Increased vulnerability of European ecosystems to two compound dry and hot summers in 2018 and 2019

Bastos, A., Orth, R., Reichstein, M., Ciais, P., Viovy, N., Zaehle, S., Anthoni, P., Arneth, A., Gentine, P., Joetzjer, E., Lienert, S., Loughran, T., McGuire, P. C., O, S., Pongratz, J., and Sitch, S. *Earth Syst. Dynam. Discuss*

Response to Reviewer #2

R2C1: The manuscript by Bastos et al explores the existence and extent of compound effects of subsequent dry and hot summers on vegetation, using the case of 2018 and 2019 in Europe. The manuscript is of general interest to a large audience, across several disciplines. Overall, the manuscript is generally well-written and clear (but see below). I have however three main suggestions
We thank the reviewer for the evaluation of our manuscript and for their comments and suggestions.

R2C2: The manuscript is dense, both in terms of methods and results. The results and even more so discussion are thus inevitably complex, but, unfortunately, at times not very clear, with apparently contrasting statements. A general suggestion would be to give descriptive names to the four clusters, to facilitate the reader throughout the text. Further, I list here some examples of apparently contrasting statements or unclear sentences, but I suggest that the entire discussion is revised for better clarity.
We agree that the manuscript is dense. We thank the reviewer for pointing out areas for improvement and for the suggestions. We have now changed the names of the clusters to: $C_{Decline}$, C_{HighV} (from high-vulnerability), C_{PRecov} (partial recovery), $C_{Greening}$. We also revised the discussion in an attempt to improve clarity, and corrected the imprecisions pointed out by the reviewer below.

R2C3: I find of particular relevance the question of the responses of different land uses. This point is relatively prominent in the abstract, but then only briefly touched upon in the results and discussion. I think the manuscript would gain in terms of impact (and interest within a broader audience) should this question be explored in more detail. To this aim, I would suggest redoing the analyses at the basis of Figure 6, but separating land uses. This could give some insights in how the different vegetation types respond to temporal compound events. Of particular relevance would be the correlation with previous year's EVI anomalies, temperature, soil moisture and soil available water capacity. This additional analysis would for example reduce the speculation behind statements like that in L349, by allowing exploring the effects of available soil water.
We agree with the reviewer that evaluating the results by different land-cover/uses increases the relevance of the study. Separating between land-uses at the spatial scale of the EVI (5km) is challenging, though, because of the substantial, small scale landscape heteorogeneity in Central Europe. This is also the reason for our analysis of LC selectivity for each cluster, rather than simple categorization into LC types. To address the reviewers concern, we propose a new version of Figure 6, in which we show results separately for pixels with high tree cover vs. pixels with low tree cover (reproduced below). We also compare residuals for pixels with high and low tree cover fractions.
The updated figure supports a contribution of the warm spring legacy effects in explaining residuals in 2018, and a strong difference in the water-limitation (given by the relationship with SManom in summer) between pixels with high vs. low forest cover in 2018. Previous year's EVI becomes more important in 2019, as do the summer climate variables and isohydricity. We have accordingly revised all the results and discussion referring to Fig. 6.

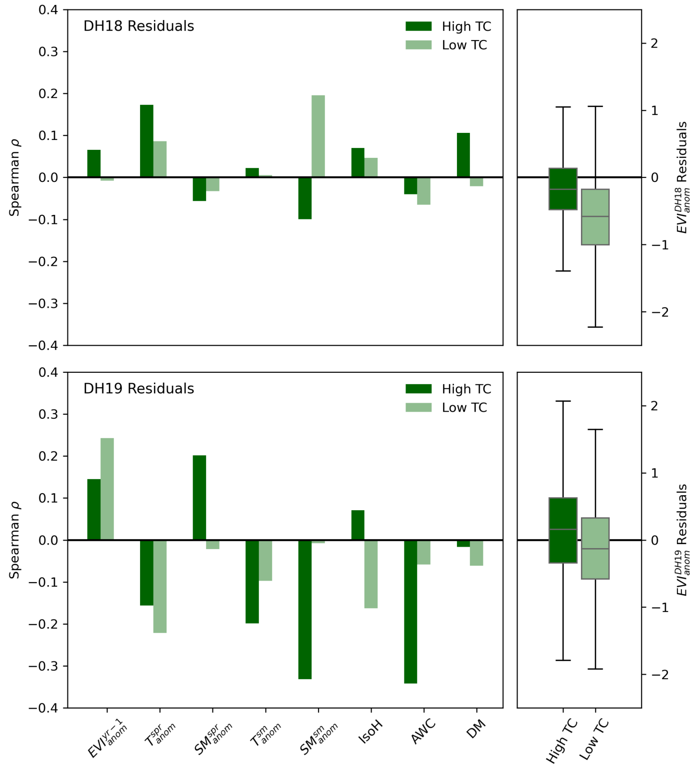


Figure R1: Spatial partial correlation (spearman) between EVIanom residuals and environmental variables in DH18 (top panels) and DH19 (bottom panels), for pixels with high (dark green) and low (light green) tree cover. The variables considered are: spring and summer Tanom and SManom (indicated by superscripts *spr* and *sm*, respectively), EVIanom in the previous growing season (EVI^{yr-1}), plant isohydricity (IsoH) and the number of dry months (DM). Because of the large number of pixels considered, all correlations are significant (p-val<<0.01).

