Dr. Hannah Cloke
Editor
Hydrology and Earth System Sciences

February 24th, 2015

Dear Professor Cloke,

Re: Manuscript reference **No. hess-2014-453**

Please find attached a revised version of our manuscript entitled "*The water balance components of undisturbed tropical woodlands in the Brazilian Cerrado*", by Paulo Tarso S. Oliveira, Edson Wendland, Mark A. Nearing, Russell L. Scott, Rafael Rosolem, and Humberto R. da Rocha. We are also sending the items: Changes Noted and Response to Reviewers' comments.

Note that original reviewer comments are in black and author responses are in blue throughout. In the "Changes Noted" we yellow highlight for Responses to the comments of Reviewers.

The manuscript has been revised in accordance to the reviewers' comments, which were highly insightful and enabled us to improve the quality of our manuscript.

We hope that the revisions in the manuscript and our accompanying responses will now meet the requirements for publication in *Hydrology and Earth System Sciences*.

Thank you again for your consideration.

On behalf of the authors,

Paulo Tarso S. Oliveira

(Remarks to Author – in black; Responses in blue):

Anonymous Referee #1

The paper is generally well structured and written, the conclusions are supported by the analysis of the data presented. The paper could be accepted for publication after considering my comments below:

We first would like to thank the reviewer for carefully reviewing the manuscript and for his/her positive feedback on our study. We also appreciate his/her suggestions on how to improve the clarity of the manuscript with inclusion of tables and recommended changes to figures.

1) Since the authors analyze data sets from two sites with similar characteristics, a summary table would help the reader to have a clearer view. As it stands now is rather confusing.

2) Page 12992 (sub-subsection 2.1.2): Authors mention here that the some characteristics of the IAB site (density of trees and diversity index) are similar to those found at PDG site and refer to Fidelis and Godoy 2003. It is better to include that information in the previous section, when describing the PDG site (carrying the reference as well).

We summarize the main characteristics of each site in a new table. (Table 1, P22 L632)

3) Page 12994, lines 6-8: The software/tool used in the analysis is irrelevant if the authors are confident in the method. If so, the information can be omitted.

Ok. We deleted the used software.

4) Page 12977, lines 22-23: It is not clear why authors present the results of Juarez et al. (2008) without making any other comment or comparison. Would their results (in Amazon rainforest) be comparable with this work? If so, a further discussion should be included. If not, the phrase can be removed.

We deleted the sentence with the results from Juarez et al. (2008).

5) Page 12977, lines 24-27: How would you evaluate Ruhoff results with this work? Do you have a possible explanation on why the present study results are better for R2 and

RMSE values? Would the issue be in their analysis, flux tower data quality or MODIS products?

Ruhoff et al. (2013) compared MOD16 ET product with eddy covariance data from the PDG site during the year of 2001. The authors found values of $R^2 = 0.61$ and RMSE = 0.46 mm d^{-1}, which were not as good as for the present study results. A better result from our model than from the MOD16 ET product was expected because MOD16 runs across the world while our empirical model has been calibrated locally with specific conditions. In other words, a calibrated local model tends to provide better estimates than a global model.

6) Since there is flux data available from PDG since 2000 (used in Ruhoff et al.), why that particular year was not included in this study?

In fact, Ruhoff et al. (2013) compared MOD16 ET product with eddy covariance data from the PDG site only for 2001. Data from the PDG site are available for 2001 through 2003 (see da Rocha et al., 2009). We corrected it in the text. (P10 L274-275)

7) Page 12998, lines 13-14: Authors mention that they observed a significant number of rainfall events in the dry season. Would you label this particular dry season as atypical? If so, would that compromise your analysis?

We found a total of 333 mm in the dry season of 2013 (which is similar to the historical mean in this season of 307 mm) and 92 mm between the months April through July of 2014 (Fig. 3a). (P10 L291-293) Thus, we cannot classify this dry season as atypical.

8) A final table with the found ET results for IAB and PDG sites would make reader's life easier.

We inserted a final table with ET results for IAB and PDG sites. (Table 5, P26 L643)

9) Figure 1: Background is too busy and it is almost impossible to see the map lines and text. Using a plain light colour would be more appropriate.

Thanks for your suggestion. We improved Figure 1. (P27 L647)

10) Figure 2: This figure is very busy and confusing. It is not possible to clearly see the instrumentation, and it does not add anything to the description given in the text. It can be totally removed without prejudice.

Ok. We removed Figure 2.

Other minor observations and suggestions:

a) Figure 4: I would suggest the use of two different colours instead of two shades (black and grey).

