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Hydrology and Earth System Sciences Discussions

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Interactive Comment

Interactive comment on "Climate-vegetation-soil interactions and long-term hydrologic partitioning: signatures of catchment co-evolution" *by* P. A. Troch et al.

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As the third reviewer and running late with my review, I find I do not have much detail to add to the comments of Drs Savenije and Gentine – the paper presents a clear methodology and results that are, on the surface of things, intriguing.

Major Comment:

I have had a nagging discomfort with this study since I first saw it presented at a conference (and I have never quite worked out how to articulate the discomfort until forced to by writing this review, so I apologize to the authors for not bringing up the nagging concern at the time).

I think I have now worked out what the cause of my discomfort is: The study doesn't clearly articulate its main hypothesis, and nor are relevant, plausible alternative hypotneses presented. While Budyko's hypothesis is tested, I don't think this is central. The paper aims to find a signature(s) of co-evolution. Thus hypotheses regarding co-evolution and what its fingerprint must be presented.

It seems to me that the authors have 2 scenarios in mind: a) Catchment morphology & function is independent of climate (i.e. no co-evolution, or limited co-evolution). b) Catchment morphology & function co-evolve with climate, leaving an interdependence between the two.

Given these two broad alternative hypotheses, how do they translate into predictions about the space-for-time substitution experiment the authors have performed?

Accepting that the Budyko hypothesis is valid, it seems that the authors hypothesize that in the absence of catchment - climate co-evolution, shifts in climate and specifically aridity should cause catchments to only shift along the Budyko curve, not off this curve. The corresponding hypothesis might then be that shifting catchments while holding climate constant should not result in any change in Budyko properties.

These seem quite strong requirements considering the baseline noise around Budyko's curve, and perhaps I have overstated them – I am not suggesting precisely that these should be the authors' null hypotheses, but rather setting up some obvious alternatives. I do strongly suggest that a null hypothesis regarding the presence/absence of co-evolution is necessary so that the space for time experiment would obviously translate as a hypothesis test.

I also think that there should be some more refined hypothesis presentation about the behavior of the anticipated co-evolution. The authors infer a fingerprint of co-evolution in their results about the correlation between vegetation efficiency and catchment evaporation metrics – less efficient vegetation = more evaporation – but why is this an indicator? What would the authors have concluded if more efficient vegetation = more

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evaporation? Would that not be a fingerprint of co-evolution? Or would both be fingerprints of co-evolution but one indicating a different pathway than the other?

I do not mean to be dismissive, rude or pedantic in these comments. I completely agree that the study is interesting and that the results are intriguing, and quite possibly important. But the meaning of the results is obscured by the lack of a conceptual framework that sets up the logic of our expectations about co-evolved systems. In the absence of clear hypotheses, it is hard for a reader to interpret the significance of the results. I leave the paper convinced I've seen something interesting, but not 100% sure clear what it was. I think that a restructure and re-write that advances the basic hypotheses upfront and tests them, commenting on any unexpected or hard to explain observations along the way would lead to a much more impactful paper, and I hope the authors consider a rewrite along these lines.

Minor comments: 1. I echo Dr. Gentine's suggestion that a little more discussion of mechanisms (even if only in the context of the model) might be illuminating. 2. I have minor concerns about using only 12 sites which limits the statistical power of the results and generates a lot of sensitivity to outliers in the plots presented – there are a few plots (the plot showing the significance of the drainage timescale being a prime example) where the existence of a trend is strongly reliant on a single point. I assume model run time has limited the analysis to the 12 catchments, but wonder whether the results would really look different if another 12 sites were added in? 3. Is there a way to distinguish "real world" versus "model derived" outcomes? After all, all we are really able to test here is whether the model itself requires co-evolutionary constraints to be imposed. Ok, I recognize this is far too philosophical to be answer-able. Perhaps the authors could explicitly suggest repeating similar experiments with a wide array of models to try to avoid idiosyncracies from a single model structure?

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