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Response to "Do dead trees tell tales?" – Johnson et al.'s critique of van Mantgem et al.'s "Widespread increase of tree mortality rates in the western United States"

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In a presentation posted to the online proceedings of the 2009 Western Mensurationists' Meeting, Johnson et al. (2009) critiqued our recent publication describing pervasive and rapid increases in background tree mortality rates in old forests of the western United States (van Mantgem et al. 2009). We have subsequently been contacted by colleagues who viewed the online presentation and asked for our response, sometimes asking whether Johnson et al. had revealed serious flaws in our work. We therefore invited Johnson et al. to participate in a formal scientific exchange in the peer-reviewed literature. Johnson et al. declined (email of 31 August 2009 from Greg Johnson et al. to P.J.V.), so we present our response to their critique here.

In their conclusions (slide 21), Johnson et al. summarized six primary criticisms of our work. Below we provide point-by-point responses, showing that three of their criticisms were based on inappropriate methods, metrics, or assumptions, and the other three offered no evidence or analyses in their support, although we have offered contradictory evidence and analyses. We conclude that Johnson et al.'s work does not successfully challenge our findings or alter our conclusions.

1. Johnson et al. (2009) stated that their analysis "… indicates that mortality rate has been stable over the last decade or more on the plots used by [van Mantgem et al.]."

Contrary to what is implied by this statement, Johnson et al. reached this conclusion for only a subset of our data, from the Pacific Northwest. Thus, their analyses examined the nature

of the increase in tree mortality rates in one of our three study regions, suggesting that in the Pacific Northwest mortality rate leveled off after an initial increase.

To reach their conclusion, Johnson et al. inappropriately used LOESS regression, which effectively fit a temporal moving average through plot-level mortality rates. Unlike the generalized nonlinear mixed models we used (van Mantgem et al. 2009), LOESS regression and related approaches are vulnerable to site-switching bias (Hall et al. 1998, Phillips et al. 2004), which can arise when the set of plots analyzed changes through time (Fig. 1). Johnson et al. appear to have acknowledged and warned about potential for site-switching bias (see their slides 11 and 16).

The period Johnson et al. identified as having stable average mortality rate in the Pacific Northwest – from the early 1990s to the end of the record – shows particularly strong geographic patterns in plot turnover, and thus enhanced potential for site-switching bias (Fig. 2). British Columbia, the northernmost and most maritime of our sites, first enters the data set at the beginning of this period. Washington begins to drop from the record in 1999, and by 2003 the data set is almost completely dominated by Oregon and British Columbia (Fig. 2). By 2007 only British Columbia remains.

For LOESS regression and related approaches, we know of no robust way to determine the magnitude of, or correct for, site-switching bias in data sets such as ours. Unlike the hypothetical example illustrated in Fig. 1, mortality trends in individual plots are highly variable through time due to local insect outbreaks, windstorms, random treefalls, and so on (see Figs. S3-S5 in van Mantgem et al. 2009). The signal-to-noise ratio for individual plots or small groups of plots therefore tends to be low, and we have no good way of predicting what mortality rates would have been for specific plots during periods in which they were not recording data.

It is precisely for these reasons that we fitted our data using generalized non-linear mixed models, with plot identity as a random effect (van Mantgem et al. 2009). Our models fit a common slope parameter to all plots, but effectively allowed intercept to vary from plot to plot. Polynomial forms of our models (Supporting Online Material in van Mantgem et al. 2009), potentially able to reveal non-linear changes in mortality, do not distinguish between declining or increasing mortality rates during the last decade of data in the Pacific Northwest (i.e., confidence intervals are large for this region of the model), perhaps due to data limitations such as small samples and relatively coarse temporal resolution.

Thus, while it is highly unlikely that tree mortality rates in the Pacific Northwest have increased in a perfectly linear fashion [if anything because environmental drivers such as climatic changes have not changed linearly (cf. Bigler et al. 2007, van Mantgem and Stephenson 2007)], (1) the approach taken by Johnson et al. to reveal non-linear changes was vulnerable to site-switching bias, and (2) our approach, while consistently showing an overall increase in mortality rates, is ambiguous concerning the shape of the mortality trend for the final decade in the Pacific Northwest, perhaps due to data limitations. The fact remains that over the entire period of study mortality rates increased in the Pacific Northwest as well as in our other two study regions (van Mantgem et al. 2009).

2. Johnson et al. (2009) claimed that a much larger sample size would be needed to achieve "sufficient statistical power to draw conclusions from the data."

