Authors Response to Reviewers (HESS 481 Second Round Minor Revisions): Hydrometeorological Effects of Historical Land-Conversion in an Ecosystem-Atmosphere Model of Northern South America

November 2, 2014

1 Responses to Reviewer 1

Referee comments will be shown in quotes.

"This paper explores the hydrometrological differences between a potential vegetation simulation and a simulation including anthropogenic land use change, in the great Amazon region / northern South America. Using four years of high resolution data, the paper explores the regional and site differences between the two simulations. This is a much improved paper from the part 2 version which I reviewed previously. However, it still is not sufficiently clear what the original contribution of this paper is. The results are consistent with what would be expected from the numerous previous regional studies of this area, and do not offer any further insight. This isnt helped by the fact that the results mentioned in the abstract are not well substantiated by the rest of the text (see point 5 below). A thorough re-writing and structuring of this manuscript that highlighted the aspects of originality and explained what the results meant as well as what they were, might be appropriate for publication in HESS."

Authors' responses to reviewer 1 general comments. In response, attention has been placed on removing reference to statistical procedure that was not used. Specifically the word "significant" has been removed completely. The reviewer also emphasizes that there is a combination of weak statistical analysis to support that significant pattern differences are evident, and that they do not believe significant differences are there. The authors have considered this. Firstly, the authors believe that presenting the simulated patterns and evaluating their consistency is of value and may be appreciated by a reader. The annual pattern precipitation anomalies in their own right, have a story to tell. We also believe the standard score is usefull to gauge how consistent the differences (anomalies) are. We do not deny that more statistical analysis could answer new questions, or would be usefull for inquiry beyond the scope of the current study. This manuscript will no longer make the argument that anomalies are significant, and care has applied to make sure that this point is no-longer conveyed.

The reviewer raises questions that the manuscript seems to be generally consistent with previous work, yet may not offer any novel contribution, insight, nor are the results remarkable. In response, as far as the author's know, novel contributions of this work include both making a connection between the severity of hydrometeorological changes experienced and the sensitivity of the ecosystems in question. Moreover, work similar to this has either relied on massive wide-spread (unrealistic) deforestation, or has made a different comparison completely (ie this work evaluated historical land-conversion and isolated the hydrologic effects). Another novel contribution, which admitedly was not emphasized nearly enough in previous versions, was uncovering the the shift in forest functional composition (from late successional to early successional) following abandonement of degraded lands, could significantly alter the evapotranspiration rates of large portions of land. Another novel contribution as far as the authors know, is identifying and characterizing the underlying forces that mitigate the differences in hydrometeorology at the two locations of interest, particulalry emphasizing and contrasting if differential surface fluxes or differential lateral fluxes are the driving force. The authors now emphasize these points in the conclusions and the abstract.

Specific comments by reviewer 1, and author response:

"1. Generally, this paper suffers from frequently presenting only the results, with no context, comparison, analysis or explanation of the importance of the information being given. This makes the paper somewhat informative, but not enlightening. The authors should endeavor to explain what results mean as well as simply explaining what the results are. More acknowledgement of the limitations of the study would also be beneficial."

A better connection between the results and the meaning can and have been made per changes to the manuscript. Moreover, the section in the discussion on uncertainties has been expanded. It also must be emphasized that in an attempt to convey a large amount of information in a stream-lined mannner, which was used to address issues with readability form the first round of reviews, the discussion section where the meaning of results is covered may seem dis-jointed from our presentation of the results.

"2. There are many unclear and/or unsubstantiated assertions, particularly in the introduction. These need to be clarified and properly referenced. For instance, the sentences on lines: 63, 65,67, 77, 80, 89, 105, 107, 130, 147, 381,397, 634,696,842,"

The reviewer has indeed identified a number of instances that were overlooked, and assumed to be common knowledge. Each of the sentences indicated by line numbers has been re-addressed and fixed. The manuscript has been re-read for the purposes of unsubstantiated and unlear assertions as well. Many new references have been added as well.

