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Comment on egusphere-2022-425

Anonymous Referee #1

Referee comment on "Meteorological history of low forest greenness events in Europe in 2002-2022" by Mauro Hermann et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-425-RC1, 2022

In their paper, Herrmann et al. use MODIS NDVI at 0.05° (roughly 5 km) resolution to characterize 'meteorological storylines' preceding extraordinary forest summer NDVI over the period 2000-2020. The defined events are backed-up using a Landsat-based product which allows for identifying forest disturbance following an event. In addition to studying single events, they also investigate on two consecutive events in a row. The meteorological storylines of the identified events – which represent the core of the study – are quantified using ERA5 reanalysis temperature and precipitation integrated over 90 day seasons to better understand the triggers of extremely reduced canopy greenness. Moreover, they study sea-level pressure-based cyclone-anticyclone frequencies to get an idea of predominant circulation patterns of identified events. In general, the topic of investigation –describing triggers of extreme drought impacts on forest ecosystems – is of broad interest to the public and scientific community and thus deserves publication.

However, I yet recognize some major issues that have to be addressed to allow for a scientifically sound research paper. I have to mention, that I already reviewed an earlier submission of this manuscript to another journal, where I already raised quite a few of those issues. Altogether, I acknowledge that the authors have put quite some efforts into the manuscript to tackle some of the points I raised previously. However, some major points of my earlier review have unfortunately not been considered in the revision of the manuscript, which I still deem mandatory for a solid and sound analysis. I will outline those in the following:

- The authors based their analysis on monthly MODIS NDVI with a spatial resolution of 0.05°. As mentioned in my previous review, I wonder why the authors do not use MODIS NDVI at 250 m resolution. In combination with a fine-grained land-cover map (as used in their study) this would allow for masking most of the non-forest areas within the grid-cells under investigation. At current, the roughly 5 x 5 km grid-cells may still contain up to 50 % of non-forest area, which may substantially affect the corresponding grid-cell NDVI. As shown in other studies (some of which are referenced by Herrmann et al.), the response of different land-cover types to drought varies substantially. In particular, agricultural land-cover – which probably dominates the noise in the considered pixels – is known to respond earlier to extreme drought and consequently the effect of drought on forests might be overestimated not to mention the effect of different harvest dates if different crops were planted. The authors admit this caveat in section 4.5 and mention that increasing the threshold of forest proportion did not change the identified event hot spots while reducing the sample size. However, reducing the sample size would possibly remove some of the spotted just-significant 'pre-cursors' of droughts (e.g. the just significant percentage of dry periods 24 months prior to an event in Fig. 6) and thus, the meteorological storylines might be interpreted differently. If MODIS at 0.05 ° resolution were the only available remote sensing product for these analyses, I would probably accept it. But since there is a relatively simple solution to the problem, I believe the authors should do their best to maximize their analytical precision. That is, instead of using the coarse MODIS resolution, simply analyze MODIS at 250 m resolution, mask non-forest areas and then – if desired (but I doubt this is needed and you would lose information) – aggregate the remaining pixels to the target resolution, i.e. 0.05 °. By doing so, the precision of identified forest stress would certainly increase, likely resulting in a clearer picture of the whole study since artefacts resulting from non-forest land-cover can be largely ruled out.
- A less critical – but yet important – point refers to the statistical pre-processing of data, i.e. a standardization of non-normally distributed data (NDVI and precipitation). While the authors stress that the standardization is not used to derive any probabilities or return intervals, relying on normal-distribution related parameters such as mean and standard deviation is inappropriate, even if Shapiro-test indicates normal distribution. A bounded distribution as NDVI (between -1 and +1) cannot be normally distributed. Also, the arbitrarily chosen threshold of -2 suggests that the authors are targeting at the lower margin outside the 95 % confidence interval (-1.96). While this probably does not severely affect the outcome of the analyses, appropriate data treatment should nevertheless be the aim in a scientific study (and is actually easy to obtain). For the NDVI an approach based on proportional differences to the median might render an appropriate alternative solution. In terms of selecting a threshold for defining events a sensitivity analysis should be carried out to reflect the dependence on the selected threshold. This should in any case be done, if selecting any threshold (i.e. also for the currently chosen z-transformed data and -2). Regarding precipitation, I was wondering why the authors did not utilize some of the more frequently used drought-metrics, e.g. SPI or SPEI which automatically standardize the water balance between precipitation and PET and consequently better resemble actual plant water availability. Recall that 100 mm of precipitation in northern Europe means a lot more water available as plant water in comparison to the same amount in the Mediterranean just because of the large differences in PET.
- I am not convinced by the meteorological storylines, at least not in the way the authors interpret them. To me, it reads as if the authors believe to have found a 'recipe' for single and consecutive drought events. While I agree to the finding that concurrent (spring-summer) drought results in extraordinarily low NDVI (this has been shown in several studies before) I doubt that a Central European single drought event per se needs previous summer precipitation to be below average and previous winter precipitation to be above average as suggested by Fig. 5 (or taking Fig. 6 a high chance of drought occurrence 2 years in advance of a drought). It is at least very counterintuitive why an overshoot precipitation in the previous winter should be an