Changes made according to reviewer's suggestion. (P29 L654)

b) Figure 5: I suggest the use of colorblind-friendly colours instead of the chosen ones (red+purple+blue).

Changes made according to reviewer's suggestion. (P30 L661)

c) Figure 6: Also here I suggest the use of different colours to be colorblind-friendly. In addition to that, although it may be obvious for a reader that already went through the whole text, the figure caption should bring the description of the used variables. For example "(...) where P is the precipitation, ET is the evapotranspiration, and dS is the water storage change (...)".

Changes made according to reviewer's suggestion. (P31 L667-669)

d) Page 12990, line 18: "(...) flux tower measurements and vegetation (...)" (no comma).

Text corrected. Thanks.

e) Page 13001, line 10: " evapotranspiration".

Text corrected. Thanks.

Thank you very much for helping us to improve the quality of our manuscript!!

Anonymous Referee #2

In the introduction the deforestation advance appears as the main justification for an article that analyzes only the water balance of a natural savannah ecosystem. This is reflected in the absence of a well-defined goal. The work creates an expectation for the comparison of the water balance in the soil components in a natural savanna site and a near deforested area, which does not happen in the development of the text. The work points the evapotranspiration estimate (by empirical model) as a major focus, when in fact what is presented is the evaluation of the soil water balance components.

We thank the reviewer for all the comments. In the introduction section, we have highlighted the deforestation problem in the Cerrado and the lack of basic information about the hydrological processes in the undisturbed Cerrado. Therefore, to understand pre-deforestation conditions, we investigated several components of the water balance that are still poorly understood for undisturbed tropical woodland classified as "cerrado sensu stricto denso". We believe that our results provide benchmark values of water balance dynamics in the undisturbed Cerrado that will be useful to evaluate past and future land uses in different sceneries of water scarcity and climate change for this region. The importance of such new information/understanding has been recognized by Reviewer #3 for the Brazilian Cerrado.

The paper indicates that the sites represent pre-clearing conditions. But in fact do not represent natural ecosystem?

We believe that our sites are representative of natural ecosystems as both studied locations are characterized by undisturbed tropical woodland classified as "cerrado sensu stricto denso".

The article has a confusing structure in the presentation of the methodology and analysis of results, which need to be better organized. Two sites are indicated as study area PDG and IAB, but only the second has analyzed the water balance effectively.

In modeling evapotranspiration item. In experimental IAB site several micrometeorological data was measured in high temporal frequency. Why not used the penman-monteith parameterization for estimating evapotranspiration, or even a land surface model (e.g. SiB2, CLM3, etc.) that would also examine soil moisture? Recent

studies have incorporated physical considerations in estimating the evapotranspiration o in combination with remote sensing data to study area, such as:

da Silva B. B., Wilcox B. P., da Silva V. d. P. R., Montenegro S. M. G. L. and de Oliveira L. M. M.(2014) Changes to the energy budget and evapotranspiration following conversion of tropical savannas to agricultural lands in Sao Paulo State, Brazil, Ecohydrology, DOI: 10.1002/eco.1580.

We did not have a flux tower installed at the IAB site but fortunately there were eddy covariance ET data available at the nearby PDG site, which had very similar vegetation and hydrometeorological characteristics (as presented in Table 1 in the new version of the manuscript). It was very important to have a well-calibrated model to estimate this important component of the water balance. We chose to use and calibrate the MODIS EVI model as it has been successfully applied to determine ET in several natural ecosystems (Glenn et al., 2010 and 2011) and capably reproduced the measurements at the PDG site. It is important to note that was the first evaluation of this approach in the Brazilian Cerrado. This approach includes both an available energy component ("quantified by ETo") and an estimate of green biomass (EVI) which is an advantage over a pure "available energy" type model like Priestly-Taylor. Likewise, calibrating a more complicated model like Penman-Monteith, or even a land surface model (LSM), would have been unnecessary and much more difficult.

LSMs like SiB2 and CLM3 are much more complex and have to represent several aspects of land surface interactions, including the hydrology but also biogeochemistry, vegetation dynamics. Calibrating such models can be quite difficult and due to interactions between individual parameterizations, "competing" parameter values can pose undesired difficulties to model simulations (please refer to Rosolem et al., 2012 and 2013).

