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Interactive comment on "Impact analysis of dynamical downscaling on the treatment of convection in a regional NWP model – COSMO: a case study during the passage of a very severe cyclonic storm "OCKHI"" *by* S. Roshny et al.

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Discussion paper

Review of the paper ’Impact analysis of dynamical ... a case study during the passage of a very severe cyclonic storm ’OCKHI’

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1 General comments

The paper reviewed is about a very severe cyclonic storm in the Arabian Sea. In the framework of a downscaling experiment, the author investigates the impact of model resolution and convective parameterization on the results.

The English language in the text is IMO proper to achieve a good flow of reading and to get the context right. The structure is clear and the figures and tables are well done.

However, main issues appear with the text which call at least for a major review. If the author is not adequately tackling that issues, the scientific content will still be questionable (i.e. very likely a rejection of the content).

1.1 The authors intention - analysis of sensitivity for initial conditions

In the paper presented a sensitivity to lead times is done. However, to identify sensitivity for initial conditions, other forcing data have to be considered. In the case of the Arabian sea I would prefer ERA-analysis, ERA5-reanalysis, MERRA2 reanalysis or NCEP analysis as well as reanalysis. Thus, having three different types of analysis, the sensitivity study is much more convincing. One would incorporate the spread originating from the different physical parameterization schemes and the assimilation techniques.

1.2 The authors intention - analysis of sensitivity for parameterization of convection

From previous studies it is already clear that parameterization for deep convection can be switched-off for resolutions smaller than about 3-5 km. The interesting question is the 'about'. Therefore, I see no reason why to add the DPC experiment with 2.8 km resolution. However, the sensitivity analysis would get more meaningful if the author decides

- to deal with an experiment in the convective 'grey zone' and performs a simulation at 4 to 5 km with parameterization for convection switched-off and switched-on.
- to investigate the need for parameterization of shallow convection. That means to add a simulation at 2.8 km resolution deactivating the shallow convection (which is active in the standard configuration).

To clarify, the recent content of the paper is somehow '2.8 km resolution leads to more details in the CAPE and precipitation fields, compared to CPC experiment with 7 km resolution. The experiment DPC is unnecessary because the patterns are smoothed and the area-averaged precipitation is the same as for CPC. The CAPE values are off compared to ERA-Reanalysis and DNC, CPC.' However, addressing the research questions the author mentioned in the introduction, it is required to go beyond the experiments introduced in the recent version of the paper.

2 Evaluational basis

The evaluation of the results is superficial. First, ERA-reanalysis data are not helpful in measuring the quality of the high-resolution model. The author should consider satellite data from TRMM as remote sensing observations. In addition, the data from IMD are referred but at no time a quantitative comparison is provided to the reader. Without such a comparison, the author cannot raise arguments like 'the downscaling did not improve rainfall prediction' or 'the CAPE magnitudes obtained from ECMWF fields were always overestimated'.

The basis for evaluation could be more improved by incorporating radiosonde data or satellite data about the cloud structures. The ERA-reanalysis can be useful for qualitatively analyzing those meteorological parameters, which are more or less instantaneously assimilated, as the mean sea level pressure.

3 Robustness of the analysis

The author confines himself to the analysis of precipitation and CAPE. Much more meteorological parameters have to be evaluated to get a clue about the differences in the model results and the related performances. It would be very beneficial to study the vertical structure of the cyclone along the path or as a cross section, to visualize the path of the eye (distance to observed position) for all configs in one figure, to look at the cloud structures, the simulated vertical velocity and the vertical integrated cloud content as well as moisture-flux divergence (as a precursor for the convection parameterizations).

Furthermore, the idea of downscaling is to add some value to the forcing model, which is the ICON in your case. The author misses to analyze which of the configurations is superior over the forcing simulation. It is not fair and not useful to compare the high resolution simulations with a global reanalysis, which cannot hold as a reference for a 'global prediction' as well as an observational field. It is much too coarse compared to the models the author deals with.

