

Interactive comment on “Land–atmosphere interactions in the tropics” by Pierre Gentine et al.

Pierre Gentine et al.

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We thank the reviewer for his/her positive comments.

Review: Land-Atmosphere interactions in the tropics, by Gentine et al. As a review paper, there are not really any new findings here, but rather a summary of previous work. The paper does a fine job of this, and I believe it will be a valuable resource for others. I recommend acceptance with minor revisions. That being said, I do have some comments. Comments: indeed as the reviewer rightly mentioned we aimed at reviewing the state of the art in the field.

Comment: Lines 202-203: When I looked at the WECANN papers I did not see proof that seasonal cycles were reproduced. I see r-squared values in the tables, but not evidence showing seasonal cycles (also: I think the sentence should have 'variability' added at the end). I'm not sure I'm convinced that WECANN is better than other models

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in the tropics (really, Brazil. Do we have enough tower data in Africa or the Maritime Continent to really make an assessment?). Is there a way to establish this? Reply: this is a good comment of the reviewer. We will now use additional products in addition to WECANN.

Comment: This brings up something else: By using the words 'wet tropics', the implication is that tropical forests do not experience any water stress. I don't agree with this. There are a couple of papers [da Rocha et al., 2009, Costa et al., 2010] that discuss the variation in 'environmental control' (light limitation) and 'biotic control' (water limitation across precipitation and vegetation gradients in Brazil. Yes, the cerrado (savanna) is generally water-limited, and the wettest forest is light-limited, but the transition is not binary, nor is it limited to the cerrado (transition forest). I think a discussion of light-and water-limitation across gradients, and our uncertainty about the relative importance of each is an important part of tropical land-atmosphere interaction that is missing from this manuscript. George Vourlitis and coauthors have done some good work in the cerrado that should be described (I'm not going to list them all here). Baker et al. (2013) put forth a conceptual description of this gradient in a modeling study. Whether describing Brazil, the forest-to-desert transitions in Africa (both north and south), or the ecotone in Australia, there is important ecophysiological information in these transitions (and their response to changing climate) that is ignored here. Response: we completely agree with the reviewer and it must have been an improper discussion but indeed tropical forests are not binary in their response nor are they uniformly responding (e.g. forest age, biodiversity, height... are key to explain the response of different parts of the Amazon forest). We will correct this.

Comment: On a potentially related note, I'm curious if the European Centre Amazon conversion papers need to be mentioned (Cox, Huntingford, Jones, et al.). I understand a review paper is not the same thing as a history paper, but these HADGCM papers got a lot of attention, and actually initiated quite a bit of investigation. The fact that there has been somewhat of a retreat from the initial findings means that these

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papers are no longer the 'state of the science', but they were seminal, along with the Saleska 2003 paper (in my opinion) in the initiation of some pretty important lines of investigation. Response: this is indeed a good point and we will highlight the fact that previous modeling efforts might have overestimated stress and incorrectly represented the feedback with the atmosphere

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-12>, 2019.

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