Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-337-AC4, 2021
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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.

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Received and published: 12 January 2021

Dear Anonymous Referee 2,

Thank you for your review and suggestions. We will respond here to your comments:

1) The Referee asks whether it is possible that the reduction of soil moisture in the glasshouse experiment was not enough to trigger the physiological response inducing earlier senescence. He therefore questions whether an increased VPD can trigger the upregulation of ABA. Literature shows that ABA, which is known to control earlier senescence, is indeed upregulated as a response to the stomatal changes corresponding to changing vapor pressure deficit levels (McAdam and Brodribb, 2016;McAdam et al., 2016;Bauerle et al., 2004;Xie et al., 2006). We agree that the treatments

+0ËŽC and +3ËŽC did not result in large differences in soil water content. However, we will test this statistically, as suggested (for example, see the supplementary file 'TEST_SWC_markdown' and Rose et al. (2012) for additional information on the possible statistical methodology). On the other hand, it is likely that larger differences were present between the reference plots and the treatments, as the reference plots were irrigated more (L. 160), and such irrigation regime showed values of soil water content of up to ca. 0.20 m^3/m while the values of 0.05 m^3/m^3 were reached in the treatments (see Fig 1; unfortunately, sensor malfunctioning did not allow us to gather soil water content data for the reference plots). Given that we observed a high mortality, it might have been the case that our +3 ËŽC treatment was too extreme, triggering necrosis instead of earlier senescence (Munné-Bosch and Alegre, 2004).

2) The Referee suggests we talk about heat stress rather than drought stress. As mentioned above, the reference plots were irrigated more than the treatments plots (L. 148 - 149; 159 - 160). Therefore, the more appropriate definition would be "treatment based on warming, less irrigation and increased atmospheric aridity". We could use this definition (although longer and somewhat impractical it is the closest to reality). In reference to L. 469 - 470 ("…the drought of 2019, which coincided with several heat waves, might have been less damaging for late summer leaf dynamics, than the drought of 2018…"), a more detailed comparison between experimental manipulation and mature trees in years 2018 and 2019 would have required a factorial approach separating drought and warming, while our design was more basic. In addition, as shown in figure 3, the rainfall deficit was high in all years. It is true that the rainfall deficit was extremely high in 2018 – 2019, but the rainfall deficit was also high in 2017 – 2018 and 2019 – 2020. Likely, more site specific measurements on the soil water content would indeed have been useful. Note, however, that figure 2 and table 1 also indicate that there was not only little precipitation but also that this precipitation fell in irregular patterns, making potential droughts more likely.

3) The Referee asks to comment on the effect of the 20% reduction in light due to

the colorless polycarbonate roof in the glasshouses (L. 162 – 163; "A draw-back of the experiment is that the saplings in the reference plots received more incoming light (i.e. ± 20%) than the saplings in the glasshouses (Van den Berge et al., 2011)"). The Reviewer raises an interesting point: can a reduction / change in the light affect the photoperiod? Preliminary tests suggested that the ratio of light in different wavelengths (e.g. R/FR) during civil twilight (i.e. what is required for phytochrome to detect the photoperiod) does not change seasonally significantly in our study area. This provide indirect evidence for us to believe that our light reduction (limited to 20%), combined with the fact that very low light intensities are needed for plants to detect photoperiod (Legris et al., 2019;Poorter et al., 2019;Franklin and Quail, 2010), would not have caused significant changes in photoperiod. We agree that it could be interesting to test the effect of the roof alone. However, this is not feasible in the short term. The effect of the roof is also partly captured by the results on the saplings in the +0 °C treatment glasshouses.

4) The Referee asks to consider restructuring section 2.2 and 2.1.2. We will consider this in the revision.

5) The Referee asks to provide more information on the meteorological stations. We will add the following information to the manuscript in the revision. (1) The station of Ukkel is located within a green area in the suburb of Brussels (thus, classifiable as "urban park"). The microclimate is expected to be different than at our study sites. However, data from Ukkel were used to describe the intra-annual variability and long-term trends (Table 1 and Fig. 3), which are less affected by microclimate. (2) The meteorological station of Brasschaat is very close to our sampling site in the Park of Brasschaat and in the Klein Schietveld (± 3 km and ± 4 km, respectively). The meteorological station in Brasschaat is a 40 m high scaffolding tower, at which measurements are taken at various heights, and stands in a patch of mixed forest covered mainly by Scots pines and deciduous tree species, such as oak and birch (see Carrara et al. (2003) for more information). Data of the temperature, precipitation and humidity were

taken at the top of the tower. Data from Brasschaat were used to describe the seasonal pattern in 2017, 2018 and 2019, and as input to the models. (3) The station of Woensdrecht is located in an open field at a local airport surrounded by heathland and urban area. It is located near the Markiezaatsmeer, an enclosed swamp ecosystem, within the river mouth of the Schelde. The measurements in both Ukkel and Woensdrecht are taken at a height of 1.5 m. However, these data were only used as gap-filling in case of short term gaps in the long-term Brasschaat series. In terms of differences in the microclimate, it is indeed not ideal that we needed to use data from the meteorological stations of Ukkel and Woensdrecht. However, we are limited here by the availability of the data and the meteorological stations of Ukkel and Woensdrecht are closest (and most representative) for our sampling sites.

6) The Referee comments on the style of the model notation and suggests to better define the variables at first use. We will define the variables further at first use and avoid inconsistencies. However, both the descriptive style and mathematical notation are based on examples and suggested notation in the specific literature (Zuur et al., 2007;Zuur et al., 2010;Zuur et al., 2011;Zuur et al., 2016;Simpson, 2018;Pedersen et al., 2019;Wood, 2017) and readers interested in background references might find it easier if style consistency is respected. Perhaps the Editor can comment on the journals preference?

7) The Referee notes there is an error in the units of the equation on the vapor pressure deficit. Thanks, we will correct this in the revision. The actual and saturation vapor pressure deficit are indeed in Pa, while the relative humidity should be noted as a fraction. The data was indeed calculated using the correct equation.

8) The Referee points out some typo's. The will be addressed in the revision.

9) The Referee suggests writing R packages in a different format. If preferred by the Editor, we will address this in the revision.

10) The Referee suggests using only the mathematical notation for model 1 and 2.Con-

sidering the literature (see the response on comment 6) and the preference of the Editor, we will address this in the revision.

11) The Referee suggests to remove the reference to the R package “DPLYR” as the breakpoint analysis is done only using the R package “SEGMENTED”. While “DPLYR” was used for data wrangling, we agree “SEGMENTED” is indeed the package that is used for the breakpoint analysis. We will remove the reference to “DPLYR” in the revision.

12) The referee asks to elaborate on L. 464. (“For the mature trees, the different drought response of the autumn pattern of chlorophyll (no effect) and the loss of canopy greenness (advanced and enhanced) is probably an important reason of confusion still present today in the literature on the relationship between drought and autumn senescence”). We thank the referee for this suggestion and will consider this in the revision. While the detoxification of chlorophyll is a prerequisite for the expression of different coloration values, chlorophyll does not degrade at the same speed as other leaf pigments. In fact, not even all leaf pigments degrade (or are formed) at the same velocity throughout the senescence process (Keskitalo et al., 2005). Consequently, observations of changing coloration levels are difficult to interpret. Moreover, note that coloration measurements also take into account leaf yellowing and mortality due to hydraulic failure.

Kind regards, The authors

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2020-337/bg-2020-337-AC4-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-337, 2020.