

Interactive comment on “High-resolution fully-coupled atmospheric–hydrological modeling: a cross-compartment regional water and energy cycle evaluation” by Benjamin Fersch et al.

Anonymous Referee #2

Received and published: 25 November 2019

Review of "Fersch et al. - High-resolution fully-coupled atmospheric–hydrological modeling: a cross-compartment regional water and energy cycle evaluation" The authors present a comprehensive effort in setting up, running and validating a WRF-hydro model run and comparing it to a non-hydro WRF setup. I have suggested major revisions and would like to see the following addressed before publication, but I would like to highlight, that with the changes made, the manuscript is indeed worthy of publication. I hope the comments can also be seen as improvements in readability.

Introduction: P2L2-4: The introduction seems to have an abrupt start. Does this first paragraph really have the phrasing to initially frame the story? At least, add "... and

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magnitude. . .” after “spatial distribution”.

P3L1: The newest general HOBE publication is “Jensen, K.H., Refsgaard, J.C. (2018): HOBE – the Danish Hydrological Observatory, Vadose Zone Journal, 17:180059, doi:10.2136/vzj2018.03.0059” This could be changed or added.

General intro: I believe an entire paragraph is missing here where the following is addressed: - What is the key relevance of this paper to the public and/or research domain? Why are you/we doing this? This is important but often overlooked. - What is new compared to previous Fersch/South-German studies as well as other studies? A lot of work is mentioned, but the relevance and new story here is overlooked. Please relate.

P7L8: Radolan reference?

P7L9-10: What is the uncertainty in doing this compared to using observations?

P7L3 to P8L6: I think this sections needs a paragraph that explains the key differences between WRF classic, WRF-H-SA and WRF-H-FC to be able to better understand the reasoning here.

P8L5: This “parameter set” has not yet been explained and confuses. What are these parameters? They belong to the atmospheric realm I believe and are therefore not hydrologic calibration parameters.

P9L2-11: Really hard to link between text and table (1). The schemes/physics as well as references do not match up and several domain levels are referred to. Please elaborate/correct.

P9L17: “only subsurface and surface overland flow routing is. . .” -> ok, but then state what is not taken into account in the atmosphere link – the non-WRF-hydro expert does not know this (me included), but would have to guess.

Figure 1: Many site abbreviations, which are not really ‘learned’ when reading the

manuscript. I don't know if a systematic naming approach could be thought of.

P10L26-27: "Furthermore. . ." -> How was this done? Based on what? Which data?

P11L28: You could also add: Larsen, M.A.D, Refsgaard. J.C., Jensen, K.H., Butts, M.B., Stisen, S., Mollerup, M. (2016): Calibration of a distributed hydrology and land surface model using energy flux measurements, Agricultural and Forest Meteorology, 217, 74-88, doi:10.1016/j.agrformet.2015.11.012. And; Stisen, S., McCabe, M.F., Refsgaard, J.C., Lerer, S., Butts, M.B. (2011): Model parameter analysis using remotely sensed pattern information in a multi-constraint framework. Journal of Hydrology, 409, 337-349. doi: 10.1016/j.jhydrol.2011.08.030.

P12L17: Computational resources -> should these not be mentioned in the manuscript?

P12L20-P13L1: Why/how did you add the baseflow. I see it was necessary, but please elaborate why/how etc. Also – please link the the “_sh” runs in fig 4 + 5.

Table 3: For Am-PEI “coeff” and “expon” the values are larger than the suggest span in table 2.

Table 3: For the two parameters mentioned above as well as $z_{max} = 1$ (3 instances) and $ret_{dp} = 5$, the autocalibration seems to have hit a boundary limit, which could imply model deficiencies. Please mention/elaborate or re-run, using more sound limits.

P13L10: First sentence: Not understood. Limited – how?

Table 4-9: I think you should add MAE.

P20L6-13: Isn't these just the latent heat results again? They look the same. If not, how are they different and why?

P20L6-9: Unclear which scenario is most realistic? Why not just use the best? Better word than scenario?

P22L3-4: Please see my previous comment on table 3 about the resulting parameters. “. . .often tuned to unrealistic values. . .”

P22L14-15: Why? Discuss/relate! This is a discussions section.

P26L19: Please write “(NSE)” after Nash Sutcliffe efficiency.

P28L8-9: Why was this the case?

P29L2: “For the validation period” -> has this been addressed previously?

P29L9-10: Why is this a problem (the 1h time steps)? If it corresponds to “SA” mode.

P29L24: Reference? On alpine foothills soil texture.

P30L15: I do not like the use of “hopefully”. If you had framed the relevance of the study in the introduction, then this paragraph would be more easily written.

Minor: Abstract: “Nominal” – use better wording?

P12L1: where “the” model. Sounds better to my taste.

P26L25-26: “difficulty in reproducing” instead.

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