Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-612-RC1, 2018

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Assessing the Added Value of the Intermediate Complexity Atmospheric Research Model (ICAR) for Precipitation in Complex Topography" *by* J. Horak et al.

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Received and published: 17 December 2018

In their manuscript Horak et al. assess the skill of the Intermediate Complexity Atmospheric Research Model (ICAR) for downscaling mean precipitation amount, in a domain located over the South Island of New Zealand. Model evaluation is performed using established techniques, a range of observational datasets and two skill scores. Their main findings are: (a) ICAR provides additional skill over the main Alpine ridge, while results over coastal stations are deteriorated. (b) Added value is typically largest for stable upstream flow, impinging on the ridge at a 90° angle. These results seem related to the model's roots, which is built on linear theory of orographic precipitation.

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Discussion paper

The article is generally well written and suited for publication in HESS (also for GMD). I particularly appreciated its modest and plain language. All review criteria are met, and I did not detect major scientific flaws, considering the manuscripts scope.

Minor Comments

(1) P2L8: “While dynamic downscaling results in a self-consistent set of atmospheric fields, the computational cost required for the fine spatial and temporal grid spacing is high, especially for long-term simulations or sensitivity studies.”

This sentence would benefit from perspective. For example, for a similar computational domain we would achieve about 240 simulation days per day when running COSMO on a single node, equipped with a P100 GPU (Leutwyler et al., 2016; Fuhrer et al., 2018, I am not implying that you should cite my studies, but used then because I know the numbers by heart). How does ICAR compare these benchmarks?

(2) P2L12: “to a lesser extent, to dynamic downscaling as well” I don’t fully understand the statement in this fragment. Please elaborate on the stationary assumptions in dynamical downscaling, and how precisely this is overcome in ICAR.

(3) Section 2.1: Adding a few plain language sentences how ICAR works and how the approach differs from dynamical downscaling would aid the wider audience. Additionally, a concise summary about linear theory of orographic precipitation and how it is incorporated into ICAR would help. I had to read Gutman et al. (2016) to understand this Section.

(4) Section 4.1: Maybe it would be good to discuss the known biases for mean precipitation in ERAI and outline weather it is difficult to beat it.

(5) Section 4.3 (a) Unfortunately, the chosen calibration period overlaps with the analysis period and employs the same stations. Cross-validation with other periods or station replacement would make the arguments more robust. (b) “Potential reasons for the observed behavior are discussed in Sect. 5.” → That statement is a bit mislead-

ing, since in Section 5 you only say that the question remains open. (c) I am skeptic if the results at 2.5 km and 4 km are substantially different from each other. (d) A devil's advocate could argue that ICARCP mainly improves skill over ICAR because the latter underestimate precipitation amount (see P30L27). I.e., could the same skill be achieved by adding random noise with the right magnitude? Does ICAR beat ERAI too? Please elaborate (here or in Section 2.6) to justify your choice to add interpolated conv. precip. from ERAI.

(6) Section 4.7 Why is the underlying dataset changed to NCEP/NCAR?

(7) Section 5 (a) 1st paragraph: It might be worthwhile to elaborate on how these results relate to linear theory of orographic precipitation. (b) P30L6: "Therefore these two instances are considered as outliers." I think there is a problem here.

Suggestions for optional extensions

(1) Downscaling low-resolution global climate simulations (rather than re-analysis), along major mountain ridges could more evidently illustrate the added value of the approach.

(2) From an application/user point of view, employing the outlined techniques to obtain higher-resolution fields is still a somewhat cumbersome procedure. It will therefore only be performed operationally if the added value is rather substantial. Therefore it would be interesting to see the added value over low-resolution precipitation climatologies such as, e.g., GPCC or GPCP..

Technical comments

P1L1: climate downscaling → downscaling techniques P1L7: the eleven-year period from 2007 to 2017 → an eleven-year period, ranging from 2007 until 2017 P1L9: diagnosed → assessed P1L14: In the abstract, I would use a more general term for "flow of higher linearity" P1L17: tuning → calibration (tuning has a negative connotation). Same applies to the rest of the manuscript. P2L21: Maybe add weather generators to

the discussion? P2L31: due → emerging from P3L23: storing → stores P4L5: no data are → no observations are P5L7: ERAI have → ERAI employs (I think ERA-Interim reanalysis is singular). Also P4L11 P5L19: “convective precipitation from the ERAI” Add the name of the field. Also, add a reference to your Table 1. P6L5: New Zealand, P6L7: ranges → maybe “ridges”? P7L12: In case of → For P9L10: Move sentence “ The aim is not a downscaling ...” to end of paragraph P9L27: HSS is defined as → The HSS P12L4: I relate “occurrence” to precipitation frequency. Maybe better use magnitude? P15L5: For lazy or tiered readers it might be helpful to re-state that VCSR are the observations. P16L6ff: Maybe indicate which months these seasons are (DJF ..)? P19L15: Cloud you add these regions to Fig. 1? P29L9-17: (a) Maybe move this Paragraph to Section 3.2? (b) Does undercatch not affect HSS(P>50)? P29L9-33: I would move the caveats to another place such that the paragraph currently starting at L34 follows after the current L8. P30L21-24: Could you elaborate why you think this issue is a likely candidate?

Table 1: Outline in caption where the uncertainty estimates come from (+/- 0.1). Figure 2: Are these MSE of the annual sums (Add to the caption)? Maybe add the mean values so the results can be put into perspective. Table 2: These are mm/day (e.g. RMSE (mm)), correct? Figure 5: (a) NIWA (top-left) → VCSR (b) Maybe mean magnitude over land to panels? Figure 6: Why do the no. samples (circles) differ among the various thresholds in HSS? Explain in the caption.

Best regards David Leutwyler

References

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