Johnson et al. calculated a variance inflation factor for the non-independence of trees in plots, and concluded that we required about a four-fold larger sample of trees to detect trends in

tree mortality rates. This conclusion seems to be at odds with the fact that they conducted analyses in support of their first conclusion (above) using an even smaller subset of our data.

Regardless, Johnson et al.'s argument ignores the fact that our generalized nonlinear mixed models explicitly accounted for both variability among plots and repeated measures within plots. Thus, the low *P*-values presented in Table 1 of our paper (van Mantgem et al. 2009) serve as *prima facie* evidence that our approach had sufficient statistical power.

3. Johnson et al. (2009) asserted that "The post-hoc use of existing permanent plots does not create a sample design able to make inferences over the geographies studied."

Johnson et al.'s fourth slide suggests that this assertion reflects their concern that our 76 plots were not originally established as part of a unified subcontinental-scale research design – one that stratified plots by "bio-geo-climatic zones" and by "species, stand density, natural catastrophic events, and management effects" within bio-geo-climatic zones.

However, post-hoc use of existing data does not preclude inference; if it did, a large and pivotal body of epidemiological studies would not be available to contribute to improvements in human health (to name just one example). While *a priori* stratification is highly desirable because it can minimize potential for bias, lack of *a priori* stratification does not automatically mean results are biased – it means extra analyses should be conducted to determine whether bias exists. As discussed in van Mantgem et al. (2009 and Supporting Online Material), we carefully controlled for catastrophic events and past management actions; analyzed our data separately by broad geographic regions, elevational zones, tree size classes, major tree taxa, and past fire histories; used a modeling approach that avoided site-switching bias (see #1, above); and assessed the potential for bias due to spatial autocorrelation among plots, differences in plot sizes, changing census interval lengths, and differences in stand densities. As we reported, in no case did we find evidence of bias, and in all cases we found evidence of pervasive increases in tree mortality rates, regardless of the particular subset of trees examined.

4. Johnson et al. (2009) stated that they "did not find empirical support for a logistic model to describe the observed changes in mortality rate over time" and that "the choice of the logistic form made it very difficult to come to any other conclusion than that mortality rates were increasing."

The logistic model is a widely-used standard in analyses like ours (Manly 2001). Johnson et al.'s LOESS regression results cannot be considered evidence that our use of a simple, flexible logistic model was inappropriate; as we described (#1 above), Johnson et al. applied their LOESS regression to only a subset of our data, and the analysis was inappropriate because it was vulnerable to site-switching bias. Additionally, Johnson et al. present no evidence that they attempted to fit a logistic model to any of our data. We can therefore find no evidence that Johnson et al. conducted analyses capable of demonstrating that they "did not find empirical support for a logistic model."

Unless we have misinterpreted its meaning, Johnson et al.'s second statement is clearly false. Logistic models are not intrinsically biased toward showing increasing mortality rates – or, for that matter, decreasing mortality rates. Furthermore, our non-parametric tests comparing first and last census intervals independently confirmed the increasing mortality rates revealed by our logistic models (see Supporting Online Material in van Mantgem et al. 2009).

5. Johnson et al. (2009) stated that they "… did not find convincing evidence that endogenous factors could be ruled out in the explanation of observed mortality on the plots in question."

Johnson et al. apparently reached this conclusion by finding no significant change through time in Reineke's stand density index (SDI) (Reineke 1933) – an index of potential for competition. However, this argument is flawed. Rather than remain constant, potential for competition would need to increase through time to reasonably be considered a possible endogenous cause of the observed increases in tree mortality rates.

Regardless, Reineke's SDI is an inappropriate metric because it applies only to even-aged, monospecific stands, not to mixed-aged, mixed-species stands such as those of our study (Shaw 2006). Additionally, Johnson et al. inappropriately included all 76 plots in their analysis rather than limiting analysis to the 61 plots with complete recruitment data (van Mantgem et al. 2009, and Supporting Online Material).

As we reported (van Mantgem et al. 2009), both forest density and basal area declined slightly but significantly during our study period ($P \leq 0.028$), suggesting that potential for competition declined slightly, and thus the increases in mortality rates could not reasonably be attributed to endogenous increases in potential for competition.

6. Johnson et al. (2009) concluded that "No clear case can be made for changing climatic effects on tree mortality from these data."

Johnson et al. presented no evidence supporting this statement. Specifically, we can find no evidence that the authors analyzed climatic and hydrologic trends at our study sites; correlated these trends with tree mortality rates; interpreted results in light of related studies in the same study regions; considered the biological mechanisms by which climate affects tree mortality; or formally compared climate to other potential drivers of increasing tree mortality rates, both endogenous and exogenous. We did all of these things (van Mantgem et al. 2009). While we have never claimed to have proven a cause-and-effect link between climatic changes and increasing tree mortality rates, for the reasons we have presented we believe that the effects of regional warming and consequent changes in the hydrologic cycle are the most likely contributors to the observed rapid increases in tree mortality rates (van Mantgem et al. 2009).

