

# Author Response to Reviewers (HESS 480): Hydrometeorological Effects of Historical Land-Conversion in an Ecosystem-Atmosphere Model of Northern South America (CHANGED TITLE AFTER CONDENSING TO SINGLE MANUSCRIPT)

July 21, 2014

The authors believe we have addressed the points brought up by the editor and reviewers. Per requests by reviewers from the companion manuscript to HESS-480, this manuscript has been condensed into a single manuscript. The following changes have been applied to this new version:

To summarize the major changes: 1) The manuscript was combined from a double paper into a single paper. 2) A significant number of figures have been removed 3) The general structure and flow of the narrative has been changed to improve readability. This includes making the results section both more concise, ordered and providing only information relevant to points in the discussion and conclusion. 4) A significant effort has been made to generally improve readability 5) The discussion section now has a clear set of topics that are supported in the results section 6) The conclusions section is now more concise and direct 7) Figures that were mentioned in the comments have been addressed for technical improvements, some figures have been combined as well 8) The reviewers' list of technical comments have been checked off. 9) Added validation with Jung et al. 2010 land-surface flux data 10) Images using the "jet" color scheme were evaluated through a limited color perception filter. With limited color sensitivities to red, green and blue, the defining features of these plots are adequately represented. After reviewing the alternative palettes on colorbrewer, the authors agree that their alternative color palettes provide good contrast and color maps for people with limited color sensitivities. At this time, we will hold on changing the color pallet, unless the reviewers and editors deem the change is necessary. 11) An appendix describing specifics of the model coupling was added. 12) A discussion section covering uncertainty, variability in lateral boundaries and inter-comparison with other similar research was added. 13) The introduction has been re-worded to enhance and convey the motivation for this work.

The following will address specific reviewer comments. Reviewer comments will be given quotes, author response without quotes.

From Anonymous Reviewer 1:

Specific comments: "1.6 p.15302: This sentence and the next sentence are contradicting. Please clarify."

This has been corrected, scaling only occurred from 1990-1999, the discrepancy was an artifact that resulted from a previous mis-communication between co-authors.

"1.7ff p.15312: Please clarify this paragraph. What is the criteria that you used for stating that this comparison is good."

This statement was indeed misleading and unclear. This paragraph was re-written: "Making a rigorous comparison of the model estimated cloud water and observation is a significant challenge. Consider that the simulation time frame does not overlap with the CloudSat mission time-frame, so these comparisons are treated as proxies to climatology and not weather validation. CloudSat measurements are known to have signal loss, attenuation and clutter during moderate to intense rainfall; events such as these could not be filtered from the comparison. It must also be assumed that the cloud classification algorithm is not without error. Nonetheless, the purpose of the comparison was to get a sense of whether the simulations estimated

reasonable mean ranges of water contents and cloud fractions, and also if the phase transitions (liquid to ice) were occurring at reasonable elevations.”

Technical comments:

“1.18 p.15300: It should state ...Quesada et al. (2011).”

Corrected

“1.14 p.15302: The sentence should start with Regional maps....”

Corrected

“1.6 p.15303: Please clarify that you refer to Table 1 in Baker et al.”

Corrected by removing the confusing reference to Table 1, reader is directed to the papers as a whole.

“1.21 p.15309: It should read ...of the significance....”

This paragraph has been re-worded, the manuscript was also checked for ”double the’s”

“1.24 p.15310: It should read Spatial maps of the....”

Re-wording has removed this text

“1.24 p.15311: It should read ...output matches....”

Re-wording has removed this text

Response to anonymous reviewer 2:

Paraphrasing: ”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.”

General response to reviewer: A considerable amount of work has been put into making this document more concise, and making it follow a central narrative more closely. It is our hope that these major changes will rectify the concerns of the reviewer.

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..”

The introduction has been re-worked. Specifically the first paragraph seeks to quickly identify the significance of Amazonian deforestation, and then use this to motivate the need for the current research.

“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.”

This manuscript was combined with it’s companion manuscript, and the major conclusions were presented in a more concise format. To do this, the total amount of text in the conclusions was paired down, and consequently more appropriate topics for discussion were moved to the ”discussion” sections. The three major conclusions of this manuscript are 1) the identification that land-use in South America has led to distinctive consistent pattern differences in the regional precipitation, 2) land-use has not had significant differences in continental precipitation but have significantly altered the continental water balance, and 3) that location where strong pattern differences in precipitation were detected were attributing those differences to both local land-atmosphere affects and changes in continental circulation.

“3. How ED2 and BRAMS are coupled is not clearly presently in the manuscript. Is it offline coupling or on-line 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.”

A description of the coupling has been added to a new section in the appendices.

“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.”

The authors agree that reanalysis driver data sets are highly variable, and that the scale of the forcing data, its potential biases and variability can have significant influence on the resulting vegetation structure.

