Dear Dr. Vamosi,

We hope that you will consider the revisions to our manuscript "Reconciling competing hypotheses regarding flower-leaf sequences in temperate forests for fundamental and global change biology" for publication as a Viewpoint article in _New Phytologist_.

Deciduous woody plants of the temperate zone exhibit considerable variation in the order of reproductive and vegetative events, or flower-leaf sequences (FLSs). Several long-standing hypotheses suggest FLSs are under strong selection and critical to fitness, yet research efforts have yet to yield a consistent, well-supported explanation for this variation. We argue that advances may have stalled because FLSs are typically classified into categorical descriptors that do not reflect the underlying biology of the FLS hypotheses. We present and test a new conceptual framework for the study of FLSs built on 1) quantitative measures of FLSs and 2) observations of FLSs below the species level. Our treatment of this topic charts a path forward to better understand both the evolutionary origins of FLS variation and the consequences of observed FLS shifts associated with climate change.

We are pleased that two of the reviewers found previous revisions to the manuscript satisfactory and appreciate both Reviewer 1's acknowledgement that the manuscript and its analyses are much improved as well as their continued input towards its refinement.

Reviewer 1 asked us to elaborate on the sensitivity of our models to the use of different phenophases, which we have done both in the main text of the manuscript as well as in the Supporting Information. They also asked us to clarify our usage of drought metrics to represent the water limitation hypothesis. We explain our decision-making process for choosing metrics to represent this hypothesis at length below and have added an additional analysis using a robust aridity index to represent this hypothesis. We are pleased to report that the inferences from our models are robust to the use of any one of the four alternative predictors we now provide to represent the water limitation hypothesis. We are grateful to the reviewer for pressing us to think harder about this hypothesis.

As with our previous re-submission, we feel that the reviewer's input has helped shape a new submission that is much improved, and we detail our specific changes in the following pages with reviewer comments in _italics_ and our responses in regular text.

We hope that you will find it suitable for publication in _New Phytologist_, and look forward to hearing from you.

Best,

Daniel Buonaiuto

I appreciate the authors have considered all comments from the 3 reviewers, which definitely improves the strength and robustness of the analyses and of the manuscript itself. I only have a few minor questions/ suggestions:

We thank the reviewer for recognizing the improvements we made to our manuscript, and appreciate their continued input. We found their comments and suggestions informative and helpful, and have done our best to incorporate them into an updated version of the manuscript. Our specific responses to their questions and suggestions can be found below.

I like the new Figure S7, but do not entirely agree with the authors regarding its interpretation. In my view, it seems that the choice of the leaf and flower phenophases does modify the relationship between the FLS and the pollination syndrome. Would that be possible to add a word on this in the main text and discuss this result in Supp mat?

We thank the reviewer for calling our attention to the need to clarify our description of this figure and we agree with their interpretation. In our previous submission we highlighted the sensitivity of our analyses to the specific phenophases used to define FLS (line 286):

"We found the estimated effect of traits (representing different hypotheses) varied when FLSs were defined based on different sub-phases of flowering and leafing; for example, days between flower budburst and leaf budburst vs. days between peak flowering and leaf expansion (Fig. S7)."

We have modified this text in line 289 as the reviewer suggested to specifically reference the differences in the pollination syndrome estimates:

"We found the estimated effect of traits (representing different hypotheses) varied when FLSs were defined based on different sub-phases of flowering and leafing; for example, using days between flower budburst and leaf budburst vs. days between peak flowering and leaf expansion modify the strength of the pollination syndrome effect estimates (Fig. S7)."

Additionally, our revised manuscript now discusses this sensitivity more explicitly in the caption of Fig. S7.

I appreciate that the authors tried to explain the relationship between annual P-ETP and the inter-annual FLS variation; but I still do not understand why they did not include such variable in their multi-species hierarchical model instead of mean precipitation (L249-251). P-ETP can be calculated at stand-scale and not only at landscape scale. Also, what does "species moisture's use" mean?

