

## ***Interactive comment on “Climate response to Amazon forest replacement by heterogeneous crop cover” by A. M. Badger and P. A. Dirmeyer***

### **Anonymous Referee #3**

Received and published: 20 March 2015

The authors provide a study to assess the climate feedback in Amazon with complete deforestation. The major claimed contribution is the use of realistic crop modules, compared with the previously used grassland PTFs. The other two reviewers have provided indepth comments on various aspects of the study. I want to comment more on the aspects related to crop model, land cover scenario and seasonal scale variation (esp. in hydrological variables). In general, I concur with the other two reviewers to recommend major revision for this work before it goes to the next stage.

First, since the major contribution of this work is the use of realistic crop models, there is a need of validation for the performance of the model, which is missing here. Ideal situation is to use the flux-tower based measurements to validate the crop module, or use some country- or provincial-level yield data to validate. Another alternative would

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be to use large-scale GPP data (e.g. from spaceborne fluorescence) for the existing crop fields. Without the validation, this claimed novelty is rather weak.

Second, I don't see any reason for a complete deforestation scenario here. I understand that's what people did before in their literature, but if the authors tried to propose more realistic scenarios, they should follow the type of work by Soares-Filho et al. (Nature, 2006) with more realistic landcover projections. The authors provided little rationale for why the scenario is more "realistic". Besides, why irrigation should be considered is also unclear for me. There is little evidence that Amazon agriculture are using irrigation even for now. The whole "realistic" scenario (Fig. 2) is very confusing and lack of justification.

Third, the authors used CESM with the CLM as the land surface model. CLM model has a well known issue that it could not well reproduce the ET seasonality in wet Amazon regions. Some possible mechanisms are missing (e.g. groundwater storage). I suggest the authors also look at the seasonal-scale change in hydrological variables (e.g. ET, precipitation, Bowen Ratio) before/after deforestation. It should be fine that the work won't resolve the CLM issue, but it would be useful to include the analysis for seasonal variations.

Ref: Soares-Filho, B. S. et al. Modelling conservation in the Amazon basin. Nature 440, 520–3 (2006).

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 879, 2015.

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