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Natural Hazards and Earth System Sciences
Discussions

# *Interactive comment on* "Convection-permitting regional climate simulations for representing floods in small and medium sized catchments in the Eastern Alps" *by* Christian Reszler et al.

**Anonymous Referee #3**

Received and published: 17 April 2018

Title: Convection permitting regional climate simulations for representing floods in small and medium sized catchments in the Eastern Alps Authors: Reszler, C., Switanek, M.B., and Truhetz, H. Manuscript number: NHESS-2018-17

In this study the authors investigate the ability of regional climate models, run at convection permitting resolutions, to simulate localized flooding in small and medium catchments. Their methodology is to run a small ensemble of models at resolutions of 50km, 12.5km and 3km the output from which are fed into a distributed hydrological model. The hydrological model is shown to be quite accurate when calibrated and tested. They find that value is mostly added for the higher resolutions over the small-



est catchments (<200kmˆ2) and sub-daily time scales (e.g. hourly). Despite these improvements in performance bias adjustment is still required. However, due to the temporal smoothing of the bias adjustment the higher resolution does not always yield improved results. Also, the results are highly dependent on the driving regional climate model. The CCLM results are far superior to those obtained with the WRF modeling system.

This study represents an important step in the application of convection permitting models. Much of the promise of these platforms rests on their ability to drive downstream impacts models and it is nice to see them applied in this manner. The authors do a good job demonstrating that there is significant added value in modeled precipitation at sub-daily time scales (e.g. figures 4 and 5) and that the flood representation (both maximum and seasonality) is generally, though not always, improved, at least for the medium to small catchments. Unfortunately applying the bias adjustment degrades the performance over the smaller catchments for the convection permitting simulations. While the bias adjustment improves the outputs from the 50km and 12.5km simulations it is difficult to see any improvement in the 3km simulations. I understand that there is some degradation due to the temporal smoothing but then one must ask: why bother?

I think this makes an important contribution but it raises more questions than it answers and as such I think the authors need some more nuanced discussion and also to tone down the conclusions a bit. The conclusion that ∼3km is "essential" for catchments smaller than 200km is overstating the fact as only two catchments are below 200km and neither shows marked improvement in either maximum flood peaks (figure 10) or seasonality (figure 11) once the bias adjustment is applied. Also the sample of two catchments is too small to make any generalizable conclusions. Maybe the authors could add a few more small catchments to the study to help bolster their case. Also there is the fact is that these results appear to be highly model dependent. The authors offer some explanation by way of the fact that CCLM is well tuned and widely used over this region while WRF is not. However, recent studies show comparable performance by WRF over the Greater Alpine Region and Europe more generally (Awan et al., 2015; Knist et al., 2018; Kunstmann et al. 2018). Rather than a hand waving generalization about model family rather an more detailed description of which processes the simulations reproduce correctly and why might be more informative.

What would help greatly would be a more in-depth look at the bias correction method, discussion of the effects of the different nesting strategies, the addition of more small catchments and more nuanced discussion and conclusion sections. The bias adjustment technique is described as "novel" and as such readers may not be as familiar with it as they are with more common quantile mapping approaches. The use of a non-stationary approaches is well justified however the reader needs some more information especially in light the rather modest improvement the bias adjustments affords. Also the conclusion section should be rewritten with a more nuanced interpretation of the results. Is convection permitting modeling really needed if, after bias adjustment, results are no better or only modestly improved compared to coarser resolution simulations? Should multi-model, multi-realization, ensembles be employed or rather one highly tuned simulation? For present climate this might be sufficient but such tuning has well demonstrated shortcomings at climate time scales. Clearly there are substantial challenges remaining before these types of simulations can reliably be used for impacts models. At present the authors fail to acknowledge this and I think somewhat overstate their results. Also, the authors claim that recommendations can be made but then fail to deliver on this promise. What general recommendations, if any, can be made based on this study? Does convection permitting modeling only provide added value over particular areas, for particular cases, particular time scales and particular cases/phenomena? The results here certainly seem to point towards such a limited use or at the very least a need to balance expectations with current capabilities. I recommend a major revision as there is considerable additional discussion/clarification needed and potentially additional analysis is required. Specific comments follow below.

