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Hydrology and Earth System Sciences
Discussions

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.

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General comments by the authors in response to the anonymous referees. All referee comments are presented with quotes, author comments without quotes.

The authors would first like to thank the anonymous referees for their time and diligence in reviewing our manuscript. Both referees provided thoughtful and useful feedback. Their consensus indicates that the manuscript would benefit from 1) being condensed into one paper, 2) removing a great deal of ancillary analysis that does not contribute

to the central narrative, 3) clarifying, motivating and identifying the significance in each element of the central narrative and 4) addressing technical problems. The authors agree with the referees suggestions, and are in the processes of making best efforts to accommodate these suggestions and make this a better paper. This includes condensing the double manuscript into a single streamlined manuscript. Author responses to specific comments follow:

From Anonymous Referee #1, 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 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."

Part of this deficiency may be explained by the format of two separate paper submissions. Part of this deficiency may exist in that it was not properly articulated. The regional analysis of hydrologic fluxes identified areas where consistent differences (namely precipitation) manifested between the two simulations. This analysis of the two focus areas to sought out to understand and explain the physical basis behind this. As far as significant goes, these two case studies highlight that the change in precipitation can be manifested from primarily localized effects (driven by changes in equivalent potential temperature as a result of surface albedo) as well as shifts in the large scale continental circulation (as evidenced by changes in advective water flux, ie the Gran

Chaco Site).

Revisions to this manuscript would address this by emphasizing this point in the conclusions.

"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?"

It is true that the scope of this paper does not extend to generalize the results of this study, and make extensions beyond the region of interest (Northern South America). This paper attempts to use the actual disturbance history of the region to compare with a potential. The various regions of the earth have experienced distinctly different disturbance history. It also does not preclude using these results to form a generalized comparison, which could be reserved for future 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."

The major advancement detailed in this manuscript is the incorporation of an advanced land-surface model as the lower boundary of a coupled regional simulation, and particularly more realistic representation of human land disturbance. It is believed that this is the first simulation of its kind that evaluates the land-atmosphere hydrometeorological effects with land-conversion at this granularity and with a realistic treatment of forest succession. For instance, the ability to generate the regional patterns of above ground biomass from a fully demographic and dynamical model are unprecidented.

A great deal of this paper emphasized the non-linear hydrologic response of the land-surface: such as the completely different through-fall response to land-conversion in the two cases, or the seasonal timing of latent heat release and moisture storage which are regulated by vertically resolved model constructs (such leaf evaporation and soil moisture uptake). However, it is acknowledged that the authors could make more efforts to elucidate how these processes actually effect the land atmosphere coupling.

"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."

This is a valid point, and has been considered by the authors and could be provided. There has also been significant emphasis in the review to reduce the content in the paper as a whole and significantly reduce the figure count (although that was mostly requested by reviewer 2). Products are emerging that combine eddy flux networks with remote sensing and machine learning, i.e. Jung et al. 2010. This overcomes some of the sampling deficiencies that have hindered satellite energy flux measurements in the tropics. At the site level, one potential validation source could be the compiled flux data of the Large Biosphere-Atmosphere Distributed Model Intercomparison Project (LBA-DMIP). A comparison products such as these will be investigated and one could be chosen.

"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."

This may be speaking to using a fully mixed model of the atmosphere as thought experiment to understand moisture convergence and divergence and how it influences pre-

cipitation. While this may be conjecture and should be addressed/removed, it should not affect the "take-home" message revealed in the data. For instance at the Para site in September 2003, precipitation rates were higher in the potential vegetation simulation. However for that month the net divergence of total precipitable water was both negative and more negative than the actual case. Moreover, the mean advective flux vectors show that upwind moisture transport into the region of interest is lessened in the potential simulation. These are very strong arguments for the final conclusion that increases in potential scenario precipitation are driven by surface fluxes.

That being said, upon further review of the manuscript by the authors, examples of unsubstantiated conjecture exist. Such statements need to be removed or amended to connect with the results either from this study or published manuscripts.

