Hydrol. Earth Syst. Sci. Discuss., 10, C1600–C1604, 2013
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Hydrology and Earth System Sciences
Discussions

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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Received and published: 11 May 2013

Reviewer's comments are printed in between "", our response is printed in normal fonts.

"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".

We appreciate very much the time and effort Dr. Thompson spent on reviewing our paper.

"Major Comment:

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Discussion Paper

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 hypotheses 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".

First, let me point out that the results presented at the conference mentioned were not the results presented in this paper, but work that is published in Carrillo et al., 2011, but since our study builds on that one we do appreciate the honest assessment.

The comment about the main hypothesis is an excellent comment and one that allows us to explain the purpose of our study better. Testing Budyko's hypothesis is central to our paper, not to find signatures of co-evolution, which is the result of our testing of the Budyko hypothesis. To restate our null hypothesis: Water and energy availability control the long-term hydrologic partitioning at the catchment scale. The fact that the 12 selected catchments plot on or very close to the Budyko curve suggest that this is a reasonable hypothesis to test with our study catchments. The way we test the hypothesis is by moving climate forcing and filter properties that capture hydrologic functioning of the catchments around. What we find is that if a catchment is exposed to a different climate than the one it experiences the Budyko hypothesis is not accepted, that is all catchments deviate from the Budyko curve. This implies that certain model components assume specific dynamics that filter non-local climates in different ways than assumed under Budyko's hypothesis. The surprising finding of our study was that

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those model components, expressed as parameter values or dominant time scales of response, vary systematically across different catchment characteristics, such as vegetation and soils.

We will modify the abstract and introduction of our paper to more clearly state the central hypothesis.

"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".

This is correct; these are the consequence of assuming that to first order only water and energy availability controls long-term hydrologic partitioning. Our study was designed to address the question why so many catchments across the world show behavior in line with this simple hypothesis.

"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".

Since our null hypothesis is not about co-evolution this comment is no longer relevant.

"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 evap-

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oration metrics – less efficient vegetation = more evaporation – but why is this an indicator? What would the authors have concluded if more efficient vegetation = more 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?"

Our results are what they are, and we can only interpret or try to explain what the data/simulations show us. We agree that both could have been expected and that both may be expressions of co-evolution under different initial and boundary conditions.

"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".

We totally agree that stating clearly our null hypothesis (Budyko, not co-evolution) will help to make the paper stronger, and we will modify the abstract and intro.

"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".

We could only add more discussion on mechanisms in the context of the model since we do not have detailed field observations at the scale required to address mechanisms properly. That's why we tried to explain the systematic deviations from the Budyko curve by looking at systematic variations in parameter values or characteristic time scales. These of course represent certain mechanisms (such as the time water is present in the perched aquifer, leading to more water available to vegetation) and we

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believe that we have added sufficient detail in our discussion to help the reader understand what the underlying mechanisms are. But, if we would have done the analysis with another physically-based yet parsimonious model (as Dr. Savenije suggested) our interpretation of the mechanisms could have been quite different. For these reasons we hesitate to add more details as these could easily be interpreted as mere speculations.

"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?"

This is a comment that obviously we have thought about a lot. For reasons indicated it was impossible to start the study all over with 12 other catchments, but we sure hope that we (and possibly others that are intrigued by our findings) will be able to do this in the near future. But let me add that 12 catchments is not too bad, from a statistical perspective, and all relationships that were presented were tested on significance.

"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 answerable. Perhaps the authors could explicitly suggest repeating similar experiments with a wide array of models to try to avoid idiosyncrasies from a single model structure?"

This is an excellent suggestion for future research that we have added to our conclusions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 2927, 2013.