

Interactive comment on “Predictors and mechanisms of the drought-influenced mortality of tree species along the isohydric to anisohydric continuum in a decade-long study of a central US temperate forest” by L. Gu et al.

Anonymous Referee #3

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This study investigates long-term trends in tree mortality (2004–2014) observed in a central hardwood forest that experience a range of hydrologic condition during the study period, including two severe droughts in 2007 and 2012. The 2012 drought was particularly severe not only in this site, but also in many other parts of the U.S. The mortality trends are linked to a similarly long time series of pre-dawn leaf water potential, and various other proxies for variability in hydrologic stress. The principal findings are that mortality of all species tended to increase in years following a drought event, and that oaks experienced mortality at a higher rate than other species. The latter re-

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sult is especially interesting given that oaks are generally believed to be more drought tolerant than other canopy co-dominants (see, for example, Abrams 1990). Linking mortality to the long-term pre-dawn leaf water potential is also a novel feature of this manuscript; and the effort necessary to collect those data for a decade is substantial and should be applauded. I am sure that researchers from a wide range of fields will find these data interesting, as they have the potential to advance our understanding of drought-induced tree mortality in forests like the Missouri Ozarks flux site that lie on the transition between water-limited and energy-limited systems. This is particularly true in the case of the oak species.

However, I have some significant concerns about the way the data are interpreted. Principally, I disagree that pre-dawn leaf water potential a reliable metric with which to classify species as isohydric or anisohydric. The authors are correct in defining isohydric species as those that regulate leaf water potential closely, and in defining anisohydric species as those that allow leaf water potential to drop during periods of hydrologic stress (with an associated higher risk of xylem cavitation). However, classification of species along this continuum should reflect the trends in leaf water potential during periods when gas-exchange is occurring (i.e. mid-day), and not during periods of relatively little water flow through the stem (i.e. pre-dawn). The recent work by Martinez-Vilalta et al. (2014), for example, uses variation in mid-day as compared to pre-dawn leaf water potential as the principal diagnostic for isohydric-to-anisohydric behavior; the slope of that relationship is relatively shallow for isohydric species, and relatively steep for anisohydric species. The pre-dawn value alone is insufficient alone to permit a classification of plant water use strategy using this framework.

I also disagree that pre-dawn leaf water potential is sufficient to diagnose the occurrence of xylem cavitation. Stem water flow is usually represented with an Ohm's law analogy:

$$\text{Water flux} = K(\text{PSI}_{\text{soil}} - \text{SPI}_{\text{leaf}} - \text{pgh})$$

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where K is the hydraulic conductivity, PSI_{soil} and PSI_{leaf} are soil and leaf water potentials, and pgh represents gravity headlosses. If the water flux approaches zero (as is often assumed to be the case in pre-dawn periods), then that implies that PSI_{soil} and $(PSI_{leaf}+pgh)$ are equivalent. The value of K is irrelevant if the water flux is zero. In this idealized scenario, the main determinants of species-specific differences in PSI_{leaf} will be differences in the effect PSI_{soil} relevant for each tree (i.e. rooting depth), as well as differences in tree height across species. The latter is not addressed in this manuscript, and tree height data are not presented.

In the case of non-zero pre-dawn water flow, species-specific differences in PSI_{leaf} may incorporate information about K , but importantly these differences will also reflect: a) variations across species in nocturnal stomatal or cuticular conductance and/or temporal variation in vapor pressure deficit (which could promote a non-zero nocturnal stem flow), or b) the extent to which plants refill depleted water stores during the night, which is a widely recognized feature of plant water use for many species (e.g. Scholze et al. 2011). These processes are not addressed in the manuscript.

Ultimately, I think that the way the authors have interpreted the data, which is not consistent with recent advances in the field, detract considerably from what are really novel and interesting results on species-specific susceptibility to drought-induced mortality, and also novel and interesting results about species-specific differences in pre-dawn leaf water potential (which to a first order reflect species-specific differences in rooting depth and canopy architecture, with some caveats as listed above).

A few other minor comments are:

- 1) It is a concern that much of the analysis is linked to Gu et al. (2015), which is under review. Has there been any change to the status of that paper?
- 2) In discussing future drought trends, the authors may want to consider citing the new work by Cook et al. (2015, Science Applications).

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3) Do the author's classification of species as isohydric/anisohydric agree with other relevant literature on the topic? If not, can reasons for the discrepancy be discussed? (see, for example, Thomsen et al., 2013, Forests).

4) The authors state on page 1304 that "no accepted mechanism exists for" xylem refilling in the absence of rain. Some recent work (e.g. Sala et al. 2012) suggests that NSC carbohydrates can be deployed to assist in xylem repair by affecting osmotic potential.

5) It is unclear to me why some of the regressions (i.e. those Figure 8) are linear, whereas others (i.e. Fig 11) represent a non-linear function.

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