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Geoscientific Model Development Discussions

Interactive comment on "Description of the uEMEP_v5 downscaling approach for the EMEP MSC-W chemistry transport model" *by* Bruce Rolstad Denby et al.

Anonymous Referee #1

Received and published: 22 July 2020

General comments:

This paper presents a novel methodology to downscale a chemical transport model (CTM) result to local scales (i.e., tens of meter resolution) over large geographical domains without double counting of emissions. A Gaussian dispersion kernel is implemented in the uEMEP model to estimate the local contribution of emission sources at fine resolutions while still considering the contribution of the EMEP CTM. The new capability of the EMEP model to quantify the local and non-local contribution of neighbouring emission sources to air pollutant concentrations in a specific grid cell of the model is used. A refined combination of the non-local contribution of the EMEP con-

centrations with the result of the Gaussian model within uEMEP provides significant improvements in places near emission sources like traffic sites. The new uEMEP provides a flexible treatment to refine the results of the CTM as desired over large domains, with still some limitations in street-canyon environments. The manuscript describes in detail the downscaling methodology and the Gaussian model implemented in uEMEP. Some parts of Section 2 describing the coupling of the models are difficult to follow and some clarification would help the reader. The work is well presented and should be accepted for publication in Geoscientific Model Development after minor revisions.

Specific comments:

- Line 46: the Authors could mention the OSPM model as an example of a street-canyon model to complement the overview of local scale models.

- Line 95: clarify if this option implies that emissions in the EMEP grid cell are not consistent with the ones in the sub-grid cells.

- Line 101: some discussion on the implications of using such inconsistent chemistry treatments in uEMEP and EMEP would be appreciated. As uEMEP is intended for applications over wide regions with significantly different chemical regimes, the simple chemistry may perform better in some environments than others.

- Line 116 and 140: the term Csg_nonlocal(i,j) is the more complex to understand. Perhaps, an equation describing how is computed would help the reader. I appreciate the effort of the Authors to explain the method with Figure 1 and 2 and Section 2.3, but it is still confusing how the local and non-local contributions of the EMEP grids are used in the computation of the Csg_nonlocal term.

- Line 147: More details on Wind et al. (2020) methodology would be appreciated in the manuscript. Considering that the local fraction estimate links emissions with concentrations, the Authors could clarify how the chemistry is treated once the tagged emissions are dispersed in the EMEP grid cells. Are tagged primary pollutants emitted as inert tracers or limited chemistry is considered? The details are provided in Wind et al. (2020), but the reader would appreciate some further descriptions of the method and limitations in the present manuscript.

- Line 152: Provide which fraction of the total contribution is missed in the local fraction estimate when using few EMEP grid cells.

- Line178 Eq. 6: Why this is not divided by the sum of the weights? Following the example in Fig. 1, you use more than 9 EMEP grid cells (adding their concentration) to obtain the local contribution of the moving window over the i,j sub-grid cell. This results with a local contribution overestimated somehow if nmw>1. For the case nmw=1, the expression seems good as the sum of the weights would be 1.

- Line 233 Eq. 10: Why Cg(i,j,s) is divided by nsource if it is already the concentration of a specific source?

- Line 295: I suggest introducing in this section the meandering and traffic term described in the supplementary material. Some variables in the equations are not defined just before or after presenting the equation. It would help the reader to introduce all the terms after the equations and specify which ones will be further described in subsequent sections.

- Line 330: Mention the floor value of the wind speed imposed in the model in this part of the manuscript. Some details are only presented in the supplementary material.

- Line 414: a table with the sigma_init_y values per emission source would be appreciated.

- Line 518: an order of magnitude of the maximum distance allowed in the dispersion of the Gaussian model would be appreciated (i.e., 250 m).

- Line 583: Is ozone also a product used from uEMEP? Is there any evaluation done for this pollutant?

- Line 631: Some discussion about the improvement in the daily cycle of the uEMEP results compared with EMEP would be appreciated. Local models use to improve the traffic peaks but also may inherit issues with the temporal profiles and the boundary layer evolution. The validation section could be improved introducing some discrimination between types of sites (rural, industrial, suburban, urban). I suggest presenting all the material of subsections 5.1.1, 5.1.2 and 5.1.3 under section 5.1 as those sections consist only in a single paragraph.

- Line 653: What missing processes could explain the remaining bias during the summer period in both PM10 and PM2.5?

- Line 700: it is counter-intuitive having more non-local EMEP contributions with smaller moving windows. Could the Authors clarify this in the text? If less EMEP grid cells are used in the moving window, less non-local contributions would be expected.

- Line 796: There are still some street-canyon processes that uEMEP cannot represent, particularly in compact cities with high street aspect ratios. The Authors should mention this in this last concluding remark.

Technical comments:

- Line 29: the acronym CTM is used several times in the manuscript but defined in Line 71. Please, define the acronym already in the introduction and use directly the acronym in the rest of the manuscript.

- Line 51: use coma instead of a semi-colon in the reference

- Line 58: the reference Wind et al. (2020) is not provided in the reference section.

- Line 154: fix the Section number. Here and in other parts of the manuscript, the number of the reference to specific sections is 0.

- Line 245: Use Eq. instead of Equ. in the Figure caption.

- Line 362: Monin–Obukhov is mistyped in different parts of the manuscript.

- Line 362: the Monin-Obukhov length and the surface roughness have already been used before in the manuscript. Define them there only once.

- Line 371 Table1: please, use consistent notation for the boundary layer height and Monin-Obukhov length. Both have been introduced before as H and L.

- Line 407 and 574: fix the section number that appears in the reference Sect. 0.

- Line 646: the statistics presented in panel (a) should be introduced in the caption specifying for which model are computed. In panel (b), the Authors could remove the shipping and industry labels in the legend as no information is shown in the figure.

- Line 661: There is too much information in Figure 11. I suggest presenting the non-local contribution of EMEP and not the detailed composition of it. Though of interest, it is impossible to appreciate EMEP4NO line and some artefacts appear as the white contribution above EMEP PRIMARY blue fraction.

- Line 679: avoid using subsections that consist of a single paragraph.

- Line 719: to be consistent with the supplementary material the coefficient of determination of the station mean time series of uEMEP should be 0.79, not 0.80. Harmonise the number in both documents.

- Line 728: I suggest merging Sections 6 and 7.

Comments Supplementary material:

- Line 13: Use section S1 instead of S3 and number accordingly the rest.

- Line 106: It should be Eq. (15a).

- Line 242: Why the inverse of the wind speed is used instead of wind speed?

- Line 314: In the figure caption, it should be Fig. S4 instead of S2.

- Line 328: The observation measurement could be provided in Fig. S6.

- Line 368: Why Figure S8a is different from Figure 10b? The caption describes the same results.

- Line 385: A value of 0.1 would likely provide an even closer fit to observations.

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