We appreciate the reviewer's continued thoughts on the issue of incorporating drought sensitivity into our models. Climatic water balance (Precipitation-potential evapotranspiration, P-PET or P-EPT) is one among several aridity indices that accounts for both water supply and evaporative demand to characterize aridity. As noted in Speich (2019) potential evapo-transpiration is an ambiguous metric and there are several formulations to calculate it. We converted our estimates of evapotranspiration from measurements of latent energy flux (Knauer *et al.*, 2018) from an eddy covariance tower close to the site where the phenological data we used in our analysis were collected.

We agree with the reviewer that ETP can be calculated at the stand level. For us to do so on a scale that would relevant for our analyses would require significant additional field work over multiple seasons including

extensive soil water and atmospheric measurements and additional analyses (Garnier, 1952; Allan *et al.*, 1998).

We have also tried to find established estimates of the range of P-ETP values that our species of interest tolerate in their home ranges from the literature in order to characterize differences among them. This effort has not been successful, but if we have missed any relevant data that we can be directed to we'd be happy to use them in a model.

In our search for established species-level metrics of aridity tolerance, we identified a data source that estimates each species aridity tolerance across their native range using a similar aridity index: the actual evapotranspiration/potential evapotranspiration ratio (AET/PET) (Thompson, 2012). Similar to climatic water balance, the AET/PET ratio is considered to be a more realistic depiction of the moisture conditions experienced by plants in comparison to seasonal or annual precipitation (Thompson, 2012). In fact, AET/PET can be very similar to soil based measures like P-ETP (Speich, 2019).

We have updated our analyses to included this additional predictor for the water limitation hypothesis. We ran our models using the lowest 10% quantile of AET/PET across a species' range as a proxy for the water limitation hypothesis and it did not impact the inferences of our models (see fig. S7 "aet/pet ratio"). We also note that for the species in our analyses, the lower 10% quantile of the AET/PET across a species range is highly correlated with the predictor we used in our main model—minimum precipitation across a species' range (correlation coefficient of 0.82).

We feel that the use of the AET/PET ratio in our analyses captures water limitation in way that is comparable to P-ETP. We hope that this additional analysis is satisfactory, but, if critical, we could attempt to calculate P-ETP over the range of each species in our study using grided climate data and flux tower measurements though we would appreciate guidance on how best to estimate it and several additional weeks to pull the climate data and write the code to construct the variable. Because we now provide three alternative predictors (four total) for the water limitation hypothesis, we feel that constructing this additional metric would more appropriate to a research paper focused on moisture limitation or metrics of moisture limitation, but can work to pursue it if critical.

Additionally, one of our other alternative predictors for the water limitation hypothesis—"species moisture use"—is a metric used by the USDA, which they defined as "Ability to use (i.e., remove) available soil moisture relative to other species in the same (or similar) soil moisture availability region" (USDA/NRCS, 2020). We have added this definition to our methods in the Supporting Information (line 69).

L45: change "are most depleted" to "their lowest level". Plants rarely deplete their NSC reserves. see Martinez-Vilalta, J., et al. (2016). Dynamics of non-structural carbohydrates in terrestrial plants: a global synthesis. Ecological Monographs, 86(4), 495-516.

We agree with the reviewer's point here and have made this change in the manuscript (line 48).

L81: Not correct, Change "more resistance to drought' to "more sensitive to drought-induced xylem embolism"

We have also made this change (line 85).

References

Allan R, Pereira L, , Smith M. 1998. *Crop evapotranspiration-Guidelines for computing crop water requirements-FAO Irrigation and drainage paper 56.* vol. 56.

Garnier BJ. 1952. A simple apparatus for measuring potential evapotranspiration. *Nature*, **170**: 286–287.

Knauer J, El-Madany TS, Zaehle S , Migliavacca M. 2018. Bigleaf - An R package for the calculation of physical and physiological ecosystem properties from eddy covariance data. *PLoS ONE*, **13**: e0201114.

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Thompson RS. 2012. *Atlas of relations between climatic parameters and distributions of important trees and shrubs in North America : modern data for climatic estimation from vegetation inventories.* U.S. Department of the Interior, U.S. Geological Survey, Reston, VA].

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