In addition, I am asking myself why not to choose a model domain capable of resolving the initiation of the storm. I.e. the extension of the domain in Southern direction by 1 degree and in Eastern direction by 2 degrees captures the whole intensification stage of the storm. Doing so, one gets more independent from the global forcing regarding lateral conditions.

However, it is still a big challenge to extract some general scientific implications from a single case study for the scientific community. Thus, it would be worth to look at other comparable events which would extend the study in a reasonable manner and which would result in a more robust statistical and scientific basis.

4 Specific comments

4.1 Introduction

The introduction is well written and with a nice literature review. However, it is too general, i.e. a literature discussion about tropical storms is missing as well as the performance of models resolving them. Furthermore, I miss a section overview at the end.

page 1, line 17: 'to name a few' can be skipped

page 2, line 1: Start a new sentence

page 2, line 21: 'meteorological data' can be replace by data. The forcing data are much more than meteorological data (hydrological, ...)

page 2, line 31: Regarding the discussion of resolution needed to achieve a complete explicit representation of convection, the author should refer Bryan (2003) [Resolution Requirements for the Simulation of Deep Moist Convection].

page 2, line 34 - page 3, line 2: reading is lost due to large bracket text

4.2 COSMO model

IMO the section 'COSMO model' should be divided into '2.1. General description' and '2.2. Parameterization of Convection'

page 3, line 22: 'The equations are solved numerically on a Arakawa C-grid (Baldauf, 2011)' - this is all you need here. Everything else would be too complicated.

page 3, line 22: 'The temporal integration of the governing equation is done with' ...

page 3, line 23-24: Reformulate the sentence with the vertical layers. Please skip the number 50, because you are later on explaining the model configuration.

page 3, line 27-28: Please skip the sub-clause about diagnostic variables. This would be a list without end.

page 3, line 30-31: 'formation of precipitation fields' is a little bit too misleading. I would recommend to use 'The formation and modification of clouds and precipitating constituents'.

page 4, line 2-3: The sentence about Tiedtke can be skipped. The section 2.2. is discussing all details about moisture convection.

page 4, line 14-18: The sentence is too long.

page 5, line 14-18: The first two sentences should be shifted to section 3. The last sentence should be placed in section 2.1 (I suggested).

4.3 Methods and Data

This section should be rearranged. At first, a renaming to ’Methods’ and Data’ would be beneficial. Second, a good naming of section 3.1. is IMO ’Configuration of the Model simulations’. Third, the recent Section 4 should be Section 3.2. named ’Sensitivity experiments with NWP model’. Fourth, the recent section 3.2. should be Section 3.3. ’Observations’. Regarding the COSMO model, it is needed to explicitly tell the version number. Having this version number, the community exactly knows about bugfixes and the state of research with your model version.

page 5, line 21: You do not explain ’VSCS’. I think it is very service convective storm.

page 5, line 26+27: Two commas would be helpful after ’km’ and ’latitudes’.

page 5, line 30-31: I do not understand this last sentence here.

page 6, line 2: ERA data are not an observation. It is a model forced to the atmospheric state observed. This is fully different than an observation. You can call it a reanalysis. Not more like this.

page 6, line 10-19: This paragraph should be shifted to Section 3.1. ’Configuration of model simulations’.

page 7, line 2-3: Please skip everything starting from ’respectively’. You have already explained about that detail.

page 7, line 5-12: I never read before something complicated like this. Please reformulate that paragraph in such a way that it is clear ’only the resolution changes compared to CPC.

page 7, line 19-20: This last sub-clause is redundant information. You have already explained that for the other configurations.