R2C4: One could expect that crops, by being mostly annual plants, would have a radically different response to temporal compound events from all other perennial vegetation. Specifically, one could expect no substantial legacy effects, or even a positive EVI anomaly in 2019, as the result of reduced nutrient use and losses via leaching in the previous year, characterized by low production and low soil moistures. While the discussion is definitely more focused on vegetation dominated by perennial plants, there are few hints at crops also having some legacy effects. I think it would be helpful to discuss in more detail why this is the case (if this is the case), thus better grounding the results in our ecophysiological understanding of plant and ecosystem response to (repeated) heat and drought. The additional analysis suggested above could shed some clarity on this matter, making the discussion less speculative. Authors could also consider how the LSM performance in 2019 is affected by land use: it could be expected that, if LSMs fail to represent the carry over effects, then they would be performing better in ecosystems where carry over effects are intrinsically more limited.

We agree with the reviewer that crops should not show strong legacy effects, at lest from a physiological perspective, and it was not our intention to convey this message. We have carefully revised the text to avoid such confusion. We find a high correlation with previous seaason's EVI for pixels with low tree cover, though, which is surprising. We added the following explanation in the revised manuscript:

> *The stronger correlation found in low tree cover pixels is surprising, as crops and many grasslands are mostly annual plants. The high correlation between EVIanom residuals and* $EVI^{yr\text{-}1anomin}$ *DH19*

can indicate either that pixels strongly impacted by DH18 were associated with amplified impacts by DH19 (negative residuals), or that pixels affected moderately by DH18 (less negative EVI^{DH18}anom were associated with positive residuals, i.e. stronger recovery.

Damage to roots and tissues or depletion of carbon reserves from DH18 leading to higher vulnerability to DH19 could explain the positive correlation in high tree cover pixels in $C_{Decline}$. Conversely, the moderate DH18 impacts in $C_{Greening}$ may have resulted in increased resistance to DH19. The strong correlation found in low tree cover pixels is, though, surprising, as European crop species tend to be annual plants, and annual species can also be found in many grasslands. For these pixels, it is more likely that the positive correlation is explained by management practices, e.g. through earlier harvest or active reduction of stand density in DH19 (Bodner et al., 2015).

Evaluating the performance of the LSMs per land-cover type would require being able to separate the land-covers in the observations. In the study region, the maximum grid-cell average forest cover is 70% and only 4% of the 5x5 km^2 pixels have forest cover above 60%. Therefore, when remapping to the 25km resolution of the LSM models, one would be left with too few pixels, if any, for appropriate model evaluation.

L313: Isn't the high correlation in contrast with the statement in L 305?

Indeed, there is an apparent contradiction, but these two sentences refer to different patterns: average anomalies in the two DH events (L305) and the temporal evolution of the anomalies (L313). In line with R1C3, we now include RMSE as well. We agree, however, that these points were not clear. In the restructuring of the results and discussion, we moved this point to the discussion section and rephrased as:

The LSMs perform well in simulating the magnitude and evolution of productivity anomalies in 2018, but not in 2019. The recovery simulated by LSMs in DH19 can be partly explained by a strong recovery of modelled soil-moisture (Fig. B7), but may also result from limited ability of LSMs in simulating changes in ecosystem vulnerability during the two DH events. The latter is supported by the fact that simulated SManom shows good agreement in the temporal evolution of soil-moisture anomalies with both observation-based datasets but not of GPPanom (Table 1).

L322-324: This is an important point, but it is not at all clear in the text.

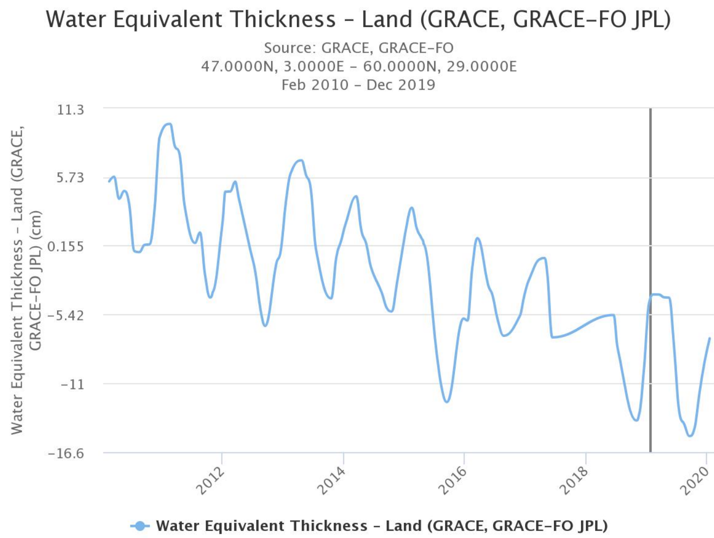
We have now emphasized this point in the revised version of the discussion:

In DH18, we find a positive effect of spring warming in vegetation growth, leading to weaker departures from long-term vegetation--climate relationships (observed EVIanom more positive or less negative than modelled), but with associated water depletion amplifying the impacts of DH18 in summer in pixels with low tree cover. These results are in line with Bastos et al. (2020a) that showed contrasting seasonal legacy effects of warm springs in crop versus forest dominated regions.

