

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

**Anonymous Referee #1**

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### **General comments:**

This paper examines the impacts of deforestation on climate in Amazonia. The authors implement new plant functional types (PFTs) in CLM4.5 in order to represent typical tropical crops, under both rainfed and irrigated practices. Using a fully coupled Earth System Model, the authors compare two simulations: one with present vegetation cover, and one with a deforested Amazon. The differences fall within the range of previous studies in spite of the more realistic prescribed changes from forest to crops.

This study includes a more detailed and realistic depiction of deforestation via the inclusion of tropical crops, and is therefore a welcome addition to the existing literature. The manuscript is generally well-written and I believe that the work deserves publication.

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However, some aspects require substantial improvements and I thereby recommend major revisions. In particular, the authors mainly assume that the observed changes are due to local changes rather than changes in large-scale circulation. Similarly, some aspects such as the cause of changes in the evaporative fraction are not convincing enough at this stage. Finally, apart from the more detailed representation of the crops used in deforestation, the work is very similar to previous studies. It could easily be slightly expanded to include e.g. the impacts of deforestation on temperature extremes, which would certainly be a welcome addition to the existing literature.

### Specific comments:

1. Page 881, line 6: 1991 is now over 20 years ago. Is there any more recent data available?
2. Section 1.3: The literature overview provided here could benefit from a few additions. For instance, Lejeune et al. (2014) have provided a comparison and synthesis of 23 GCM studies relevant here and provides useful findings in this regard. This study could be relevant to facilitate the comparison to previous studies on page 891, lines 13-19 and on page 892, lines 10-18, as well as when discussing the presence of bipolar temperature changes (p. 890, lines 20-24). Recent studies that make the link between land-atmosphere coupling and sensitivity to deforestation could also be worth citing (e.g., Lorenz et al., 2014, etc.).
3. Irrigation: I find it quite difficult to understand what and where is irrigated. For instance, was irrigation only applied to “irrigated rice” but not to other crops, or to any crop? I think this requires clarification. For example, at lines 5-12 on page 886 there is an explanation that (1) irrigated area fraction is defined based on a dataset of areas equipped for irrigation, and that (2) irrigation is only applied to the soil beneath irrigated crops. This does not seem to exclude that irrigated area fraction might exceed the irrigated rice PFT area fraction, and thereby implies that other crops can be irrigated (also implied by line 5-6 on page 887). On the other

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- hand, in the rest of the paper, irrigation only seems to refer to rice (e.g., lines 12 p.888; line 26 p.891; line 7 p.892; line 15-17 p.895).
4. Section 2.3: over which period/with which forcing are the simulations run? The Qian et al. forcing used repeatedly is mentioned for the spin-up simulations but it is not clear to me whether this forcing has also been used for the actual simulations (250 years including 125 last years used for the analysis). A layer of confusion is added at L7 (p. 889) when the year 2000 is mentioned as initial conditions. Please clarify.
  5. Section 3: The analysis only considers changes in mean quantities. I think that showing changing in the distribution (e.g., changing in temperature quantiles from the distribution of daily temperature) would be a valuable addition to this paper, even if only for temperature. I leave this up to the authors to take up this suggestion but it might also help understanding some of the observed features.
  6. Section 3.3: The authors argue that changes in net radiation directly drive the changes in the partitioning between sensible and latent heat fluxes (page 892, line 24: “. . .impacts the partitioning between sensible and latent heat fluxes”). I would expect a reduction in net radiation to reduce both fluxes without necessarily changing their partitioning, and the authors do not present any convincing evidence that the reduction in net radiation is the cause of decreased latent heat / increased sensible heat fluxes. Instead, I suspect that reduced precipitation (Fig. 5) is likely to lead to drier soils and thereby reduced evaporative fraction. Alternatively, modified vegetation parameters might also impact evaporation via plant physiology without necessarily impacting net radiation. The authors need to present a more detailed analysis here and/or more cautious conclusions (see also the first few sentences of the discussion and conclusion, which clearly assume that modified albedo directly change the partitioning EF, further changing the climate, although this could also result from indirect effects via precipitation

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- or from vegetation parameters other than albedo). A map of changes in evaporative fraction might be useful here, as it is difficult to assess how changes in turbulent fluxes translate in changes in EF if both are reduced or enhanced at the same time.
7. Page 895, line 10-14: The authors seem to imply that changes in precipitation result from local interactions via PBL growth, and vertical moisture transport. What about horizontal moisture transport (i.e., convergence)? It is not clear, in my opinion, whether changes in large-scale moisture convergence due to e.g. atmospheric subsidence can be excluded based on these analyses. Did the authors observe any change in wind, atmospheric moisture and resulting moisture convergence that could also explain changes precipitation (as well or even better than local PBL drying/deepening alone)? Much of the manuscript assumes that changes are local, but in fact non-local changes linked to circulation could also play an important role in the shown changes and relationships between those.
  8. Section 4 and Figure 9: Much of this section focuses on analysing the impact of irrigation (page 895 line 15 until page 896 line 19). I would recommend having this as an additional subsection within the results (i.e., a new section 3.5) rather than merged in the conclusions. Moreover, Figure 9 is difficult to read, especially the lower row, as many dark dots mask lighter dots, and it is difficult to verify the claims made from p.895, line 25 onwards. Could these results be presented in a different way? For instance, one could use multiple boxplots in the respective colors for different ranges of initial tree cover; the choice is of course left to the authors.
  9. Table 1: The date format is not intuitive and changing it would facilitate understanding. Also, could you distinguish between crops that already existed and those that have been implemented (e.g. with an “\*” or with bold or italic font)?
  10. Figures 1 and 2 display a smaller domain than the other figures, which extend



further south. Although this is not really a problem, I was wondering if there was there any reason for this.

## Technical corrections:

- Page 889, line 1: “with precipitation is centered” -> remove “is”.
- Page 893, line 13: I assume “SD” means “standard deviation” but I think it has not been defined.
- Page 894, line 10: replace “,” with “:” ?
- Page 894, line 25: replace “and” with “an”.
- Page 896, line 1: “SH-PBLH” should be defined.

## References:

- Lorenz, R., and A. J. Pitman (2014), Effect of land-atmosphere coupling strength on impacts from Amazonian deforestation, *Geophys. Res. Lett.*, 41, 5987–5995, doi:10.1002/2014GL061017.
- Lejeune Q., E. L. Davin, B. P. Guillod, S. I. Seneviratne (2014), Influence of Amazonian deforestation on the future evolution of regional surface fluxes, circulation, surface temperature and precipitation, *Clim. Dynam.*, doi: 10.1007/s00382-014-2203-8.

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