hess-2013-480 comments

General comments:

I read through the manuscript (MS) by Knox et al. with interest. This study uses a coupled terrestrial ecosystem model and atmospheric model to investigate the impacts of land conversion on regional hydrology. The work highlight the land conversion induced patterning change in precipitation and bias in evaporation and runoff. It will enhance the understanding of land-atmosphere interaction in the context of human intervention and will benefit the Earth System Modeling community if the authors present their work and convey their key messages in a more communicative way. The subject is within the scope of HESS, and would be appealing to the peers. However, the significance of this study is not clearly seeable as it currently stands, it will be appreciated if the following concerns have been addressed.

Specific comments:

1. The motivation of this study is not clearly presented. The authors mentioned some intent in the Introduction, but justification of this work is not explicitly conveyed to readers. I believe the authors have good reasons to defend why they have done this work, please articulate these reasons and let the readers know why this work is of interest and importance. For example, the authors could talk about why Amazon forest is important, why using a coupled ED2-BRAMS model is superior, why people should concern about effects of land conversion, what new questions have been investigated that have not been examined in previous studies, and etc..

2. What are the new findings of this study? As it has been pointed out by the authors in the Introduction, there are many studies on the deforestation in Amazon forest. Then what new messages or novel insights this study provides to the science community and how this study advances the understanding of land-atmosphere interaction in the context of human interference? These key questions are not well addressed in the MS.

3. How ED2 and BRAMS are coupled is not clearly presently in the manuscript. Is it offline coupling or online coupling? I believe using the coupled system to investigate the effects of land conversion would be an advantage of this work over studies via land surface model which does not take the feedback of land conversion into consideration. More detailed documentation of the coupling system will benefit the modeling community.

4. The quality of climate forcing data DS134 for ED2 is not assessed. It is well known that the accuracy of forcing data is critical for model output, as the uncertainties in the input will propagate through many processes and will be cascaded to the output. An assessment of the climate forcing data will benefit the understanding of uncertainties in the output.

5. Following the 4th point, there is a lack of section discussing the uncertainties and limitation of this study, such as uncertainties in the forcing data and output, whether disturbances have been considered, discrepancy and consistency with previous studies.

6. There is a lack of quantitative analysis supporting speculations in Section 3, especially in Section 3.2. Using more quantitative analysis will make the speculations more solid, it is mostly descriptive narration as it currently stands.

7. This study shows a lot of patterning variations derived from land conversion, whereas the temporal variation is not included, plus the step size of the coupled model is not clearly stated in the MS. I would expect a temporal (seasonality) change in hydrology due to land conversion, and it is also in the scope of regional hydrological difference as it reflects in the title of this MS. If the authors intend not to include the temporal analysis, please justify your choice.

8. To better understand why land conversion would impact the regional hydrology, detailed scrutinizing of the mechanisms in site level will be beneficial. As the authors indicated in the responses to reviewers' comments for the Part 2 companion paper that the two MS would be combined into one MS, then the two case studies would improve the understanding here.

9. The authors introduced MSI in Section 3.3, but the analysis regarding MSI is few and not clear, I do not see the significance of MSI here. Moreover, there is MSI related conclusions (Lines 12-15 in page 15310) in the Conclusions section, however, there is no analysis in the Section 3 leading to that statement.

10. 15 figures tend to be too many, and some of them could be combined or condensed. For example, Fig. 1 and Fig. 4 could be combined. Moreover, as two MS would be combined into one, then the total number of figures should be constrained.

11. Line 2 in page 15308, it is not clear why there is increased cloud in the far east Brazil region. The authors pointed out that the latent heat flux is decreased although I do not see much decrease in that region in Fig. 8, then decreased ET will less likely to generate increased cloud. Is it due to enhanced turbulence derived from increased wind speed?

12. The Conclusions Section does not highlight the new messages of this study to the science community. I would encourage the authors to think more about the scientific contributions of this study to the community, to depict a bigger picture for the significance of this study, such as how this study improve the understanding of land conversion induced hydrology variation, what are the superiority of using a coupled model system, what are the implications of deforestation and land conversion in Amazon forest to water resources in that region, and etc.

Technical corrections:

1. Line 1 in page 15302, what does 1700-1999^2 mean? Is it 1700-1999 km^2 ?

2. Figure 5, it will be beneficial if use dashed polygons to show locations of the dipoles when talking about dipoles.

3. Lines 11-13 in page 15305, the authors indicate that there is a persistent dipole pattern, but I do not see the persistency between 2002-2003 and 2004-2005.

4. Line 8 in page 15306, use AV instead of Actual Vegetation. Please use the acronyms once you introduced them.

5. Lines 12-13 in page 15306, I do not see the patterns of differential ET and transpiration were more pronounced compared with that of precipitation. Besides, are you comparing Fig. 5 with Fig. 8? If yes, please also cite Fig. 5 together with Fig. 8 in Line 13. Similar case for other comparisons.

6. Lines 17-end in page 15307, I do not understand the logic of this paragraph. The authors first present the wind speed has the potential to enhance surface heat and energy flux, but later indicate that it is unlikely. However, the authors do not explain why this happen.

7. Line 1 in page 15308, the authors indicate the latent heat flux also has strong decrease in the far eastern Brazil, however, I do not see significant ET decrease for that region in Fig. 8.

8. Heading of Section 3.3, how "significance" is defined in this study? Is it different meaning from statistically significant ($p < 0.05$)? Please specify to avoid misleading.

9. Lines 22-24 in page 15309, it is not clear how do the authors get to that statement. Besides, I would expect MSI have apparent effects on ET, an analysis between ET and MSI might be of interest to readers.

10. Fig. 2, "early tropical", "mid tropical" and "late tropical" in the label of Y-axis is not clearly defined in the figure caption.

11. For the color ramp, intuitionally red color stands for less water and hotter and blue color stand for more water and cooler, but currently the color ramps are the opposite for many figures.

12. Figure caption for Fig. 8, is it total transpiration and ET during 2002-2005 (sum of ET in year 2002, 2003, 2004 and 2005)? It is not clear, and it is not clear what "Totals" stand for here. Similar case for other figure captions. Please specify explicitly.