Response to Reviewer #3

We have written a very long manuscript, so we would first like to thank reviewer #3 for taking the time to review our paper and for giving a lot of very useful criticism on different aspects of the paper. Their lead to a significant improvement in its quality. We have quoted the relevant text from the review (shown in italics) and have responded below each comment in times new roman. In addition, we have prepared a revised manuscript showing the revised text in red.

General comments

In general the paper suffers from too much lengthy and unnecessary descriptions, repetitions and, in some sections, too many details. A more concise language and a better structuring of the paper is needed. Thus the authors should work out a more concise version before publication to increase readability. Some examples on how the paper can be improved are given in the detailed comments below.

We thank the reviewer for this advice. We have revised the paper to remove the unnecessary text and repetitions. We have tried to make sentences as concise as possible. We have also removed many of the unnecessary details within the paper. Lastly, we followed advice from another reviewer and have removed the section analyzing daytime/nighttime, weekend/weekday, and summer/winter differences.

In the manuscript the model is presented as "new", which is somewhat surprising since the EPISODE model is well known for many project applications in the Nordic areas during the last 15-20 years. It should be made clearer what is new in the present version compared to earlier model descriptions (for example Slørdal et al., 2003). At the same time, it is acknowledged that it is important to publish a model description including new revisions.

We have now made it clearer in the introduction that a primary motivation to publish this article is to provide a comprehensive and definitive peer-reviewed description of the current version of the EPISODE model, i.e., version 10.0. Further, we make clear in section 2 that version 10.0 bases much of its heritage on the EPISODE version described in Slørdal et al. 2003 and have documented the key advancements in v10.0.

Detailed comments

Abstract

Page 1, 14: It is somewhat surprising that PM2.5 and PM10 is not included in the paper since the health concerns probably are stronger for these two components, and since the model EPISODE also largely has been applied to PM modelling (as documented in reports from NILU etc.)

We acknowledge that PM is a very important pollutant due to its significant health impacts. There are a few reasons PM was excluded from the case study in Sections 3 and 4 of the paper:

- Work prior to the submission of the manuscript identified problems with missing pollution source processes, i.e., road dust resuspension and domestic heating emissions linked to meteorology. The addition of both emission processes was planned to be documented in separate more focused research papers.
- We have several planned upgrades to the model representation of PM that we believe will significantly improve the simulations. These include PM below cloud scavenging, sedimentation, and the inclusion of different PM size bins.

We plan to carry out a case study focused on PM in the near future after we have completed the proposed upgrades and this will involve the new emission processes for road dust resuspension and domestic heating.

Despite this, we should make it clearer that the capability to simulate PM is included in the current implementation of EPISODE, and that all of the model components, barring the PSS, are relevant for simulation of PM as well.

Page 2, 2-4: The model seems not to be applicable to a range of policy applications in local air quality, but rather to more specific policy applications involving NOx. Please rewrite this.

Thank you for this recommendation. We have altered the text to express this more specific application.

Main body

Page 2, 2: Replace "...assess of trans-boundary..." to ...assess transboundary...

We have correct this error, thank you.

Page 3, 8-15. References to the model EPISODE is missing here. The model has been applied (but may be not documented in refereed journals) for quite some time. For example gives Slørdal et al. 2003 a quite thorough technical description of the model. Please add references.

We have now added this reference. Previously it was cited later in the paper but not included in the reference list.

Page 3, 23-26. It is rather unclear what the authors mean with micro-scale modeling, it is not necessary to run a LES-model in order to model on the micro-scale. Please define micro-scale properly, or remove.

We have modified the text here in order to explain micro-scale and give examples of these methods.

Page 4, 20. Sentence "Episode consist of ..." repetition of what is said in the introduction, please revise and make the paper more concise (see also general comment above).

We thank the reviewer for this recommendation and have acted upon it along with other changes to reduce redundancies. Specifically, we have moved details presented in the introduction into section 2.

Page 4, 29. Explain acronyms NWP, AROME, WRF

We have now explained these acronyms. Please note that NWP was defined already on page 3.

Page 5, 1-2, 10-11, 19-20. Examples on unnecessary repetition. Page 5, 7. The sentence "We also .." appears as an unnecessary statement.

We have now addressed these examples and also carried out a more extensive revision of the document to remove redundancies.

Page 6, 20-21. How is convection solved by bulk transport? Please explain or give a reference to how this is parameterized.

We provide an example for the case using AROME meteorology. At the 1 x 1 km scale of the AROME meteorological simulations it is possible to resolve individual deep convective on the bulk Eulerian grid. We have now included a reference to support this. Shallow convection is represented within

AROME using a parameterization, and we have now included a reference to this scheme. Thus, the wind fields provided by AROME already include vertical motions due to convection.

Page 6, 26. ": : :. very low artificial numerical diffusion...". How low? For very steep gradients numerical diffusion should be expected from any Eulerian scheme. Please discuss this issue in more detail and explain how it may affect the simulations close to large sources.

Consistent with the original Bott 1989 paper, we have now changed "very low" to small and we have now given a quantitative explanation of small, i.e., <1% with reference to Bott 1989. We have noted that numerical diffusion will occur with very steep concentration gradients, e.g., close to large sources.