"6. The Conclusions section is not concise and straightforward. Only the key findings 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."

The general flow of the paper is being completely re-worked. For instance a section dedicated to discussion is being introduced. The conclusion section has been reserved only for the concise description of key findings.

"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."

An attempt has been made to improve the readability. Specifically, we have removed figures that have not contributed to the central findings of the paper. Previously, the two case studies were presented in sequence. The format has been changed. such

that each section addresses a concept and the two case studies are used to address that concept.

The change in flow will be combined with a general re-working of the narrative and fixing technical problems related to figure labelling, ancillary information, acronym usage and condensing figures.

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

Several figures have now been removed in the latest draft. Of note, if the analysis on the land-surface hydrologic balance is not removed in the final draft, this section can be reduced to 3 figures. This would include one figure for each site that has 4 panels, featuring integrated water balance, differential water balance, differential energy balance and evaporative fraction. Several figures related to the land-atmosphere coupling will be removed and are discussed further down in response to reviewer 2.

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.

This is acknowledged. These technical issues should not had made it to this phase. Much of these technical problems are a result of 1) swapping between submitting a single manuscript or a double manuscript (this was originally written as a single paper, which we are now revising per suggestions to again be a single paper), and 2) that the original methods presented differential results as (PV-AV) instead of (AV-PV).

Response to technical corrections:

The authors identified that the many of the technical corrections are very straight forward and can easily be addressed. Two comments are selected and elaborated upon:

"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."

This is somewhat connected to the issues the reviewer brought up in general comments 5. In addition to removing unsubstantiated comments, this section will be reworked for clarity.

"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?"

In the new manuscript, there will be no figures concerning land-atmosphere coupling that are not shown for both sites. Regarding surface hydrology, there may be one exception. The soil moisture time series will be presented for the Gran Chaco focus area, but not for the Para focus area. The reason for showing it at Gran Chaco is to convey how vegetation inpacts the timing of the wetting and drying of the soil column, and that this temporal variability is having an affect on the overall water balance. This is used to partially explain why there is increased soil evaporation in the AV case, even though there is a decrease in precipitation throughfall. This explanation is not necessary at Para. If the referee's and editor feel this would be usefull for Para as well, we would oblige.

Technical corrections:

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

will fix

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

wil fix

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

You are correct. This is an articulation mistake and is being fixed

"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."

These mistakes are being fixed

"5. Table 1. For Mpw, 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."

Yes, this is what is meant. It is the total time integrated flux of mass into the column over the month. This will be clarified.

"6. Page 15343, line 6, "both sites" refer to the two sites in case study 1 and case study 2? However, this section only talks about site 1."

this is misleading, will fix

"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."

This has been fixed*. (*This is a holdover from initially using the reverse method of comparing the two simulations, i.e. presenting the AV case and then showing the PV-AV differential.)

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

This will most likely be removed. But if not, the caption will identify the results are recorded over April 2003.

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

Fixed.

"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."

Agreed, that does not make sense and was the result of poor articulation. The language in this section has been re-worked, this line is no longer present.

"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)."

Agreed, this is confusing. It was intended to mean that less energy "in the form of sensible and latent heating" is transferred back to the atmosphere with increased surface albedo. This has been addressed.

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

All captions have been reviewed, and these problems with incorrect labels have been addressed.

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

Fixed. It was a black box. A red box was used in the following figure, and was correctly added to the reference.

"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."

fixed

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

agreed, fixed.

Anonymous Referee #2:

"General comments:" "This paper follows straight on from part 1, also submitted for review in HESSD, extending the regional analysis of Northern South America in part 1 to two case study sites. Though these case studies are interesting and valuable, they do not constitute a separate study. Therefore this paper has a lack of clear, justifiable, significant conclusions. Neither does it have a methods section, or engage with the literature. Ultimately, this paper does not stand on its own.

