Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-195-RC2, 2016

Geoscientific Model Development Discussions
Open Access
EGU

Interactive comment on "Continuous high resolution mid-latitude belt simulations for July–August 2013 with WRF" *by* Thomas Schwitalla et al.

Anonymous Referee #2

Received and published: 25 November 2016

The authors investigate the benefits of convection permitting modeling by employing the WRF modeling system in a channel configuration over the Northern Hemisphere (between 20° N and 65° N) at resolutions of 0.12° (LOWRES) and 0.03° (HIRES), respectively. In HIRES the deep convection parameterization scheme is turned off. The necessarily short integration period covers the summer of 2013 (July and August). This period was notable for exhibiting a strongly positive phase of the North Atlantic Oscillation and generally weaker subtropical highs over the Atlantic and Pacific basins. The driving data is from the ECMWF operational analysis data at the northern and southern boundaries and the OSTIA 5km SST data set at the sea surface. The authors then compare their results to reanalysis and gridded combined data products (e.g., E-OBS,

Printer-friendly version

Discussion paper

CMORPH). They aim to answer the three questions: 1) What is the benefit of a CP resolution with respect to the spatial representation of large-scale features in comparison to coarse resolution? 2) Does the higher resolution lead to an improvement of surface variables such as 10m windspeed and 2m temperatures? 3) What is the benefit of the CP resolution with respect to the spatial distribution and amount of precipitation?

Given large channel domain and the fact that convective permitting simulations are still relatively rare this study can potentially make a useful contribution to our understanding of how and why simulations at these grid spacings are useful. However, the experiment is not designed in such a way that it can answer the questions as they are posed. The shortcomings are detailed below as are suggests for improvement. I will focus on what I see as the two major issues that must be addressed before the manuscript can move forward. There are likely more specific comments but these can be addressed in the next revision.

General comments

Due to the channel set up the model simulations are largely "free". In other words internal variability can account for much of the difference that we see between the simulations and the reference data sets. In fact there is little reason to assume that they would in anyway resemble each other. In the absence of nudging the one-to-one comparison of the model simulations with the reference fields for the large-scale circulation is doomed to fail. This is illustrated most clearly through examination of the Figures 4 and 5. The dominant anomalies in the large-scale circulation for 2013 are a weakening of the subtropical highs over the ocean basins and a strengthening of the low-pressure anomalies over the Eurasia. The so-called model biases wipe out this weakening over the subtropical highs and intensify the low-pressure anomalies over the Eurasia. From there the rest of the comparisons are uninformative at best. Therefore, I would suggest the authors focus on whether this type of simulation is fit for purpose. In other words, can the model perform the task for which it is intended and does the HIRES simulation perform this task better, or more accurately than the LOWRES simulation? One way the authors could do this would be to show, via hatching for example, areas where the modeled field falls outside the +/- 2 standard deviation confidence bounds of the observations. Given that these simulations are basically single realizations of internal variability, weakly constrained by the lower and north/south boundaries this is a more fair and appropriate comparison. Another solution would be to re-run the experiments, but constrain the flow so that expectation could be that the model, in the absence of internal model errors, would reproduce the temporal evolution of the weather over the course of July and August 2013.

The other issue relates to added value. Given the issues described above and the face that this is a single model, case study experiment, it is very, very, difficult to convincingly argue for added value. Rather than focus on added value using such measure as Taylor diagrams and RMSE, the authors could perhaps focus more on processes that are more accurately captured in the HIRES simulation. Examples are diurnal cycles of winds and precipitation, blocking associated with heat waves, etc. The authors should not focus on spatial comparisons as there is little reason to expect high spatial correlation between the simulations and the reference data other than that due to the fact there are climatological patterns that the simulations will somewhat follow. If the authors are really set on showing added value then I would recommend they use something like the Perkins skill score which assesses the similarity of two pdfs (Perkins et al. 2007). This metric is quite a bit more informative than the approaches shown used in the manuscript, which rely heavily on visual inspection.

Perkins, S. E., Pitman, A. J., Holbrook, N. J., & McAneney, J. (2007). Evaluation of the AR4 climate models' simulated daily maximum temperature, minimum temperature, and precipitation over Australia using probability density functions. Journal of climate, 20(17), 4356-4376.

Presentation Quality

Some context for the study is lacking. Why is summer 2013 chosen? Some information on datasets and calculations is missing. The resolution of OSTIA is about 5km. This does not appear in the text. Some of the reference data sets are described in the experiment set up section. Some description only comes later in the results. It is a bit confusing. Better would be to describe all the reference data sets their strengths and weaknesses, resolution, etc. in a subsection of the experimental set up.

Specific comments The abstract is much too long and without critical insight. The abstract should not just be a laundry list of the results but a brief exposition of key findings. The reader should immediately grasp why this paper is of interest. The contribution this study is making should come through in the abstract.

Page 5 L3-20: The authors go on about how important soil moisture is but then choose not to spin up soil moisture? This is confusing if, as the authors claim, only 10-14 days are required for spin up. Given that there was a heat wave over Europe in 2013 having the correct soil moisture field would be critical to get the proper atmospheric circulation.

Page 8 L14: "low pressure" should be replaced with "negative bias"

Page 8 L19: How is the standard deviation calculated? On mean daily values? Something else? This lack of clarity on calculations appears in other areas of the manuscript as well.

Page 8 L26: Delete "significantly". Unless describing the result of a hypothesis test this term should not be used in such a context. There are other areas of the manuscript where this is used.

Figures

As stated in the general comments the figures could benefit from inclusion of confidence bounds from the reference data.

Figure 9 can be removed, as there is no reason to expect these simulations to match the temporal march of the reanalysis.

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-195, 2016.