Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-203-RC3, 2019

Hydrology and Earth System Sciences
Discussions
Open Access
EGU

Interactive comment on "Intra-catchment variability of surface saturation — insights from longterm observations and simulations" *by* Barbara Glaser et al.

Anonymous Referee #3

Received and published: 30 July 2019

Review of Glaser et al. HESS-2019-203

The authors use a novel and large dataset of hydrometric data and thermal infrared images to describe and simulate the extent of surface saturation at seven locations across a small catchment in Luxembourg. The work is very interesting and novel and the simulations are well described. Based on the title and the first research question, I had expected more analysis and discussion of the observed patterns but instead the manuscript mainly focuses on how well these patterns of saturation can be simulated with a physics-based model. I highly command the authors for this study but think that the impact of the manuscript can be improved by focusing more on specific research

Printer-friendly version

Discussion paper

questions. Currently, the questions (particularly question 2) are very broad and the reader can't escape the occasional thought that the manuscript 'just' describes a model application. That is a real petty because the dataset and the work are very novel. I, therefore, recommend that the authors adjust the title, rewrite the research questions and focus the discussion and also the conclusion on these research questions so that it is clearer what the take home messages are. Overall, the manuscript is well written and particularly well-illustrated but the wording can be clearer and more to the point at some locations (see attached annotated pdf for some suggestions).

Specific comments:

P1L1: Adjust the title so that it is clearer that the manuscript mainly focuses on the simulations and what "part" of the patterns can be simulated based on topography and climate inputs. Now the reader may expect a more in-depth analysis of the observed patterns of surface saturation.

P1L23: It is unclear from the abstract which part of the observed variability is important but not explained by topography or groundwater exfiltration. Please add additional info.

P2L5-15: This is a nice list of what has been done previously but it would be more useful if a short summary of the main findings from these studies (and thus also the remaining open questions) is given as well.

P3L8-9: Make the research questions more specifc! Now they are very general and not so helpful, particularly question 2. Then focus the discussion and conclusion more on these questions, rather than the different model steps or the overall discussion of the goodness of fit of the model.

P5 section 2.2: Add more information on the georeferencing of the images. How was it done and how accurate is it? What is the size of a pixel and how many pixels do you think you are off?

P5L16: How many pictures (or what percentage of all pictures) were excluded?

P5: You show that topography and microtopography are very important for the patterns of saturation and frequency of occurrence of saturation. However, I seem to have missed how this topographic information was obtained. Lidar data? What is the resolution and what smoothing algorithms did you use? What is the size of a mesh grid cell and how much of the microtopography does the mesh reflect?

P6L6: A bit more information on this previous calibration is needed. What data were used for it? Which data for the calibration are the same as used here for the validation? Which riparian zones correspond to this area and were these pictures of saturation used already for this calibration? Please provide more info for the reader who hasn't read this paper yet.

P7L3: The sentence on 'spatial heterogeneity' could be clearer. I think that you only added a different soil parameterization for the riparian zone but as it is written now the reader could think that you added spatial heterogeneity within the riparian zone. In that case, there are different parameters for different parts of the riparian zone, which would of course significantly influence your results.

P7L29-P8L1: This is unclear to me. Either you used the KGE as the evaluation criterion or the Pearson correlation. Did you add a different weight for the Pearson correlation in the KGE? If you did, then this needs to be described in the text.

P11L5: I guess that you are showing the less reliable data with a different symbol but that is not clear from the caption or legend.

P15L33: Provide a bit more details on the Antonelli et al (2019) study. How did they statistically analyse the images to show this?

P15L32: While it is interesting that the model suggests that the ponding is due to saturation and groundwater exfiltration, your model parameters (i.e., the Ksat at the surface and near surface) would already have told you that infiltration excess overland flow is unlikely for this parameterization. How were these parameters derived or calibrated?

Make sure that you don't present results that are directly related to the parameter values without referring back to the choice of these parameter values.

P16L42: But isn't part of this variation due to micro-topography that is not included in your mesh?

P17L16: So what was the resolution of the topography data that was used here? And the resolution of the model mesh?

P18L6: Yes this is in line with this result but doesn't explain why this is the case.

P19L12: Why do you need additional water sources? Why are spatially variable soil properties (and thus parameters) not sufficient?

P19 conclusions: I find this study really nice but the conclusion doesn't seem to have a clear take home message – or perhaps simply too many messages. This is a petty as this reduces the impact of this manuscript. This may be due to the lack of clear well-defined research questions. I thus suggest that the authors reflect on these questions and to add clearer take home messages to the conclusion.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-203/hess-2019-203-RC3-supplement.pdf

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-203, 2019.

Printer-friendly version

Discussion paper