Response Reviewer 1

General comments:

Papastefanou et al. assessed the extent and severity of the 2005,2010, and 2015/2016 droughts over the Amazon basin using 10 precipitation data sources and 3 drought indexes (MCWD, scPDSI, and RAI) with different assumptions. The main results show an increasing disagreement across datasets for more severe drought signals (in terms of both frequency and location). PDSI which consider variable ET shows a much stronger drought impact in 2016 compared with MCWD while RAI based on dry season rainfall shows a weaker drought impact in 2016. In addition, the research explored the consequences of estimating biomass loss from uncertainty across different precipitation using an empirical drought-mortality relationship. The resultant uncertainty in total carbon loss can reach 1.4 PgC (1.3-2.7) for the 2015/2016 drought. The authors conclude with a recommendation of using an ensemble of precipitation data sets when assessing the impact of drought. Overall, I think the analysis is a useful contribution to the study of drought impacts over the Amazon or more generally the tropical forests. The research provides a comprehensive overview of the differences across rainfall datasets, an issue that any analysis or modeling studies over tropical drought will struggle with. I feel the key figures showing dataset agreement are helpful. However, I think the manuscript can benefit from more in-depth discussion and a stronger conclusion. Please see the below specific comments for details. Hopefully, they will help to improve the manuscript and make it more useful to the scientific community.

- *We thank the reviewer for his constructive feedback. We addressed all comments in detail in the sections below.*

Specific Comments:

1. The manuscript focuses on the disagreement among drought indices across different precipitation data sets, which are ultimately driven by the differences in precipitation. It would be helpful to show the difference (e.g. systematic biases and spatialtemporal correlation) across the raw precipitation data sets using paired scatter plots for each precipitation data combination (could be put in the supplementary). This can help to understand why there are disagreements in MCWD (is it just because of a systematic bias so certain data set generates lower MCWD or due to disagreement in the spatial distribution of rainfall, etc.) Such analyses can help to illustrate.

- *We agree with the reviewer that analyzing the precipitation datasets in more detail will improve the understanding of the differences of the MCWD.*
- *We added an additional plot (Fig. S1) that shows the empirical cumulative density functions (CDFs) of monthly precipitation. We find no obvious biases between the datasets, with only ERA5 showing consistently higher rainfall rates.*
- *We further created empirical CDFs for each drought index and across all grid cells (Fig. S2). We could, however, also not identify any obvious biases between the precipitation datasets. By comparing the CDFs we were able to express our absolute MCWD anomaly classifications with relative MCWD anomaly classifications (Methods S1) which we further used throughout the manuscript. Using the relative anomalies also enabled us to better cross-compare the three drought indices.*
- *We now refer to the additional analyses in the main text in lines 171- 174 and 382-384.*

A related point is how to compare different drought indices. Current categorization into moderate, severe, and extreme seems too subjective. Why not show the scatter plot between different drought indices across the drought (from selected precipitation dataset or averaged across all precipitation datasets), which can show the scaling between MCWD, scPDSI, and RAI and demonstrates their differences. Or maybe use percentile (e.g. lowest 5% to indicate extreme) to compare across indices?

- *We agree with the reviewer that our categorization is subjective. This was also pointed out by reviewer 2. We refactored figure 1-3 and 5,6 and now use relative MCWD anomaly (in units of standard deviations) to better describe the agreement of the datasets. We only used absolute MCWD anomalies for Fig. 6 (now figure 4) and the potential biomass losses for 2005.*
- *We have now adjusted the text on multiple occasions, e.g. in lines 217, and 269 - 274 where we now write: "Because no relationship between the anomalies of aMCWD and aAGB could be verified for 2010 (Feldpausch et al., 2016) we did not estimate the impacts on AGB for the other drought years 2010 and 2016."*

