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

Anonymous Referee #1

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The authors have driven the atmospheric model WRF with analysis fields of the operational forecasting system of the ECMWF to simulate the period July to August 2013 in a mid-latitudinal belt (20°N to 65°N) around the globe with two grid sizes: 0.12° and 0.03°. Both simulations make use of sea surface temperatures from the Operational Sea Surface Temperature and Sea Ice Analysis (OSTIA) data. In the 0.03° simulation, parameterisation for deep convection is turned off. In order to evaluate the belt simulations, they are compared to various reference datasets (analysis fields of the operational forecasting system of the ECMWF, EOB-S, CMORPH) and in several subdomains. In addition, the authors ask specific questions about the added value in the 0.03° simulation.

General Comments

The paper combines two relatively new innovations, convection permitting simulations and belt simulations. The strengths and weaknesses of both innovations are largely unknown and hence, the paper is worth to be published. However there are two major issues that need to be clarified first:

(1) Scientific quality

The solution of a local area model is partly predominated by its lateral boundary conditions (LBCs). The larger the model domain becomes, the weaker becomes the coupling to its LBCs and the larger become large-scale deviations from its driving data in the interior of the model. Kida et al. (1991) and Paegle et al. (1996) are often cited in this context. More recently, Becker et al. (2015) demonstrated that a local area model creates artificial flows to compensate those large-scale deviations in order to achieve physical consistency with the LBCs along the lateral boundaries and that an increase of the model domain does not change this – the artificial flows simply become more complex.

In the presented belt simulations, there are no western and eastern boundaries and hence, the decoupling becomes an important factor. This can be seen in principal in Fig. 9: the model creates significant anomalies in MSLP (low and high pressure systems are created that do not exist and vice versa) in the Atlantic region, but from time to time (e.g. around July 27 and August 10 to 15) the influence of the driving data becomes dominant. The fact that there is some coupling to the LBCs at all comes from the location of the sub-domain: the Atlantic region touches the northern boundary. In the interior of the model domain, the coupling might be much smaller.

Hence, the simulations may be affected by large-scale decoupling to such a large extent that the entire evaluation in its current stage is flawed. Shifts in time/space between modelled and observed phenomena are limiting the applicability of traditional statistical analysis. Biases and other error measures are showing the summary effect of large-scale decoupling and model deficiencies (which should be the only focus in a model evaluation study). A common approach to overcome this mismatch is to extend the simulation period to multiple decades and evaluate statistical measures in a climatological way (as it is done for climate models, for instance). However, in the face of high computational costs, this might not be feasible. For the purpose of model evaluation it would be enough to demonstrate that the simulations are lying within the bandwidth of possible realistic developments. The climatological year-to-year variability (on a monthly basis), which could be derived from ERA-Interim, or extended ensemble forecast data (e.g. http://www.ecmwf.int/en/forecasts/datasets/set-vi) – as the forecast model runs without ingestion of observation – could be used to define such space of possible developments.

Because of the large-scale decoupling that makes the simulations partly incomparable to observational or observation based data, the investigation of added value is flawed, too. In addition, the asked questions about added value are way too generally expressed. With one coarse and one fine resolved simulation of the same model, no robust conclusion on added value can be drawn. In such a case, the added value analysis is limited to this specific case. By the way, for demonstrating added value, it is not enough to show that biases on a monthly basis are reduced, because monthly biases are the result of multiple processes and phenomena that may take place at the same time and also in sequences. So, a reduction of a monthly bias can be the result of enlarged process and phenomena related biases that are simply cancelling out each other. Hence, demonstrating added value includes a thorough investigation of the underlying processes and phenomena plus a demonstration that these processes and phenomena are more properly captured by the finer resolved model. To solve this issue, the authors could either include such a thorough process and phenomena based analysis or should put more effort on the model evaluation and its problematic (see above) and do not announce added value in such a prominent way.

Since large-scale decoupling plays such an important role, it needs to be an integrative

part of the discussion (Section 5) and summary (Section 6) sections.

Becker N., U. Ulbrich, R. Klein (2015), Systematic large-scale secondary circulations in a regional climate model. Geophys. Res. Lett., 42, 4142–4149. doi: 10.1002/2015GL063955. Kida, H., T. Koide, H. Sasaki, and M. Chiba (1991), A new approach for coupling a limited area model to a GCM for regional climate simulations. J. Meteor. Soc. Japan, 69, 723–728. Paegle, J., K. C. Mo, and J. N. Paegle (1996), Dependence of simulated precipitation on surface evaporation during the 1993 United States summer floods. Mon. Wea. Rev., 124, 345–361.

(2) Presentation quality

Reference data, error measures, including an explanation why theses reference datasets and error measures are selected and how data from different grids is remapped onto a common evaluation grid is missing in the experimental setup (Section 2). Instead this information is (partly) given at other places, for instance at the beginning of the result section (Section 4). Having an evaluation concept in section 2 summarising all of this would increase the readability of the manuscript.

Specific Comments

The following study would nicely fit into the introduction section:

Prein, A. F., A. Gobiet, H. Truhetz, K. Keuler, K. Goergen, C. Teichmann, C. Fox Maule, E. van Meijgaard, M. Déqué, G. Nikulin, R. Vautard, A. Colette, E. Kjellström, and D. Jacob (2015), Precipitation in the EURO-CORDEX 0.11° and 0.44° simulations: High resolution, high benefits?, Climate Dynamics, 46, 383–412, 10.1007/s00382-015-2589-y

Page 3, lines 14, 15: It is unclear how errors in the large scale circulation patterns can be traced back to the applied physics schemes, especially in the light of large-scale decoupling.

