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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Nudging technique for scale bridging in air quality/climate atmospheric composition modelling" *by* A. Maurizi et al.

Anonymous Referee #2

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Using high resolution simulation results to improve results obtained at low resolution is a highly important subject, and therefore the literature contains a variety of suggestions on how this could be materialised in an effective manner. In particular, nudging of concentration fields as a method for dynamical upscaling from local/city geometries to the regional-to-global scale has an enormous potential for studies aiming to quantify the impact of urban plumes on climate modelling or similar applications.

The present paper deals with a statistical assessment of "synthetic" calculations with and without nudging, where only results of a single model (BOLCHEM) are being used. The results shown are interesting, as they provide a quantitative estimate on how nudging can modify the concentration fields at the regional scale. As stated by the authors themselves, it would have been possible to apply their concept to two different models,

an approach very frequently followed in practical applications. Undoubtedly, the restriction to a single model is a serious weakness of the paper, and the same is true also for the authors' decision not to apply the nudging technique to the meteorological fields as well: This reviewer believes that the latter could prove more decisive for improving low resolution air quality simulation results compared to the application of nudging to the concentration fields themselves.

In spite of the above deficiencies, and although the manuscript in hand also lacks any comparison with observations, the paper contains a number of innovative elements and should be accepted for publication after revision for adhering to the comments listed below. In addition, the authors should remove the few typing and language errors in the manuscript.

Comments

1. The scientific value of the paper could be considerably enhanced if comparisons with other model results would be added. This would allow better assessing the potential accuracy improvement achieved through nudging. As a matter of fact, the Po Valley has been the subject of model application by several research teams, so it should not be a problem for the authors to find appropriate results for comparison purposes. Most probably, this extra work would make it possible for them to compare their findings, at least to some extent, also with observational evidence.

2. Given the purely statistical character of the paper, the authors should thoroughly analyse how their results depend on the relaxation time and the frequency of updating c(sub[HR-r]), i.e. the concentration values derived from the high resolution run and averaged over the low resolution grid.

3. In view of their own comment in this respect, the authors seem being aware of the fact that the use of the same model for both scales is a weakness of their work: This approach simply suppresses questions related to interpolation and scale consistency in the vertical direction which can be expected to arise in any practical application

involving coupling of different models. They should therefore at least discuss in some detail these issues in their manuscript.

4. Apparently, nudging is implemented by the authors individually, i.e. in a cell-by-cell manner. As an alternative, the authors could have also applied, for example, Gaussian spreading, a well-established method extending the impact also on neighbouring cells, the intention being to avoid numerical artefacts. They should justify their approach and discuss in some detail the pros and cons of the cell-by-cell nudging implementation against Gaussian spreading or other similar procedures described in the literature.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 17177, 2011.