Page 7, 14-15. What about the bulk vertical convection, is this also solved by use of the upstream scheme? Please explain.

It would be more precise to say that the upstream scheme does not solve convection but solves the vertical motions of tracers based on the three-dimensional wind fields, which includes both shallow and deep convection. As noted earlier, both convection processes are calculated within AROME and the resulting meteorological wind fields therefore include these motions. The upstream method implicitly assumes that there is no net divergence or convergence within the three-dimensional field, and it is therefore used to ensure full consistency and mass conservation during an EPISODE simulation. However, the parameterizations and treatments of advection within AROME should produce wind fields with no net convergence or divergence and that conserve mass and momentum.

Page 7, 20-21. Please explain better what is meant with " ...dependence on spatial structure of the flow field ...".

To simplify this and make it clearer we now state "depends on the properties of the flow field".

Page 7, 26. Smith, 1985, is not found in reference list?

We have now added this to the reference list.

Page 7, 32. ": : :.K-theory..." should be " : : : Monin-Obukhov similarity theory: : :..

We thank the reviewer for this recommendation and have changed the text accordingly.

Page 8, 1-4. Is the vertical profile of K prescribed? K(chem-comp) = K(heat) which I would expect to be found from the meteorological data based on what is previously said in the paper? The descriptions and assumptions in this section needs to be made clearer.

The original wording in the manuscript was not very precise. Prescribed is a poor choice of wording, and we now make it clear that Kz is reconstructed indirectly from the input meteorology via estimation of the Monin-Obukhov length, L and the friction velocity, u*. We do indeed assume that K(chem-comp) is equal to K(heat).

Page 8, 26. It is said "The new urban : : :..", please explain better what is new compared to the description in Slørdal et al. (2003). Also since this is a new parameterization, refererence to a previous validation or a comparison of the new method to local turbulence observations are missing. Please include.

We are planning to carry out a comprehensive and focused evaluation of the new urban Kz in a dedicated separate study in the near future. This is dependent on obtaining suitable observations, which we plan on gathering at the earliest opportunity. We have now explained this planned future work in section 6.

We regret that it is not possible to provide an evaluation of each feature of EPISODE presented here but we have had to make compromises in the choices of what to present.

Page 9, section "Area Gridded Emission". This sections has unnecessary many details, for example the units of the emissions, ASCII format etc., details rather to be entered in user manuals or an appendix.

While we agree with the overall aim of reducing the length of the paper and improving its readability, one of the reviewers has asked for more detailed information in the section on line sources. We have therefore made a compromise and moved all of the details relevant to the emission input files to a new appendix rather than leaving this information to a manual.

Page 10, 23-25. How large fraction of the emissions are assumed to be NO2? This is not clearly stated. Diesel engines could have as much as 10-20 % direct emissions of NO2, so if all emissions are NO it should be argued why.

This information is already expressed in Table 5, but we have now tried to make this clearer in the text at the point referred to.

Page 15, Section 2.3. There are lots of details in this section that should be put elsewhere or excluded to improve the readability of the text.

We thank the reviewer for this recommendation and have now removed many of the details here to improve the readability.

Page 16, section 3. The importance of the paper would have been larger if PM2.5 and PM10 had been included in the case studies.

We agree. However, as we explain the future work section, at the time of this work there were too many strong limitations on the processes (both emission and loss) governing PM. We prefer to address these concerns in future work.

Page 18. Section 4.1.1. These section also have several unnecessary repetitions and statements, partly "essay style". Please make the text more concise. Just as an example, first sentence of line 15 is clearly unnecessary.

We thank the reviewer for this recommendation and have now improved the text to improve the readability.

Page 20, 29. Units of RMSE?

We have now corrected this.

Page 22. A discussion of the uncertainties in wintertime NOx emissions from cold engines, and the uncertainties this may imply in the model results, are missing.

The NOx traffic emissions do not consider cold start discussion, so we have now added a short discussion on this specifically with regard to the low biases identified in Oslo.

Page 25, section 4.2.1 and 4.2.2. A quantitative comparison with local meteorological data (both model data used in the EPISODE model and local measurements) must be given and may shed light on what is happening in these two cases. Please include.

We have now added a quantitative comparison of the local meteorology at Drammen (Berskog) and Oslo (Blindern) on both events to provide additional context within a new section in the supplement (S8). This evaluation includes comparisons between the observed and simulated wind and temperature data at the two selected observation stations. We have also added a short discussion on these comparisons within 4.2.1. and 4.2.2.

Figures and Tables

Figures 3-6 are hard to read and must be improved. Geographical information must be added and the different concentration classes on the maps must be made clearer. The same applies to Figures 16 and 18, although the concentration levels are more clearly seen in these figures. Also, for the time-series, avoid legends overlaying the curves. Apart from this the Figures and Tables are satisfactory.

We have now remade the mapping figures to improve the colour scale and the geographical information presented (labels are now included for roads and important geographical features). The colour scale now shows the different concentration classes more clearly. In addition, we have now highlighted exceedances above the 40 ug m-3 annual mean limit value.