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 #3

The manuscript talks about a relevant topic showing the importance of consecutive dry and hot events for ecosystems. The paper investigates why temporally-compound compound extremes can amplify the damage to the ecosystem focusing on 2018 and 2019 in Europe.The paper addresses a relevant scientific question and it is within the scope of ESD with the potential to have an audience from a broader community of climate impact studies, agricultural systems, hydrology, and multi-sector dynamics.The paper concludes that process-based models miss the legacy effects of consecutive compound hot and dry summers. The final results are based on a) regression and correlation analysis, and b) LSM outputs. However, the data preparation has heavily relied on satellite scans from MODIS and simulation outputs of Land Surface Models.

We thank the referee for the constructive review of our study.

R3C1: The paper describes the data sources in great detail which is good. I understand that this is a critical part of the research, but it distracts the reader from the main message. I suggest moving the data construction information to the appendix.

We will move the description of the simulation protocol to the Annex and we have attempted to condense the description of the observation-based datasets, since they are published elsewhere.

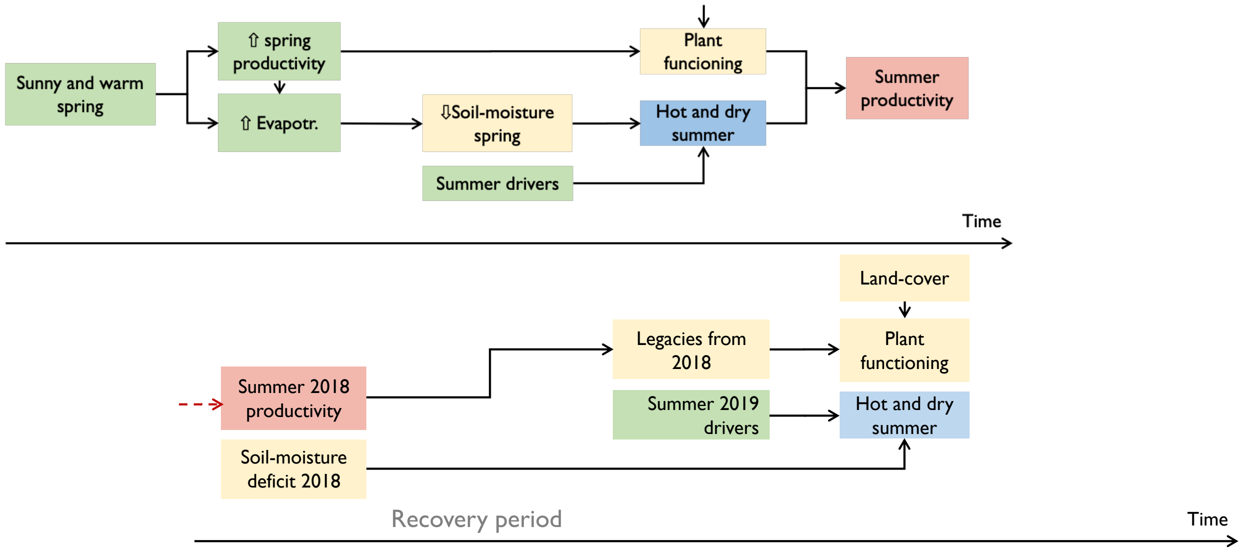
R3C2: Finally, I suspect the results are reproducible. It would be beneficial if the authors could share the major constructed datasets for regression analysis and related outputs of LSM models.

All datasets used in the paper are freely available, at exception of the extension of the LSM outputs for 2019. The SoMo.ml dataset has been published recently (https://www.nature.com/articles/s41597-021-00964-1). We have added a section on data availability with the links to the public repositories and will make the model outputs for 2019 available as supplement to the paper.

R3C3: The title of the paper talks about compound dry and hot summers. The model and data do not include metrics of compound extremes. The T and SM are often separated in the paper and are considered individually. I expected to see some compound indicators. I am not sure the term "compound" in the title is well-represented within the paper.

The whole analysis proposed here is based on the conceptual framework of compound event analysis. From a meteorological perspective, DH2018 and DH2019 extreme summers were "compound events" in that both high temperatures and strong drought conditions were observed. Each taken individually can be considered a multivariate compound event (Zscheischler et al., 2020). Additional effects that make each of these events "compound" from an ecological perspective are the preconditioning effects of the warm/sunny spring in 2018 (Bastos et al., 2020a), and of the impact of DH2018 in preconditioning the response to DH2019.

An expanded version of our Figure 1 is shown below:



Where green represents drivers of hazards, blue the hazards, red the impacts, and yellow preconditioning effects (Zscheischler et al., 2020).

In the paper, T and SM are considered separately only in the specific case of the linear regression shown in Figure 4. For the subsequent analyses, T and SM are analyzed jointly as predictors of EVI anomalies. The goal of the analysis in Fig. 4-6 is to separate the preconditioning effects of DH2018 in explaining the EVI anomalies in DH2019.
We acknowledge that the text might not have been clear in this respect, and we made an effort to improve the clarity in the revised version of the manuscript:

> *From a hydrometeorological perspective, the dry and hot summers in 2018 and 2019 (DH18 and DH19, respectively) could be considered individually as two compound events in that both high temperatures and strong drought conditions were observed (Zscheischler and Fischer, 2020) Taken together, they could also be analysed as a temporally compound event (Zscheischler et al., 2020).*

R3C4: The paper is written well. Still, the overall presentation requires revisions. Please describe the main variables in more detail in a table in the appendix. The reader deserves a clear description of the main variables of this study and the main model evaluating the relationship between them (e.g. variables explaining EVI in regression models). There is no single equation neither a table showing the underlying model of the study. While the main work is based on regressions and correlation analysis, the reader should wait until page 7 to learn about them. The problem is that the regression strategy has a vital role here. Are you estimating the marginal impacts for each pixel or a set of clusters? These should be clarified with a written equation with clear indexing of all the variables. In addition, this can be a useful reduced-form model for future studies. In its current form, the regression section looks pretty weak.
We have now re-written the methods section explaining the regression analysis. We would like to note that the methodology was also mentioned in the abstract. We have nevertheless revised the abstract and introduction to clarify the approach used.

