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Interactive comment on "Does drought advance the onset of autumn leaf senescence in temperate deciduous forest trees?" *by* Bertold Mariën et al.

Anonymous Referee #2

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The article analyzes the impact of drought on the onset of autumn senescence and the difference featured by different temperate deciduous tree species. The authors used a manipulative experiment of tree beech sapling and three years of data on beech, birch, and oak trees. The authors show that drought did not affect the onset of senescence. Tree saplings showed high mortality with drought, and mature trees showed higher leaf mortality. No significant differences across species were observed. The manuscript deals with a significant subject, senescence, about which not much is yet known. Understanding the senescence process, particularly in relation to drought, is fundamental to predict the phenological cycle of temperate trees better.

Regarding the greenhouse experiment, I have a methodological concern. From the data reported in Fig 1 seems that the "drought" treatment does not have a significant

(should be tested statistically thought) effect on soil moisture (Fig 3c). Instead, the effect was mainly an increase of VPD that is not drought but an increase of the atmospheric evaporative demand. One of the factors linked to the earlier senescence in the case of drought is abscisic acid accumulation (ABA). Long term ABA responses should be more induced by soil moisture. Root perceives reducing soil moisture and upregulate ABA synthesis. ABA is a factor controlling earlier senescence (and stomatal regulation). I am not aware of studies showing the high VPD can trigger the same response in terms of upregulation of ABA. It could be that the lack of response observed was simply de to the fact that the reduction of soil moisture was not enough to trigger the physiological response inducing earlier senescence. Also, in general, I would not call drought the treatment. Given the data shown in Figure 1, I think it is more heat stress. Please provide more insights to understand whether the treatment can be indeed called drought treatment. If not, I would suggest talking about heat stress and increased atmospheric aridity. This would not diminish the paper. There is a lot of discussion on the different repose of plants to decreasing soil moisture and/or increasing VPD, and here I think the authors are looking at increased VPD and not necessarily at drought. This can also support the discussion of the differences between 2018 (more soil moisture stress) and 2019 (more heat and VDP stress) – see discussion at line 469-470. The 20% reduction of incoming light should also be better addressed (Line 162-163): though unclear, it seems that senescence is controlled by photoperiod. How does 20% - decrease in incoming radiation affect the photoperiod? The authors should check this and evaluate if the reduction of light has an impact on the results.

In the methods section 2.1.2, when the CCI is mentioned the first time, I expected a description of the sampling (that comes later). I think it would be beneficial to move section 2.2 above, where the CCI is mentioned the first time. Preferentially, put a reference in paragraph 2.1.2 to paragraph 2.2. The meteo stations are 20 and 60 km from the sites. But there is no information about where these stations were located (in a city, in a forest, in a grassland, at which height). Even if the climate regimes can be similar at a distance of 20-60 km can we be sure that the microclimate is comparable?

At the moment all this info are missing. I suggest the authors carefully check all this information and provide a methods description that can prove the study's robustness.

1) The equation and symbols do not follow the scientific format. I suggest to rewrite them. Also many variables have names that are more for a programming language but not following the scientific notion. I suggest to follow the IUPAC standards, or at least try to go close to that format. Avoid using "Leaf_place"Also please define the variable the first time is used, and then stick with the symbol: one example is the "day of the year" that in the equation 2 (model 1) is Doy and in the text is "day of the year"

2) If I am not wrong there is a mistake in Eq 1. First if rH should not be expressed in % as indicated but as fraction (rH[%]/100)

Here the result of VPD with the current equation > T <- 25 > rH <- 50 > e0 <- 613.75*exp((17.502*T)/(240.97+T)) > e <- rH*e0 > VPD <- e0-e > VPD [1] -155829.6

Moreover, even if the rH is used in the correct unit, the VPD unit is wrong. The resulting VPD from this equation is in Pa and not kPa as indicated at line 144.

> T <- 25 > rH <- 50/100 > e0 <- 613.75*exp((17.502*T)/(240.97+T)) > e <- rH*e0 > VPD <- e0-e > VPD [1] 1590.098

The VPD reported in the figures seems correct, therefore please verify is there is a problem in the Equation.

3) There are few track changes and typos in the manuscript. Please edit careful the article a. Line 249, line 415, 416, 417, 4) The reference to the R package is a bit strange R/ggpubr etc. Please modify in: "we use the R package ggpubr (Reference)". But it is very nice that the authors cite all the packages. This is important and often overlooked. 5) Please report "Model 1 and 2" in a less R script style. Please use mathematical notation 6) 6) I think the breakpoint analysis was achieved with the "segmented" package and not dplyr", correct? 7) Line 464-465 – this is interesting, please elaborate more this point.

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