

Interactive comment on “Rapid reduction in ecosystem productivity caused by flash drought based on decade-long FLUXNET observations” by Miao Zhang and Xing Yuan

Anonymous Referee #1

Received and published: 11 May 2020

Reviewer Recommendation: Major Revisions

General Comments:

1) Terminology – Because the definition for flash drought recovery focuses on changes in soil moisture, this framework introduces some confusion when also used to examine changes in GPP given the lag between the onset of soil moisture drought and its impact on vegetation health. For example, it is counterintuitive to refer to periods of “recovery” as those that also have substantial reductions in GPP. I think the framework used in this study is okay, but that different terminology needs to be used when referring to these periods because the “recovery” is only with respect to soil moisture conditions.

C1

The new terminology will need to be used in the abstract, and throughout the paper. It would also help to remind the reader at various stages of the paper that “flash drought” refers to “soil moisture flash drought”.

2) Definition – I think it's fine that you chose to add a maximum length threshold (lines 128-131) to the flash drought definition if you also want to solely focus on sub-seasonal drought events. However, this choice, and its impact on the resultant analysis, needs to be clearly noted in the revised text. For example, limiting flash drought duration to no more than two months means that situations where a period of rapid intensification preceded development of a longer-term drought will be excluded from the climatology because the soil moisture will not rise to greater than the 20th percentile within the chosen period of time. In fact, many of the most notable flash drought events discussed in the introduction (such as the 2012 U.S. flash drought) would presumably not be classified as flash drought with this methodology because the period of rapid intensification itself lasted for two or more months in many regions, with subsequent drought conditions lasting for many more months after that. In reality, the method used in this study only examines a subset of flash droughts, where not only must they exhibit a period of rapid intensification over 1-2 months, but then the drought conditions themselves must also be completely eliminated within another month. So, these are sub-seasonal events in their entirety. This is alluded to at lines 193-195. To reiterate, I think the methodology itself is okay, but that it needs to be clearly stated at various points of the text (abstract, methods, results, discussion, conclusions) that the goal is to look *only* at flash drought events that develop and decay over a single season, and that the method will exclude flash droughts that subsequently develop into long-term drought.

3) Section 3.3 – This section needs to be substantially revised. Given that the focus elsewhere in the paper has been to evaluate the results based on the vegetation type, it is confusing why this section primarily focuses on analyzing the results accumulated over all vegetation types in Fig. 5, before then very briefly discussing vegetation-

C2

specific results in Fig. 6. It would be much more insightful, and consistent with the rest of the paper, if you were to instead expand the existing brief analysis for each of the vegetation types into something more substantial. This would result in the removal of Fig. 5 that focuses on all of the stations in aggregate and redoing the bottom panels in Fig. 5 so that they can be added to Fig. 6 for each individual vegetation type. This will then allow you to continue to examine the time series for each vegetation type as has been done elsewhere in the paper.

Specific Comments:

- 4) Line 37 – Insert “future” before “land carbon uptake” in this sentence.
- 5) Line 58 – Please add the Svoboda et al. (2002) reference for the U.S. Drought Monitor.
- 6) Line 59 – This drought also impacted parts of southern Canada.
- 7) Line 78 – Few studies, or no studies, have investigated this parameter? If there are previous studies, please cite them here.
- 8) Introduction – It would also be good to cite the Otkin et al. (2018; WCAS) paper because they examined the impact of a flash drought on vegetation health across the north-central U.S.
- 9) Line 99 – Please add some additional information about the soil moisture sensors, such as their type, their accuracy, and how they are sited. It would also be good to know what the soil type is for each of the stations.
- 10) Lines 103-106 – How were these vegetation classifications determined? I think it would also be good to briefly discuss the phenological characteristics of these classifications.
- 11) Line 106 – Please make this sentence explicit rather than simply stating “etc”. Also, this would be a good spot to point the readers to the top panel in Fig. 2 to see

C3

the locations of these stations.

- 12) Lines 106-108 – Please provide some justification for why these three particular sites were chosen for the case study analyses. It would also be helpful to mention here where these three stations are located, and a brief overview of their climate characteristics. For example, are these stations located in regions that are known to frequently experience flash droughts?
- 13) Line 116 – Does the first day of the flash drought occur at the beginning, middle, or end of the 8-day period used to compute the mean conditions? Please clarify.
- 14) Figure 1 – The label between steps 2 and 3 should be “true”. The box for step five should also be expanded to include “and < 2 months”. Please correct these errors.
- 15) Please 119 – It would be good to note here that these differences are also being computed at 8-day increments to match the cadence of the 8-day mean periods.
- 16) Lines 123-125 – “Recovery” is imprecise here because a decrease of 4% from one period to the next does not represent recovery; instead, it simply means that the deterioration is not fast enough to meet the threshold for a flash drought used in this study. Please change this term to “stabilization”, or something similar, because that will permit some degradation to still occur. Note that this only refers to the soil moisture status “stabilizing”; thus, the inconsistency with respect to the vegetation parameters (see Major Comment #1) still remains and will also need to be properly addressed.
- 17) Line 132 – Please change the start of this sentence to “At least decade long”.
- 18) Lines 132-140 – It would be good to reiterate here that the percentiles themselves are still only computed over an 8-day period, but that the use of the surrounding 8-day periods are used to increase the sample size. These surrounding time periods though are certainly not completely independent, so please also comment on how much this approach does or does not increase the effective sample size when computing the percentiles.