2. I like the idea of translating uncertainty in MCWD into the uncertainty in AGB changes (ln 215). However, it should be acknowledged that the empirical relationship itself subjects to large uncertainty. For example, Feldpausch et al. (2016) find that the mortality-MCWD relationship identified in 2005 disappeared during the 2010 drought. Feldpausch T R, Phillips O L, Brienen R J W, Gloor E, Lloyd J, Lopez-Gonzalez G, Monteagudo-Mendoza A, Malhi Y, Alarcón A, Álvarez Dávila E, Alvarez-Loayza P, Andrade

A, Aragao L E O C, Arroyo L, Aymard C. G A, Baker T R, Baraloto C, Barroso J, Bonal D, Castro W, Chama V, Chave J, Domingues T F, Fauset S, Groot N, Honorio Coronado E, Laurance S, Laurance W F, Lewis S L, Licona J C, Marimon B S, Marimon-Junior B H, Mendoza Bautista C, Neill D A, Oliveira E A, Oliveira dos Santos C, Pallqui Camacho N C, Pardo-Molina G, Prieto A, Quesada C A, Ramírez F, Ramírez-Angulo H, Réjou-Méchain M, Rudas A, Saiz G, Salomão R P, Silva-Espejo J E, Silveira M, ter Steege H, Stropp J, Terborgh J, Thomas-Caesar R, van der Heijden G M F, Vásquez Martinez R, Vilanova E and Vos V A 2016 Amazon forest response to repeated droughts Global Biogeochem. Cycles 30 964–82 Online: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015GB005133 In addition, I am not sure whether directly plugging in MCWD based on different rainfall data set makes sense. eqn 2 was derived using a specific rainfall data set. I think it would make more sense to remove the systematic biases between the specific data set and all the data set used in this study before converting MCWD to AGB. One way to find the mapping between MCWD data sets is simple regressions between the data sets as suggested in my comment above. Will such cross-data set calibration reduce AGB uncertainty?

- *We thank the reviewer and author for highlighting the Feldpausch et al. 2016 study which we missed when writing our manuscript and we agree that the linear relation between AGB and MCWD does not hold for 2010 and 2016. This was also pointed out by reviewer 2. We removed the AGB loss estimates for 2010 and 2016.*
- *We appreciate the suggestion of the referee to deeper investigate the MCWD-AGB relation using multiple precipitation datasets and we would be happy to work on this topic together in a follow-up study.*

3. Current conclusion recommends using an ensemble of different rainfall data sets when analyzing drought impacts. However, is there strong evidence that the ensemble would perform better than individual data sets? I wonder whether there are ways to evaluate the performance of each rainfall data set in terms of estimating drought impact. For example, is it possible to compare the spatial and temporal patterns of AGB loss based on different rainfall data sets with the observed spatial-temporal patterns from microwave remote sensing data (Liu et al. 2015; Saatchi et al. 2013; Wigneron et al. 2020) or lidar data (Yang et al. 2018)? Some more detailed details on the potential biases of MCWD that do not include ET variability?

- *We thank the referee for these important remarks. While we do not want to state that an ensemble (collection of datasets) generally performs better than one single dataset, our point is that drought stress can differ substantially between datasets. So for studies assessing impacts of droughts on the Amazon rainforest it may be worth considering multiple datasets to test for climate uncertainty purely arising by the choice of precipitation dataset. We reformulated our manuscript accordingly, it now reads in lines 36-37, 455-456 and 470-472: "Communicating the uncertainty in the estimation of drought events and their impacts on the Amazon rainforest is highly relevant and thus, multiple datasets should be applied by any large-scale study on drought impacts on vegetation."*
- *We appreciate the reviewers' idea regarding the comparisons to remotely sensed AGB data. While this would probably go beyond the scope of this study we think that it would be interesting to investigate in a follow-up study.*

ln 369, I thought microwave data is mostly free from cloud cover effect, which mainly influence optical remote sensing products? I think some of the challenges are the limited penetration depth in the dense tropical forests (Chaparro et al. 2019) and the influences of vegetation water status (Xu et al. 2021)