"I therefore strongly suggest that this paper should be integrated into the ‘part 1’, also currently in HESSD. As a section in that paper, a concise version of the case studies presented here would be a good addition. There is no apparent reason for the case studies to be a separate paper and the quality of this paper suffers as a consequence." "With that proviso, I support the comments of reviewer #1 and add the following comments. Please note that though the comments below are quite general, they apply to almost every paragraph. Specific comments:" "1. The paper shows the results from the model, but frequently presents only the results, with no context, comparison, analysis or explanation of the significance of the information being given. It is somewhat informative but not enlightening. It is not explained why the reader should care."

Both referees make a valid argument that the paper should be condensed into one paper. This will be done, as the second paper does not stand alone. A combination of efforts will address conveying significance (i.e. why care), reducing the number of figures and reducing text that does not support the central narrative. The key to this is identifying and sticking to the key concepts: 1) emergence of dipole differences in precipitation, 2) the insignificant differences in continental mean precipitation yet significance in regional evap/runoff, 3) key complexities in surface hydrologic feedbacks such as canopy throughfal and soil moisture storage, 4) physical mechanisms behind the differntial precipitation.

"2. Further to the first point, it seems likely that the length of these results could be

reduced considerably (by approximately 50%) were only the most salient ones that added to the argument presented." 3. Similarly, 18 figures is an unnecessarily large amount of figures, most of which are presented with no analysis. By combining figures and removing figures only mentioned in passing, it would be feasible to present this work in 3 or 4 figures."

The number of figures can be reduced. To get the figure count down to 3 or 4 (just concering the second portion of the double paper) would probably require the removal of the land-surface hydrologic analysis. The most critical figures tables from section two (in somewhat order of importance): the tables showing the mean hydrometeorology at the two focus sites, the flux vector maps of total mixing ratio, the maps of differential precipitation and evaporation. The histogram of convective failures could probably be conveyed in text. The profiles of equiv potential temperature and TKE are not considered beyong their effects at the surface, so the TKE value could be incorporated into tables 3 and 4 and the figures removed. The histograms of key convective events can be removed as well.

"4. This paper does not engage adequately with the literature. A much more extensive use of the existing literature on the Amazon regional climate, tropical deforestation and tropical climate is needed. It is unacceptable that most assertions are left unsubstantiated. They should either be properly referenced from the literature or explicitly refer to the results quantitatively."

The bulk of literature review was presented in paper 1. The re-combining of the two manuscripts, and how they present an overview of literature will have to be re-evaluated. Most scrutiny will be placed on the discussion and interpretation of results, and comparing these results against the results of peers.

The author believes that the technical issues brought up by the second reviewer are all addressable. The authors have experience with colorbrewer and can use these color schemes. One note; some of the map figures used multiple panels of the same

map projections, in these cases the axis labels were omitted from interior plots to save space. The authors felt it reduced clutter, and preserved the ability to reference position on the map. However, we can defer to the suggestion and add axis labels to the interior panels if that is the consensus.

"1. I would strongly encourage the authors to review the figure colour schemes. Figures 1, 6 and 15 particularly would be very difficult for someone who is colour blind. A diverging sequential colour scheme would also make it easier for all readers to see the positive/negative areas. You may want to consider www.colorbrewer2.org for finding alternative colour palates. 2. The colour schemes in the figure plots (e.g. figure 9) are also not very colour blind friendly. Neither is the colour explained in the figure caption or the legend. 3. The map figures (1, 6, 15) need units and axis labels. 4. Many of the figures need explanations of the acronyms used (AGB, LAI, ET, LT, etc.) in the figure caption. 5. Many of the figure numbers are out of sequence compared to where they are first referred to in the text. 6. The use of acronyms for the two simulations (AV and PV) should be explained once at the beginning and then used consistently thereafter. The use of 'converted lands', 'forested', 'deforested' etc. does not aid comprehension. 7. Similarly, the naming of the two regions should be consistent. As it is, Site 2 is referred to variously as 'Chacco', 'Site 2', 'Bolivia', etc. This is confusing for the reader."

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