In response to the reviewers, there are several points that are now reinforced in the manuscript: 1) The focus of this study is to evaluate land-atmosphere hydrometeorology and the spin-up (that uses the DS314) only serves to generate the initial condition, 2) other driver data sets (including the native NCEP reanalysis and native ERA40 reanalysis) had been tested with poorer results and 3) regarding the coupled simulation, the lateral boundary conditions provided by the ERA-interim are uncertain and variable, and this uncertainty and variability influence the outcome of this study. This last point has been communicated with a new sub-section at the end of the discussion section.

“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.”

Similar to the response in reviewer comment 4, a new subsection on uncertainty and inter-comparison with other research has been added to the discussion section.

“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.”

The results and discussion sections have undergone major overhaul, removing speculative statements has been a priority.

“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.”

The step-size of the model is now stated in appendix 1 and the table describing coupled model parameters (30 seconds). The authors agree that a temporal analysis has its merits. Now that the manuscript companion papers have been unified, the focus on specific locations take place in the dry season. The choice not to include a regional temporal analysis was a hard one, we have evaluated the monthly precipitation and radiation differential maps, but chose not to present them in the interest of keeping the size of the manuscript down. The narrative and amount of material covered is already very long, and it seems the general consensus among all reviewers is that the number of figures and deviations from the “plot” needs to be reduced. However, in new additions to the discussion section we acknowledge that the differential responses were most significant during the dry-season. This is generally consistent with other findings.

“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.”

Agreed, this has been done. We feel that this is a significant strength of this paper, as it goes more into exploring the mechanisms of pattern differences in regional precipitation than its predecessors.

“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.”

The explanation of MSI has been cleaned up and improved. The authors have clarified why MSI, is significant. In summary, MSI is significant because it indicates regions in the area where plants experience relatively high water stress. As this research focuses on hydrometeorological response, including precipitation, it is useful to have a sense for regions where plant response may be governed by water limitations.

“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.”

The figures have been condensed and decreased considerably.

“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?”

This is an interesting difference, yet we feel it does not really fit within the central narrative of the paper anymore, and the discussion on this has been removed.

To summarize the difference, actual scenario conditions experienced increased surface solar radiation, decreased clouds, weak to no increase in latent heat flux, no discernible changes in surface wind-speed but significant decreases in both sensible heat flux and transpiration. We feel the data is there to investigate this further and uncover the reasons for the difference, but would require an in-depth analysis.

“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.”

The conclusions have been refined such that a small selection of key points are highlighted and clearly presented to the reader.

Technical corrections: “1. Line 1 in page 15302, what does 1700-19992 mean? Is it 1700-1999 km<sup>2</sup> ?” corrected, to “from the years 1700-1992”

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

See below.

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

The dipole in Gran Chaco region is not as clear as the dipole in Para. There is a pattern change occurring. In introducing this pattern, we have changed the language. We refer to the pattern at Para as a dipole. We describe the pattern at Gran Chaco, and leave it to the reader to interpret it as a dipole or not.

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

Decided to use Actual and Potential consistently throughout the article.

“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.”

This was previously referring to the spatial correlation between ET and changes in aboveground biomass, and precipitation and changes in aboveground biomass. This section has been reworked, the text that was at issue is no longer there.

“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.”

It is agreed that this discussion is confusing. It also loses focus, so this part has been removed.

“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.”

This is true. Transpiration had a more significant change, yet differential latent heat flux (ET) was weak. Similar to response to point 6, this discussion has been removed because it loses focus.

“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.”

Significance was established by using a standard score statistic. It is understandable that this may be confused with significance in a probabilistic sense, but no claims to that are made. The authors could consider using a word synonymous with "significance" if the reviewers feel this is an issue.

"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."

This sentence was confusing and has been removed. The intent of that paragraph was to indicate there is some connection between change in precipitation, and ecosystem response. That is why we used the word comprehensive. Susceptibility to water availability is conveyed in the MSI metric. This metric is very powerful, it explains the fraction of time stomata are regulated due to water stress, which is tracked by the ecosystem model.

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

This has been fixed, explanations added to the caption.

"11. For the color ramp, intuitively 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."

The reviewers point is understandable and well received, but it can also be argued that red (hot) colors are traditionally used for positive or higher values, while blue (cold) values are used for low values. The manuscript is consistent in using this system.

"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."

This has been reworded. Left Panels: Mean annual transpiration and evapotranspiration in the *Potential* vegetation (PV) scenario, from 2002-2005 [mm]. Right panels: difference in mean annual transpiration and evapotranspiration between the *Actual* Vegetation case and *Potential* Vegetation case (AV-PV). Left Panels: Mean annual transpiration and evapotranspiration in the *Potential* vegetation (PV) scenario, from 2002-2005 [mm]. Right panels: difference in mean annual transpiration and evapotranspiration between the *Actual* Vegetation case and *Potential* Vegetation case (AV-PV).