- The reviewer is correct. We fixed the sentence and thank the reviewer for pointing out some challenges of microwave data which we included in the text: "However, conducting analyses over the Amazon rainforest based on VOD is difficult, because of the limited penetration depth of microwaves in dense tropical forests (Chaparro et al. 2019), and the influences of vegetation water status (Xu et al. 2021)." (Lines 372-374)

Stylistic Comments and Technical Corrections:

ln 63: ’altering the carbon cycle of the Amazon forest already today’ -> ’already altering the carbon cycle of the Amazon forest’

- We fixed this styling issue.

ln 80-100: I wonder whether it is better to just briefly talk about the usage of ten different data sets here and move the details into Methods

- We acknowledge that this part of the introduction might be long, but we think that the details presented are useful in the introduction as they give the reader a short overview of the datasets used in this study. Hence, we would like to keep this part in the introduction.

ln 122: 0.6 Mio -> 0.6 million?

- Fixed!

ln 402: ’In addition, also’, the also is extra

- Fixed!

ln 419: ’average annual carbon uptake’ global or regional? Please specify I wonder whether Table 2 and Table 3 are more suitable for SI… Especially if additional figures on the difference across rainfall datasets are added in the revision.

- We thank the reviewer for his suggestion and moved Table 2 and Table 3 to the supporting information, they are now Table S2 and S3, their reference was updated throughout the manuscript text.

Response Reviewer 2

The authors present a comparison of drought metrics, calculated with different rainfall products. The study region is focused on the Amazon basin, and an extrapolation is made of aboveground forest carbon loss from drought. The authors end with a message that evaluation of drought through an ensemble is better. I think the comparison of rainfall products and evaluation of drought metrics could be useful, especially if it is more developed in the revision. This section could use some more analysis, especially with respect to defining anomalies per pixel location rather than absolute thresholds. However the section concerning the extrapolation of forest carbon loss from drought is a large overreach and does not help advance the state of the science. Please see the following general comments, and line comments.

- *We thank the reviewer for his/her very constructive feedback and detailed assessment of our study. We have addressed all comments below.*

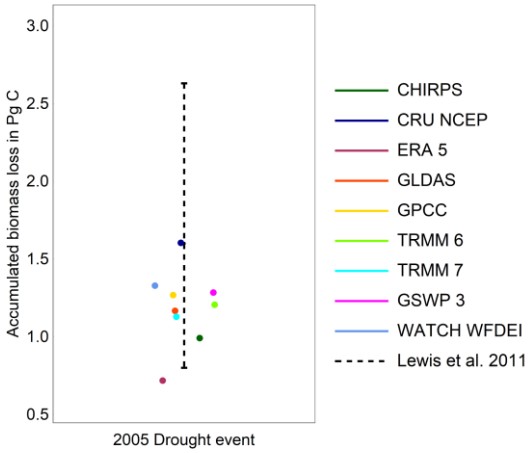
General comments:

Carbon loss from drought - I will start with my strongest objection to this study, which is the extrapolation of forest carbon loss from drought. Accurate estimation of tropical forest carbon loss from drought is a highly sought after goal for tropical ecosystem ecology, but the methods this study uses are not robust or defensible in the present day. The standing biomass and forest sensitivity to drought differs dramatically across Amazonia. This point is even acknowledged (Line 435) in the manuscript. This study does not present any new field data to evaluate this very simplistic empirical relationship (from Lewis 2011), and therefore this study does not have the substance to make these claims. Even Lewis (2011) states this is a first approximation approach and does not include any goodness of fit statistics, the number of plots used to derive this estimate, or even specific information about which RAINFOR plots were included. Lewis extrapolated the relationship beyond the MCWD observed within the RAINFOR plot network from the 2005 drought through the 2010 drought to produce a quick estimate of carbon loss. In this study, the simplistic linear relationship is extrapolated even further beyond the original Lewis 2011 extrapolation. Even if this original relationship was remotely accurate for the 2005 drought, there is no evidence that it was accurate for subsequent droughts in 2010 (or 2015/16). It is difficult to make these forest carbon loss estimates regarding the 2015/16 drought without new field observations and validation, therefore I do not agree that the AGB loss estimates presented here are justifiable and object to their inclusion.

