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Natural Hazards and Earth System Sciences
Discussions

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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article [a4paper,top=1.0in,bottom=1.0in,left=1.0in,right=1.0in]geometry fancyhdr setspace color hyperref graphicx textcomp soul Response to the Reviewer's Comments: NHESS-2018-288

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:
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Ms Reference No.:NHESS-2018-288

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Summary of Revision (to the Editorial Board, NHESS)

Dear Dr. Fabrizio Masci,

On behalf of myself and my co-authors, I would like to extend my sincere thanks to you and your supporting Editorial team for your efforts in evaluation of our manuscript. We would also like to place on records our sincere appreciation to Dr. Ronny Petrik and other anonymous reviewer for their valuable comments and suggestions, which have helped us in extending the scope of paper and improving the quality of scientific content of our manuscript. We have addressed almost all the suggestions/queries raised by both the reviewers and have made necessary modifications in the manuscript.

After incorporating reviewer's suggestions, the revised manuscript includes fine-resolution (i.e., 0.25° grid spacing) ERA5 and NCEP FNL reanalysis fields for assessment of initial conditions and validation of CAPE and other meteorological fields simulated through different numerical experiments of COSMO. We have also

included satellite-based IMERG precipitation measurements (available at 0.10° horizontal resolution) for validation of rainfall simulations. As per the suggestions of both the reviewers, numerical experiments with COSMO model are also re-designed. For investigation of the vertical structure of cyclonic storm, a new figure of vertical cross section of equivalent potential temperature is also included in the Results and Discussion.

Having addressed most of the queries/suggestions pointed out by the reviewers, we are now quite optimistic that you will find the revised version of our manuscript acceptable for publication in NHESS Journal. Point-to-Point Response to the Reviewer's comments and summary of modifications carried out in the revised manuscript is attached as an Appendix to this letter.

Thanking You,

Dr. D. Bala Subrahamanyam
Corresponding Author
(On behalf of all the co-authors)

Dated: September 05, 2019.

POINT-TO-POINT RESPONSE & SUMMARY OF REVISION

REVIEWER#2

Comments from Referee

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).

Author's Response

We thank Dr. Ronny Petrik for his critical appraisal of the manuscript and numerous suggestions. We have taken into account of all the suggestions, and have revised our manuscript substantially by addressing all queries raised by both the reviewers.

Author's changes in the manuscript

- Below, we provide Point-to-Point Response to all the queries and concerns raised by REVIEWER#2.

Comments from Referee

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.

Author's Response

We agree with the reviewer's suggestions, and accordingly we have included ERA5 and NCEP FNL reanalysis fields in the revised manuscript. Different meteorological parameters, such as: CAPE, sea level pressure, and wind vectors extracted from these reanalysis data, are compared with the COSMO model simulations. Also an intercomparison of these global fields is carried out for the assessment of initial conditions. Since, our primary goal of this research article is confined towards assessment of convection parameterization scheme at different horizontal grid resolution, we have not extended the scope of this article towards data assimilation, or the spread originating from different physical parameterization schemes. Nevertheless, we have adequately rephrased the write-up in the revised manuscript to make the scientific contents of this article well-focused.

Author's changes in the manuscript

- **Data:** Details of ERA5, NCEP FNL Reanalysis and IMERG observations are added.
- **Results and Discussion:** Details on meteorological fields, such as: CAPE, sea level pressure, and wind vectors from reanalysis fields are discussed in line with the COSMO model simulations.
- **Figures:** Above-mentioned meteorological fields are included in the revised figures.

Comments from Referee

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:

- *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.

Author's Response

We agree with the reviewer's suggestions. DPC Simulations are replaced with CNC (Control simulations, with No Convection parameterization) simulations, wherein the grid resolution of COSMO is kept as 0.0625°, and the convection parameterization scheme is switched off. Furthermore, we have made use of fine-resolution reanalysis data from ERA5 and NCEP FNL for comparison of CAPE, and other meteorological fields. In addition to this, we have also utilized satellite-based precipitation measurements in the revised manuscript for validation of rainfall simulations.

