HESS-2013-481 comments

General comments:

The overall quality of the revised manuscript (MS) has been substantially improved, including the logic of story line, the organization of sections, the readability of narrative, the presentation skills as well as the justification of significance of this MS. The time and efforts that the authors dedicated to this work are much appreciated. I believe the merits of this work – coupled ecosystem-atmosphere simulation, scrutinization of land-atmosphere interactions via two case studies, detection of dipole pattern in hydrometeorology – will also be appreciated by the community. In response to the authors' hope, yes, the substantial changes made to this work have rectified my major concerns regarding the original two companion papers in HESSD. It is my hope that my comments and particularly the author's dedication could enhance understanding of the feedback of human-induced land conversion to hydrometeorology in the very important Amazon region, and advance progress of science. I recommend acceptance after addressing some minor issues below.

Specific comments:

1. It might be worthwhile to mention why the Northern South America domain and especially the Amazon region is important in regulating the global climate (e.g., via water cycling or carbon cycling) in the Introduction section, to strengthen the motivation of this work.

2. It is good to see evaluation of model performance by comparing against a range of observations. In Figure 23, it seems there are large discrepancies between the model results and Jung Ensemble L, the readers might benefit from an explanation of possible reasons for the disparities.

3. The word "equilibrium" appears a lot in the MS, but it is not clear how the authors define this term in your work. In some ecosystem models, equilibrium refers to NEP=0 or NPP=RH. Please make it clear.

4. Following comment 3, the authors compared the model estimated equilibrium AGB and BA against measurements in Appendix A2, I speculate that the authors assume that the forests where the measurements were taken are in equilibrium. If yes, please clearly state this in appropriate section. In addition, what does "equilibrium soil moisture" in line 767 mean? If I misunderstand anything, please also clarify.

5. When the PV and AV are simulated via ED2, how do the authors compromise the spatial resolution mis-match between different datasets such as DS134, SIMAMAZONIA?

6. In the 4-years coupled simulation, do the authors assume that the AV and PV simulated by ED2 are static during 2002-2005? If yes, this might need to be discussed in the uncertainty section. Please clarify.

7. Regarding the use of “significance”, if it does not refer to the traditional statistical meaning ($p<0.05$), the author could use synonymous words such as “apparent”, “noticeable”, “obvious” or any other words that the authors think might be more appropriate to avoid misleading. Alternatively, if the authors choose to use this word, please let the readers know that it does not refer to $p<0.05$.

8. Regarding Equation (1) and (2), is the σ of each variable calculated from its corresponding 4 annual values (2002-2005)? If yes, the readers might complain that the sample size is small, but it would be understandable due to limitations in data availability and expensive computation load and so on.

9. In the beginning of Section 4.3, the authors raised a hypothesis that “differential precipitation response is driven by differential surface energy fluxes associated with the land-conversion”, but later on the author do not answer that whether it is valid or not. Please make it round.

10. In Section 5.5, the authors might want to include the uncertainty that cascaded from forcing data.

11. In recent years, an issue regarding the effects of elevated CO_2 concentration on stomatal conductance and consequently on ET and runoff has been raised in the community (e.g., Lammertsma et al., 2011; Gedney et al. 2006). It is not clear whether the stomatal closure effect as well as the fertilization effect of elevated CO_2 concentration has been considered in the ED2. If not, this might lead to bias in the outcome of AV and PV from the ED2, and then the author might need to discuss this issue in Section 5.5.

12. In Conclusions, the author highlight that differences in precipitation at the site Pará are more connected with localized difference, whereas those of Gran Chaco is more manipulated by teleconnections. The authors allude to this in lines 854-872, several additional sentences either in this part or Sections 4.2-4.3 providing the readers a more intelligible context will be helpful, as this is an important finding and should be stressed rather than be diluted.

13. Recall my comment regarding the representativeness of these two sites for the original Part 2 companion paper in HESSD, while the finding regarding these two sites – the precipitation pattern differences in one site is mostly manipulated by local effects, the other one has also been affected by teleconnection – is not applicable to other regions, does it transferrable to areas in the Northern South America beyond these two sites? I leave it to the authors whether to add some commentary on this.