Specific Comments P3L7-8: The community is well beyond "first attempts". WRF-



Hydro (a fully coupled distributed hydrological model within WRF) is far enough in its development to be the core model for the United States' National Water Model. https://ral.ucar.edu/projects/wrf_hydro/overview. Other such systems in operation are TerrSysMP which features a 3-D groundwater model coupled to COSMO-CLM (e.g. Keune et al., 2017). Note that I do not make any on their reliability over climate time scales (i.e. simulations around a decade or more).

P3L13: Please be clear that when you write "coupled" you mean limited one-way coupling (I actually wouldn't call this coupling at all) wherein there is no feedback between the hydrological model and the atmospheric model and the atmospheric model only passes temperature and precipitation.

P4 L1-7: What are the potential ranges of observational uncertainty? In addition to sensor errors and under catch there is also uncertainty resulting from interpolating to a grid from point based station data. How are these taken into account?

P4L8: The version of WRF used here is quite old (almost 8 years!) and many of the issues related to this version have been corrected. In fact WRF is now 6 full versions more advanced as of this writing. How might this have affected to the results?

P6-Error Correction: More details on the bias adjustment are needed given that it is being pitched as a "novel" approach. How does it perform relative to other approaches? What are its limitations and/or tradeoffs? What are the implications of univariate approach to bias adjustments when the two variables corrected, temperature and precipitation, are related to each other? Also more explanation of the issues/limitations behind current approaches is needed. Maraun et al (2017) have an excellent overview of the current state of bias adjustment shortcomings and placing the SDM approach among these would be helpful to readers.

P8L29: "relatively".

P12L1: Specify which figure/panel you are referring to.

P13L10: I don't believe it has been demonstrated that CPM is "absolutely necessary". I would suggest either making a stronger argument with stronger supporting evidence or modify this claim.

P17L23: The authors write that "performance using 3km data is still best" but it is hard to discern this from the figures. Figure 11 show 24 seasons in total and of those the 3km is closest to observations in only 11 of these. In the other seasons either the 0.11 or 0.44 degrees simulation is closer to observations or the performance across resolutions is equal.

P16L10: Performance after bias adjustment is degraded over the smallest catchment. Yet this is precisely the type of application (i.e. small catchments) where the authors argue we see the greatest added value of convection permitting modeling. Here it appears that the two techniques, high-resolution modeling and bias adjustment, are not working in concert but in opposition.

P20L30-31: This is in direct contradiction to earlier, and later statements, that CP scales are "absolutely necessary". I would say rather that it is clear that there is still quite some work to do before these models can be reliably used for these sorts of applications.

P24L10: See previous comment. I do not think the authors have presented evidence sufficient to make this statement.

P25L17-18: Clearly CCLM has higher performance than WRF in this study. However, the authors never discussed the nesting strategy (see table 2), which is different for each model system. Specifically, WRF goes through an additional intermediate nest, a step that will certainly have an impact. How then are then are the WRF and CCLM simulations directly comparable?

P25L4: What "recommendations", specifically, can be made?

P25L5: The modeling systems used here are not "coupled" they are used in a model



chain.

Figures and tables

Figure 1. It is almost impossible to make out the catchment boundaries and initials in this busy figure. I suggest moving the catchment labeling to the larger figure 3.

Figure 3. Place catchment labels here in bold. Also bold lines around the catchments themselves so that the six catchments under investigation are clearly delineated.

Figure 4. What region is shown here?

Figure 5. Remove the empty panel in the lower right corner.

Figure 7. It is very hard to distinguish between the blue and black circles. Also including red and green is not colorblind friendly. I suggest a different color scale that has greater separation. This comment applies to Figures 7-12 and 14.

References

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