63: added references forest cover has different albedo than bare ground, Chappin et al.
65: removed "cooling", add Eltahir and Bras reference 1993
67: modified discussion on precipitation interception to reflect the complexity of the process, and added several references
77: modified to emphasize changes in surface infiltration connected to agricultural practices, and cited key references
80: added reference to seminal paper by Raupach
89: re-worded, improved clarity
105-107: re-worded, improved clarity
130: re-worded, improved clarity
147: included examples
381: added correlation
397: removed suggestion that differences were significant, re-worded
634: clarified reference to reduced "equivalent potential temperature profiles"
842: increased boundary layer growth was not observed in these simulations, re-worded to reflect that increased sensible heat flux and turbulent kinetic energy was associated with the actual scenario. Also added references to Wang et al. 2009 and Fisch et al. 2004. to continue discussion on the meaning of these anomolies regarding convective triggering.

"3. It is not established that the Potential vegetation scenario vegetation was in equilibrium," We ran the model for a maximum of 508 years (15002008), and we assumed equilibrium if: 1. The polygon became a desert, or 2. We tracked the changes in biomass from one year to 40 years later, and assumed equilibrium if the biomass was the same within 0.5% tolerance for each PFT separately. We used one point every 40 years, because if the site burns, then the biomass never reaches a steady state equilibrium, but rather a dynamic equilibrium. Under these situations, the spin up process was deemed completed, otherwise the simulations continued to 2008.

This explanation has been included in the text.

"as its not mentioned whether the dataset used has a climate warming signal over the 35 year period that it is looped through. How much was the vegetation varying year to year at the end of that simulation?

The DS314 served as the forcing dataset for the spin-up procedure. This dataset uses downscaling and bias correction techniques on the NCEP reanalysis, the interannual time-series of that data is provided in Figure 8 Sheffield et al. J Climate. Volume 19. July 2006. The inter-annual trend in temperature originating from the NCEP was preserved through temperature bias corrections (using CRU) and downscaling. The interannual variability in mean 2m air temperature at South America was strong compared to the long-term multi-decadal trend. In model simulation output, we also did not see any "saw-tooth" patterning in time series biomass which would have 35 year periodicity if warming trends were significant.

"It would also be very helpful for the reader if figure 01 included the PV and AV vegetation distribution maps. Currently it shows human land use, but it isnt possible for the reader to see whether those areas would otherwise be forest, or grassland, or etc., which will affect how large a difference one would expect between the PV and AV scenarios."

Maps of potential biomass and the differences in biomass from the actual scenario are in Figure 2.

"4. Although the term significant is used frequently, it isnt clear how the statistical significance is (or isnt) established in this paper. The standard score is used, but its not clear why the t-statistic hasnt been referred to and used, given that it seems unlikely that the population parameters are known. Having said that, obviously a better choice would be the Wilcoxon rank sum test (see Sawilowsky 2005). However, it is very dubious whether any statistical test will yield meaningful results from only 4 years of data, given the level of noise and the spatial and temporal autocorrelation. I would recommend acknowledging this more explicitly and stating clearly that the results of the tests were not significant. If the authors prefer to try to show that the results are significant, then the maps all the other data presented needs to be properly statistically tested and the results stated."

This is acknowledged, and the use of the word significant has been removed. Please see general comments above regarding the meaning we find in the analysis.

"5. The abstract says (line 16) that the land use change significantly affects the precipitation. However, looking at figures 3, 4 and 2, it seems clear to me that at a regional scale, the signal of the anomaly is not consistent (fig 4), that the spatial patterns are not consistent between the four years (fig 3) and moreover that such patterns that do emerge in figure 3 are not well correlated with the representation of the land change given in the right hand column of figure 2. If there is a significant relationship, or even a correlation, it needs to be much more clearly shown."

