantitative.

Good suggestion, we have modified this paragraph into sections 4.2, 4.3.3 in the revised manuscript.

Page 12556, lines 21-27: This discussion implies that the gas-phase mechanism is the key issue in accurately representing secondary organic aerosol formation. However, there is good evidence to suggest that significant SOA formation may occur in association with particle surface reactions, cloud chemistry, etc. This should be mentioned here. It would be good to reference Gong et al (2011) here in that context as well as in the latter discussion in the manuscript.

Good suggestion, we have modified this paragraph along with the reference in section 4.3.3 in the revised manuscript.

Page 12559, lines 1-6: Note that each new mechanism requires a new set of emissions data, due to the VOC speciation changing between mechanisms. Also, the connections to other processes (e.g. deposition) will be species, hence mechanism dependant. The point being that changing the mechanism in the gas-phase portion of the model is only part of the setup required for switching mechanisms in a 3D model.

Good point, we have modified this paragraph in section 4.2 in the revised manuscript (see below):

“Changing the chemical mechanism also has implications for other aspects of the modelling system. The introduction of new species may require new emissions and associated speciation, as well as information on dry and wet deposition rates. “

Page 12559, lines 8 to 20: There are a number of statements being made in this paragraph which need some more justification and/or references. “NWP does not depend on detailed chemical processes that may be necessary to predict air quality” – in the absence of long-time period simulations with and without feedbacks in a fully coupled model, how can this statement be made? Certainly current NWP models do not include detailed chemistry, but the statement reads as if for NWP, the feedbacks are not important. Has this been proven? “Needs just enough complexity to be able to model aerosol effects on radiative and precipitative processes” – how much complexity is that? Determining the extent to which NWP may be improved through coupled models with feedbacks is an ongoing and interesting topic of current research – I think that the “jury is still out” with regards to their importance. So these statements are perhaps premature.

We have modified this paragraph in section 4.2 in the revised manuscript.

Page 12559, last paragraph: yes, a central database for gas-phase mechanisms would be helpful – it would also be helpful to have methodologies for SOA formation be a part of the same or associated database.

Yes, we agree. This point has been incorporated in the revised manuscript.

Page 12563, top paragraph describing inorganic heterogeneous chemistry approaches. It is important to note somewhere in the text that these modules (even when solving sub-spaces of the problem as in the case of ISORROPIA-1,-II) usually are used in “bulk mode”, that is, inorganic aerosol mass across the size distribution is added up within each species, and partitioning is with respect to this bulk mass. This is done for computational efficiency, rather than solving local problems at different locations within the size distribution. Also page 12565, line 21: “is in a bulk thermodynamic equilibrium” is more precise.

Right, this is an important point. We have modified the text according to the reviewer.

Pages 12563, 12564, discussion on SOA formation. It should be noted that all three of these approaches tend to assume that all of the SOA formation results from gas-phase reactions, while recent laboratory evidence is starting to suggest that other mechanisms (e.g. surface reactions, cloud chemistry, acid catalysis of organic polymer formation) may be required to adequately represent the formation of organic aerosols.

We agree, this point has been incorporated in the revised manuscript.

Page 12565, line 14 – would also be good to reference Gong et al 2011 here as a review on this topic.

This suggested reference has been added in this statement.

Page 12574, line 13-15: how many AQ models explicitly model Fe, Al, and PAH compounds? Is this based on observations or on models which have evaluated these impacts?

p. 12574, line 12-15: Very few online models are explicitly considering metals or PAHs. Details on their treatment can be found in the references provided for each of the models. However, very few models explicitly consider metals, and none of the online models is treating PAHs. However, it turned out to be practically impossible to collect this information directly from the modellers themselves during the review process.

Page 12574, section 4.5.1: might be better to title this “Direct radiative effects of gases and aerosols”, since section does not deal with aerosol indirect effect.

p. 12574, section 4.6.1 title: While this section does focus on direct radiative effects, new references and text have been added relating to indirect effects which are treated in more detail in other sections (e.g. 4.5.2).