Technical corrections:

1. Line 40, "(Nepstad et al., 2001)" should be "(Nepstad et al., 1994)", it would be two "(Nepstad et al., 2001)" in the citation otherwise.
2. Line 55, change "This enables a better understand" to "This enables a better understanding of".
3. Line 288, does "the start" here refer to 1900? If yes, please let the readers know that.
4. Line 378, "a sliver of space in southern Bolivia", it looks more like "Peru" rather than Bolivia.
5. Lines 518-520 and Lines 523-525, the former says human land use promoted a complete collapse, whereas the latter says land-conversion has not lead to a collapse, it seems there is a paradox.
6. Line 529, "was dominated leaf evaporation and transpiration", it should be "was dominated by leaf evaporation and transpiration".
7. Line 542, lower left panle of Figure 08 does not directly support the point you are making (e.g., decrease of EF might be due to increase of H; decrease of EF does not necessarily mean decrease in leaf interception surfaces).
8. Line 553, I think it should be "the surface albedo **increases**" rather than "the surface albedo **decreases**".
9. Line 571, I believe it is "while leaf evaporation and transpiration equally combined to represent the other half."
10. Line 586, it is "indefinitely" rather than "indefinately".
11. Line 612, insert "an" before "increased mean surface albedo".
12. Line 627, remove "the" in the beginning.
13. Line 701, "as precipitation through-fall", here a transitional word such as "whereas" and "however" should be used rather than "as", which is usually used in a cause-effect relationship.
14. Line 720, for the statement "This was verified by observing the model spin-up.", here model spin-up results might not be suitable for verification. More traditionally, people use observations to verify their points.
15. Line 765, either change "weakened" to "increased" or change "throughfall" to "interception".
16. Line 791, it is "highest" rather than "higherst".
17. Line 872, it is Figure 16 rather than Figure 13.
18. Line 958-959, "water availability" here might be open to different interpretations, as in some literature this term refers to the difference between precipitation and evapotranspiration (e.g., Milly et al. 2005). I think the authors intend to mean "soil moisture" here. Please clarify.

19. Line 1116, I think it is an "overestimation" rather than "underestimation", please double check.
20. Figure 08-09, EF is "L/(L+H)" rather than "L/(H+H)".
21. Right panels in Figure 13 and Figure 16, it is more like "differential in vertically integrated total water advective flux vectors" rather than "differential in vertically integrated advection of total precipitable water", please double check. In addition, please provide the site name in the figure caption as well.
22. Figure 17, there should be a period "." before the words "Circle size".
23. Table 03, it is better to use site name rather than "case study 1" in the caption.
24. Figure 20, the comparison of "mean wind magnitude" is shown in the figure caption, but it is not shown in the figure.
25. Figure 22, which version of SRB is used? Please indicate in the text as well as the figure caption.
26. Figure 21-24, the y-axis labels are not consistent.
27. Several awkward sentences might be confusing or misleading, please rephrase them to statements that the authors exactly want to convey and to be more readily intelligible for a broad range of readers. The rule of thumb is the simpler the better. These sentences include lines 187-191, 228-232, 475-477, 780-784, 794-796, 803-804 (more likely be "the open canopy forest of higher stomatal density") and 944-945.
28. Finally, please carefully go through the whole manuscript to make sure there are no technical errors, typos and awkward sentences before resubmission.

References

Milly PC, Dunne KA, Vecchia AV (2005) Global pattern of trends in streamflow and water availability in a changing climate. Nature 438:347-350.

Gedney N, Cox PM, Betts RA, Boucher O, Huntingford C, Stott PA (2006) Detection of a direct carbon dioxide effect in continental river runoff records. Nature, 439(7078), 835-838.

Lammertsma EI, de Boer HJ, Dekker SC, Dilcher DL, Lotter AF, Wagner-Cremer F (2011) Global CO2 rise leads to reduced maximum stomatal conductance in Florida vegetation. Proceedings of the National Academy of Sciences, 108:4035-4040.