Please see the general response to the reviewer in response. Specifically to this point however, the authors agree that significant is not the correct terminology to be used. However also note that the abstract may had been mis-interpreted, although we acknowledge it falls on the authors to make such points sufficiently clear. The old text read: "Model output is assessed for consistent and significant pattern differences in hydrometeorology.

Results show that South American land conversion can drive consistent changes to the regional patterning of precipitation.“ At the locations we have identified for focused analysis, the anomolies did show consistency over the four years of evaluation.

Technical corrections

”1. I would strongly encourage the authors to review the figure colour schemes, particularly for the maps. The current color scheme used in the maps is not accessible to those who are color blind, etc. Please consider using a color blind friendly color scheme (see for instance Light and Bartlein (2004), for further information on this subject).”

The color images using the jet gradient scheme (the one in question) have been converted to a red-white-blue spectral, which should approximate Type D in Figure 2 of Light and Bartlein 2004 (EOS).

”2. The authors need to take more care with the use of the word significant. Here, it mainly appears to be used to mean larger. Ideally, it should only be used in the context of meaning statistically significant, followed by the method of testing (ttest, Wilcoxon rank sum test, etc.) and the p value.”

This has been addressed and the word significant (which had indeed been intended to convey larger) has been removed.

”3. I would be helpful if the authors would explain the rationale behind eschewing the usual term for a different between two simulations anomaly in favour of differentials.”

An anomaly is used to express a deviation from normal. In this manuscript, a positive change is a deviation from a hypothetical (ie the natural scenario). ... However, after considering this, the usage of the word anomoly instead of differential may be more approachable and intuitive for the reader, and the meaning may not be misconstrued. Differential has now been changed to anomaly in most all cases.

”4. Line 55:”

Removed, odd sentence.

”5. The references are all rather awkwardly formatted, with brackets around each reference and spaces missing between the text and the reference.”

Spaces have been added and the latex natbib formatting has been modified for multiple continuous citations.

”6. Line 797: this is not an appropriate use of the word biodiversity. Model parameterization of plant structure is (presumably) what is meant here.”

Changed to: ”Intersection of Seasonal Hydrology and Represented Plant Functional Types on Canopy Process”

2 Responses to Reviewer 2

All response to the considerable feedback provided by reviwer 2 will be provided in-line.

"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."

Added to the introduction. Massive deforestation effects through teleconnections, references included.

"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."

This is a good point, an (extended) discussion of the discrepancies has been added.

The greatest discrepancies between benchmark and model output was a latent heat flux bias (higher in the EDBRAMS model) among the three zones covering the Amazon basin. There are several possible explanations for this, outside of uncertainty inherent in the benchmark product. Latent and sensible heat flux contribute a portion of the total energy flux balance through the land-surface, which also includes contributions from change in storage, diffusive ground heat flux, net radiation and the enthalpy contained in the mass flux of precipitation and runoff (enthalpy flux from density and pressure changes can be assumed near zero). Latent heat flux also contributes to a portion of the water mass balance at the land-surface, which also includes precipitation mass flux, change in storage and total runoff. During previous experiments with the ED2 model used in offline simulations, we found that the surface water balance was sensitive to the scale of the precipitation input. When driving the land-surface model with precipitation resolved at coarse scales (such as native NCEP and ECMWF products), the leaf evaporation rates were dispoportionately high. It was found that low but continuous precipitation rates from these products promoted a slow wetting of canopy leaves, and as a result the canopy leaves overflowed to the point of generating throughfall with less frequency and magnitude. Point scale precipitation rates from raingauges in the Amazon showed a much larger variability in precipitation intensity. After using downscaling routines based on Lammering and Dwyer 2000 to preserve the monthly volume of grid-cell precipitation and creating point-scale precipitation intensity, these products (specifically the DS314 from UCAR) elicited a shift in canopy throughfall rates thereby decreasing latent heat flux and increasing surface runoff. Precipitation scale and how it affects the distribution of intensity, storm duration and the time-between storms is a challenge in couple model simulation, that cannot be overcome using the same techniques as offline simulations. The spatial and temporal resultion of the simulations used in this work (40 kilometers with 15 minute time-step between convective precipitation calls) are smaller than the reanalysis models (larger than 1 degree), yet they cannot generate point-scale precipitation. There are various approaches to ameliorating precipitation scale effects, such as using multi-atmosphere multi-land (MAML) sub-grid methods and by employing creative ways at the land-surface to generate throughfall volumes that match observations even when driven with precipitation rates that cannot match those that are observed. Regional and meso-scale couple simulations such as the work presented here, could benefit greatly from advances in this area.

