

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 #1

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The manuscript by Van Pham et al. presents a first reanalysis-driven evaluation run of the new ICON-CLM regional climate model which originates from the limited-area version of the global ICON model. The performance of the new modelling system is compared to the one of COSMO-CLM, the regional model previously employed by the German Weather Service and the CLM-Community. Although ICON-CLM is a comparatively new development and options for model calibration have probably not been fully exploited yet, its performance is comparable and partly even superior to the well-tested recommended COSMO-CLM configuration.

Overall, the work certainly fits into the journal’s scope. One can expect ICON-CLM

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to become one of the major RCMs employed in Europe, and the documentation and comparison of its performance is an important part of scientific progress. The paper is well written and well structured, data and methods are well explained. The quality of the figures is acceptable for most parts. The presentation of the results is rather descriptive, which is however acceptable for this kind of work. The conclusions are well based on the results obtained and the use of the English language is acceptable (though it could still be improved).

I can therefore recommend a publication of this work after a couple of minor issues have been fixed (see below). With kind regards.

MINOR ISSUES

- Model naming conventions and use of abbreviations. The reader of the paper easily gets confused with the names of the modelling systems (COSMO, COSMO-CLM, ICON-CLM, ICON-A, ICON-NWP, ICON-LAM, ICON-O, ICON-EUclim, ICON-EU-Nest, ICON-GCM etc). This is in part unavoidable as the names reflect the complex history of the ICON model. Figure 1 is certainly helpful in this respect, but as a reference I would appreciate an additional table that lists the individual configurations of ICON and their basic characteristics. Such a table could also serve as a reference for further publications. Furthermore, the paper also introduces the abbreviations ICLM-REF and CCLM-REF, which do not denote model systems but individual simulations but could well be confused with the model system names. I believe this could somehow be simplified, although – frankly - I currently have no idea how. One possibility might be to mention the simulation names in the suggested table as well. In any case, the reference to the simulations is not consistent throughout the paper, especially within the figures. Figure 3, for instance uses "ICLM-EOBS" in the headers, which should actually read "ICL-REF – EOBS" (same for CRU). Same for Figures 5 to 10. Figure 4 just uses "ICLM" and "CCLM" as headers (should be "ICLM-REF" and "CCLM-REF"). Also Tables 5 to 7 use "ICLM" and "CCLM" instead of "ICL-REF" and "CCLM-REF" in the headings.

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- page 1 line 10: "with the setups similar
- page 2 lines 9-10: It is not really clear to me whether ICON-EU-Nest is a global model with regional refinement over Europe or a higher-resolution version traditionally nested into a global ICON model.
- page 2, line 10: "COSMO-EU" needs an explanation
- page 2 lines 13-14: As written, the unification was scheduled for end of 2019, which is already in the past. The sentence should hence be modified to "This implies that the last unification of COSMO and COSMO-CLM (COSMO 6), carried out at the end of 2019, was the last one."
- page 2 line 20: Wouldn't it be better to speak of "climate projection" here instead of "climate prediction" to highlight the longest time horizon for application of the model?
- page 2, line 23: Did ICON-NWP inherit any parts from COSMO (for instance, the physics package)?
- page 2 line 34: Why was CCLM 5.0 clm9 used for the comparison and not the latest (and final) unified version COSMO 6 (see above)?
- page 3 lines 13-14: Could you briefly explain why this is the case?
- page 3 lines 20-21: I suggest to replace this expression by "... time dependent GHGs as provided by corresponding GHG scenarios".
- page 5 line 13: It is not really clear which "necessary changes" are meant here.
- page 7 line 12: How were the data transformed/regridded?
- page 7 line 18: Would be better to speak of "ICLM-REF" and "CCLM-REF" here as the simulations themselves are meant.
- page 7 line 28: "a very good performance" -> this statement actually needs some quantification or should, alternatively, be reformulated

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- page 7 lines 28-30: This paragraph actually summarizes the results described later on. It should not precede the presentation of the results I believe, but should rather be moved to the end of the results chapter or even to the conclusions chapter.
- page 8 line 17: "trends" is misleading here. I'd rather speak of "biases" or "results".
- page 8 lines 17-18: Any ideas WHY?
- page 9 line 5: "over the whole evaluation period" -> this expression is misleading as the figure shows no time series of the bias.
- page 9 lines 15-16: The numbers obviously refer to events per 20 years. Without providing the length of the period the numbers are, however, not interpretable. I'd suggest to use the unit [days per year] for these numbers and, actually, for the entire Figure 11. This is much easier to understand and to compare to other studies.
- page 9 line 20: "too low values" -> you're obviously referring to gauge undercatch and evaporative losses here, this should be mentioned (and supported by some reference).
- MSLP evaluation in Chapter 4.3: MSLP is evaluated in the same fashion as the other variables, but I see rather little value in this. What is most important here is the spatial MSLP pattern (in addition to the general magnitude), so the evaluation should consider the mean spatial field. The authors might think about replacing their MSLP evaluation by some more informative MSLP analysis.
- Figure 17: In addition to the naming of the simulations (see above) the variable names in the headers refer to the internal model names of the respective parameters. This is rather cryptic and could be replaced by the full names (2m temperature, cloud cover etc). Alternatively, the abbreviations should be mentioned in the caption.
- Figure 4: These maps obviously employ some uncommon projection and the European continent seems a little "distorted". Why don't you use the same projection as in Figure 2, for instance?

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- Figures 8 and 9: The y-axis of these figures misses a label and the unit of the bias
- Figure 11: As explained above I'd suggest to use the unit [days per year] or [events per year] for these indicators (instead of [days per 20 years]). Furthermore, in topographically structured sub-domains such as AL or SC, the spatial averaging of the number of days defined by a temperature threshold makes little sense in my opinion as it completely neglects the large spatial variability. One way to improve on this might be to present the bars as medians with whiskers on top of it reflecting P5 and P95 of the spatial variability within a subdomain. Furthermore, I suggest to place the black EOBS bars to the left of the green simulation bars, not between them (also modify the legend in this case)
- Table 1 is hardly readable, the space between the table lines should be enlarged

SPELLING AND GRAMMAR

- page 1 line 7: "tests"
- page 2 line 1: "the CLM-Community developed"
- page 3 line 16: "with a user-defined"
- page 3 line 19: "the greenhouse gas"
- page 3 line 23: "retrieve" instead of "get"
- page 4 line 3: "for the European domain"
- page 5 line 14: "was tested"
- page 5 line 33: "Tiedtke/Bechthold" (with a "t")
- page 6 line 34: "British Isles"
- page 7 line 12: "for the purpose"
- Chapter 4 "Results": Past tense is used for describing the simulation results in this

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chapter. Use of present tense would be more appropriate and clearer in my opinion.

- page 8 line 12: "... British Isles, Mid-Europe, ..."

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