Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-57-AC2, 2019

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Multi-decadal Hydrologic Change and Variability in the Amazon River Basin: Understanding Terrestrial Water Storage Variations and Drought Characteristics" *by* Suyog Chaudhari et al.

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Received and published: 4 June 2019

This study applied a physics-based hydrological model and GRACE product to investigate the hydrological changes in the Amazon basin, especially the water storage and how it related to droughts, for 36 years period. The results of this study are comprehensive and the findings are significant, which improve the understanding of hydrology in Amazon. But there are still some concerns in the manuscript need to be addressed.

Response: Thank you for your positive evaluation of the manuscript. We found sig-

nificant improvement in the quality of the manuscript following your and the other reviewer's comments. Below we provide detailed responses to your comments along with the references in necessary locations. Please note that following the first reviewer's comment, supplementary figure numbers have changed in the revised manuscript.

GC1) The first two questions regard the modeling approaches. Firstly, it was mentioned that the atmospheric forcing data are spatially interpolated using a bilinear interpolation method to the model grid. The issue is, for example, rainfall events are usually local and spatially discontinuous, whether the bilinear interpolation is appropriate for some of the climatology data.

Response: The Leaf-Hydro-Flood (LHF) model version we used in this study interpolates WFDEI forcing data from 0.5 degrees to the 1 arc minute model grid ($\sim$2km) using bilinear interpolation. We agree that more sophisticated interpolation techniques, such as kriging, yield more accurate results compared to the bilinear approach, especially with the rainfall data. However, these sophisticated methods also come at a significant computational cost. Moreover, several previous studies (Fan et al., 2017; Miguez-Macho and Fan, 2012; Pokhrel et al., 2013, 2014) along with this study have shown that even with the bilinear interpolation LHF model yield accurate results. Hence, we firmly believe that implementing a more sophisticated interpolation technique would be unnecessary given the current accuracy achieved from the model. Further it would add significant computational burden as the interpolation is done within the model over a very large domain ($\sim$4 million grids).

GC2) Secondly, regarding the LULC change applied to the model, LAI higher than 5 are considered as forest canopy. Then the question is, how does this approach deal with the seasonal variation of LAI as for LULC change?

Response: Thank you for pointing out this issue. The threshold of LAI=5 for the forest transition is the mean annual LAI calculated by aggregating the 8-day composites from GLASS data for every year. We use this mean annual LAI estimates only for deriving the 1980-1991 (years not included in the ESA-CCI data) annual land cover maps. The seasonal variations in LAI are separately incorporated in the model framework at a monthly scale. To elaborate a bit more on the method we used to back extrapolate land cover, we have added more information in Section 2.3 of the revised manuscript. Further, the threshold value was mainly based on the study conducted by Asner et al., (2003) which presents a synthesis of global LAI values for different land cover types. Asner et al., (2003) showed that the evergreen broadleaf and needleleaf forests, which are the major forest types in Amazon, have average LAI values greater than 5 (5.8 and 6.7, respectively). Other studies also classify the evergreen forests in the same LAI range (Myneni et al., 2007; Xu et al., 2018); for example, Myneni et al., (2007) studied the seasonal swings in LAI values and showed that the mean annual LAI is $\sim$5 over the entire Amazonian rainforest (Figure 1A of the citation). Hence, we used the threshold of LAI=5 to get a first-hand approximation of the past forest cover in Amazon.

GC3) The manuscript consists of 5 parts, but the model descriptions in Section 2 should belong to Section 3, methods. Thus, it would be better to re-organize the contents and the structure of the manuscript.

Response: Thank you for the suggestion. We agree that moving the model descriptions to Section 3, methods, will result into a better structure. We thought of improving it further by combining the current sections 2 and 3 together. In the revised manuscript, we have revised the structure as follows, 2. Model, Data and Methods 2.1 The Leaf-Hydro-Flood (LHF) model 2.2 Atmospheric Forcing 2.3 Land Use Land Cover and Leaf Area Index 2.4 Validation Data 2.4.1 Observed Streamflow 2.4.2 GRACE Data 2.5 TWS Drought Severity Index 2.6 Occurrence and Duration of Drought 2.7 Dry Season Total Water Deficit 2.8 Simulation Setup

GC4) In addition, Figures S3, S6, and S8 are not referred nor discussed in the manuscript. Moreover, there are also some specific comments as below.

Response: Thank you for pointing this out. We have added supplementary figure

references for the in required locations in the revised manuscript.

SC1. P3L13-15, some of these ‘more recent’ literature are still more than 10 years old. The author should cite some real more recent papers.

Response: Thank you for the suggestion. We have removed the old citations and have added more recent literature in the revised manuscript. New literature added to the revised manuscript consists of studies conducted in 2010s such as Fan et al., (2019), Shin et al., (2018) and Wang et al., (2019).

SC2. P8L27-29, the description of the symbols in the figure should also be presented in the figure caption.

Response: Thank you for the suggestion. We have added the symbol descriptions in the Figure 1 caption. Also, please note that, according to the other reviewer’s comment we have removed the figure description from the main text.

SC3. P9L7, this conclusion is not easy to clarify from the figures. Please describe more clearly and specifically.

Response: Thank you for the suggestion. River basins, such as Japura and Negro, are characterized by high topographic gradients, resulting into an uneven seasonal streamflow pattern. These gradients are not adequately represented in the model framework due to the limitation in model resolution, hence causing higher discrepancies with the observed values. We have added this information in a concise manner in the revised manuscript to have a better understanding of the conclusion inferred from Figure S2.

SC4. P9L14, the discrepancies in some basins cannot be seen from Figure S2, for example, by which metrics?

Response: Thank you for pointing this out. We specifically wanted to point out the discrepancies in the simulated and observed magnitude of peaks in seasonal streamflow cycle. Xingu, Tapajos and Tocantins sub-basins show significant differences between the simulated and observed seasonal peak of streamflow (smaller right panels of each basin, Figure S3 of the revised manuscript) due to the higher hydropower activity compared to the other river basins. We have edited the statement by including additional information to avoid confusion.

SC5. P12L13, the method of t-test should be described in the methodology section unless it is an ordinary t-test.

Response: The t-test methodology we used is the ordinary t-test. We decided to skip its description from the methodology section as the test is very commonly used.

SC6. P14L14, it should be 'Figure 10'.

Response: Thank you for pointing this mistake. We have corrected it to "Figure 10".

SC7. Figure 5, the color change of the rivers is not clear. The line widths of the rivers should be increased.

Response: Figure 5 shows the interdecadal difference in TWS components at the original model resolution (∼2km). The data presented in the figure is gridded data, hence we cannot represent the rivers in a polyline format. For better visualization we have removed the inland water and country borders.

SC8. Figure S1 lacks the north arrow and the scale. Moreover, the author should mark all sub-basins and major rivers in this figure.

Response: Thank you for the suggestion. We have marked the sub-basin borders in Figure S2 of the revised manuscript; however, we believe that adding the scale and north arrow to the figure would become redundant due to the presence of the geographical co-ordinates.

SC9. Figure 7, y-axis label is missing.

Response: Thank you for the suggestion. We have added a y-axis label in Figure 5.

SC10. It would be better to include geo-coordinates for all spatial plots, e.g., Figure 3,

4, 5, 6, 8, 9, S4, S5, S7, and S9.

Response: Thank you for the suggestion. We have made the suggested changes in the figures.

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