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Interactive comment on "Water availability limits tree productivity, carbon stocks, and carbon residence time in mature forests across the western United States" *by* Logan T. Berner et al.

Anonymous Referee #3

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In my view, this paper makes an important contribution in quantifying the relationship between forest characteristics (net primary productivity, NPP; live biomass, BIO; mean carbon residence time, CRT) and climatic moisture regimes in the western United States. The analysis is strengthened by the inclusion of two fundamentally different data sources and methods, including forest inventory measurements from 3 states (WA, OR and CA) and satellite-based estimates across an even larger area (11 western states). The results are striking as both methods show that forest characteristics in this region are governed primarily by spatial gradients in climatic moisture regimes (as represented by a simple climate moisture index, CMI_wy). Although this general conclusion is not new, the work provides valuable quantitative estimates of forest-climate

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relationships that are likely to be useful in improving models of forest responses to the climatic drying that is already evident in this region. Overall, the paper is clearly presented and the methods seem appropriate, given the major challenges of spatial scaling in this mountainous and climatically diverse region.

My main questions relate to a) the justification for including only mature stands > 100 years, and b) unstated assumptions and potential sources of error in estimating CRT (see specific points, below).

Specific comments:

L110 What is the justification for restricting the analysis to mature stands older than 100 years? Is this age considered to be a threshold, beyond which the variables BIO, NPP and/or CRT remain constant over time?

L127-128: Do these inventory sites represent forests across the full range of elevations in this region? If they exclude sampling of unproductive forests in climatically cold, wet sites near the upper timberline then I'm wondering if this could explain the observed differences in response to CMI for inventory sites versus satellite-derived estimates (Fig. 2).

L138-140: I believe that the equation used for carbon residence time (CRT = BIO / NPP) is based on the assumption that BIO is constant over time (e.g., see equation 1 of Friend et al 2014, reference cited in the MS). If so, then this assumption (and any others) should be stated explicitly. Overall, I'm wondering how much of the variation in the reported estimates of CRT is driven simply by variation in stand age, given that in my experience, older stands tend to exhibit increasing (or stable) values of BIO along with age-related declines in NPP. On a related point, using the above equation, I would expect estimates of CRT to be inflated in forests with anomalously low NPP over the 10-year period of calculation (e.g., during droughts or insect defoliation episodes that are not sufficiently severe to cause a proportional decline in BIO). Again, it would be helpful to at least acknowledge the potential sources of bias in the reported CRT

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estimates.

L396-400: The discussion includes reporting of the large percentage difference in BIO across the climatic moisture gradient (CMI_wy) using the two methods (from Fig. 2) but I expect that the percentage difference would be even greater than this if dry, naturally unforested areas (with zero forest BIO and NPP) were included in the analysis. In this respect, it would be interesting to see how %forest cover varies as a function of the binned values of CMI_wy across this region. I recognize that such an analysis would go beyond the scope of this paper, but it could provide an interesting additional indicator of how forest NPP and carbon stocks may respond over the long term under the projected (and ongoing) climatic drying, i.e., drought-related loss of forest cover in addition to drought-related decreases in BIO, NPP and CRT in those sites that continue to remain forested.

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