Interactive comment on "Presentation and discussion of the high resolution atmosphere-land surface subsurface simulation dataset of the virtual Neckar catchment for the period 2007–2015" *by* Bernd Schalge et al.

Anonymous Referee #1

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Review of Schalge et al. "Presentation and discussion of the high resolution atmosphere-land surface-subsurface simulation dataset of the virtual Neckar catchment for the period 2007-2015"

This study introduces a high-resolution dataset of the hydro-climatology of the subsurface, surface and atmosphere obtained with the coupled TerrSysMP model. It covers the area around the Neckar catchment in south-western Germany, and the time period 2007-2015. The dataset is validated in terms of several variables covered by respective observations, and is described to be a testbed for exploring land-atmosphere

interactions at high spatial resolution.

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Recommendation:

I think the paper requires major revisions.

I like the idea of introducing a comprehensively validated, modeled high-resolution land-atmosphere dataset, but regarding the present version of the manuscript and dataset I have several concerns which need to be addressed.

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General comments:

(1) The applicability and purpose of the dataset is not entirely clear to me. The authors only briefly comment on this in the conclusions section, stating it could be used for exploring land-atmosphere feedbacks, investigating potential model simplifications, and data assimilation testing. Honestly, I do not see how the dataset can be useful in such analyses as (i) land-atmosphere interactions are known to be hardly robustly captured by models in general, and (ii) the testing of model simplifications and data assimilation would require different versions of the dataset with respective different model configurations in my opinion. In this context, I ask the authors to expand and clarify their discussion on the applicability of this dataset.

(2) CLM3.5 is used here as land surface model. This is outdated. By now, CLM5 is clearly more advanced in terms of simulating processes related to vegetation and hydrological dynamics at the land surface (Lawrence et al. 2019).

(3) Throughout the manuscript there are various inaccurate statements limiting the reproducibility of the model simulation (e.g. 'increased by about', 'increased by approximately', 'set considerably higher', 'needed to be increased from its standard values', various pedotransfer functions used without explaining criteria, see also respective

specific comments below). More information needs to be provided in each of these cases to ensure the reproducibility of the entire analysis, either in the manuscript or in an appendix.

(4) There are several arbitrary choices made throughout the study which need to be (better) motivated. This includes modifications of the modeling setup of which the purpose is not clear or the magnitude is arbitrary (e.g. 20% increase of sand fraction, ignoring of karst layers, conceptualization of alluvial layers as gravel and bedrock layers including the assumption(s) of values for various involved parameters, modifications of the LAI data). When making these modifications to adapt the model behavior in particular respects (more sandy soils to enhance infiltration) it should be kept in mind that even if the particular purpose is fulfilled, the land-atmosphere system is highly interconnected such that unforeseen side effects can occur. Further, the arbitrary choices include the approach(es) used to validate the modeled dataset (e.g. spatial averaging of model data across 25 grid cells for validation of atmospheric boundary layer characteristics, seemingly random time intervals of the soil moisture, evapotranspiration and runoff validations).

(5) Soil types are an important ingredient for hydro-climatological model simulations. The downscaling-based derivation of soil types in this study is (i) difficult to understand and (ii) contains several assumptions which are not motivated, among which are the amount of considered 1995 locations, the 20% increase of sand fraction (see above), the choice of an exponential model, the choice of conditional co-simulation versus kriging, the focus on first three soil horizons (first means uppermost I guess?). I wonder what is the impact of the choices made here on the final dataset?

(6) The validation of the model simulation in terms of evapotranspiration is very limited. While it is reassuring that the ET and groundwater dynamics are broadly coupled according to expectations this is not a quantitative assessment. The modeled ET could instead (or additionally) be compared with state-of-the-art evapotranspiration datasets such as GLEAM (Martens et al. 2017) or FLUXCOM (Jung et al. 2019) at larger spatial

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scales.

(7) I like the comprehensive validation of the dataset in terms of several variables - an overview table summarizing the determined strengths and weaknesses would be helpful for users I think.

(8) There are too many figures in my opinion, diluting the main messages. Figures 3-5, 7, 14, 17 could be moved to supplementary, and Figures 9 & 10 could be combined.

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Specific comments:

line 35 and throughout: 'simulated' would be more straightforward than 'virtual', using such terminology the term 'real' (line 34 and throughout), referring to observations, can be removed from the manuscript

line 56: test a disaggregation method

line 75-78: you do not aim to reproduce to observed catchment dynamics but still validate the model in some respects - this seems contradictory to me; what is the aim here if not validating the model against observations? how useful is a modeled dataset for the community if is not resembling observations?

lines 139 & 145: the chosen time period and simulation catchment/area are not motivated

line 172: please give more information on the 'software restriction'

line 183: please give more information on the location of the grid cells and the artificial elevation modification to ensure reproducibility, here or in an appendix

lines 195-196: 'about 20%' & 'about 3.3', please be more accurate

lines 194-197: in the abstract of the Tian et al. 2004 paper I found "On average, the model [...] overestimates FPAR over most areas in the Northern Hemisphere com-

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pared to MODIS observations during all seasons except northern middle latitude summer.“ “The MODIS LAI is generally consistent with the model during the snowâĂŘfree periods. . .“ which makes me wonder why the authors modify LAI in summer? Further this could create jumps in the LAI time series from May-June and August-September. More importantly, you state here that LAI is used “for the year 2008“. Does this mean there is no interannual vegetation variability? This would affect evapotranspiration and thereby many related variables and would need to be stated as a serious shortcoming.

