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Interactive comment on "ICON in Climate Limited-area Mode (ICON Release Version 2.6.1): a new regional climate model" *by* Trang Van Pham et al.

Anonymous Referee #2

Received and published: 1 September 2020

Dear Editor

I have reviewed the manuscript entitled "ICON in Climate Limited-area Mode (ICON Release Version 2.6.1): a new regional climate model" by Van Pham et al.

The manuscript presents a new regional climate model (RCM) based on the recent ICON model family developed by DWD and other institutes in Germany. This model is thought to replace CCLM. The manuscript recalls the context and history of this new development, presents the model configuration and the simulation setup for an evaluation run and shows the evaluation results with respect to state-of-the art reference datasets (observation-based gridded products) and a CCLM reference run. The birth

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of a new RCM is rare and worth to be published. GMD seems to be adapted for such publication. The manuscript is generally well written, well organised and often informative. However I feel that it does not really succeed to full-fill its goals, that is to say to become the foundation of the new RCM for a wide community. In particular, it sometimes looks like a technical note or an internal document of the COSMO-ICON community where not everything is understandable for a reader not participating to it. Also it focuses a lot on the comparison with CCLM for the evaluation run that is not so interesting by itself (a comparison to the Euro-CORDEX RCMs on the whole would be more relevant) whereas at contrary the technical tests of the model are not fully exploited and illustrated in the manuscript.

Finally I'm advising the publication of this manuscript after major revisions. See a detail of my main concerns below as well as a list of minors comments

Main concerns:

1. The manuscript often looks like a technical note of the COSMO community where not everything is understandable for a reader not participating to it. So either this manuscript is a technical note of the COSMO or ICON communities and it does not deserve a GMD publication, or it should be revised to avoid this impression.

1.1 Not all the terms and projects referred within the manuscript are understandable by a reader not used to the COSMO model. Please consider that most of the readers are not part of the COSMO or ICON community and does not know the related projects.

Ex: COPAT project very often mentioned.

Ex: R2B8 configurations and other RxBy.

Ex: Figure 1 is incomplete and not completely self-sufficient. Could you please make the text and the figure 1 consistent: ICON-NWP or ICON-ESM are in the text, not in the figure. Same for the Large-Eddy Simulation version for completeness. Please rework the figure 1.

1.2 Some figures do not have the quality required for a scientific article. It seems that everything was done a bit too quickly without careful final checking by all co-authors.

Ex: figure 2: missing top line. Please defined the PRUDENCE box abbreviations in the captions

Ex: figure 3: names of the variables in brackets are not standard names. Please use standard names and consistent naming between figures, tables and text.

Ex: many figures without units (fig 4, 5, 8, 9, . . .). Please check

Ex: some figures with y-axis labels, other without (cf. Figure 7 vs 8). Please check for consistency.

2. The manuscript focuses a lot on the comparison with CCLM for the evaluation run. I'm not sure that this should be the major point of such an article.

2.1 I understand the will of the authors to focus on this comparison but I would expect that the reader is likely more interested by a fine description of the behaviour of ICLM itself. So at least in the section 4, please spend more time in describing the ICLM behaviour and less time to the description of the CCLM behaviour. You may even want to cut section 4 in two parts, one dedicated to the new model evaluation and the other to the quick comparison with CCLM.

2.2 In addition, personally I would find more relevant to compare ICLM with all the EURO-CORDEX evaluation simulations performed at 12 km. You can either use the results obtained in Kotlarski et al. (2014) if values are numerically available or recompute some of the key scores using data downloaded from the ESGF. Doing so, you will place the newly-developed RCM within the state-of-the-art of the RCMs in Europe. I know that this request requires massive additional work but I hope that the authors will consider it.

Kotlarski, S., Keuler, K., Christensen, O. B., Colette, A., Déqué, M., Gobiet, A., ... & Nikulin, G. (2014). Regional climate modeling on European scales: a joint standard

evaluation of the EURO-CORDEX RCM ensemble. Geoscientific Model Development, 7, 1297-1333.