- *We appreciate the reviewers' very comprehensive comments. We agree that we overlooked the study of Feldpausch et a. 2016 which shows that the 2005 AGB-MCWD relationship cannot be applied for 2010 and no evidence exists which would justify the application of the relationship for 2015/2016. Hence, we removed the impact of the droughts on AGB for 2010 and 2015/16, but kept the estimate for 2005. For 2010 and 2015/2016 we focused on the comparison of the drought indices instead of the AGB estimates. We adapted the method section and wrote "To calculate the AGB anomaly in Eq. 2, we calculated the MCWD anomaly of each gridcell in 2005 for each of the precipitation datasets in our study." (lines 216-217) and also the result and discussion sections in multiple occasions, e.g. in lines (272-274) where we wrote "Because no relationship between the anomalies of MCWD and AGB could be verified for 2010 (Feldpausch et al., 2016) we did not estimate the impacts on AGB for the other drought years 2010 and 2016."*
- *Generally, the point of our study is not to give better estimates of AGB loss during drought, but rather to show how the choice and version of a climate (forcing) dataset also can have large influences on the drought impact and representation. In addition, we wanted to highlight that despite having better satellites and more sophisticated techniques the uncertainty can even increase for recent drought events (such as 2016). We added a better explanation about this into the discussion and conclusion e.g. in lines 453-455 and 468-470: "We therefore recommend applying several climate (precipitation) datasets as well as drought metrics to account for model uncertainty when assessing the spatial extent, duration, and location of droughts. We regard it as an important step when assessing drought impacts on tropical rainforests also under current climate conditions."*

Next, it is worth noting that a large-scale squall line also crossed the Amazon basin during the period of measurements presented in the original Phillips 2009 Science paper. This was estimated to have killed hundreds of millions of trees (Negrón-Juárez et al., 2010 Geophysical Research Letters), so even the empirical AGB~MCWD loss relationship presented in Lewis 2011 has a heavy bias from wind mortality. I strongly urge the authors to drop this aspect of the manuscript. Estimating Amazonian forest carbon loss from drought has long been a difficult endeavour, and many groups have been physically collecting field observations to quantify this. I worry this aspect of the study adds more noise than value to the current state of the science.

- *We thank the reviewer for this detailed comment. As stated in the comment above we agree with the reviewer and removed the AGB-MCWD relationship for 2010 and 2016 from figure 6 (now figure 4). Figure 4 is now:*



Defining drought - I think the evaluation of different precipitation datasets concerning the drought is mostly fine and could be useful. However the way drought is defined here is a bit simplistic, especially regarding the MCWD anomaly. The mean annual precip spans from 3500 mm + in the northwest Amazon to less than 1700 mm in the southeastern peripheries. I think it is difficult to justify a definition of drought based on absolute thresholds for the MCWD anomaly. The northwest Amazon rarely experiences a dry season, whereas the southeast Amazon does not receive rainfall for more than half the year. Forests are adapted to some level of water stress, which is why simple absolute thresholds are unlikely to characterize vegetation water stress. Assessing drought anomalies based on the number of standard deviations (calculated per pixellocation) is one commonly used way to assess drought with respect to the baseline climate and interannual variability of precipitation. Absolute thresholds (e.g. MCWD >25) vs. relative anomalies (e.g. MCWD > 2 standard deviations). The older papers using MCWD (e.g. Aragão et al., 2007) used a fixed value because there was not enough information at the time of actual ET. Now it is well understood that actual ET can vary substantially across the Amazon and has seasonality in most regions. It no longer makes sense to use a fixed value of ET for both the everwet northwest Amazon and the seasonally dry southwest Amazon. I suggest the authors could use newer spatially resolved ET estimates such as from GLEAM, MODIS MOD16, Fluxcom, etc.