line 204: please give the spatial resolution instead of the scale 1:1000000

line 221: ’approximately 20%’, please be more accurate; further, and more importantly, please motivate this modification and its magnitude

lines 229-231: please give detailed information on where which pedotransfer functions have been used

lines 239 & 242: repetition of the information that karst is not considered

lines 239-240: ’to avoid the manifold hydrological challenges related to its modeling’, please be more specific here, also please comment on the impact of this simplification of the approach on the final dataset

line 245: if these alluvial bodies are so relevant, why does this study use datasets which do not include them?

line 249: evapotranspiration errors in models can be significant and might be underestimated here

section 4: please discuss for the performed validation analyses how the determined performance of the study dataset compares generally with the performance of other regional climate models im similar hydro-climatic regions

line 277: ’simulated realistically’, please give more details here on how this is quantified

line 301: 1km2, is this referring to the spatial resolution being 1km x 1km?

line 323: ’quite well’, please be more specific and objective. Further, in Figure 9a between 6h-17h the pattern in the model is actually opposite to that of the observations, I would not refer to this as fitting “quite well“.

line 331: the potential (dis)agreement of simulated and actual land cover can be checked using high-resolution land cover datasets such as provided by ESA CCI

line 343: how are the temperature standard deviations determined?

line 361: ’very well’, please be more specific and objective

line 367: why not using the ESA CCI soil moisture dataset derived from observations of various satellites for this validation?

lines 394-395: I do not really understand why this daily matching is applied here? Also it is not clear how this is done.

lines 391 & 396: I guess you are referring to Figure 15 here, not Figure 16 as stated.

line 415: ’adequate agreement’, please be more specific and objective

line 422: ’will always be replaced’ needs to be toned down in my opinion

line 415: ’good distribution’, please be more specific and objective

lines 447-450: I do not understand how the “fluctuations“ are “scaled“. Do you divide by the inter-annual standard deviation to obtain normalized anomalies (or z-scores)? If so, please name it this way as the term “fluctuations“ is rather unclear.

line 450: I guess this should be “according to Figure 19b“ and not 19c?

lines 451-452: How is this trend computed?

line 454 and following: I like this discussion of limitations and issues

line 458: to me it seems three challenges being discussed here (?)

lines 502-512: As there are multiple concrete ideas to improve the model setup and

consequently the dataset, why not implementing them before publishing this dataset? comment if this will be game-changers

Figure 2: This figure is the same as in the Gasper et al. 2014 paper, with the reference given in the caption. I think it is uncommon to use figures from previous papers, so I would remove this and only refer to the figure in the reference paper in the main text.

Figure 3: Maybe I missed that but what is domain 1?

Figure 4: "e+00" can be removed

Figures 4-6: Please label the color bars.

Figure 6: Please harmonize "evaporation" and "evapotranspiration" in the caption and the axis label. The same applies for the main text in section 4.1.

Figure 8: Values are quite far apart from color bars. Also, it would be nice to also express the difference as percentage.

Figure 11: y-axis label missing Please explain what is meant with the "temperature standard deviations"

Figure 13: Please specify from which times the reference radiosonde observations are taken. Further, please explain how the standard deviation is derived.

Figure 15: It would be insightful to quantify the agreement of the temporal dynamics with e.g. a correlation.

Figures 16-18: Please use the station names throughout instead of the position numbers.

Figure 19: Panel a is not labelled, as well as color bar and axes therein The terms "model" and "reality" are not consistent with the terminology used throughout the manuscript.

References:

Martens, B., Miralles, D. G., Lievens, H., van der Schalie, R., de Jeu, R. A. M., Fernandez-Prieto, D., Beck, H. E., Dorigo, W. A., and Verhoest, N. E. C.: GLEAM v3: satellite-based land evaporation and root-zone soil moisture, Geosci. Model Dev., 10, 1903–1925, doi:10.5194/gmd-10-1903-2017, 2017.

Jung, M., Koirala, S., Weber, U., Ichii, K., Gans, F., Camps-Valls, G., Papale, D., Schwalm, C., Tramontana, G., and Reichstein, M.: The FLUXCOM ensemble of global land-atmosphere energy fluxes, Scientific Data, 6, 74, doi:10.1038/s41597-019-0076-8, 2019.

Tian, Y., Dickinson, R. E., Zhou, L., Zeng, X., Dai, Y., Myneni, R. B., Knyazikhin, Y., Zhang, X., Friedl, M., Yu, I., Wu, W., and Shaikh, M.: Comparison of seasonal and spatial variations of leaf area index and fraction of absorbed photosynthetically active radiation from Moderate Resolution Imaging Spectroradiometer (MODIS) and Common Land Model, J. Geophys. Res., 109, D01103, doi:10.1029/2003JD003777, 2004.

Gasper, F., Goergen, K., Shrestha, P., Sulis, M., Rihani, J., Geimer, M., and Kollet, S.: Implementation and scaling of the fully coupled Terrestrial Systems Modeling Platform (TerrSysMP) in a massively parallel supercomputing environment-a case study on JUQUEEN (IBM Blue Gene/Q), Geoscientific model development discussions, 7, 3545-3573, doi:10.5194/gmdd-7-3545-2014,2014.

Lawrence, D. M., Fisher, R. A., Koven,C. D., Oleson, K. W., Swenson, S. C.,Bonan, G., et al. (2019). The Community Land Model version 5: Description of new features, benchmarking, and impact of forcing uncertainty, Journal of Advances in Modeling Earth Systems, 11,4245–4287, doi:10.1029/2018MS001583, 2019.