With respect to use of LSMs to infer soil moisture quantities. It is not entirely accepted by the LSM community that simulated soil moisture by such models represent actual soil water content conditions or, instead, a "index of wetness" as reported, for instance, by Reichle and Koster (2004). We reproduce the text for clarity "…, *simulated soil moisture contents reflect the many necessary simplifications imposed in the land surface model and should arguably be considered model- specific ''indices of wetness'' rather than quantities that can be measured in the field …*"

The presented evapotranspiration estimation method does not allow the conclusion "…Our findings indicate that the fitted equation may be used to compute ET at daily, monthly and annual scales. ", only by comparing with literature data, longer time series analyzes need to be considered. This results in a fragile analysis.

We calibrated and validated the model for one scale (16 days) using measured ET from eddy covariance at the PDG site. From this empirical model is possible to compute ET at 16 days and these results may be interpolated and/or summed to estimate daily, monthly or annual values. We changed the text to make it clear to the readers. (P10 L283-285 and P14 L413-415)

In pg 12992, line 20: forest no fores

Thanks. We fixed it.

Pg. 12994: Equation 2 is not correct to describe the function that minimizes the mean squared error.

Equation 2 is correct for an objective function of the sum of squared differences.

Pg. 12996: In equation 4, S is soil water storage.

Done. (P9 L243)

Pg. 13016: Figures 4(a,b,c) are very small and illegible.

We thank the reviewer for making this point. We improved the quality of the figure and the size of the numbers. Furthermore, we also changed the colors in the Figures following the suggestion of Reviewer #1. (P29 L654)

In figure 5 soil moisture data are presented but not cited in methodology. The values of the volumetric water content in the soil (y-axis) are in different scale of variation of which is presented in the text (pg 12999).

We corrected it keeping the same units that were used in the text (m^3 m^{-3}). (P30 L660)

The results show the effect of greatly reduced rainfall over the region in the years 2013 and 2014, which limit their comparison with other times and nearby areas. The great influence of climate variability prevents the comparison of the components of the water balance of the soil, with measurements in deforested/grown areas in different periods.

The annual rainfall during the period of study (1248 and 1139 mm for 2012 and 2013, respectively) were approximately 20% less than the historical mean of the 1500 mm; however, this did not compromise our conclusions because our findings of ET, canopy interception, throughfall, and stemflow were similar to those from previous studies in different periods of study (Tables 4 and 5, P25-26). We did not find other runoff studies in the undisturbed cerrado to make a direct comparison. In addition, periods of hydrological extremes (drought and flood) are often more important than mean values in the water resources studies. These extremes can help to know how a specific hydrological component can explain various changes of the system.

The conclusions reproduce the results without discussions.

A thorough discussion about the results is provided in Section 3 (Results and Discussions). We have limited the Conclusions section to include only the main findings of this study.

Anonymous Referee #3

GENERAL COMMENT:

The manuscript presents new information about water balance over the brazilian Cerrado based on observations. The study analyses a wide range of hydrological fluxes as precipitation, evapotranspiration, interception, surface runoff, infiltration and soil moisture. The authors conclude that 4-20 % of precipitation is intercepted in the canopy, a small fraction runs off and most of the water infiltrates. It was not clear how water flows out the soil (evaporation, subsurface flow or groundwater recharge). Also, observations and previous studies show that removing Cerrado vegetation may generally increase runoff. The contribution of the paper is to bring new information about hydrological processes over an important region (Cerrado) that is still not fully studied. The questions addressed in the paper are important as Cerrado is an important region of Brazil/South America that may experience important transformations, which can cause important impacts over hydrology of major/important river basins. The paper is generally well written, most of the methods are appropriated and conclusions are supported by analyses. I would be pleased to see this work published at HESS. Meanwhile, I have some important comments/suggestions that hopefully will help the authors to improve this manuscript.

We thank the anonymous reviewer for his/her kind words in support of our article and for suggesting some improvements to the manuscript.

SPECIFIC COMMENTS:

-Findings from two sites vs Cerrado:

How these findings (typical values of hydrological fluxes) from 2 sites can be generalized to the Cerrado region? Can it be generalized over a typical catchment of Cerrado? For example, should we expect similar runoff rates at different parts of a catchment (close to a stream or upland)? Or should we expect most of surface runoff generated close to streams at saturated areas, following Dunnian concept of flow generation processes?

This is an interesting point made by the reviewerand we thank he/she for that. We believe that some of hydrological fluxes in the Cerrado may vary in a typical catchment influenced mainly by the water content into soil (e.g. close to a stream or upland). Further, Villalobos-Vega et al. (2014) concluded that water table depth has a strong

influence on variations in tree density and diversity, i.e. regions with deep water tables such as the IAB site (35 m) tend to exhibit greater tree abundance and diversity than sites with a shallow water table. Therefore, if there is variation in the vegetation characteristics we can expect changes in the hydrological fluxes on different parts of a catchment. However, this is not an exclusive characteristic of the Cerrado. In generally, land around a stream (riparian areas) exhibits different hydrological fluxes than in the upland. This is the expected scenario in many catchments.