- *Thank you very much for raising this point. While we generally agree with the reviewer that precipitation is very heterogeneously distributed across the Amazon rainforest, the constant ET of 100mm is still being used frequently in recent publications (e.g. Flack-Prain et al. 2019, Biogeosciences; Koch et al. 2021 Earth's Future). While we developed the methods for this study we also looked into this topic acknowledging the spatially large difference in annual precipitation.*
- *We already also did some analyses (Fig. S3) where we tested the sensitivity of the 100 mm ET threshold and used the ERA5 ET product instead. We found that using variable*

ET can significantly reduce the MCWD anomalies. However as (to our knowledge) the majority of the studies conducted in the Amazon rainforest still use constant ET of 100mm we would like to keep the 100mm ET threshold.

- *To account for this important effect of variable ET on MCWD we added the following sentence to the discussion in line 335-340: "We investigated the effect of choosing variable evapotranspiration and a longer baseline in our MCWD calculation (Fig. S3). Using variable evapotranspiration consistently reduced the moderate drought-affected area by 10-20% per drought event (Fig. 3a, b, c). It also affected the intensity of the drought stress, e.g. areas previously classified as extreme drought affected were now classified as areas with severe drought stress. This reduction is expected as ERA5 takes the above-mentioned lower ET values in the highland tropics into account which overall leads to higher MCWD values in this region. Because of the strength and consistency of this effect we recommend testing the MCWD calculation regarding its sensitivity to variable ET in the tropical rainforest in future studies."*
- *While initially, absolute thresholds are useful for deriving the absolute impact of AGB changes, we agreed switching to relative thresholds throughout the study is more meaningful. This further enabled a better cross-comparison of MCWD to the two other indices which we now compared directly. We hope with the current changes on the use of the MCWD we have fully addressed this point.*

The comparison of precipitation products and drought metrics could be a useful contribution, however this is currently muddled by putting all the estimates together in an ensemble. I suggest the authors focus on presenting a more organized comparison of (1) precipitation products, and (2) drought metrics. What is the justification for using an ensemble of precipitation datasets? Why is this better than using the best evaluated precipitation dataset? Consider the timing of the development of these products. Some of them have been operational for over 20 years. Statistical methods, data assimilation and climate reanalysis models have improved dramatically since then. I think it is difficult to argue that an ensemble method is better, especially when including where a coarse resolution earlier generation product (e.g. GPCC) has as much vote as the latest generation of products (e.g. ERA5, GPM IMERG6).

- *We are not quite sure if we fully understand the reviewers critique regarding our approach. We use the term "ensemble" to reflect a collection of datasets that have overlapping spatial and temporal resolutions. The collection of climate forcing data sets that we use include state-of-the-art reanalysis data sets (NCEP, ERA5 and 20CR), remotely sensed data sets (TRMM v6 and v7), widely used climatology data sets which are interpolated from station data (CRU) and merged data sets (GSWP3 and WATCH-*

WDFEI). Our aim is to show the range of climate forcings and the resulting simulated ET and drought response. We therefore analysed the time period which is covered by all data sets. However, we still consider each dataset individually as shown, e.g., in Fig. 2. As mentioned in the text (lines 75 - 76), the scope of our study was to conduct "a systematic analysis of how the most frequently used precipitation datasets differ regarding the spatial extent, location and severity of recent extreme drought events". Obviously, we were not clear enough about this scope and tried to make this clearer throughout the text, for example in lines 36 to 37, where we now write: "We conclude that for deriving impacts of droughts on the Amazon Basin based on precipitation, multiple datasets should be considered."