Page 12576, last sentence of 4.5.2: What data is available for this validation? Also, what data are available on fundamental aerosol properties such as complex refractive index as a function of chemical speciation within the aerosol? The section does not mention issues regarding internally mixed, versus core/shell, versus multiple core/shell approaches to working out the impact of aerosols (recent work by Binkowski and by Jacobson). It would be useful/interesting to compare the different complex refractive index values used in the different models in a table.

We have modified paragraph 4.5.2 towards the direction suggested. We have added two references that deal with the effects of different treatments in the mixing state of aerosols. The sentence about validation has been removed based on suggestion by David Schulz. Comparing refractive indexes of different models was not feasible as this would again require a new set of queries to modellers.

Section 4.7.1, second two paragraphs. One aspect of the "natural" emissions is that no matter how good the emissions algorithm is, the model will be dependent on the accuracy of the underlying input data, which in this case is land use type, vegetation type, etc. Given that this is the case, the authors should comment on the availability and accuracy of that data, and whether any improvements are required to improve on- line model accuracy. Also, many of the wind-blown dust processes make assumptions regarding soil type and soil moisture – parameters that may be available in meteorological model input databases, and in any event a point for further coupling between the meteorology and chemistry.

Good suggestion, we have addressed this at the end of this section.

The importance of good knowledge of and coupling to land use/vegetation/soil moisture models has now been elaborated on a bit further, and is referred to from both the emission and deposition perspective. It has also been mentioned in the conclusions. However, no further attention has been given to the availability, except for a statement on soil moisture, which is a crucial but rather uncertain factor.

Page 12581, final paragraph of 4.7.1: note that on the local or urban scale the contribution of anthropogenic VOC emissions may be quite significant, since those emissions occur over a very limited spatial domain. Consequently, their impact at the locations where most of the human population resides may be quite large. Referencing just the large-scale impact may thus underestimate the local impact (which may be of greater importance from the standpoint of policy implications of the model results).
A remark has been added on the possibly higher impact of VOC to SOA downwind of densely populated areas.

Page 12582, line 2: Suggest "only possible pathways to remove" should be "only possible pathways (aside from chemical transformation) to remove".

In the sentence the possible chemical transformation is already mentioned. We don't think an additional bracket is necessary.

Page 12591, line 18: need to define what is meant by a "coupler" in this context.
Coupler is now defined as "process that exchanges information between model components and may execute diagnostic or remapping tasks".

Page 12591, line 27: "that exchange information" The word "exchange" implies a two-way interaction, while the sentence is in reference to offline models, where the information passing is one-way, and via input/output files. Clarify.
The sentence has been reworded as
"…The main characteristic is that the exchange follows only a one-way direction and no feedbacks are possible".

Page 12592: The authors introduce “slightly coupled” models here for the first time. What does this mean – how is this defined? It would be better to stick with the earlier terminology and/or define this in the earlier section where off and on-line models are defined.

This terminology is used by Zhang (2008) to differentiate the degree of complexity in the online coupling of the models. As indicated in the manuscript, coupling in online models varies in complexity, and the Zhang (2008) classification is presented (slightly-, moderately-, or fully-coupled). There is no consensus on the exact definition of such terms within online modellers. However, in the present paper we only introduce these terms in Section 5.2, but use throughout the text online access and online integrated models.

To address the review comment, the following statement was added in Chapter 5.2:

The slightly- or moderately-coupled models only couple selected species (e.g., O_3 or aerosols) and/or processes (e.g., transport) and may not account for all important feedbacks among processes, they are named in the present paper online access models. The fully-coupled models couple all major processes at every main time step and simulate a full range of atmospheric feedbacks (corresponding to: online integrated models).

Page 12593, last sentence of section 5.2. Computational demands also rapidly increase with off-line models; this is not something that is off or on-line specific, so the statement can be removed.

Following the referee’s comment the sentence has been removed.

