



Interactive comment on “Effects of land-conversion in a biosphere–atmosphere model of Northern South America – Part 2: Case studies on the mechanisms of differential hydrometeorology” by R. G. Knox et al.

Anonymous Referee #1

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General comments:

Although it is not a novel topic, it is still good to see this article exploring the feedback of land use and land cover change on hydrometeorology, as a lot of land surface models are lack of this consideration. The subject is within the scope of HESS, and it could be appealing to the science community if the authors interpret the results well. The authors made a lot of efforts to investigate the consequences of land-conversion and the feedback on hydrometeorology, nevertheless, many deficiencies and flaws in

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this paper make it hard to follow the story line and see the significance of this study. There are many concerns should be addressed before consideration of acceptance for publication at HESS.

Specific comments:

1. The motivation of the two case studies is not clearly justified in the Part 2 paper, and the significance of this study to the science community is not presented.
2. The conclusions drawn from the two case studies are not necessarily applicable to other regions due to high heterogeneity of landscapes and climate in the nature. Then, to what extent would these conclusions be legitimate in a broader area, or only valid in the two sites involved in this study?
3. What are the superiorities or distinctions of this study over many previous studies on the impacts of land-conversion on climate or hydrometeorology? What's new information that has been missing in previous studies has been provided in this study? Any knowledge gap has been filled? The authors should think more about how this study advances science instead of just listing out the results. Otherwise, it's just another model application study and may not be worthy of publication at HESS.
4. Apart from Appendix A in the Part 1 paper, there is no validation for the estimation of energy and water fluxes (e.g. latent heat flux, sensible heat flux, evapotranspiration, runoff, soil moisture) at the study region, which are fundamental and critical for model simulation. An assessment on the accuracy or uncertainties of those fluxes at the study region or at site levels is needed.
5. Too many speculations in the text, the authors should use quantitative data analysis to support your points or base on the peer's work to corroborate your thoughts. Besides, the number of references in this paper is very small, more peer's work should be involved, either to raise contradicts or confirm your points.
6. The Conclusions section is not concise and straightforward. Only the key findings

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and implications should be presented and explicitly conveyed to the readers, but it is not the case as it stands; a lot of information should be in the Discussion section instead of Conclusions.

7. The way that the authors tell the story is not enjoyable and sometimes is hard to follow. The organization of text is loose and the text itself is not concise and sharp enough, some description of the results should be condensed. Too much information compiled together without clear story line and condensation would make the reader lose track. Make sure the story line is clear and straightforward. Besides, land-atmosphere interaction flow charts would be very helpful for readers to better understand the essence.

8. Too many figures, some figures can be combined or compressed.

9. There are a lot of errors in the text, I pointed out some visible errors in the section “technical corrections”, but there might be more. It is the authors’ responsibility to go through the entire paper carefully and make sure there are no errors.

Technical corrections:

1. A lot of acronyms (e.g. AGB, DBH, LT in Fig 2) have not been introduced in the first use.

2. Fig 5, left panel does not have label on x-axis.

3. Page 15342, line 12-13, conversion of forest to pasture, normally surface albedo will increase, however, here you indicated decrease, wrong?

4. Fig. 6. The description of the lower left and lower right panel in the caption should be the other way around. Moreover, it is shown that the ET map is for case AV in the figure caption, but “PV” is shown in the lower left panel, not consistent.

5. Table 1. For ΔMpw , change is relative to what (the end of the month relative to the beginning of the month?)? It is not clear. The same case for Table 2.

6. Page 15343, line 6, “both sites” refer to the two sites in case study 1 and case study

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2? However, this section only talks about site 1.

7. Fig. 8. The second line of the title, "Actual Vegetation case (PV)" is not consistent. Please indicate explicitly that it is referring to AV or PV.

8. Fig. 9. What's the time period for these counts?

9. Page 15344, line 23, the first "the" is redundant.

10. Page 15344, Line 22-28, how do the authors get to this statement that "the moisture flux at this location is divergent and the rainfall is convectively driven" and "more moisture divergence in PV scenario"? What do the "factors" in Line 25 refer to? The logic in this paragraph is not very clear.

11. It's not clear why some analyses are done for one site but not for the other, e.g. difference of soil moisture profiles between PV and AV scenarios. Is it because soil evaporation is more dominant in site 2?

12. Page 15347, line 1 "even with less incident short-wave radiation", if the cloud albedo decreased, it's supposed to have more incident short-wave radiation in the AV scenario, explain line 1.

13. Page 15347, line 13, it should be more energy transferred back to the atmosphere with the presence of land-conversion, as the net radiation is decreased (shown in page 15346, line 29).

14. Fig. 15. The description of Upper left panel in the caption should be "PV" rather than "AV", similar case for lower right panel.

15. Page 15347, line 19, "it is shown by a red line in Fig. 15", no red line is delineated in the Fig. 15.

16. Fig. 16. For the second line of the title, "Actual Vegetation case (PV)" is not consistent. For the sixth line, "Quivers are scaled to 20 times", but what is shown in the figure is (Magnified 2x), not consistent.

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17. Page 15351, line 22, should be “increases in land surface albedo, decreases in leaf water interception”.

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