important 'ingredient' for a drought event, since it in fact would rather replenish soil-water resources. In contrast, a dry winter prior to an event seems more meaningful as described for the Mediterranean. A reason for these partly counterintuitive results is probably the limited number of events underlying these analyses. As pointed out by the authors, 42 % of single events refer to the 2018 drought, indicating that this specific event (with a wet winter in advance) has a strong fingerprint on the storylines. Even stronger is the effect of the 2018/2019 consecutive event which renders 82 % of consecutive events. Consequently, the meteorological conditions prior to the 2018/2019 event dominate the meteorological storyline for consecutive events, but I am convinced that also other constellations can lead to consecutive extraordinary NDVI. Given this dominance of the 2018/2019 event on these analyses, I was surprised to see precipitation to be above average in the winter 2018/2019 since in fact this winter was drier than usual in Central Europe, explaining why the drought actually simply went on in 2019 since soil water was not replenished – as usually – in winter. Therefore, I would be very careful with talking about legacy effects but rather interpret the drought 2019 as an ongoing drought. This can for instance be seen when studying soil water deficit indices or other soil-related drought metrics over the winter 2018/2019. Again, this makes me wonder whether the used climate parameter precipitation is the right candidate for quantifying drought impact. I have to stress, that the authors do discuss the disadvantage of having only a short period for their overall analyses in section 4.5, resulting in the inability to 'perform superior statistical modelling or resolve species specific responses' (the coarse resolution of 0.05° would anyhow not allow for the latter). Unfortunately, they do not mention the potential effect of single events dominating their meteorological storylines even though the numbers are given in section 3.2.3. To account for the dominance-effect, it would be interesting to see the storyline analyses based on a weighed random subsampling of events to avoid dominance of single events in the storylines. But all in all, I have doubts that an average storyline– i.e. a 'recipe' – for extreme NDVI exists beyond concurrent spring/summer conditions. The droughts of the past two decades all had in common that summers were extraordinarily warm and dry. However, the timing of drought differed and so did the conditions in the previous winter (warm-wet vs. cool-wet vs. warm-dry) as well as the previous summer. Thus, even if 100-year lasting NDVI time-series were available, I would be surprised if we were able to find a specific storyline resulting in single or consecutive drought events.

- Finally, I miss the mention of potentially additional noise on the analyses. For instance, in 2019 a late-frost event stroke parts of Central Europe which potentially affected NDVI in summer 2019. Same holds true for the winter-storm early in 2018. This also refers to my mention on the uniqueness of meteorological storylines preceding extraordinary NDVI values. While I agree that the main cause of shown events is spring/summer drought, the possible additional effects of late-frost/storm impacts and others – which is briefly mentioned at the initiation of the discussion – is not well elaborated, at least not in context of the meteorological storylines.

I regret, that I cannot be more positive. As mentioned earlier in this review, I appreciate that the authors have refined their analyses in comparison to the last time I reviewed this manuscript: compared to the previous version of the paper, it now reads much clearer and the analyses as well as their presentation have certainly improved. But given the major revision they undertook, I wonder what made the authors decide against a higher spatial resolution of MODIS data, which is freely accessible and also why they stick to the standardization of non-normally distributed data. Both issues could have been dealt with in course of the revision without extreme efforts and would allow for a more precise picture of forest drought meteorological storylines. At current, the selection of events is blurred by effects from non-forest land-cover and potentially inadequate standardization resulting in different absolute variations being treated equally (for instance if the standard deviation differs between pixels which can easily happen given the varying contribution of non-forest land-cover to the pixels but also when comparing different forest types).

As a way forward, I (again) recommend to utilize high-resolution MODIS products and mask non-forest areas from the analyses. Inappropriate standardization of data should be avoided (suggestions are given in point 2 above). Regarding the meteorological storylines, I recommend to randomly sub-select the data to avoid single events – such as 2018 or 2018/2019 – dominate the storylines. This may result in a lower overall sample size but that could be accounted for by bootstrapping the random subsamples over several iterations (just make sure this does not result in a stronger contribution of single events, i.e. keep the weighing for each iteration constant). When interpreting corresponding results, I would highly appreciate if the authors communicate their findings in a less straightforward way, i.e. avoid selling a 'recipe for droughts' but rather tone down to something like: meteorological conditions that have preceded extreme forest drought impacts over the last two decades and recall that under anticipated climate change 'storylines' may change if drought frequencies increase.

I hope my comments help the authors refining their analyses towards a more robust interpretation. At this stage, I refrain from commenting on textual aspects of the manuscript since I believe that the revisions change a larger part of the methods, results, and discussion.