Page 12598, statements on bottom of page of the transient nature of initial conditions. While the initial conditions may be transient, the use of data assimilation to improve boundary conditions in a regional run may be quite important. Consider the findings of the HTAP experiment, wherein the impact of emissions changes in different continents where evaluated based on global-model-predicted changes in that and other conti- nents. One of the important findings there was that outside of the summer season, long range transport of ozone from one continent to another could have a significant impact on ozone levels in the destination continent. Following this concept – if data assimilation is used to create better ozone boundary conditions for a regional model run, it could have a substantial impact on regional model predictions. Perhaps the discussion could be linked to 5.5.3 at this point?

We agree that boundary values are important for O_3 concentrations and that CDA could be used to improve those for better model performance. We now explicitly mention this point in Section 5.5.3 and give a couple of references highlighting the influence of the long-range transport of O_3.

References:

Zhang, L., Jacob, D.J., Downey,N.V., Wood, D.A., Blewitt, D., Carouge, C.C., van Donkelaar, A., Jones, D.B.A., Murray, L.T., Wang, Y. Improved estimate of the policy-relevant background ozone in the United States using the GEOS-Chem global model with 1/2° 2/3° horizontal resolution over North America, Atmos. Environ., 45, 6769-6776, 2011.

Waked, A., Seigneur, C., Couvidat., F., Kim, Y., Sartelet, K., Afif, C., Borbon, A., Formenti, P., Sauvage, S. Modeling air pollution in Lebanon: evaluation at a suburban site in Beiru during summer, Atmos. Chem. Phys., 13, 5873-5886, 2013.

Colette, A., Bessagnet, B., Vautard, R., Szopa, S., Rao., S., Schucht, S., Klimont, Z., Menut, L., Clain, G., Meleux, F., Curci, G., Rouïl, L. European atmosphere in 2050, a regional air quality and climate perspective under CMIP5 scenarios, Atmos. Chem. Phys., 13, 7451-7471, 2013.

Page 12600, line 23: “simulated online” versus “two-way coupled”? So is the first supposed to be with reference to online but not coupled modelling? Unclear. Similarly, page 12601, line 1 – online configuration versus current coupled system? Unclear. Also, more efficient with respect to what, data assimilation? Needs to be reworded.

We meant “online coupled”; this has now been corrected on pp. 12600 and 12601. We also specified “two-way coupled” on p. 12601. It is more efficient computationally (not in terms of CDA here).

Page 12601, line 21-23: The sentence starting with “Nevertheless” doesn’t make sense: what is probably the easiest CDA technique to implement? Something has been inadvertently deleted, here. Line 27: need to explain what is meant by parameter estimation in the context of online models.

The first two sentences of this paragraph have been rewritten as a single sentence. Some examples of parameters that could be estimated via inverse modelling are now provided.

Section 6.1: This describes past use of online coupled models, but its not clear what results from the studies were unique as the result of the use of that type of model as opposed to off-line models. A key question that I kept asking while reading this section was, “What was the unique contribution of the on-line approach”? This section would be better removed, or shortened and perhaps added to the introduction as background for on-line modelling work in Europe. The last few lines (12604, line 23 and down) talk specifically of how the on-line approach improved the results – that’s what should be the focus of this section, if possible.

The reviewer is right that often there is no distinct difference from previous studies using offline models. This is why the section was separated into two subsections 6.1 and 6.2, with the latter presenting applications including feedbacks, which are clearly distinct from studies using offline models. The idea of this section was to present a review of applications of online coupled models and thereby to document their history in Europe. The introduction to section 6 now reads as follows:

“This section reviews applications of online coupled models in Europe published during the past 15 years and thereby documents the historic evolution of this type of models. Note that in most of these studies, in particular in the early studies, the coupling was only made from meteorology to chemistry. These early applications, which do not use the full potential of coupled models except for a more consistent numerical and physical treatment of chemical and meteorological quantities, are summarized in Sect. 6.1. Studies using the advantage of online couple models to consider feedbacks of chemistry on meteorology are highlighted in Sect. 6.2. In Section 6.3 the focus is on model evaluation and in particular on methodological aspects specific for online-coupled models.”