Other comments

There are a number of typos in both the main text and figures. Some of these are highlighted in the line comments.
There are far too many acronyms in this manuscript. For example, is CHR really a useful shortening of the CHIRPS? Each new acronym makes the manuscript more difficult to read. I suggest limiting the usage of acronyms to the absolute minimum. Wherever possible, use established acronyms such as TRMM. Making up new acronyms of acronyms (TR6, TR7) is confusing and will not help readers comprehend the manuscript. A manuscript of this length does not need additional acronyms to make it shorter.

- *We fully agree and used the official acronyms throughout the manuscript instead of making up new ones.*

Section comments:
L30: This should be MCWD > 25 mm, no? Also the climatological mean MCWD across Amazonia is quite large. I don't think it makes sense to use a single value to define drought (~25 mm). MCWD >= 25 mm in the southeast Amazon does not indicate drought.

- *Similar to Lewis et al. 2011 we wanted to use the negative definition of DeltaMCWD, so in this case DeltaMCWD < -25mm would be correct.*
- *However, as already stated above we switched to relative thresholds and rephrased this part accordingly. It now reads (lines 24 to 26): "Evaluating an ensemble of nine state-of-the-art precipitation datasets for the Amazon region, we find that the spatial extent of the drought in 2005 ranges from 2.2 to 3.0 (mean = 2.7) million km² (37 – 51% of the Amazon basin, mean = 45%) where MCWD indicates at least moderate drought conditions (relative MCWD anomaly < -0.5).".*

L 170: The wet season starts at different times of the year across the Amazon. How is the choice of starting the hydrological year determined?

- *Similar to Phillips et al. 2009 and Lewis et al. 2011 we selected the 1st October as the onset of the hydrological year for each location in the Amazon.*

L 173: I am not sure Delta MCWD is a good abbreviation for the anomaly of MCWD. This can easily be taken as just the change in MCWD between two time periods, but that's not exactly what the anomaly is during a drought. Perhaps it's better to spell it out as the " MCWD anomaly".

- *We thank the referee for this suggestion and now use the term MCWD anomaly throughout the text and figure descriptions.*

L 176: Removing the drought years causes bias. There are three droughts in the span of 15 years, so these are not rare events. Just because Lewis 2011 used a method, does not mean it is defensible in the present day.

- *Please see subsequent comment because we think they are related.*

L 185: Climatologies are typically calculated from 30 year periods. Most of the data products have at least 20 years of duration, if not closer to 40. The selection of years to remove is subjective and removing the years with anomalously low rainfall will bias the standard deviation to be artificially small.

- *Regarding L176 and L185, we agree with the reviewer that removing the 3 extreme events may cause a bias. We included the baseline years 2005, 2010, and 2016 in our MCWD calculation (and also in the calculation of the other metrics) to avoid this potential bias.*

L208: Be consistent in treating MCWD as either a positive or negative quantity.

- *We corrected any inconsistent use of MCWD throughout the manuscript.*

L215+: I reject the underlying basis for the empirical carbon loss estimate from Lewis (2011).

- *We accepted this rejection and deleted all estimates of carbon losses for years other than 2005.*

L229: MCWD is misspelled L295: It is difficult for rainfall products to correctly estimate rainfall near the foothills of the Andes. Also, some areas have very little ground information for each product's bias correction algorithm. It might be worth getting into this to describe more deeply why the products disagree, and where.

- We corrected the spelling error, thanks for spotting it.
- Also, thanks for raising the issue on estimating rainfall at the Andean foothills correctly. Interestingly, when switching to relative anomalies, as suggested by the reviewer above, this disagreement at the Andes disappeared. This also supports the reviewer's suggestion of using relative over absolute thresholds.
- While switching to relative anomalies the most of the more obvious differences (e.g. in the highlands) disappeared. Furthermore, we could not find much in the literature that could explain the differences we observed in our studies. An in-depth analysis why the forcings disagree would probably go beyond the scope of this study.

L333: I would note that many studies no longer use the fixed estimate of 100 mm. I believe some have used Stephenson (1998 Journal of Biogeography) as a reference for the development of the MCWD metric.

