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Earth Surface Dynamics Discussions

Interactive comment on "Designing a suite of measurements to understand the critical zone" *by* S. L. Brantley et al.

Anonymous Referee #1

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The manuscript entitled, "Designing a suite or measurements to understand the critical zone" by Brantley et al. describes the on-going expansion of the Susquehanna Shale Hills Critical Zone observatory (SSHCZO) from the original 0.08 km^2 catchment to a much larger 164 km^2 catchment. To continue to make progress towards understanding the fluxes of water, energy, gas, solute, and sediment fluxes, the expansion of the SSHCZO demands that some measurements are prioritized above others. In their manuscript, the authors outline the rationale for their new sampling strategy and explain how discrete measurements of many properties will be used together to extrapolate across the entire SSHCZO study area.

This manuscript is not a typical research paper. Instead of discussing results and drawing new conclusions, this paper explains, in detail, an experimental design. Con-

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sequently, It is not clear to me if the manuscript is appropriate for publication in *Earth Surface Dynamics* given that it does not fit the typical model of a research or review article. That said, I *do* think that manuscript by Brantley et al. makes a valuable scientific contribution for a couple of reasons. First, the Earth science community has implicitly invested in the success of the Critical Zone Observatory (CZO) program. The CZO program has and promises to continue to provide new datasets and models that are useful to a range of sub-disciplines within the Earth sciences. By providing a glimpse into the design of the new SSHCZO, the authors are, in a way, helping the Earth science community to better understand the utility of the CZO program. Similarly, future researchers will likely also struggle with need to prioritize sampling strategies. While the effectiveness of the sampling strategy proposed by Brantley et al. is not yet known, it is likely to be useful as a comparison for future iterations.

Given that this manuscript is not a typical paper, I have only a few comments about the underlying science. In particular, I got the impression that the main purpose of the new instrumentation that the authors are installing at the SSHCZO is to provide calibration data for a suite of models that the authors are developing. Importantly, this suite contains only a single model per processes (i.e. one hydrologic model, one reactive transport model, one land-surface model, ETC). If, as the authors state, the scientific community does not yet agree on which model or which parameters we need to measure in order to understand the critical zone, how will the proposed sampling strategy make progress towards this knowledge gap if it is focused on specific models with a particular set of calibration requirements?

Similarly, the authors mention that our understanding of the key processes that govern the critical zone is far from complete and that there are likely many important and unknown thresholds and feedbacks (page 1007-line 22). How can/will the new sampling network at the SSHCZO be used to elucidate these thresholds and feedbacks? How can/will the calibration of a suite of models that do not contain these unknowns help us better understand critical zone processes? Are any of the proposed mea-

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surements useful for distinguishing between competing models with different assumptions/parameterizations?

I understand that the questions I have included above are difficult to answer and I do not necessarily expect the final version of this paper to solve all of them. It is also likely that many of these issues have already been carefully considered by the authors. I just think that this manuscript would be improved by elaborating on how the expansion of the SSHCZO can/will lead to new insights into critical zone processes independent of any specific numerical model.

Detailed Comments

1007 - Line 16: I'd be interested to know if there are any specific disagreements that the authors have in mind. This is relevant since it seems as if the proposed sampling strategy is focused on calibrating a specific set of models. If the community does not yet agree on which measurement to make, will the proposed sampling strategy for the expanded SSHZO only be useful for one modeling group? Or, are enough additional measurements being made in order for competing models to be tested?

1008 - Line 16: I am not sure what "This" is referring to at the beginning of the paragraph.

1010 - Line 10: This sentence points out that there needs to be a feedback between model collection and development, but this is not major focus of the manuscript. To me, expanding the discussion about plans for model-data feedback at the SSHCZO would greatly improve the manuscript. The authors have a lot of complex models to calibrate, but will they use any independent measurements (i.e. measurements not involved in model calibration) to assess the performance of their models? The only mention of this that I saw was on page 1020 - line 28, which stated that independent measures of evaporation/transpiration could be compared with FLUX-PIHM. Is this the only plan for model validation?

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Potentially, the authors could also use a water isotope mass balance as an additional check on estimates of evaporation and transpiration from field measurements and FLUX-PIHM simulations.

Can FLUX-PIHM be used to track fluid transit times? If so, is there a plan to compare these estimates with tracer based estimates? If they disagree, will the design of the sensor network, which was based on the FLUX-PIHM simulations, still be useful for other research questions?

Can RT-FLUX-PIHM be used to predict concentration-discharge relationships? I suppose that the reaction kinetics will always be an adjustable parameter that can be used to fit the data. However, the optimal reaction kinetics would make predictions about long-term regolith development that could be tested with elemental profiles in the soil pits. I guess that this sort of model testing is what the authors are planning, but it would be nice to get more specific details of how different models will be tested with independent data.

1011 - Line 17: At least to me, it is never made exactly clear why the distinction between planar hillslopes and swales is so important. Furthermore, the authors state later in the manuscript (1024-line 20) that swales are absent in portions of the catchments underlain by sandstone. If this is the case, is the geomorphological knowledge we learnt from the original SSHCZO actually translatable to the expanded SSHZO? Or, when we incorporate different lithologies, do we need to resume the random sampling strategy?

1013 - line 5: I guess this is where I got the impression that the focus of the sampling strategy is to parameterize a specific model. While I understand the desire to find a robust way of extrapolating discrete measurements, are the authors doing anything to independently verify model results? Also, can these parameters be used to calibrate other hydrologic models?

1025 - line 16: The authors write four rock samples here. Later on (1026 - line 14), the

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authors write 5 rock samples. Which is it?

1032 - line 19: The authors mention characterizing groundwater residence times, but never mention how this will be determined. I know that there are a variety of approaches and each has its own caveats. So, it might be interesting to know which will be tested at the SSHCZO.

1036 - line 12: The authors state that, "we are testing the hypothesis that fewer soil pits are needed because we are using a regolith formation model and geological knowledge to site the few pits that we dig". I agree that this is what they are doing. But, it is not clear to me how they will evaluate the results of this test. I'd appreciate more detail about how the authors will determine whether or not digging a few pits is all that is really necessary.

Figure 1: I got a bit confused by this figure. So, I am going to write down my impressions so that the authors can see whether or not I understood it correctly.

Why are some blue arrows pointing up and others pointing down? I get the impression that this is because the arrows pointing up refer to processes above the land-surface and vice versa, but this is not explicitly stated in the captions. It this is the case, shouldn't "other gas fluxes" be changed to "gas effluxes" ?

The left-to-right order of the blue arrows and brown boxes refers to relative time for each to reach steady-state, right?

Why does surface runoff have sediments while subsurface runoff has particulates?

Also, I am supposed to assume that the gradients on the top of the diagram (distribution of above ground biota and land surface elevation) arise from the above ground fluxes and not the below ground ones? I'd assume that the distribution of above ground biota is also sensitive to the gradients in regolith composition. These two properties seem disconnected in the figure, but I do not know if this is intentional or not.

Figure 2: The caption says black dots = trees. But, the dots are actually green. I

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assume that the inset describing the tower is located where the black box is on the map. An arrow or something could be helpful here.

Figure 8: Are the uncertainties associated with the analytical measurements and the range of parent material composition smaller than the point size in this figure? If not, it might be worth plotting them as well.

Figure 9: are there any measurements of soil depths that could be plotted for comparison?

Interactive comment on Earth Surf. Dynam. Discuss., 3, 1005, 2015.