Please also note that advantages of the online approach even in studies without feedbacks are already mentioned in Section 6.1, e.g. the sentences

“MCCM was also used to compare online versus offline simulations on cloud resolving scales (Grell et al., 2005) to demonstrate the deficiencies of the offline approach at high resolutions.” And

“The potential benefits of online coupling with respect to the quality of simulated transport and dispersion of chemical species were demonstrated by Korsholm et al. (2009). ….”

Section 6.2 will be shortened somewhat by removing the example of the COSMO-M7 model as this example was targeting climatic time-scales, which is not the focus of this paper.

Section 6.2: it may be the way the information was reported here, but there seems to be a significant lack of comparison with observations in the modelling studies reported here. That is, only a few studies are mentioned as having had an impact (positive or negative) on the accuracy of the model predictions with respect to observations. Is that because most of the on-line work in Europe has been sensitivity studies to date, or is that information available in the papers quoted? If the latter, it should be summarized as part of this section. If the former, then the need for detailed evaluation should be mentioned as one of the outcomes of this section.

Comparisons with observations were made in most of these studies, and several comparisons are mentioned, especially if they demonstrated the benefit of the online coupling, e.g. for the studies of Pérez et al. (2006), Korsholm (2009); Chaboureau et al (2011) and Bangert et al. (2012). However, it is also true that in many of the studies presented in this section, no systematic comparison with observations was performed and that there is a clear need for further evaluation of online coupled models and the effects of feedbacks. We made the following changes:

(a) In the section referring to Chaboureau et al. (2011), we changed the last sentence to “From comparison with rain gauge observations they concluded that precipitation was better predicted when including the dust prognostic scheme and radiative feedbacks in the model.”

(b) We added the following sentence at the end of Section 6.2: “In many studies mentioned in this section only the differences in chemical and meteorological parameters between simulations with and without feedbacks were highlighted, but no systematic comparisons with observations were performed to evaluate the potential benefits of considering feedbacks. There is thus a strong need for detailed evaluation studies as outlined in the next section.”

Page 12606, line 20, also page 12608, lines 7-9: for these quantitative statements of impact, mention the significance relative to, e.g. typical radiative budget. i.e. what can be said regarding the relative impact of the feedbacks compared to other factors? Are the feedbacks capable of having a significant effect, are they lost in the noise, etc.? For direct aerosol effect impact being “substantial” – can this be quantified? Were these model sensitivity tests, or was there any attempt in the studies to rate the impact relative to observations (i.e. any improvement via feedbacks)?
These were only sensitivity studies. When a publication did not clearly state, whether the effects were significant or not and e.g. improved performance, we refrained from making such a statement by ourselves.

Page 12607, line 1 – does this imply that the impact of feedbacks on turbulence had a minor effect on the predicted model results?
The specific study is not conclusive as to the reasons behind the marginal improvement of the model results. Modification of turbulence is expected to be one of the reasons affecting comparison with measurements but certainly not the only one. The authors of that study note that further investigation that will cover more extended simulation periods will probably be required to assess model behaviour more adequately.

Page 12608, lines 18 to 24: the impact on model results is described, but not whether the model results improved (either relative to observations or from a theoretical stand- point). Do these studies make the case that on-line coupled models are “doing a better job” or is there insufficient evaluation at this point to say this? Page 12609, lines 10 to 15: this is the sort of thing that should be emphasized from the other studies as well: the on-line model in this case clearly has at least the potential to correct an existing deficiency in modelling.

Again, these were only sensitivity studies and no assessment was made whether the model was doing a better job or not.

Page 12609, line 19: brightening of what? Cloud top albedo? Surface albedo? Clarify.
This section has been removed entirely as the study by Zubler et al. (2011) is addressing climatic time-scales which are not the focus of this paper.

