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Interactive comment on "A Deep-Learning Hybrid-Predictive-Modeling Approach for Estimating Evapotranspiration and Ecosystem Respiration" *by* Jiancong Chen et al.

Anonymous Referee #1

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This study tested and validated a hybrid predictive modeling (HPM) approach at eight flux tower sites and three snow measurement sites in western North America. Modeled predications of annual evapotranspiration and ecosystem respiration fluxes were significantly correlated with observations but less accurate at sub-annual resolution. These results are very promising but also demonstrate some limitations of the current HPM approach.

General comments: This is a thorough analysis and very promising study for the application of hybrid models to simulate ecosystem fluxes. It's also a well written manuscript. However, minimal effort has been devoted to generating the type of

process-based/transferrable information that's expected from a top journal. Instead, the discussion is generally couched in terms of supporting previous work to emphasize model performance. In my opinion, some re-interpretation of results is needed to move this beyond a basic model validation.

A good recent example is Wieder et al. 2017 that also compared CLM (point mode) to flux tower observations in complex terrain. Although their study only considered one site, they explicitly focused on periods of relative agreement versus disagreement between the modeled and observed fluxes to yield broadly testable ecohydrological hypotheses. Given the multi-site focus of the current study, I don't think the same level of detailed inquiry is required, but additional synthetic analysis would increase the scope and the subsequent impact of this work.

Relatedly, Figures 4-9 all show similar long-term timeseries data with scatterplots that lend themselves to similar interpretations in terms of R2 of MAE. These are useful, but perhaps they could be condensed and/or supplemented with other figure types that were more conducive to process-based interpretation. For example, I found Figures 11e and 11f fascinating insofar as they highlighted seasonal differences between vegetation types, but little explanation was provided to "unpack" these results (grasslands and shrublands not even mentioned). Likewise, Figures 12a and 12b present a rich opportunity to speak to differences between the biophysical controls on ET at the SNOTEL and East River sites. Some of the specific factors I'm left wondering about are differences in snow accumulation and melt between sites, evaporation versus transpiration, and heterotrophic versus autotrophic respiration. I understand that you don't have all these measurements, but you've generated a lot of suggestive data that could be leveraged to push this field of research.

Specific comments: L66-67: Reading back through the manuscript, this seems at odds with the practice of using single flux towers to represent the larger ecoregion (section 4.2). I don't actually have a problem with that research design, but this heterogeneity descussion may not be the best way to set things up.

L73-75: Also uneven hydrologic distribution due to lateral flow in complex terrain (e.g., Chang et al. 2018) that results in heterogeneous fluxes.

L109: Has NDVI been defined?

L142-144; L365-367: After reading through the manuscript once, I'm not convinced this objective was met or even really addressed, which was confusing because I kept expecting to come across these results. The small-scale heterogeneity results must be expanded or else it may not be a fatal flaw to just remove this language/objective if the analysis didn't work out (as you intimate on L574-577). In any case, the current manuscript introduction/objectives/results are inconsistent with respect to the degree of focus on this topic.

L143: Replace "CO" with "Colorado, USA" for the global audience.

L150: I'm curious how you defined "mountainous watersheds" for this study. I've been to the Walnut Gulch sites and they didn't strike me as the least bit mountainous. Also, with respect to my comment on L142-144, how important is the "mountainous" aspect anyway? I understand the broader impacts for water resources, but you'd reach a wider audience if the results were presented in a more general way. I see advantages and disadvantages to both mountain-specific and general analyses, but details/justification (mountain) or else re-framing (general) is needed in either case.

L162: How were the eight FLUXNET stations selected? Some justification needed here. Was it to facilitate the paired approach in section 4.2?

L164: Table 1 indicates that the Saskatchewan sites are colder than US-NR1.

Table 1: I assume the periods of record are truncated at 2015 because you used the FLUXNET2015 product? This should be specified. Watch significant figures throughout this table.

L227: Why was it necessary to treat this site different than the others? Please provide details about this "cleaning" procedure and why it was needed.

L367: The previous text makes it sound like three (not four) cases – confusing.

Table 3: You probably don't a need a table just to say that "sn" was included at three of the eight sites. Especially because you already have so many display items.

L378-380: I'm very curious as to whether this was also the case at the seasonally dry Walnut Gulch sites? If so, it speaks to systematic bias where the model captures ET dynamics during energy-limited but not water-limited periods. This strikes me as a major result (see general comments) and could be leveraged to make recommendations about the input variables that are necessary for various systems.

L393-395: Wouldn't the model overestimate (not underestimate) Reco if it can't account for moisture limitation during this time? Please clarify.

L408-411: Seemingly contradicts L66-67.

L493: Units mismatch.

L495-505: Discussion.

L516-518: In my mind, this is a missed opportunity to gain process-based (and thus transferrable) insight. What about these sites could factor into ET differences that are so much greater than the Reco differences? See general comments.

L544-546: How hard would it be to add moisture into the model? Why wasn't it added in the first place? I'm not suggesting that you re-do the analysis, but readers will be very interested in this information.

L563-568: It's not clear to me what model results "present similar dynamic trends" to the moisture limitation invoked by Hu et al. 2010 (and a host of larger scale, more recent work). My current understanding is that the model breaks down somewhat in the presence of moisture limitation, which I consider an interesting and valid result/contribution, but you can't have it both ways i.e., the model either does or does not capture fluxes during periods of relative moisture limitation. Perhaps I'm missing something.

L609: Still need more convincing about how “mountainous” was defined and why these sites were chosen, in particular with respect to other “mountainous” sites in the FLUXNET2015 database. I’m thinking of sites in New Mexico and possibly Oregon off the top of my head.

Chang, L.-L., et al. (2018). Why do large-scale land surface models produce a low ratio of transpiration to evapotranspiration? Journal of Geophysical Research: Atmospheres, 123(17), 9109–9130. https://doi.org/10.1029/2018JD028159

Wieder, W. R., et al. (2017). Ecosystem function in complex mountain terrain: Combining models and long-term observations to advance process-based understanding. Journal of Geophysical Research: Biogeosciences, 122(4), 825–845. https://doi.org/10.1002/2016JG003704

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