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Biogeosciences Discussions

Interactive comment on "As different as day and night: evidence from root lifespan" *by* W. Bai et al.

Anonymous Referee #2

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Similar to the other reviewer, I commend the authors for the labor intensive dataset that they have collected; I agree that the experimental design is sound; I think 'continuous warming' would be a better description of your 'diurnal warming' treatment; and I agree that more could and should be extracted from the multi-year nature of the dataset. However, I disagree with the primary conclusion of the paper, that day warming only affected root longevity, as the seasonal 2008 analysis clearly shows that day, night, and continuous warming all have effects on this parameter at various times in the year. I do not know why the authors didn't do the same seasonal analysis on all years of data collection, or at least do a by-year or a by-season by-year analysis, rather than the cumulative 2007-2009 approach. Clearly, based on their temperature and moisture data, they had substantial variation in precipitation across years, and that 2008, was an unusual year compared the rest (wetter and cooler). A fact that could explain the sensitivity to temperature observed in this year. I suspect that the authors

have glossed over some meaningful details in this dataset, and that interannual variability in precipitation is extremely important in dictating the effect of their treatments. Given the substantial seasonal variability in response to treatments, I don't understand how the authors are comfortable stating that across the whole timeframe day warming differed than night or continuous warming. Figure 3 is confusing. Why isn't the continuous warming treatment analyzed in the stats, if it is shown here? Why show us Fig. 5 if there weren't significant differences, you've already published this data, and you don't give us enough detail to evaluate how it was collected in the methods of this manuscript? I do not believe that correlation equals causation, and am therefore skeptical of the regression approach employed in the paper. I wondered what happened to temperature and moisture in these regressions, and thought that the overall presentation of the regression findings was very incomplete. We need to know what all was evaluated using this approach and what was found significant or not, what parameters were auto-correlated, etc. There are many mistakes in the language of the paper (as the previous reviewer also pointed out). I wondered when the root samples were taken for carbohydrate analysis. In general, I thought the methods were skimpy and certainly wouldn't allow someone to duplicate the study.

Interactive comment on Biogeosciences Discuss., 8, 10459, 2011.