We also appreciate the reviewer's suggestion on "the need for parameterization of shallow convection", however the current version of COSMO offers simulation of convection as per Tiedtke parameterization scheme. Hence, we have confined our analysis to the sensitivity of Tiedtke convection parameterization scheme to horizontal grid resolution during the passage of OCKHI storm. We will definitely try to address the second aspect in a more detailed manner in a separate research article, as inclusion of all these stuff may be beyond the scope of present research article.

Author's changes in the manuscript

- **Numerical Experiments in the COSMO Model:** We have eliminated the DPC simulations, which are now replaced with a new set of simulations, namely - CNC.
- **Figures:** New figures are drawn with new reanalysis data, as well as new simulation experiment.

Comments from Referee

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.

Author's Response

We agree with the reviewer's views about limitations in the ERA-Interim reanalysis fields. For making the comparison of COSMO simulations with reanalysis fields more meaningful, we have now included fine-resolution ERA5 and NCEP FNL reanalysis data, which are available at 0.25° grid resolution. Furthermore, we have also included

satellite-based precipitation measurements from IMERG for validation of 24 h accumulated rainfall.

Author's changes in the manuscript

- **Results and Discussions** and **Figures:** We have redrawn all the figures with inclusion of new datasets (ERA5 and NCEP FNL Reanalysis, and IMERG precipitation measurements). Accordingly, associated write-up is also modified.

Comments from Referee

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.

Author's Response

We have extended the scope of our revised research articles beyond the precipitation and CAPE. Now, the mean sea level pressure, wind vectors and vertical cross-section of equivalent potential temperature along the latitudes is also included in the Results and Discussion.

We have included fine-resolution global reanalysis data (ERA5 and NCEP FNL Reanalysis, available at 0.25° grid resolution), and satellite-based IMERG precipitation measurements (available at 0.10° grid resolution) for validation of COSMO simulations.

COSMO model domain is extended to a larger area for covering the entire track of OCKHI storm.

Finally, we agree with the reviewer that inclusion of more comparable events would extend the robustness of results in a statistical and scientific basis. However, as we mentioned in the title itself, the present work is undertaken as a case study wherein the sensitivity of convection parameterization scheme on the grid resolution, and its impact on forecast fields with designing different numerical experiments, is investigated. Nevertheless, we take the reviewer's comments in a positive manner and will carry similar studies in future to include more number of cyclonic storms.

Author's changes in the manuscript

- **Results and Discussion:** Sea level pressure, wind vectors, and equivalent potential temperature are newly included meteorological fields.
- **Figures:** A new figure showing the vertical cross-section of equivalent potential temperature across latitudes is added for the investigation of vertical structure of the cyclonic storm.

Comments from Referee (4. Specific Comments)

Below we present a summary on all the Specific Comments raised by the reviewer (*Italic Letters*), and our response/changes in manuscript just beneath the reviewer's comments. Overall, we have taken care of all these comments in the revised version of manuscript.

- ***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.

  AGREED AND INCLUDED. We have included details on performance of models resolving storms, as well as an overview of the manuscript.

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

  AGREED AND SKIPPED.

- *page 2, line 1: Start a new sentence*

  AGREED AND CORRECTED.

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

  AGREED AND REPLACED.

- *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].*

  AGREED. Bryan (2003) paper is cited and important findings from this paper are included in the revised manuscript.

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

  AGREED AND CORRECTED.

- ***4.2 COSMO Model***

  IMO the section 'COSMO model' should be divided into '2.1. General description' and '2.2. Parameterization of Convection'
  AGREED AND IMPLEMENTED. We have re-structured our sections accordingly.

- *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.*
  AGREED AND IMPLEMENTED. Complicated stuff is omitted.

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

  AGREED AND CORRECTED.

- *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.*

  AGREED AND CORRECTED.

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

  AGREED AND SKIPPED.