"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."

Agreed. Equilibrium is our sense of the definition is evaluated by long-term fluctuations in biomass. The text has been modified:

We identify equilibrium when the total biomass of each plant functional type in a grid-cell does not change more than 0.5% over a period of 40 years. If equilibrium within this threashold was not achieved, the spin-up was allowed to continue to 508 years before stopping. For reference, ED2 simulations in old-growth central Amazonian forests take roughly 250 years to reach equilibrium biomass.

"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."

This has been addressed by removing the word equilibrium in this part of the text and making some additional notes.

To section "Generation of Surface Boundary Conditions":

The *Actual* and *Potential* boundary conditions utilized a dynamic model process to generate structure and composition. However, when applied to the coupled simulations, the land-surface dynamics including the processes of mortality, recruitment and growth are turned off and only phenology (leaf flush status) is left to vary in time. For simplicity and computational benefit, they forests are treated as static. As will be covered in greater detail in the next sections, the length of the coupled simulations are not long enough to generate large changes in above ground biomass. As an example, citeauthorLewis2006 and citeauthorBaker+2004a estimate that in recent decades, the Amazon has sequestered approximately 0.6 ± 0.2 Mg C ha^{-1} yr^{-1}. Over a course of four years, this is less than 3 Mg C ha^{-1}, which is on the order of 1-2% of total forest biomass.

And now in the Appendix:

Please note that the forest inventory data makes no assumption that aboveground biomass is in equilibrium. This comparison is only intended to evaluate the present day static representation of forest structure. As applied to the coupled simulation, the were treated as static. As stated earlier, the length of the simulation did not merit the need to incorporate dynamics.

"In addition, what does equilibrium soil moisture in line 767 mean? If I misunderstand anything, please also clarify."

Equilibrium soil moisture is not clear. This has been changed to mean annual soil moisture which is more reflective of the actual condition that was intended.

"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?"

The scale of the spin-up was conducted at the scale of the climate data DS314 and land-use transition data (Hurtt 2006). The SIMAMAZONIA product is a 1-km product, and can therefore be upscaled easily by simply counting deforestation and intact cells that fall within each grid cell. Certainly, the scale at which a model simulation is conducted impacts mean results, particularly when there are non-linear processes involved. But it is not clear what is meant by mis-matches. We would be happy to continue correspondance on this issue though.

"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."

This question has been answered through responding to question 4.

"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."

Agreed, the word significant was decided to be confusing and misleading and was therefore removed. Where appropriate, synonyms like the reviewer suggests, are inserted.

"8. Regarding Equation (1) and (2), is the 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."

This has been addressed in the Uncertainty in Model Estimates Section.

"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."

Because no formal hypothesis test is made, the wording was changed to "was questioned" rather than hypothesized. This question has been addressed, specifically see the third paragraph of section: Drivers of anomalous convective precipitation.

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

This has been added.

"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."

The simulations were conducted using present CO2 concentrations, and the model photosynthesis and forest dynamics were calibrated using modern day co2, so this should not be an issue. It is now realized however, that the reader is not given much description of how CO2 has changed (or more to the point, not changed) during the spin-up. The co2 concentration has been explicitly stated (378 ppm constant).

Model calibrations, including photosynthesis parameters, following what was published in Kim et. al 2012, utilized measured carbon dioxided concentrations at calibration sites. The photosynthesis and stomatal models are physically based (based on Leuning 1995), and should respond to the actual canopy air-space CO2, as it impacts the rate of transport through the boundary layer outside the stomata. The atmospheric simulations also used the same concentrations (378 ppm), and while they will not affect forest-carbon dynamics (static biomass) it has the potential to affect water use efficiency.