Page 3, lines 22, 23: It is not clear how large uncertainties over the Atlantic and Pacific can be explained by differences in model physics, especially in the light of large-scale decoupling which is also active when model physics are identical (simply extending the model domain of a local area model by some grid cells into one direction gives different results; see Becker et al., 2015).

Page 5, line 14: What is the advantage of using OSTIA instead of SST from the operational ECMWF analyses? OSTIA is given on a daily resolution and need to be interpolated in time (see page 5 line 34 to page 6, line 3), while SST from ECMWF is already on a 6 h basis. I am not an SST expert, but a short literature research brought up a paper from Seo et al. (2014) which demonstrates the importance of sub-daily SST variability to properly capture the onset and intensity of Madden–Julian oscillation (MJO) convection in the Indian Ocean in a coupled WRF-ocean model. Seo, H., A. C. Subramanian, A. J. Miller, and N. R. Cavanaugh (2014), Coupled Impacts of the Diurnal Cycle of Sea Surface Temperature on the Madden–Julian Oscillation. J. Climate, 27, 8422–8443, doi: 10.1175/JCLI-D-14-00141.1.

Page 6, lines 11 to 17: the 0.03° and 0.12° simulations make use of pnetcdf. A discussion about pnetcdf is missing here. Does pnetcdf solve the problem?

Page 8, line 25: What is "good agreement"? What biases are acceptable? (These questions should be tackled in an evaluation concept in section 2.)

Page 11, lines 7 to 8: There must be something fundamentally going wrong with the model in this specific region. Maybe it is related to the initialisation of the soil. The authors are encouraged to contact WRF experts (e.g. Walter Immerzeel, University of Utrecht, or the CORDEX-South-Asia or CORDEX-Central-Asia communities) that are operating the model in this region. There is also a new reference dataset for temperature available. It is called WFDEI (Weedon et al., 2010; 2011) and can be downloaded from ftp://rfdata:forceDATA@ftp.iiasa.ac.at

Weedon, G.P., Gomes, S., Viterbo, P., Österle, H., Adam, J.C., Bellouin, N., Boucher, O., and Best, M., 2010. The WATCH Forcing Data 1958-2001: a meteorological forc-

ing dataset for land surface- and hydrological models. WATCH Tech. Rep. 22, 41p (available at www.eu-watch.org/publications).

Weedon, G.P., Gomes, S., Viterbo, P., Shuttleworth, W.J., Blyth, E., Österle, H., Adam, J.C., Bellouin, N, Boucher, O., and Best, M., 2011. Creation of the WATCH Forcing data and its use to assess global and regional reference crop evaporation over land during the twentieth century. J. Hydrometerol. 12, 823-848, doi: 10.1175/2011JHM1369.1.

This dataset is based on ERA-Interim, but it corrects temperatures in a way that it is consistent with the observed behaviours of glaciers in the Himalayan region. It might be a more reliable reference dataset than the ECMWF analysis fields.

Technical Corrections

Page 3, line 16: Is there a reference for the storm systems affecting Europe?

Page 3, line 33: typo – "This study is organised . . ."

Page 4, line 14: Euro-CORDEX should be referenced by Jacob et al. (2014).

Jacob, D., J. Petersen, B. Eggert, A. Alias, O. B. Christensen, L. M. Bouwer, A. Braun, A. Colette, M. Déqué, G. Georgievski, E. Georgopoulou, A. Gobiet, L. Menut, G. Nikulin, A. Haensler, N. Hempelmann, C. Jones, K. Keuler, S. Kovats, N. Kröner, S. Kotlarski, A. Kriegsmann, E. Martin, E. van Meijgaard, C. Moseley, S. Pfeifer, S. Preuschmann, C. Radermacher, K. Radtke, D. Rechid, M. Rounsevell, P. Samuelsson, S. Somot, J.-F. Soussana, C. Teichmann, R. Valentini, R. Vautard, B. Weber, and P. Yiou (2014), EURO-CORDEX: New high-resolution climate change projections for European impact research, Regional Environmental Change, 14, 563–578, 10.1007/s10113-013-0499-2.

Page 5, line 18: Are there any references for the studies that have shown the spin-up time for NOAH's land surface model?

Page 9, line 1 to 2: Is there a reference to the analysis on tropical storms of JMA?

Page 9, line 5: Is there a reference to this jet stream north of the Tibetan Plateau which is typical for the summer monsoon?

Page 9, line 10: up to now, it was not clear that RMSE is used at all. It is also not clear which RMSE is used (the RMSE of monthly means or on a daily basis or whatsoever)

Page 10, line 8: typo – “. . . due to small . . .”

Page 10, line 19: typo – “. . . caused by the inaccurate . . .”

Page 15, line 29 to 30: cumulus parameterisation is also changed in LOWRES. Is GRIMS also active in LOWRES?

Figure 2: colours of the shades are too intensive, continents can only hardly be seen. Also the structure of the plot should be consistent with Figure 4 (anomalies should be shaded and the climatology should be in solid lines).

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