2.3 Reading the text, I often feel that the authors are "too proud" of ICLM being so close or even better than CCLM. Again I understand the author point of view after so many years of work and the fear of not being as good as the old model. However the way it is phrased is not scientific (objective) enough and show too much satisfaction with themselves. See for example, conclusion, abstract, page 7 line 28-30. Please re-read the whole manuscript and rephrase keeping in mind that the goal of the paper is to present a first version of the model and not to "kill" the old one. Model developers are often not well placed to judge themselves their new model. At the end, it will be up to the readers and then to the ICLM users to decide if the new model better fit their applications. The future will tell us.

3. In a first paper describing a new RCM, I m expecting much more illustrations concerning the technical tests performed with the model before the evaluation run. Many tests are mentioned (time steps, domain decomposition, different computing system) but not really exploited and illustrated. Even if those tests are very appealing to present, for me, they should be at the heart of such paper and each test should be documented by a table or a figure. Currently we need to trust the authors blindly concerning the test results without any proof or trace.

3.1 I'm advising to create a section dedicated to the model tests. That is to say to split section 3.1 in two sub-sections, one describing the tests and one for the evaluation run.

3.2 I'm also advising to add at least the "1+1=2" test. That is to say, checking if running 2 months in one job or in two jobs with a restart between the months give the same results or not. This allows to verify the restarting procedure.

3.3 Later (not for this specific article), I'm also advising to test the model in the Big-Brother / Little-Brother framework what is for me a mandatory step for any new RCM (see for example Denis et al. 2002)

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Denis, B., Laprise, R., Caya, D., & Côté, J. (2002). Downscaling ability of one-way nested regional climate models: the Big-Brother Experiment. Climate Dynamics, 18(8), 627-646.

4. Not enough information on the model configuration and simulation setup. In such article, I'm expecting more information about the model itself and its configuration for the evaluation run. The information given in section 2.1 and in section 3.1 are not complete for me.

4.1 First, clarify what should fit in section 2.1 and what should fit in section 3.1. For me everything general concerning the model itself should go in 2.1 whereas the specific model setup for the simulation (domain, resolution, time step, physical choice, tuning, forcing choice) should go in 3.1. The separation is not always easy but deserve some attention to ease the reading.

4.2 For the model description (section 2.1), I'm expecting more information and related tables and figures on the horizontal grid (how does the icosahedric grid look like ?), the distribution of the vertical levels, the output procedure (do you output on the icosahedric grid or on a more classical grid ? See the text page 7, line 11). Do you have the option of spectral nudging in addition to the upper boundary nudging? Also add more information about the relaxation zone and lateral nudging procedure (width, variable nudged, strength of the nudging, filtering tricks if any...) for example in the paragraph page 4 line 12-16.

4.3 For the simulation setup, I'm expecting there the number of grid meshes for the EURO-CORDEX configuration, the way to define the grid, the numerical cost (compared to CCLM at least), the resolution (explain what R2B8 is) but also the description of the forcings of the run. In particular, in addition to the GHG, SST and sea-ice cover (described in section 2.1), I'm expecting some information concerning the aerosol representation (3D+time variation) that can be very variable from one RCM to another (Gutiérrez et al. 2020), the tropospheric ozone and the evolution of the land-use-land-

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cover if any (Davin et al. 2020).

Gutiérrez C., Somot S., Nabat P., Mallet M., Corre L., van Meijgaard E., Perpiñán O., Gaertner M.A. (2020) Future evolution of surface solar radiation and photovoltaic potential in Europe: investigating the role of aerosols. Environ. Res. Lett.,15 (3), 034035, https://doi.org/10.1088/1748-9326/ab6666

Davin, E. L., Rechid, D., Breil, M., Cardoso, R. M., Coppola, E., Hoffmann, P., ... & Raffa, M. (2020). Biogeophysical impacts of forestation in Europe: first results from the LUCAS (Land Use and Climate Across Scales) regional climate model intercomparison. Earth System Dynamics, 11(1), 183-200.

