

***Interactive comment on* “The urban dispersion model EPISODE. Part 1: A Eulerian and subgrid-scale air quality model and its application in Nordic winter conditions” by Paul D. Hamer et al.**

Anonymous Referee #3

Received and published: 29 January 2020

General comments:

The manuscript documents the comprehensive and “much applied” model system EPISODE at NILU. As a model development description paper it merits publications and the presentation of results and the statistical validation is interesting and relevant. However some revisions are needed in which the main issues are:

- 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

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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.

- 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.

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.)

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.

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

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.

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.

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).

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

Page 5, 1-2, 10-11, 19-20. Examples on unnecessary repetition.

Page 5, 7. The sentence “We also ..” appears as an unnecessary statement.

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

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.

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

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

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

Page 7, 32. “. . .K-theory...” should be “. . . Monin-Obukhov similarity theory. . . .

Page 8, 1-4. Is the vertical profile of K prescribed? $K(\text{chem-comp}) = K(\text{heat})$ 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.

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,

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reference to a previous validation or a comparison of the new method to local turbulence observations are missing. Please include.

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.

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.

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.

Page 16, section 3. The importance of the paper would have been larger if PM_{2.5} and PM₁₀ had been included in the case studies.

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.

Page 20, 29. Units of RMSE?

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

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.

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

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seen in these figures. Also, for the time-series, avoid legends overlaying the curves.
Apart from this the Figures and Tables are satisfactory.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2019-199>, 2019.

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