Redundancies in the manuscript (things that could be removed or reduced):
Page 12544, first paragraph of the introduction repeats information already in the abstract – remove.
Following your and other referee comments we rewrote the abstract and the introduction, reducing the overlaps and shortening the text.

Page 12583, lines 10 through 20: this information has appeared earlier in the manuscript and does not need to be included here. In that respect, anything that has been included in previous sections need not be repeated here – it would be sufficient to start this section at “Several additional feedback mechanisms” (page 12584, line 7).Page 12588, section 5.1: This is a good summary of the state of the science for numerical methods used for modelling – but it is not specific to the topic of the paper; the on-line aspects of modelling. This section should be greatly shortened, removed, or put in an appendix. The latter might be the best route – it’s a good summary, but it detracts from the paper’s main focus in its current location.

We have modified and shortened the text. However, different reviewers offered different suggestions, e.g. Referee 6 suggested to add even more explanations for the chains of the feedbacks and to remove the paragraph on page 12584, lines 7-11. We tried our best to address comments from all reviewers in the revised manuscript to the maximum extent possible. Section 4.8 has been substantially rewritten and shortened.

Page 12593, section 5.3. As in section 5.1, this section deals with generic issues that are not specific to on-line models per se, but any model configuration. As such, it is not part of the main topic of the paper. The section should be placed in an appendix or removed altogether (with my preference being an appendix – the information is useful, but not as relevant to the main topic under discussion). Page 12594, last 8 lines: the discussion does not explain why CAF represents a better means of coding – what is the advantage, and why?

Yes, some of the discussed aspects and requirements are also important for other types of models, however, for the online models these computational requirements (as well as numerical methods, e.g., advection schemes in section 5.1) are more relevant due to higher computational costs. For example, consider the actual prognostic equation for transport used: often this is different in offline and online models (see corrected version of section 5.1). Also, the magnitude of the mass-wind inconsistency problem is much larger in offline models. However, it is also relevant in some online models, especially when the chemistry-aerosol-cloud interactions are considered. Thus, to simplify the text flow, we decided to keep this section here (not moving it to the Appendix), but modifying the text stressing more the online modelling specifics.

The following sentence (just after Eq. (2)) has been changed:

“which is actually done in many chemical transport models” into “which is typically done in offline chemical transport models”.

Page 12595, section 5.4. Again, these issues are common to both off-line and on-line models, and should be removed or placed in an appendix. One issue that I would think would be worth mentioning in the main body of the paper would be the extent to which boundary conditions provided from a global on-line model may be superior to those from a global off-line model, if any work has been done on that topic. The last two paragraphs of the section lists the different approaches used, but doesn’t interpret which methods are better / worse, which would be of interest to the broader community.

The most important aspect when BC’s are considered in the online approach is that the chemical information provided at the boundary also affects the meteorology through feedback mechanisms. That is the main difference to the treatment of the BC’s in offline coupled models. The simultaneous availability of meteorology and chemistry, e.g., provided from a larger scale or global model is important to avoid inconsistency near the boundary of the domain. Additionally, these data should be physically consistent which is achieved, if the larger scale model also follows the online approach. If the meteorology and chemistry provided at the boundaries are not consistent the respective deviations can propagate throughout the simulation.

These appreciations have been introduced in the revised manuscript. Following the reviewer comment the two last paragraphs have been removed.

Page 12610, section 6.3: Model evaluation is a very important topic, but it is not a topic that is specific to the class of on-line models. A large part of this section could be removed or placed in an appendix – the section could easily start at line 3 page 12614.

We suppose the evaluation for online models has specific issues and we try to discuss these. A problem is that there are too many questions and issues for deep analysis in this field. Therefore, we just try to open this problem for further analysis (e.g., in AQMEII). We think we should keep this section here and not move it to the Appendix.

However, we have significantly shortened this section, especially the introductory part not specific for online coupled models.