R3C5: There is no discussion on the goodness of fit for regression and the causal analysis. The lagged EVI used in the model, while can be used to prove the existence of the legacy effect, does not tell us the exact sources of legacy effects. There is a high chance of omitted variables here (e.g. soil moisture in lower layers or disease as discussed in the paper). This could be briefly addressed in the appendix.

Thanks for pointing out. Indeed, one of the drawbacks of the current approach is that is cannot fully separate between the sources of legacy effects. Arguably, some of these causes could be pinned-down if, for example, long-term spatially explicit data on disturbance sources, and specifically for 2019, would be available. We added a note on this in the revised version of the discussion:

> *Increased vulnerability may be explained by modulating effects of global change on vegetation condition (e.g., ``hotter droughts'' (Allen et al. 2015), Fig. 1) and, in the case of DH19, it may be further linked to inter-annual legacies from the impact of DH18. The first should be expressed by relationships between EVIanom residuals and climatic variables. The latter are more difficult to assess without comprehensive data about different competing factors, e.g. defoliation or damage from Ruehr et al. (2019), higher susceptibility to diseases and pests due to reduced health (McDowell et al. 2020) or increased hazard of insect disturbances due to warm conditions (Rouault et al., 2006). The relationships between EVIanom residuals and EVI^{yr-1}anom provide an approximation, but do not allow to identify the underlying drivers.*

Unfortunately, spatially explicit data about other variables is not available, especially not up to 2019. Additionally, other effects, such as physiological mechanisms explaining legacy effects may be very difficult to attribute at the large scale at which this analysis is performed (5-25km). Nevertheless, we hope that this approach can be adapted for local scale studies, in which some of the additional variables needed (deeper-level soil-moisture, sap-flow, vegetation structure and allocation to leaves, stems, roots, etc, information about pests and diseases) might be available.

R3C6: Unfortunately, the interpretations and conclusions are more than what the model results show. For example, the claim in L316-319 is too strong. The correct conclusion is that "the proposed model" and current LSMs did not capture this legacy effect. This effect can be captured in future studies in models with different variables, metrics, and methods. This can be a shortage of the methods of this study. The results can be biased by omitting some variables (e.g. disease, nutrient, radiation, etc). This is the nature of science. Further investigation is required to have such a strong conclusion.

With this study, we show that considering inter-annual legacy effects of a given extreme (DH2018) is important to understand the dynamics of vegetation in response to a subsequent extreme event (DH2019). Such effects have been conceptually described, e.g. in (Ruehr et al., 2019; Gessler et al., 2020), but quantifying them at large scales remains a challenge (Kannenberg et al., 2020). Our modelling approach based on EVI is designed to detect legacy effects, if existing, and not necessarily to model them. Our residual analysis (Fig. 6) provides some hints of possible variables needed to understand these effects, but is by no means an exhaustive list. More data is needed to test different hypotheses about sources of legacy effects (see comment above). On the other hand, such processes can be implemented in LSMs – some of them are under ongoing development – so that their relevance to the observed dynamics can be evaluated. In line with the comments from R2, we have thoroughly revised the results and discussion and we hope that this point is now clearer.

R3C7: The paper employs the soil moisture data based on volumetric soil moisture of the top 28 cm. I expected to see other metrics of root-zone soil moisture depending on the vegetation dominance. The soil moisture in lower layers can be "a" major source of legacy effect which is ignored apparently. As mentioned in the paper: "total water storage was lower in 2019 due to the water storage deficit resulting from the 2018 event". Probably, a more precise hydrological product should be used to capture this.

The soil-moisture dataset (SoMo.ml) used here represents the top 50cm, and thereby already a significant portion of the root-zone. Furthermore, it is to be expected that the temporal variability in (slightly) deeper layers is similar and thereby somewhat reflected in our analysis. While we agree with the reviewer that it would be preferable to use an observation-based soil moisture product covering even deeper layers, we are not aware of such a dataset. For example, total water storage from GRACE includes groundwater, snow, lakes and rivers, so not really the plant-available water. Other remote-sensing based datasets (e.g. ESA-

CCI, SMOS) are limited to the surface layer. Therefore, we think that SoMo.ml is, in our perspective, the most adequate product to use here.

Other issues.

Define vulnerability

Vulnerability is now defined:

> *Vulnerability to DH is defined as the impact of the physical hazard (hot and dry conditions) on vegetation and assessed by remotely-sensed EVI and modelled GPP anomalies.*

L3: "though" does not seem the right word here.

Corrected.

L10: Please revise the sentence.

It now reads: *"These estimates correspond to expected EVI anomalies in DH18 and DH19 based on past sensitivity to climate."*

L29: "hot and dry" is better to be replaced by "dry and hot" to better represent the DH abbreviation.

Done.

L65: extra parentheses

Corrected.

L124: This is the data section. Maybe change the title to show this.

Corrected.

L268: is this your finding? If not move it to the discussion.

We rephrased for clarity: *"[…] and estimates summer water limitation and negative legacy effects from spring warming, consistent with process-based modelling studies."*