

General comments

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 isn't 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.

Specific comments

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.
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,
3. It is not established that the Potential vegetation scenario vegetation was in equilibrium, as it's 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? 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 isn't 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.
4. Although the term "significant" is used frequently, it isn't clear how the statistical significance is (or isn't) established in this paper. The standard score is used, but it's not clear why the t-statistic hasn't 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.
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.

6.

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).
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 (t test, Wilcoxon rank sum test, etc.) and the p value.
3. I would be helpful if the authors would explain the rationale behind eschewing the usual term for a difference between two simulations – anomaly – in favour of “differentials”.
4. Line 55: *understanding of*
5. The references are all rather awkwardly formatted, with brackets around each reference and spaces missing between the text and the reference.
6. Line 797: this is not an appropriate use of the word biodiversity. Model parameterization of plant structure is (presumably) what is meant here.
- 7.

Light and Bartlein (2004) The End of the Rainbow? Color Schemes for Improved Data Graphics. *Eos*, Vol. 85, No. 40. Available from:
http://geography.uoregon.edu/datagraphics/EOS/Light-and-Bartlein_EOS2004.pdf
Sawilowsky, S. Misconceptions Leading to Choosing the t Test Over the Wilcoxon Mann-Whitney Test for Shift in Location Parameter. *Theor. Behav. Found. Educ. Fac. Publ.* (2005). at <http://digitalcommons.wayne.edu/coe_tbf/12>