C4

- 19) Line 150 – Please add the Crausbay et al. (2017) paper in BAMS that discusses ecological drought.
- 20) Line 154 – You highlight an example with 19 years of data; however, most of the stations only have around 10 years of data. This is a short period for computing standard deviations. Please comment on how the short period of record will impact the anomalies and their subsequent use in this study.
- 21) Lines 154-157 – The example provided in this sentence implies that ecological drought always happens one period after the flash drought first develops. Is that the true intention here? If not, please clarify this sentence. I would expect there to be more than a one period lag because in many situations, the vegetation roots will extend much deeper than the 10-cm topsoil layer used in this study to identify flash droughts, thereby allowing them to remain healthy despite a rapidly drying topsoil layer. This needs to be highlighted in this section – a flash drought in the topsoil layer may not correspond to an ecological drought because of the depth of the roots.
- 22) Lines 150-162 – It would be helpful if each of these indices were assigned separate names to be used in the results section.
- 23) Line 187 – Please add “or equal to” before 24 days.
- 24) Line 190 – The station-level average lengths are not helpful because many of the stations only have one or two events. It would be better to show the average length over all of the stations, or for all of the stations within a particular ecosystem type. Please do this in the revised text.
- 25) Lines 192-193 – Is this sentence meant to imply that some stations may have multiple flash droughts because a single event is broken into two because of a rainfall event that temporally improves things? If so, please describe it as such, otherwise it is not clear what this sentence adds to the paper.
- 26) Line 192 – What is meant by “variability of soil moisture”? Please describe this

C5

more clearly. Also, this really means variability of precipitation since it is the ultimate cause of the variability in soil moisture.

- 27) Figure 2 – The panels on this figure are difficult to read. For example, the spatial heterogeneity briefly mentioned in the text is impossible to see in the top panel because most of the stations are crammed into central Europe or North America, and it is impossible to relate the results shown in the bottom panels to the map shown in the top panel. I suggest breaking this panel into separate panels for North America, Europe, and the other four stations individually, while still taking the same amount of space as the current panel. This will allow you to zoom into all of these regions and therefore more clearly show the spatial heterogeneity.
- 28) Lines 204-206 – This sentence is imprecise. A decrease in ET will indeed limit the loss of soil moisture; however, it does not represent an alleviation of drought conditions. For one thing, soil moisture will still be decreasing in the absence of rainfall, albeit at a slower rate. Secondly, decreasing ET actually means that agricultural or ecological drought conditions are worsening. Please clarify this statement to account for these considerations.
- 29) Lines 210-211 – Please add some information describing where these stations are located, and why these events were chosen for closer analysis.
- 30) Figure 4 – Please change the top and bottom rows so that the precipitation and temperature anomalies can be both positive and negative, otherwise, the analysis is incomplete since only one part of the anomaly time series can be shown.
- 31) Line 220 – This statement is too strong because it is based on a single case study.
- 32) Line 228 – Is there a reference that supports this statement? The variability in the time series for this station is very similar to the other two time series shown on Fig. 4.
- 33) Lines 230-231 – This statement is not supported by the bottom row of Fig. 4 where the ET anomalies for this savanna station are actually less severe than those for the

C6

forested site. Please fix this in the revised text.

34) Lines 212, 224, and 236 – It would help if you pointed the reader toward the appropriate panels on Fig. 4 in the introductions to each of these paragraphs.

35) Figure 5 – Please move the legend on panel a to panel b since that is where both of these lines are shown.

36) Line 252 – It would be good to clarify that this is “flash drought as determined by soil moisture reductions.”

37) Line 279 – Why “down to its normal conditions”? I assume this is a mistake since you’ve already shown in the previous section that GPP anomalies become negative during a flash drought.

38) Line 284 – This ratio is reversed compared to that shown at line 172.

39) Line 288 – Again, this terminology is confusing – how can “recovery” be accompanied by “significant reductions” in GPP and ET. Those reductions show that vegetation conditions have deteriorated, not improved. This is also repeated at lines 319-320. This terminology needs to be changed to reflect that the “recovery” is only respect to soil moisture.

40) Line 315 – Please change “intensity” to “reduction”.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-185>, 2020.