Conclusions

Of Johnson et al.'s (2009) six primary criticisms of our work, three (1, 2, and 5) were based on inappropriate methods, metrics, or assumptions, and the other three (3, 4, and 6) offered no evidence or analyses in their support, although we have offered contradictory evidence and analyses in our original publication and here. We conclude that Johnson et al.'s work does not successfully challenge our findings or alter our conclusions.

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This Web transmittal has received Bureau approval for release.

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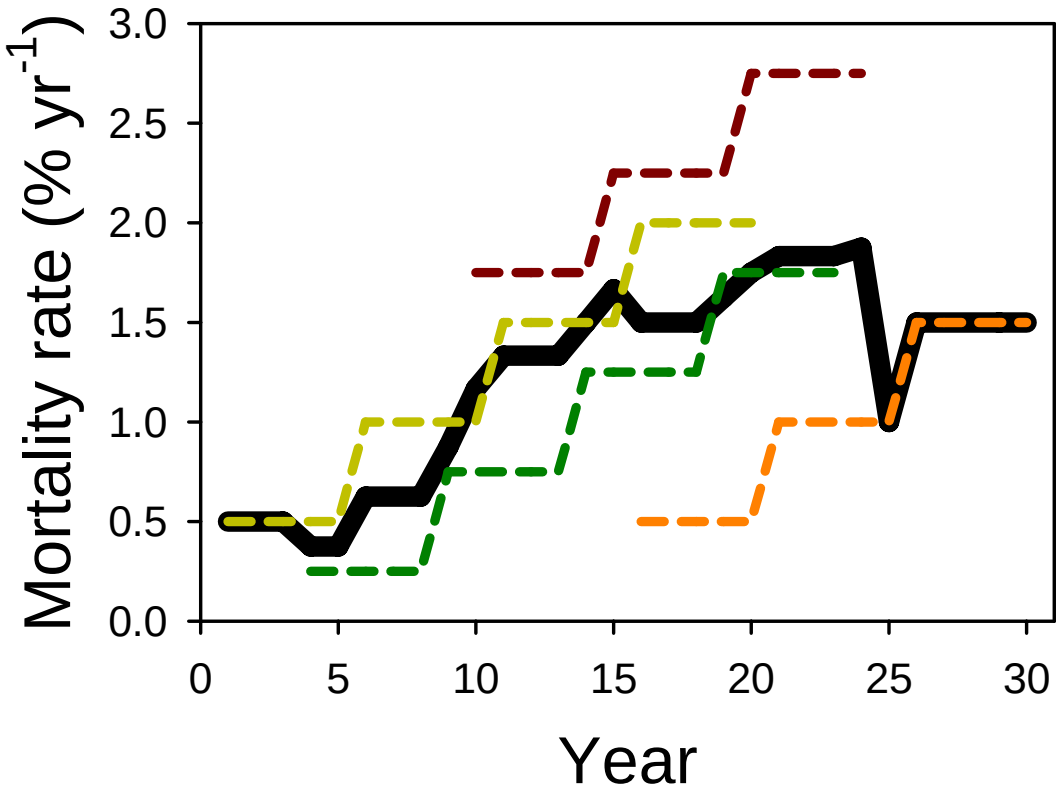

Figure 1. Site-switching bias. In this hypothetical example, for each of four plots (dashed lines) tree mortality rates have consistently increased by 0.5% in each subsequent 5-year census interval. As in slides 14-16 of Johnson et al. (2009) and in Figures S3-S5 of van Mantgem et al. (2009), horizontal lines indicate the duration of each census interval for a particular plot. At any point in time, mortality rates differ among the plots due to plot differences in site productivity, species composition, minimum tree size measured, and other factors (Stephenson and van Mantgem 2005).

However, when mortality rates are averaged across all plots that have data for a given year, the resulting curve (solid black line) indicates that mortality rates increased during the initial 15 years of record, but showed no consistent directional trend during the final 15 years of record. The spurious conclusion that mortality rates leveled off during the final 15 years is a consequence of two things: (1) the last plot added to the data set (orange dashed line) has the lowest intrinsic mortality rate, thereby reducing the mean mortality rate in the later years of record, and (2) the other three plots – those with the highest intrinsic mortality rates – sequentially drop out of the record before the plot with the lowest intrinsic mortality rate, meaning the latter plot has progressively more weight in determining mean mortality rate through time. Comparable examples could be constructed showing other possible site-switching biases, such as spurious increases in mortality rate when none actually exists.