- *We mentioned Stephenson (1998 Journal of Biogeography) as a reference for MCWD in line 319.*
- *We now mention that there are better alternatives for ET instead of assuming constant ET = 100mm/month in lines 330 to 332, where we state: "In the last decade, better products of spatially and temporally resolved evapotranspiration data (e.g. ERA5) have been developed and an increasing number of studies are now estimating MCWD based on such data (e.g. Staal et al., 2020)".*
- *However, we also pointed out that many studies (e.g. Flack-Prain et al., 2019; Koch et al., 2021) in the Amazon still use the constant ET= 100mm/month approximation. See also our above response where we addressed the major points.*

L357: Using a better estimate of "actual ET" might reflect the impact of VPD. I would say this is a limitation of using a fixed 100mm value for ET in the MCWD calculation.

- *We thank the reviewer for this good point and added this point to our discussion in line 366 where we state: "One possibility to account for the influences of VPD is choosing temporal and spatially resolved evapotranspiration instead of constant evapotranspiration in the calculation of MCWD."*

L426: Indeed, this is another reason to drop the extrapolated carbon loss estimates.

- *See our response to the first major point (above).*

L453: I don't think the case for assessing drought with an ensemble is made clear.

- *We rephrased this part to make it more clear (see also comment below). It now reads (lines 454 to 455): "Therefore, we recommend using multiple climate forcing datasets to test for climate data uncertainty also under present climate conditions.".*

Why is it not better to just use the product that has the lowest RMSE in the region of interest?

- *We again want to highlight that the purpose of this study is not to find the best dataset for locations at which we have exact measurements and can evaluate RMSE, but to give a broad picture of how different precipitation datasets represent drought stress across the complete basin. We rephrased (e.g. in the introduction and conclusions) some parts of the manuscript to make this more clear. For example, we now state in the introduction (lines 107 to 111): "The goals of our study are (1) to analyze and quantify the uncertainty in strength, extent, and location of three recent Amazon droughts in the years 2005, 2010, and 2015/2016 in precipitation from nine state-of-the-art precipitation or climate datasets based on MCWD; (2) to examine differences among these drought events by taking two additional drought indicators RAI and scPDSI into account; and (3) to give an estimate of the impacts of the 2005 drought on the carbon cycle by estimating potential biomass losses."*

L458: The code in the repo looks to be incomplete. Ideally the complete code for analysis and figures should be hosted prior to the review process. An incomplete repository hinders the review process.

- *We will put all the files and scripts on the repository so that they can be easily reproduced.*

Figure 1: Is this MCWD, or anomalies of MCWD?

- We replaced Figure 1 with a new figure representing the impacts of the 2016 drought across all datasets. Figure 1 now is:

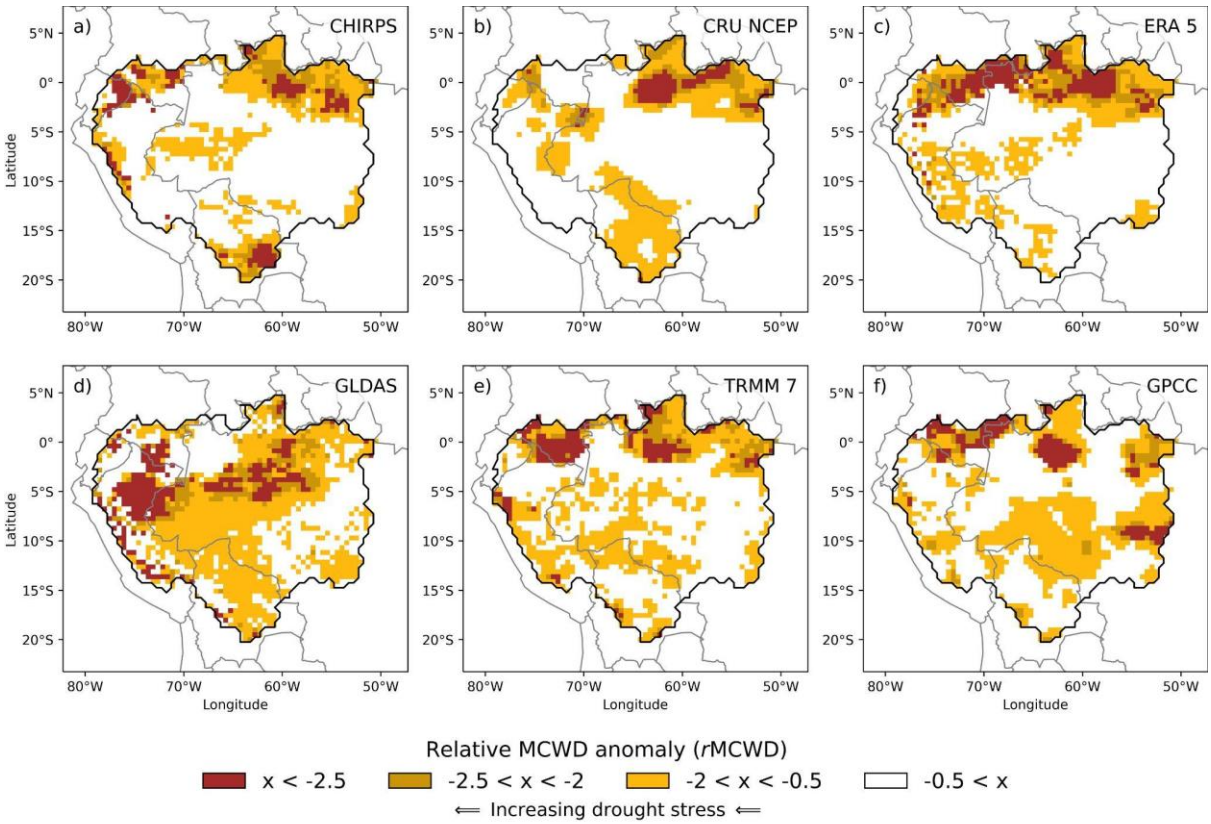


Figure 1: Relative MCWD anomalies (from October to September) as an indicator for drought stress in the Amazon basin during the record-breaking drought event in 2016. Displayed are only the datasets that include the year 2016 in their temporal range. The baseline period of the MCWD calculation is 2001 to 2016.

Figure 2: Why is WAT not included in panel C?

- Because WATCH_WFDEI (or at least the version we have of it) does not go beyond 2010.

Figure 3: This is a useful figure. It might be useful to add another two columns indicating where the satellite-based products agree, and where the climate reanalysis modeled products agree.

- We thank the reviewer for finding this figure useful. We agree that it would be interesting to compare against satellite-based products. However, this would go beyond the scope of this study as our purpose was not to find the “best performing” dataset, but rather to show the uncertainties that purely arise by the selection of a dataset (and drought indices).

Figure 4: Is "PA" (y-axis label) supposed to be "RAI"?

- This is correct and we fixed it.

Figure 5: Is "PA" (y-axis label) supposed to be "RAI"? The delta MCWD supposed to be the Anomaly of MCWD? Might be better to spell this out.

- This is correct and we fixed it.

Figure 6: I suggest removing this aspect of the study, and this figure.

- (Now Figure 4). We removed the 2010 and 2016 aspects from this figure. However, we still would like to keep this figure and also to show the differences of the potential impacts on the carbon balance for 2005, where the Lewis et al. 2011 (Phillips et al. 2008) relationship is valid.

Table 1: I suggest dropping the abbreviations of abbreviations, and adding a column about how the product is derived (e.g. Remote sensing, interpolation of ground data, atmospheric process model, etc).

- Thank you for this suggestion. We dropped the abbreviations of abbreviations from the table and added the column about how the product is derived.

Table 2: RAI?

- Correct and fixed. Please note that we moved the big tables 2 and 3 to the supporting information to follow a suggestion from reviewer 1.