[GMDD]

Interactive

comment

Interactive comment on "A sub-grid model for improving the spatial resolution of air quality modelling at a European scale" *by* Mark R. Theobald et al.

Anonymous Referee #2

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1   General Comments

This is an interesting paper describing a downscaling approach to model sub-grid variability of NO2 and NH3 concentrations at a resolution of 1km, starting from a regional scale CTM with 50km grid size. This is undoubtedly an important topic, as air pollution impact indicators such as population exposure to NO2 or PM show strong local variations with source strength, so that a grid size of 50km is clearly too coarse for impact assessment.

Although the paper is well written, I have a few comments that in my opinion need to be addressed before publication. I have doubts about the use of non-local meteorological

data that are inconsistent with the coarse CTM, and about the missing chemistry in the downscaling scheme when addressing reactive gases like NO2. Somehow there is an inherent mismatch between the aim to "resolve the large horizontal gradients found close to sources of relatively short-lived pollutants" (p.2 l 18) and the use of an approach that explicitly does not include photochemistry, formation of secondary aerosol or deposition processes. This method would be perfectly fine if the focus was on chemically relatively inert species like PM (particularly primary) or NOx. The methodology needs to be better justified there (if not improved) and the potential implications discussed.

2 Specific comments

P2 l20. One issue that is not addressed here nor anywhere else in the manuscript is that (to my understanding) the lowest vertical layer of the EMEP model extends from the surface to about 90 meters. In this context, it would be important to explain what is meant by 'the mean atmospheric concentration in each grid square': Is this the estimated surface concentration calculated by applying some standard vertical distribution? But if so, is it then justified to assume that the modelled mean concentration on the 50km grid is 'correct' and just needs to be re-distributed spatially within the grid cell?

P4 l29ff. I find it hard to understand why meteorological data from just one station was used here, instead of grid-specific meteorology consistent with the fields driving the CTM. Even the two "domain-specific" meteorological data sets come only from one station each. I think this needs to be better justified and compared to the effects a grid specific met data set would have.

P5 l16. I agree absolutely. I would not expect a 1x1km grid cell to be representative of a hotspot traffic station such as the high-concentration cases in Scotland. I don't see

how these could be resolved without a proper street canyon increment calculation. In this light, some of the discussion of the model performance later on (p9 l3 ff) could be a bit rephrased.

p.5l 27 – whole Section 3: I would find it helpful if the downscaling equations were written out as equations and not only described verbally. I do appreciate the process flowchart in the supplementary material which is very helpful in understanding the procedure and should definitely be kept, but I believe also a formal treatment could help here.

p.6 l1. I have difficulties understanding why NO2 photochemistry is not needed here. By using the NOx fine-scale emission pattern and distribution kernel for redistributing NO2 concentrations this approach assumes a constant NO2/NOx concentration ratio in the whole EMEP 50km grid cell, which is hard for me to believe. Typically this ratio should show a dip close to NOx sources due to the high NO ratio in primary emissions. Also this fraction of primary NO2 in NOx emissions varies strongly between different sources and this should play a role at the local scale. I thought that at least ADMS includes a NOx photochemistry formulation, why is this not used? Ideally such a chemistry scheme should be used, or at least the potential errors discussed. Along similar lines, also the formation of NH4 can influence local NH3 concentrations - it would be interesting to know what the potential errors are when these processes are ignored.

p6 l19. This is a part of the model formulation I have not understood. Why is it justified to assume rotationally symmetric dispersion kernels, should not the local predominant wind direction have a significant influence? How big is the error introduced by this rotationally symmetric formulation?

P6 l24 Why moving window? Is the final subgrid distribution pattern at a given 1km grid cell not simply made up from the sum of all emissions times kernel within the range of the kernel? Perhaps a better formulation could be to say that the kernels are not cut

off at the EMEP grid boundaries (I hope). Again, formulating this with equations would help here.

P7 l16. Does this mean the downscaling decreased agreement to NO2 non-traffic stations, which are the ones we would actually expect it to improve? As mentioned above, I would not expect the 1x1km model to be representative of traffic stations.

P10 l17. In case of PM10 to my knowledge the limit on daily means (35 exceedances of 50ug/m3) is the more stringent one.

P10 l22. Since the authors mention this as a possibility I would suggest that they add a sensitivity analysis using grid-specific meteorology consistent with the CTM.

P10 l31. ...for other pollutants for which there is a strong relationship between emissions and concentrations (i.e. which have very localized sources) and – in my opinion – which do not undergo fast chemical transformation. Therefore I agree in the case of PM2.5, especially primary PM, but I have doubts for NO, SO4 and O3.

3   Technical corrections

p6 l24-26. I wonder whether the quotes are really necessary here.

P11 l2. SO_2^{4-} should be SO_4^{2-}?

Figure 4: I would suggest to split up the two regions into separate figures, since the model performance is so different.

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