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Interactive comment on "Effects of heat and drought on carbon and water dynamics in a regenerating semi-arid pine forest: a combined experimental and modeling approach" *by* N. K. Ruehr et al.

Anonymous Referee #2

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This study by Ruehr et al. uses an impressive range of methods to explore the climate sensitivity of ponderosa pine carbon and water fluxes in a semi-arid environment. At its core, the study is centered on ecosystem modeling but it involves a small-scale watering treatment as well. It builds on a wealth of past research at one of the Metolius flux tower sites, involving detailed field measurements and finely tuned ecosystem process modeling of the coupled dynamics of carbon, water, and energy fluxes and balances. This study is of high quality overall and will certainly merit publication after some interpretations and conclusions are revised to accurately reflect the study's quantitative

results. Critiques and suggestions for improvement are detailed below.

1) Section 3.1: P563, L27+ Presentation of results here suggests that simulated daily transpiration matched observations, but there is a sizeable high bias in the simulation that should also be explained here. Maybe this could be achieved by simply changing the structure of the next sentence to start with something like, "The high bias in simulated relative to observed transpiration was intentional because...".

2) Section 4.1 and others: One might think that the model's underestimation of ET, despite overestimation of transpiration, implying significant underestimation of soil evaporation, is all cause for some concern in the model's ability to represent pine response to drought, warming, and associated water stress. The possible causes are openly discussed in the discussion section 4.1, but the potential implications for the model experiments are not discussed as broadly as they might need to be in other sections of the manuscript. Is it also possible that this contributed to the mismatch between observed and modeled ecosystem responses to the experimental water additions? The model appeared to maintain a higher soil water content and allow greater tree transpiration. Please add additional discussion of both of these main points.

3) Please provide additional details describing how soil and heterotrophic respiration, sap flow, biomass inventory, and leaf area index were measured. In particular, how did you separate total soil respiration into heterotrophic versus autotrophic components?

4) Section 4.2: Watering a 2 x 2 m area around a tree was likely too small a treatment area, particularly for the +35% and +120% treatments. The authors themselves conclude the same. If this is the case, the experiment that was performed was insufficient to evaluate whether or not heat and low air humidity exerts tight control over stomata, preventing a response to elevated soil water content. Thus, the discussion in section 4.2 overstates the capacity of the study to "study the effects of atmospheric drought apart from soil water limitation". This needs to be revised to bring the discussion and interpretation in line with what the study's methods and findings allow.

5) Section 3.2, p565, L4-20: Soil water content was observed to decrease rapidly after the 35% and 120% watering treatments, while the model showed a sustained elevation of soil water content. This discrepancy is attributed to either soil evaporation being too low in the model, or neighboring vegetation taking up a significant portion of the water in the experiment. Is it also possible that the soil water drained rapidly and that the model's soil water dynamics do not allow sufficient vertical drainage? These effects would be more pronounced for the larger watering treatments, given that both soil evaporation and soil water drainage are greater when soil water content is greater (hydraulic conductivity is a highly non-linear function of soil water content, and so is soil resistance to water vapor release). Please add discussion of this additional explanation if deemed appropriate.

6) Could you calculate a mass balance of the amount of water added in the treatment and accounted for through the measured loss pathways as well as the change in soil water storage? This surely seems possible, so please add. You might also show vertical drainage from the simulation just to see to what degree gravity drainage is active in the framework. This might help with interpretation of which of the potential explanations for the discrepancy discussed above is most plausible.

7) Intro and Discussion: Regarding implications of hypothesis 3, it is worth thinking about whether such a compensation (gains from a longer growing season offsetting losses from summer drought) would allow these pines to escape the warming and drought-induced mortality that has been so widely reported lately. One thought is that, even if a longer growing season compensates some of the lost summer productivity due to drought, this may not alleviate mortality events, particularly if hydraulic failure is the dominant mechanism behind mortality rather than carbon starvation.

8) The statement (P571, L5+) that "these findings confirm our first hypothesis that GPP in isohydric pine is affected more by changes in atmospheric demand than summer precipitation" does not appear to be supported by the effects sizes in Figure 6. That figure shows that reducing summer precipitation reduced GPP as much or more than the rise

in temperature out to 2040 and 2080. While GPP declined 17% in the 2080 case (+4.5 deg C), GPP declined similarly or even more from -100% summer precipitation and was even responsive to the -50% summer precipitation treatment. The interpretation and conclusion about the relative importance of reduced precipitation compared to elevated temperature and VPD needs to be carefully reconsidered and revised to be consistent with the model experiment. The field soil watering experiment was not really conclusive in the sense that the watering treatment may not have been effective and because it operates in the opposite direction (additions versus reductions), despite the lack of response, it is not entirely rationale and sound to use this experiment to dismiss the importance of summer precipitation, especially in light of the model simulations which indicate that summer precipitation is, in fact, important. All of this reasoning needs to be revisited, in my opinion.

9) In the conclusions, the statement "[t]he small response of transpiration and photosynthesis to water additions in both the field experiment and modeling clearly showed that heat executes a tight control on ponderosa pine physiology" is not fully supported by the study. What results presented here evidence this? Doubling summer precipitation increased GPP by 9% to 13%, almost as large as the effect of a +4.5 deg C summertime temperature increase. This interpretation needs to be carefully re-evaluated and revised.

10) Also in the conclusions, the results do not support the next statement about decline in carbon fluxes and stocks being three times larger in response to elevated temperature than reduced precipitation. Looking at Table 5, the -100% summer precipitation experiment resulted in a similar decline in GPP as the +4.5deg C simulation. Looking at Figure 5, if we compare the GPP reduction from -100% summer precipitation (-10%) to that combined with the +4.5 deg C summertime temperature scenario (-25%), we find large responses to both. This interpretation needs to be carefully re-evaluated and revised.

Interactive comment on Biogeosciences Discuss., 11, 551, 2014.