5. A tricky point in RCMs is the capacity to keep or to modify the large-scale information provided by the driving model. Many methods can be applied to check this (Big-Brother/Little-Brother experiment, see above or GCM-RCM temporal or spatio-temporal correlations for large-scale fields often in altitude or cyclone tracking or weather regimes identification). You may want to keep it simple for this study but could you please show at least one illustration allowing to check the lateral forcing procedure? For example, you may want to correlate the Z500 anomaly or the temperature in altitude between the model run and the driver (ERA-Int) at various temporal scales (e.g. yearly, seasonal, monthly, daily, 6-hourly) or anything showing to the reader that ICLM is able to reproduce the large-scale of the driving model at least for some temporal scale (see for example Sanchez-Gomez et al. 2009).

Sanchez-Gomez, E., Somot, S., & Déqué, M. (2009). Ability of an ensemble of regional climate models to reproduce weather regimes over Europe-Atlantic during the period 1961–2000. Climate Dynamics, 33(5), 723-736.

6. Minor comments:

6.1 page 2, line 24: could you explain the difference between "one-way nested sub-domain" and "limited-area mode"? For me, it is the same thing. Is it a question of

on-line versus off-line?

6.2 page 3, line 16: for the update of the SST, could we also use lower frequency such as daily or monthly?

6.2 page 3, line 19: green house → greenhouse

6.3 page 4, line 6-11: this paragraph could perhaps include more information about the input/output procedure, the file format, the flexibility of the outputs, ... For example, is it possible to output hourly precipitation and monthly-mean MSLP from the same run? or do you need to output all variables at the same frequency before a post-processing step?

6.4 page 4, line 33: grammatical issue

6.5 page 4, line 24-30: could you explain more the restart procedure and the job management and its flexibilit? Could you perform daily run, monthly run, yearly runs ? Or do you have a mandatory time slice such as one month?

6.6 page 5, line 2: could you tell more about the tuning strategy for ICLM. What do you try to optimize?

6.7 page 5 and in many places: EU-CORDEX → EURO-CORDEX

6.8 page 5, line 20: 30 km. Give also the value in hPa.

6.9 page 5, line 29: give the list of the variables nudged and the nudging coefficient

6.10 page 5, line 31-33: The use of many unexplained grid names (R2B8, R3B8, R3B7) is confusing. Simplified or explain. Also in the paragraph, you mention tuning parameters from global settings but setup from LAM... clarify

6.11 page 6, line 8: could you compare the 120 s time step with state-of-the-art RCM time steps at the same resolution?

6.12 page 20-23: is the reference CCLM simulation published? Any reference to refer

to? If yes, cite it. If not, you need to describe it in the method section or to use a published run such as one of the EURO-CORDEX evaluation simulations performed with CCLM and available on the ESGF.

6.13 page 6, line 34: clarify that you are considering only land points.

6.14 page 7, line 12: typing issue?

6.15 page 7, line 21: In your case, if I understand well, the RMSE measures a skill related to temporal variations of the variables over the PRUDENCE boxes. So I would have dedicated STDEV to a spatial skill score by averaging in time before computing the standard deviation. Currently STDEV is spatio-temporal score if I understand well, what is therefore quite difficult to interpret. Please, consider to change this. Also table caption mentions "spatial standard deviation" whereas the text mention "spatio-temporal standard deviation". Please clarify.

6.16 page 7, line 21: For the quantitative score, I'm not forcing you to do so but it could have been a better option to plot Taylor diagrams (incl. RMSE, correlation, standard deviation) in order to be more exhaustive in the evaluation of the runs: for example a spatial Taylor diagram per season for all European land points and a temporal Taylor diagram for each PRUDENCE box. This is just an advise. In particular, it allows to put all boxes or all seasons or all variables on the same figure.

6.17 page 7, line 21: If you decide to keep the score STDEV, I propose to put in the tables the ratio of the standard deviations (Model/Obs) in order to have only 2 columns as for the RMSE allowing to easily see the best model for every line.