Site-switching bias can be avoided by fitting the data using a generalized nonlinear mixed model with plot identity as a random effect – the approach we took in our study of tree mortality rates (van Mantgem et al. 2009). Simply put, our approach would fit a common slope parameter to the four plots, while effectively allowing intercept to vary from plot to plot.

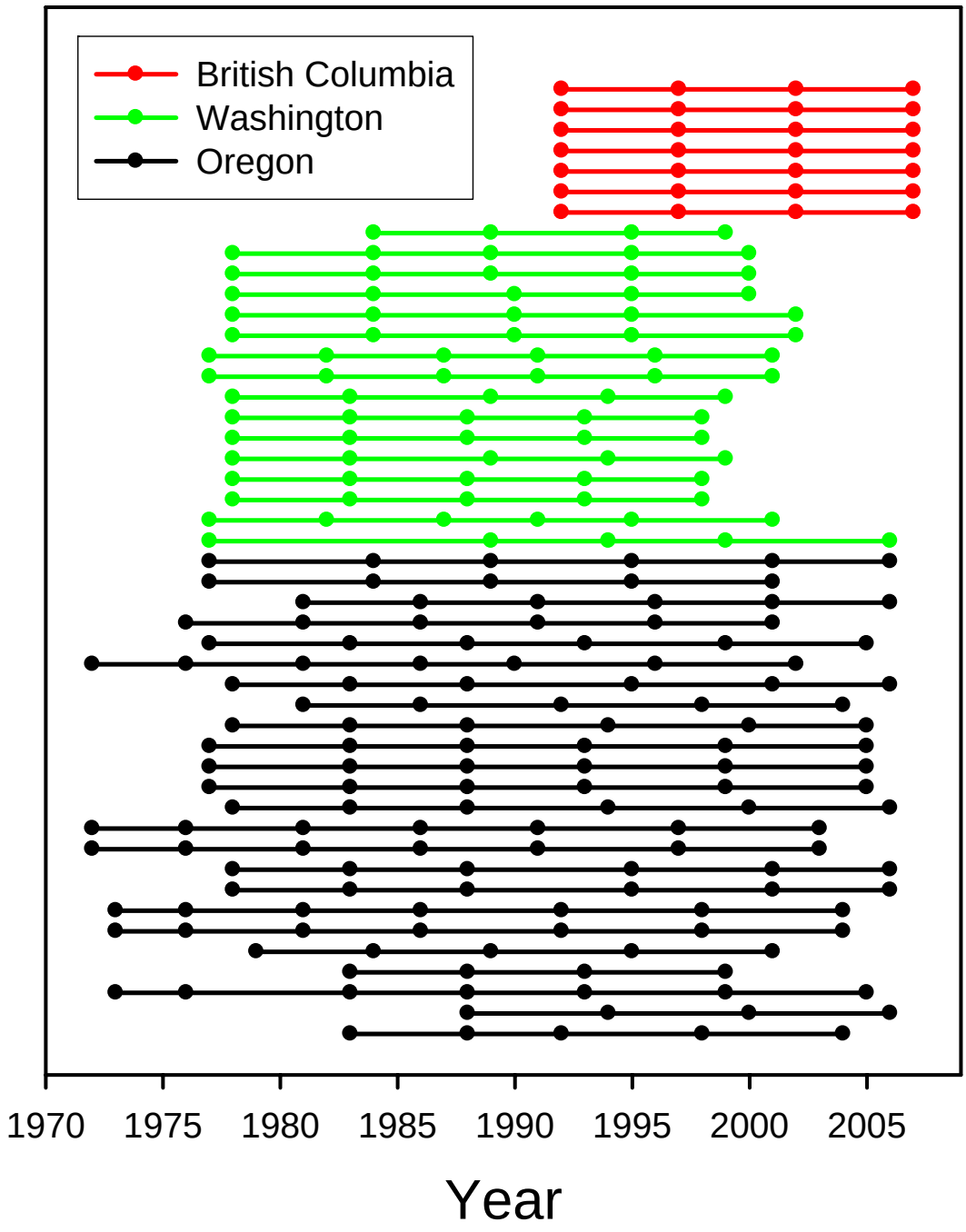

Figure 2. Periods of record for the Pacific Northwest plots used by van Mantgem et al. (2009) and reanalyzed by Johnson et al. (2009). Each horizontal line indicates the calendar years of record for a given plot. Dots represent census years. Plots are arranged latitudinally from farthest north (top) to farthest south (bottom).