On the contrary, spring and summer T^{sm}anom in 2019 (or cooling, see Fig. B1) are negative correlated with EVIanom residuals in both high and low tree cover pixels. This indicates increasing damage from heat stress, for example due to reductions in evapotranspirative cooling (Obermeier et al., 2018) or cascading impacts of compound heat and drought, such as insect attacks (Rouault et al., 2006).

L349: I think this is a potentially controversial point. Deeper roots are an advantage, if the off season has provided water recharge.

In the figure below, we show the mean value of total water storage over the study region from the GRACE Data Analysis Tool. The vertical line indicates January 2019, where it can be seen that water equivalent values had returned to values registered only before DH2018.



L354-355: On which basis can it be stated that this is 'consistent'?
We meant that forests and grasslands tend to have higher isohydricity than croplands (Konings and Gentine, 2017). The discussion section has been restructured (R2C2) and the sentence in the meantime removed.

L356: Why is the vulnerability increased? Is this because of heat? Isn't this in contrast to L350? Does this apply just to 2019? And, even if so, how could an opposite response in the two years be justified?
Indeed, the formulation was confusing. The results for DH2019 are opposite than for DH2018, which is a key result of this analysis. We have reformulated the discussion and hope the message is now clearer:

> *In DH18, we find a positive effect of spring warming in vegetation growth, leading to weaker departures from long-term vegetation--climate relationships (observed EVIanom more positive or less negative than modelled), but with associated water depletion amplifying the impacts of DH18 in summer in pixels with low tree cover. These results are in line with Bastos et al. (2020a) that showed contrasting seasonal legacy effects of warm springs in crop versus forest dominated regions.*
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> *On the contrary, spring and summer T^{sm}anom in 2019 (or cooling, see Fig. B1) are negative correlated with EVIanom residuals in both high and low tree cover pixels. This indicates increasing damage from heat stress, for example due to reductions in evapotranspirative cooling (Obermeier et al., 2018) or cascading impacts of compound heat and drought, such as insect attacks (Rouault et al., 2006).*

Minor comments:

There are several imprecisions in the text (missing or misplaced blank spaces, inconsistencies in symbols, etc.), in particular in the results and discussion sections.
We have corrected the typos.

The readability of Fig 3 would be greatly improved if it was bigger (in particular the right panels). Also, could a different set of colors be used, to highlight differences in the map? C2 and C3 are difficult to distinguish now.
We have now increased the size of the figure and adapted the color scheme so that C2 and C3 can be more easily distinguished (C2 is now redder). We have also checked that the color scheme is colorblind friendly.

Also Fig. 4 could be a bit bigger, possibly with larger and differently shaped symbols for 20018 and 2019 (which are anyhow outside the regression).
We now use bigger and different shaped symbols for 2018 and 2019.

L131: Why not also June-August 2019?
The goal of these simulations was to test the hypothesis that models might simulate too weak impacts of DH2019 because they lack legacy effects from summer 2018. The added value of running one additional simulation with fixed summer 2019 climate is, in our opinion, limited, since we expect concurrent responses to be similar, in the absence of legacy effects. Since such simulations are time consuming, we propose not to include such an experiment.

L172: In which sense there is an acclimation to drought, at the scale of one-to-two years?
Certain responses to DH18 could confer greater resistance to DH19. We have added now:

> *Impaired functioning during the recovery period can additionally increase the hazard of subsequent disturbances, e.g. insect outbreaks (Rouault et al., 200ˆ). However, reductions in leaf area, increases in root allocation (McDowell et al. 2008) or reduced growth, by reducing evaporative tissue and enhancig water uptake capacity, could also confer an advantage to subsequent droughts (Gessler et al., 2020).*

L244: I think the term 'recovery' is not necessarily correct, at least not in all ecosystems. So, I suggest using a more neutral 'less negative EVI anomalies'.
We refer to "partial recovery", not full recovery, which is justified by all pixels in C3 showing higher (less negative) EVI anomalies in 2019. For readability, we suggest keeping as is.

L 285: 'stronger' with respect to what? DH2018?
Yes we meant with respect to DH2018. This is now corrected.

L357: I find the term 'natural ecosystems' potentially confusing here. I suppose it is used to contrast forests/grasslands to croplands, but, in Europe, very few forests can be considered natural (in the sense of unmanaged) ecosystems. Their response to heat and drought is certainly mediated by species choice and other aspects of management.
Thanks for pointing out, we agree. This sentence has been removed in the revised version of the manuscript.