-Discharge from stream gauges:

The author did a good job in the analyses most of the hydrological fluxes. However, analyses concerning the sinks of soil water are not conclusive. It would be interesting to look at discharge data from stream gauges and convert it to runoff (mm/year) to compare it with the water balance terms obtained in this study. This way, it would be possible to infer about the sink of soil water (evaporation or subsurface and groundwater flow). For example, how overall runoff coefficient compares with runoff ratio obtained using precipitation and discharge from stream gauges? The conclusions concerning water storage can change depending on the results from such analyses.

This is a good point raised by the reviewer. However, we have not monitored streamflow at the IAB site, but surely will be a great point to take into account in a future investigation.

-ET model:

Eq. 4 is a nonlinear function between EVI and ET. But the authors mention that the fitted equation (5) can be used for daily, monthly and annual scales. But as it is not linear, I'm not sure if the equation fitted for one scale (16 days) could be used in other scales (daily of annual).

Our findings indicate that from this fitted equation is possible to compute ET at 16 days and these results may be interpolated and/or summed to estimate daily, monthly or annual values. We have changed the text slightly to refer that. (P10 L283-285 and P14 L413-415)

-Define DBH

DBH is Diameter at breast height, which is the tree diameter measured at 1.30 m above the ground. (P7 L202)

-Figure 4: Please improve quality.

Figure 4 has been improved as suggested by reviewer.

Thank you very much for helping us to improve the quality of our manuscript!!

References

da Rocha, H. R., Manzi, A. O., Cabral, O. M., Miller, S. D., Goulden, M. L., Saleska, S. R., Coupe, N. R., Wofsy, S. C., Borma, L. S., Artaxo, P., Vourlitis, G., Nogueira, J. S., Cardoso, F. L., Nobre, A. D., Kruijt, B., Freitas, H. C., von Randow, C., Aguiar, R. G., and Maia, J. F.: Patterns of water and heat flux across a biome gradient from tropical forest to savanna in Brazil, J. Geophys. Res., 114, 1-8, doi:10.1029/2007JG000640, 2009

Glenn, E. P., Nagler, P. L. and Huete, A. R.: Vegetation Index Methods for Estimating Evapotranspiration by Remote Sensing, Surv. Geophys., 31(6), 531–555, doi:10.1007/s10712-010-9102-2, 2010.

Glenn, E. P., Neale, C. M. U., Hunsaker, D. J. and Nagler, P. L.: Vegetation index-based crop coefficients to estimate evapotranspiration by remote sensing in agricultural and natural ecosystems, Hydrol. Process., 25(26), 4050–4062, doi:10.1002/hyp.8392, 2011.

Reichle, R. H., and Koster R. D.: Bias reduction in short records of satellite soil moisture, Geophys. Res. Lett., 31, L19501, doi:10.1029/2004GL020938, 2004.

Rosolem, R., Gupta, H. V., Shuttleworth, W. J., de Gonçalves, L. G. G. and Zeng, X.: Towards a comprehensive approach to parameter estimation in land surface parameterization schemes. Hydrol. Process., 27, 2075–2097, doi:10.1002/hyp.9362, 2013.

Rosolem, R., Gupta, H. V., Shuttleworth, W. J., Zeng, X. and de Gonçalves L. G. G.: A fully multiple-criteria implementation of the Sobol′ method for parameter sensitivity analysis, J. Geophys. Res., 117, D07103, doi:10.1029/2011JD016355, 2012.

Ruhoff, A. L., Paz, A. R., Aragao, L. E. O. C., Mu, Q., Malhi, Y., Collischonn, W., Rocha, H. R. and Running, S. W.: Assessment of the MODIS global evapotranspiration algorithm using eddy covariance measurements and hydrological modelling in the Rio Grande basin, Hydrolog. Sci. J., 58(8), 1658–1676, doi:10.1080/02626667.2013.837578, 2013.

Villalobos-Vega, R., Salazar, A., Miralles-Wilhelm, F., Haridasan, M., Franco, A. C., and Goldstein, G.: Do groundwater dynamics drive spatial patterns of tree density and diversity in Neotropical savannas?, J. Veg. Sci., 25, 1465–1473, doi:10.1111/jvs.12194, 2014.