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Geoscientific Model Development Discussions

Interactive comment on "Reaching the lower stratosphere: validating an extended vertical grid for COSMO" *by* J. Eckstein et al.

Anonymous Referee #2

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Review of "Reaching the lower stratosphere: validating an extended vertical grid for COSMO" by J. Eckstein, S. Schmitz. and R. Ruhnke

The authors present simulations with the COSMO model over an area covering Europe and parts of the polar region driven by two different reanalysis sets: the ERA-Interim and the NCEP reanalysis. The main focus of this study lies in the comparison between a nearly one year run and observations of temperature and relative humidity by radiosondes in the upper troposphere and the lower stratosphere. To this purpose the authors run the COSMO model with an upper boundary lying in 33km height.

Major remarks

A main remark concerns the title of the paper and parts of the abstract. They sug-

gest that the reader learns anything about constructing well suited vertical grids. One motivation of writing the paper was obviously to establish a new grid for the COSMO model reaching somewhat higher than the applications used e.g. operationally at the Deutscher Wetterdienst (DWD). I think that it should be better emphasized by the authors that the mentioned vertical grid structure of COSMO (or better say of the interpolation program INT2LM) is only one example among others available and that other vertical grid choices can quite easily be defined e.g. via namelists in the INT2LM input. Also DWD uses two different vertical grids for its two model applications COSMO-DE and COSMO-EU. Therefore, it is a bit misleading to talk about 'the standard' vertical grid. What I expected instead from the title and abstract are some statements about what should be the basic principles in constructing a vertical level choice. Are there situations found, where a reasonably looking choice of a grid produces problems (instabilities) in COSMO whereas another choice does not? However, I couldn't find anything about this in the paper. The exact values for the vcoord-values are given in table A2, but I think that is not what one needs to know very urgently. One could even omit this table in favor of figure 1 (the latter eventually using a logarithmic z-axis). By the way, where do the values of vcoord come from? What is the guiding principle in using these values?

The author's correctly mention other studies reaching to higher values of the model top height in the tropics and motivate this study with a high model top in the polar region. I think the reader should be informed by which reasons it is or could be problematic for a model to simulate with model heights of 33 km in the polar region.

On the other hand, other very important values of the model setup are mentioned but not so well explained/motivated. E.g. the beginning of a damping layer in 28km height (p- 486, line 7) in a model which uses a model top of 33 km at least needs some explanation. It is often assumed that the damping layer should be deeper (let's say one third of the model depth). At p. 486, line 9 it is stated that the new vertical grid is 'better' than the old one. I see that the additional 10 levels are put on top of the 'standard' grid (i.e. they lie between z=22km and z=33km). Consequently there are only the same number of levels available for z=0..22 km. Obviously, the boundary layer has lower resolution than the 'standard' grid. So again, what means 'better'?

I have also serious doubts about the beginning of flat levels in this grid. p. 489, l. 20 mentions a value of only vcflat=7 km. Since the model area contains also the Alps e.g. the Mont Blanc with roughly 4.8km height is contained, too. Though, the 'model Mont Blanc' has a lower altitude, I assume that this nevertheless produces a strong compression of the model levels at these grid points. Obviously linked to that on p. 489, l. 21-22.: why does the analysis of this study depend on the flatness of model levels? One could always interpolate from terrain following coordinates to other surfaces. Convenience of evaluation should not be the reason for dubious model setups.

Another aspect is the time step mentioned on p. 488, line 15. For a model with a resolution 0.2°, i.e. roughly 22 km, I would expect a time step for COSMO around 200 sec. In contrast, the authors use only 60 sec. which means a 3 times larger computation time than possibly needed. Normally, I would mention such things as 'minor remarks'. However, in a paper apparently focusing on the grid choice the question arises, if there are any instabilities potentially arising with different grid choices. Moreover, the total run time seemed to be a serious limitation for the whole study (p. 488, l. 23-26). Therefore, this should be better inspected and documented.

(Possibly, one big danger in choosing a misleading title and abstract is, that a paper is sent to a reviewer which has a different background and does not fully appreciate the true strengths of the paper.)

There are quite other aspects in the paper, which I find interesting: the detection of quite heavy relative humidity biases in the Russian radiosondes is an important result and important to know both for researchers and for operational data assimilation! It is mainly this point for which I find the paper worth for publishing after some major

revisions.

Minor remarks:

introduction or section 2: the reader should get a rough impression about the kind of model simulations. I guess from the remarks at the end of section 2.4 that it was done some kind of a climate run over 11 months, in contrast of performing several forecast runs. It is further a kind of climate hindcast but without assimilating data, isn't it?

p. 488, l. 6: there are many national weather services using the COSMO model for operational forecasts. In particular the Brazilian Navy uses COSMO for operational forecasts of a large part of the Antarctic region. Therefore, this statement is not entirely correct.

When comparing Fig. 4 and Fig. 6. I have the impression that at least one of both plots does not have a linear time axis. The onset of the sudden polar warming (at Jan Mayen) visually seems to set in earlier in Fig 6 than in Fig 4. Also when I try to determine the date of the sudden warming by the tic marks I end up with the beginning of April in Figure 6 whereas with mid of April in Figure 4.

p. 486 , l 4 it is 11357 m, not km

References: for a standard description of COSMO-DE often the paper by M. Baldauf, A. Seifert, J. Förstner, D. Majewski, M. Raschendorfer, T. Reinhardt (2011): Operational Convective-Scale Numerical Weather Prediction with the COSMO Model: Description and Sensitivities, Mon. Wea. Rev., 139, 3887-3905 is cited.

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