6.18 page 7, line 28-30: this small paragraph illustrates well my major comment 2.3 with terms such as "very good performance", "consistent for all six evaluated variables", "already of similar". Please rephrase in a more objective and scientific way without overstating the results obtained. Also remember that the ICON project started 20 years ago. So the model is not so new and has been already tuned and adapted at that

resolution over the European domain. I'm aware that a model used in climate mode can show biases not seen in weather forecasting mode but still, you are building on the weather forecast experience. Also note that the model performance is not "consistent" for all variables. From my point of view, it seems better for temperature-related variables than for precipitation or MSLP. Here again, a section comparing the ICON run with all the Euro-CORDEX RCM runs in evaluation mode would be more conclusive (see previous major comment).

6.19 page 8-9-10: Please reorganise the text of those sections to put first the description and discussion of the ICON biases before comparing more quickly with the CCLM reference as the reader want more information about the strengths and weaknesses of ICLM and less about CCLM. Currently I find that the ICLM description is too light and the CCLM description too fat.

6.20 page 8, line 10: "no bias" → When the median bias is near zero, it does not necessarily mean "no bias", it can mean "bias compensation in space". Rephrase.

6.21 page 8, line 17: "extreme daily temperature" → avoid to use the world extreme for min and max daily temperature. It is misleading for the reader as "extreme" is often kept for specific statistics or indices. Check everywhere. Also page 9, line 2, line 3.

6.22 page 8, line 21: "the bias was larger". All the text of the results is written at the past form. I'm not an English specialist but it would be easier to read at the present form → "the bias is larger ...". Please consider to change this everywhere in the results section.

6.23 page 9: please state that ICON is not so good for Summer day statistics. I don't understand why the representation of figure 11 is not similar to the representation of figures 5 to 10 with a box plot representation. A black box can be used for the observation in addition to the green and blue boxes in that case.

6.24 page 9, line 19: not sure I agree that CCLM overestimates the precipitation. It is

relatively well balanced over Europe contrary to ICON.

6.25 page 9, line 20. Please cite a reference for the "too low values". For precipitation, please also mention and discuss the strong model biases over the topography.

6.26 page 9, line 29: "summer had the smallest variations". Not so true if you think that precipitation is very low in summer for some regions. Computing the error in % (even without showing them) may help for discussing the results

6.27 page 9, line 34: "five out of height" → for me it is 7 out of 8. Please check in table 6.

6.28 page 10, line 3-6: for me by eye, CCLM-REF seems better than ICLM-REF for those indices. Please re-assess.

6.29 page 10, line 7: please cut the MSLP and cloud section in two sections, one for each variable for consistency and add the Table 8 for the cloud cover again for consistency.

6.30 page 10, line 8-11: any explanation for the MSLP biases in both models?

6.31 page 10, line 9: for MSLP, it seems that the biases can reach values higher than 2.5 hPa (cf. Figure 4 over Spain for ICLM.

6.32 page 10: same question for the cloud biases. Any explanation or hypothesis?

6.33 page 10, line 20: not clear where you find the +/- 5% values

6.34 page 10, line 20-21: "overestimation of the cloud cover . . . cold bias". ok for the causality for tasmax but this is often the opposite for tasmin. Rephrase.

6.35 page 11, line 11: Personally my assessment is that CCLM is better than ICLM for precipitation. Please re-assess. I agree that models are equivalent for MSLP.

6.36 figure 3: please make this figure easier to read. For example by increasing the thickness of the curves? Possibly showing only seasons or showing maps? Try to

make it simpler and more informative with the key message easier to catch for the reader.

6.37 figure 4: showing the areas where the differences are statistically significant or not may lead to a more informative figure and make it easier to describe in the text in order to focus only on signaificant biases. Please revise the map projection for figure 4 (it is ugly currently) in order to limit the zone without information in each panel. Also "shave your model" that is to say remove the relaxation zone or comment the model behaviour there in the text.

6.38 figure 5-10: I like such figures. Check the y-axis labels and the units everywhere.

6.39 Table 1: Please add more information about the physics by splitting the deep convection and shallow convection lines and by splitting the radiation in short-wave and long-wave radiation. Add in this table all useful information and references for the physics as it will likely serve as reference for many articles afterwards.

6.40 Table 8: please add a table for the cloud cover

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-20, 2020.