4.4 Results and Discussion

IMO, this section consists of two subsections 4.1. and 4.2. The discussion about the location of the storm beginning at line 21 on page 11 is worth to put in an own subsection 4.3.

page 8: Where are the paragraphs here? One suggestion from my side: line 23.

page 8, line 20-23: This deviation of the path is fully misleading here. The location of the storm is discussed later and needs in my opinion an own section.

page 8, line 27: I cannot observe from Figure 3 that the magnitude of rainfall is larger for ERA, but the spatial extend of regions with a high amounts of rainfall is much larger in ERA than in COSMO. Furthermore, I see a shift in the maximum precipitation field between ERA and COSMO.

page 9, line 10: Is this an observation from a radiosonde? If so, it should be highlighted here because then the reader knows which value is realistic (and not only a model output).

page 9, line 12-13: You mention that the eye in COSMO forecast is 40 km away from observations. Yes, but the ERA is much far away from the observations. IMO, this discussion should be placed in 4.3. Otherwise, the information falls from the sky.

page 9, line 24-30: I miss the discussion about the placement of the CAPE maximum at Figure 4. It is evident that the runs with 24 lead times place the maximum more the South compared to the runs with longer lead times.

page 9, line 32-33: You argue something about downdrafts and updrafts, but no figure or detailed text is given. What do you exactly mean? What is a realistic downdraft and updraft? Such a discussion would be a chance to improve the paper and make it more scientific.

page 10, line 12: ERA is not observations. Please skip that.

page 10, line 12-13: You argue that the CAPE values of the ERA are always overestimated, but you do not give a proof for it. IMO, this sentence can be skipped.

page 10, line 15-23: There are no observation by the IMD shown. Thus, the reader has no feeling for the differences between model and observations.

page 10, line 27: the ERA reanalysis fields show an overestimation in spatial extend but without any observations the reader would not believe that magnitudes of precipitation and CAPE are overestimated.

page 11, line 2-3: I do not understand the meaning of that sentence.

page 11, line 5: This is a barplot and not a histogram.

page 11, line 7-9: A sentence without content. Please skip it.

page 11, line 9-12: The discussion about leadtime requirements is confined to precipitation intensities but not to location of intense precipitation. I do not understand, why this is less important. Regarding lead times, this is a crucial point.

page 11, line 19-20: Again, as already said, what is the value of such sentence without having seen any observation.

page 11, line 31-35: The critical discussion about predictability only includes the model domain. However, the quality of the initial and lateral boundary conditions is of much more importance, but it is not discussed and analyzed at all.

4.5 Conclusions

The conclusions are too general and off-topic. The main content is about preconditions for high-resolution simulations and improvements or problems detected in other studies. The relation to this paper is not so clear. IMO, the conclusion should be rewritten in order to get a clue about the implications of the author for the whole scientific community.

page 12, line 9-12: Too long sentence.

page 12, line 15-17: What is the measure that indicates deep convection on 3rd of December 2017?

page 12, line 25-29: What is the line of argumentation here? The text deals initially with COSMO-DE and its graupel scheme. Afterwards, we learned something about reduced precipitation over the coastal Arabian and then, downscaling issues of the UM are referred. There is no logic at all.

page 12, line 33-34: What do you mean with that sentence? What means necessary?

page 13, line 4-5: The english text reads strange starting from 'where little ...'.

page 13, line 6-8: The author is telling about tuned parameterizations in NWP models, in particular for specific resolutions and scales. The study presented here should give valuable insight into the treatment of convection and the impact on precipitation. I am not convinced at all that we learn with this study something new and not known from former studies. We learn about model results from the storm 'OCKHI' nothing more. This study does not help to conclude about what are the problems with dynamical downscaling nor at which resolution to switch off parts of the parameterization of convection.

4.6 Figures and Tables

figure 1: Do we need it? The text explains everything one needs.

figure 2: Which simulation is shown regarding the CAPE? The extend of the COSMO domain is not large enough to extract that information.

figure 3: Which experiment of COSMO is shown? (CPC)

figure 4: The CAPE observation at 00UTC of 3.12.2017 and at 69.15 degree East and 11.82 degree North should be marked in each plot.

figure 5: Which area is taken for averaging?

figure 6: Which area is taken for averaging? What is meant with the last sentence in the caption?

table A1: The version number of COSMO is missing. Reference for grid-scale precipitation, vertical turbulence diffusion and surface-layer turbulent fluxes is missing.

table A2: Which time is analyzed? The position at 00UTC of 3.12.2017? Why is the analysis not done for other stages of the storm?