- *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'.*

  AGREED AND CORRECTED.

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

  AGREED AND CORRECTED. This sentence is rephrased and details of Tiedtke scheme is skipped.

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

  AGREED AND REPHRASED.

- *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).*

  AGREED. These sentences are moved to section 2.1.

- ***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'.

  AGREED AND IMPLEMENTED. Section names are modified accordingly.

- *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.*

  AGREED AND INCLUDED.

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

  NO CHANGES ARE DONE. VSCS refers to "Very Severe Convective Storm" and is mentioned in the Introductory section.

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

  AGREED AND CORRECTED.

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

  AGREED AND REPHRASED.

- *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.*

  AGREED AND CORRECTED.

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

  AGREED AND IMPLEMENTED.

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

  AGREED AND SKIPPED.

- *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.*

  AGREED AND REPHRASED.

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

  AGREED AND ELIMINATED. Redundant information is eliminated.

- ***4.4 Results and Discussions***

  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.

  AGREED AND IMPLEMENTED.

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

  AGREED AND RESTRUCTURED THE NEW PARAGRAPH.

- *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.*

  AGREED. We have revised this part, and introduced the discussion about location of the storm in appropriate places.

- *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.*

  AGREED. We have rephrased this part to bring more clarity. Important observations obtained from Figure are rephrased for better clarity.

- *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).*

  AGREED AND INCLUDED. We have revised this sentence to include details.

- *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.*

  AGREED. We have taken care of this sudden jump in the revised manuscript and appropriate changes are done.

- *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.*

  AGREED AND CORRECTED.

- *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.*

  AGREED. We have included more details with a new figure to discuss the vertical structure of cyclonic storm.

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

  AGREED AND SKIPPED.

- *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.

AGREED AND 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.*

  AGREED. We have included IMERG satellite-based precipitation measurements.

- *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.*

  AGREED AND CORRECTED.

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

  AGREED AND CORRECTED.

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

  AGREED AND CORRECTED.

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

  AGREED AND SKIPPED.

- *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.*

  AGREED. We have included more details and discussed this aspects in Results and Discussion.

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

AGREED AND SKIPPED.

- *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.*

  AGREED AND CORRECTED.

- ***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.

  AGREED AND RE-WRITTEN.

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

  AGREED AND REPHRASED.

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

  DETAILS INCLUDED. Large values of CAPE and associated accumulated rainfall over the location was the important aspects concerned with the deep convection. We have addressed this point in the revised manuscript.

- *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.*

  NECESSARY CORRECTIONS ARE DONE, AND SENTENCE IS REPHRASED.

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

  NECESSARY CORRECTIONS ARE DONE, AND SENTENCE IS REPHRASED.

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

  NECESSARY CORRECTIONS ARE DONE, AND SENTENCE IS REPHRASED.

- *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.*

  SUBSTANTIAL REVISION IS DONE, AND CONVINCING CONCLUSIONS ARE INCLUDED.

- ***4.6 Figures and Tables***

  figure 1: Do we need it? The text explains everything one needs.
  PARTLY AGREED. We are still retaining this figure. However, we have modified this figure for making it more informative.

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

  AGREED. As we have extended our model domain, this aspect is addressed.

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

  NECESSARY CORRECTIONS ARE DONE AND DETAILS ARE INCLUDED IN THE FIGURE CAPTION.

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

  NECESSARY DETAILS ARE INCLUDED IN THE REVISED MANUSCRIPT.

- *figure 5: Which area is taken for averaging?*

  NECESSARY DETAILS ARE INCLUDED IN THE REVISED MANUSCRIPT.

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

  SENTENCE IS REPHRASED AND OTHER DETAILS ARE INCLUDED IN THE REVISED MANUSCRIPT.

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

  NECESSARY CITATIONS ARE INCLUDED IN THE REVISED TABLE.

- *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?*

  NECESSARY DETAILS ARE INCLUDED IN THE REVISED MANUSCRIPT.