"12. In Conclusions, the author highlight that differences in precipitation at the site Par a 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."

This has treatment in the discussion and conclusions, and has been attended to.

"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."

While I do agree these are interesting things to consider. The considerable work that has gone into addressing all of the other reviewer comments has taken precedence.

"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."

The duplicated reference to Nepstad 2001 has been removed.

"2. Line 55, change This enables a better understand to This enables a better understanding of."

Sentence structure has been changed due to general cleaning and language improvement.

"3. Line 288, does the start here refer to 1900? If yes, please let the readers know that.

Yes this is true, wording has been updated.

"4. Line 378, a sliver of space in southern Bolivia, it looks more like Peru rather than Bolivia."

This wording has been changed, due mostly to arguments from reviwer 1 that a dipole may not exist in this region. But indeed, there is a positive anomalie that occures in Southern Peru. Changes to the wording are now cognizant of this point.

"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."

The point conveyed here is very subtle, and may be construed as paradoxical. The wording has been updated to clarify this: "Human land-use, as represented in the ED2 model at this specific location, led to a collapse of the estimated tree cover, which includes natural landscapes. This specific site is undoubtedly a more aggressive representation of the differences between the *Actual* and *Potential* scenario ecosystems in this region. Human land-conversion has not lead to a collapse of the Gran Chaco's dry-forest ecosystems for the region as a whole."

"6. Line 529, was dominated leaf evaporation and transpiration, it should be was dominated by leaf evaporation and transpiration."

corrected

"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)."

Good point, this must had been an artifact of referencing that panel in a previous comment. Evaporative fraction increasing would not be directly attributed to decreased leaf area and increased throughfall. This has been corrected.

"8. Line 553, I think it should be the surface albedo increases rather than the surface albedo decreases.

yes, this also a good catch. My wires were crossed, probably because I was thinking of increased absorption of shortwave radiation. Corrected.

"9. Line 571, I believe it is while leaf evaporation and transpiration equally combined to represent the other half.

done

"10. Line 586, it is indefinitely rather than indefinately."

done

"11. Line 612, insert an before increased mean surface albedo."

done

12. Line 627, remove the in the beginning.

There may be an inconsistency between the line number the reviewer is using and the version the author has access too? I cannot identify this sentence, the others have had context.

"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.

This paragraph has been re-worded for clarity and to remove speculative comments.

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.

changed: "This type of behavior was observed in the model spin-up, where newly disturbed patches of land in the Central Amazon reached maximum leaf area over a span of a few decades, compared to the length of time (more than a century) it took for biomass to stabilize."

"15. Line 765, either change weakened to increased or change throughfall to interception."

done

"16. Line 791, it is highest rather than higherst."

done

"17. Line 872, it is Figure 16 rather than Figure 13."

done

"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."

changed to "root-zone soil moisture"

"19. Line 1116, I think it is an overestimation rather than underestimation, please double check."

again, line-numbers in my version seems to differ, but I tracked this to the comparison of short-wave radiation with SRB. This has been changed, the language was backwards.

"20. Figure 08-09, EF is L/(L+H) rather than L/(H+H)."

done

"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."

Names were included in the caption. The actual quantity that was calculated in the advective flux was precipitable water.

"22. Figure 17, there should be a period . before the words Circle size."

done

"23. Table 03, it is better to use site name rather than case study 1 in the caption."

done

"24. Figure 20, the comparison of mean wind magnitude is shown in the figure caption, but it is not shown in the figure."

done

"25. Figure 22, which version of SRB is used? Please indicate in the text as well as the figure caption."

done

"26. Figure 21-24, the y-axis labels are not consistent."

Axis on 23 and 24 have been changed to match.

"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."

Acknowledged and addressed.

"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.