

## ***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 #1**

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Examining the ability of atmospheric-hydrological coupled modeling in the seasonal prediction is an important topic in hydroclimatic research. This study explored the ability of the WRF-hydro in capturing the regional hydrological cycle. Their results show considerable changes of latent and sensible fluxes between classic WRF and coupled WRF-Hydro though the calibrated parameters were same for both models. When compared to observations, the WRF-Hydro performed better than the classic WRF in the simulation for variety of variables including ET, sensible and ground heat flux, near surface mixing ratio and temperature. The value of this study lies on providing some information in the performance of WRF-Hydro that may be useful for weather/climate forecast service. I support it to be published in HESS if the following comments could

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be addressed.

General comments:

1. For the Figure 2, since the parameter set was calibrated in WRF-Hydro (WRF-H\_SA), we can expect that when coupled with atmospheric model, the WRF-Hydro (WRF-H\_FC), which is the same model used in the calibration process, should have a better performance than the WRF standalone (WRF\_SA) which is a different model to that used in the calibration process. Therefore, how could the authors demonstrate that the worse performance of WRF\_SA is not caused by that the parameters calibrated in WRF-Hydro did not work well with WRF\_SA?

2. While compared to WRF, the WRF-Hydro had a better performance, I am also curious about if compared to other classic hydrological models, such as SWAT, TOPmodel, how the performance would be for the WRF-Hydro, especially on the stream flow modeling. Though this may be out of the scope of the manuscript, adding such information (maybe by citing other previous research) could much add the value of this manuscript.

Specific comments:

P1, line 3: What are the traditional disciplines?

P8, line 13-14: Directly using soil moisture from WRF-H as an initial condition of WRF\_SA can not guarantee the land surface has been well spun-up for the WRF\_SA start.

P9, line 5: CML->CLM

P9, line 11-13: Usually, these processes should be included in a land surface model. Does the WRD-Hydro use more advanced (or complicated) representations for those processes replacing the old ones in the Noah-MP, or there are no such processes in the Noah-MP at all?

P10, line 18: Are there any reservoirs distributed in the basins? If so, why did you turn

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off the reservoir module? What uncertainties may come from this setting?

P10, line 24-25: Please explain the meaning of "two and three dimensions of soil features representation".

P12, line 12-13: What is this setting of REFKDT used for?

P13, line 1-2: Does this simplification induce some errors in regarding to the land-atmospheric interactions?

P14, figure 4: Why there are no shifted hydrographs for Ach-OBH, Ach-OBN and Rt-RST?

P14, line 9: Could the authors show the net radiation for both observations and simulations?

P15, figure 5: Same as P14, figure 4.

P29, line 12-13: How does the lateral water transport increase the soil moisture?

P29, line 16-18: It seems to me that the decrease of soil thicknesses made the results worse in the mountainous site. So why did you change the soil layers for the slope region?

P30, line 16: In terms of Earth System Modeling, how to calibrate the Earth System Model globally is quite challenging. Observation data are so limited on this scale.

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