Page 12614, line 19: why are new and improved strategies required for online model evaluation? The reasoning for this statement doesn’t become clear until the last paragraph, and should be presented earlier.
We added the following paragraph.
“Application of process analysis tools in online coupled models would help to identify the contribution of feedbacks and other processes to the calculated concentration values. In addition, using ratios of calculated concentrations (e.g. EC/PM2.5 or NO_2/PM2.5) rather than just the PM2.5 or PM10 concentrations for validation with measurements would help to get information about the effect of feedbacks.”

Line 23 “In many cases...” this presupposes that the differences due to feedbacks are small. Are they? What evidence is in the literature to this effect? Maybe include a few references.

“many cases” has been changed to “some cases”.

Line 26: should that be “much data assimilation”, not “data simulation”?

This has been corrected as “data assimilation”.

Page 12626, line 5: I don’t recall these issues being discussed in the paper (ship emissions, aviation - why/how has this become a recommendation? Line 23: “thus should be aimed at.”? Reword this, unclear.

This part and Section 7 have been rewritten substantially. You are right, ship emission where not explicitly mentioned, but we take it up now in the Section 4.7.1 on emissions.

Appendix A: the first part of the descriptions should focus on the degree of coupling in place. This is not clear from the descriptions as they currently stand.

We discussed this issue in Section 3 and Table 4, short descriptions of the coupling degree have been included. We also added a column in Tabel 4 that states if the model is online access or online integrated.

Minor issues:

Page 12544, line 4: replace “attempt reviewing” with “review”

The suggested change was made.

Page 12546, paragraph lines 13 through 22, as a point of interest (and for potential inclusion here), the on-line model GEM-MACH has been used for operational air-quality forecasting at Environment Canada, providing North American forecasts of O3, PM2.5, and NO2 to the public since 2009 (Moran et al, 2010). Also, page 12552, lines 15-20, please add the same reference here.

Thanks for the hint, the citation was added.

Page 12555, line 4: “calculate fluxes” should become “calculate radiative fluxes” Page 12561, line 9: “deviations variable” should be “deviations of variables”

The suggested change was made.

Page 12561, line 20: GEM-MACH model, Moran et al, uses 2 bins for operational configuration, 12 for research, FYI.

In this review we consider only online models actively used in Europe, so only the GEM-AQ version, used in Poland, was included.

Page 12566, line 15: “degree” should be “degrees”

The suggested change was made.

Page 12568, line 19: “It depends” – What depends? Not clear.

This has been clarified in the revised manuscript. It means ‘the Bergeron-Findeisen process’.

Page 12568, section 4.5.1 title: perhaps this should be “Aerosol-cloud interactions in online models with diagnostic property equations”; it seems to describe the methods used in on-line models which do not include feedbacks.

We agree. It has been corrected to “Aerosol-cloud interactions in online models with diagnostic property equations”.

Line 17: the “above example” mentioned: these sorts of parameterizations are unnecessary if schemes such as Abdul-Razzak and Ghan, 2002 are used.

This comment is probably for Page 12569, Line 17. We rephrased the text.

Page 12572, line 27: “opposite is true” Unclear – i.e. concentration is high enough for global impact, or the temperature for ice nucleation for BC & OC is relatively low?

The text was rephrased and reads now: “Black carbon and carbonaceous particles, on the other hand, are much more numerous, but it is not yet clear if they nucleate ice well above the homogeneous freezing temperature.”

Page 12621, line 1: “consistency” should be “mass consistency”?
This has been corrected as “wind mass consistency”.

References:

Moran MD, Ménard S, Talbot D, Huang P, Makar PA, Gong W, Landry H, Gravel S, Gong S, Crevier L-P, Kallaur A, Sassi M (2010) Particulate-matter forecasting with GEM- MACH15, a new Canadian air-quality forecast model. In: *Air Pollution Modelling and Its Application XX*, Steyn DG, Rao ST (eds), Springer, Dordrecht, 289-292.