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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatial and temporal variability of clouds and precipitation over Germany: multiscale simulations across the "gray zone"" *by* C. Barthlott and C. Hoose

Anonymous Referee #2

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In their manuscript Barthlott and Hoose aim to present a study on the dependence of precipitation and convection patterns on the grid scaling of the COSMO model. The main messages of the publication are that convective processes are increasingly better resolved with increasing resolution. This affects the formation of precipitation, even though it was not found to actually improve the quantitative precipitation forecast (except for some indication in the summertime case).

The paper is mainly of qualitative nature. The discussion and presentation of the results are done adequately. Actual quantitative information or a clear definition of pro's and con's of increasing/changing the horizontal resolution are however not given which

reduces somewhat the scientific value of the publication. I consider the scope of the paper to be a well readable documentation of scaling effects on precipitation processes which is in a rather publicable state.

Two major issues however still remain. Since clouds are not treated in the manuscript, the title and content of the manuscript need to be modified. If the title cannot be changed, the paper needs to be withdrawn and resubmitted or the manuscript needs to be extended. Taking into account the second comment would improve the scientific value of the manuscript considerably. See below for details.

Major comments:

1) The term 'clouds' in the manuscript title is misleading because no information on the presence of clouds is given at all. This also affects the content of the paper (p17136 lines 1 and 19; p 17144, line 4). Consequently, if the authors want to stick to the presence of the term 'clouds' in the title, they actually are required to present information on cloud properties. There are probably quite some indices available to do so, but validation will be more difficult because measurements of the vertical structure of clouds are rare. Even though they are available with rather high density within the HOPE domain (from cloudnet).

2) The reason why I consider the paper to be (just) "a well readable documentation of scaling effects on precipitation processes" is the missing of quantitative information on reasons for the observed differences. Precipitation produced by a model is the result of a large number of model processes and parametrizations that interact with each other. By looking just on the precipitation and the convection fields, one cannot resolve the processes taking place in between – which are likely the reason for resulting differences. It is well known that the resolution of a model cannot be increased without checking for the validity of involved parametrizations. Keyword here is the 'effective resolution' of a model. Below that effective resolution turbulent/kinetic processes are not resolved accurately (Skamarock, 2004 (Section 3), Petrik 2012). How is this problem

dealt with within the present study? There should be a section added to the manuscript to discuss the discrepancy between actual model resolution and effective model resolution. Is it possible to show the energy spectrum for the different resolutions? If there is a dependence of the spectra on the model resolution, how will this effect the forecast efficiency? E.g., there may be increasing turbulence with increasing model resolution. But does the energy spectrum show the expected 5/3 decay?

Minor comments:

1) I would stay with either the term 'terra incognita' or 'gray zone.' Don't switch between both in the text. 'gray zone' is currently used more often than 'terra incognita'. Thus staying with gray zone would ease the readability. Introduce 'terra incognita' only in the introduction.

2) Section 3.1 only deals with the vertical velocity/convection. The section title can be adjusted accordingly.

3) Acknowledgements: Shouldn't HOPE/HDCP2 be acknowledged, too?

References:

William C. Skamarock, 2004: Evaluating Mesoscale NWP Models Using Kinetic Energy Spectra. Mon. Wea. Rev., 132, 3019–3032. doi: http://dx.doi.org/10.1175/MWR2830.1

Ronny Petrik, 2012 : Physical validation and bracket-based dynamical cores for mesoscale NWP models. Phd thesis in "Berichte zur Erdsystemforschung", 121, ISSN 1614-1199. Available at http://www.mpimet.mpg.de/wissenschaft/publikationen/berichte-erdsystemforschung.html

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