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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 #3

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This manuscript addresses a very timely and important topic, which fits well to the scope of Biogeosciences Discussion: ecosystem responses to climate change in the form of drought and heat. The manuscript uses a rigorous modelling scheme combined with some evaluation data obtained from a precipitation manipulation experiment to simulate ponderosa pine forest responses to changing climate in terms of carbon and water cycles. The climate change is induced by using the A1B greenhouse gas emission scenario.

Generally the study seems to be made carefully and I applaud the amount of atten-

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tion paid on error analysis and model evaluation in this manuscript. However, with this amount of focus on the model description, evaluation and error analysis I find the title of the manuscript slightly misleading. Instead of a study of effects of heat and drought on carbon and water dynamics of a semi-arid pine forest that uses both experimental and modelling approaches, this manuscript reads as a modeling study or model evaluation/description for such an ecosystem and all the conclusions and hypothesis testing is done based only on the model results. This also raises the question whether models really can be used for testing the hypothesis stated in the manuscript. Ultimately models give answers that are in line with the equations inserted in them. Therefore, I think that truly testing the hypotheses of this manuscript one should use experimental data, rather than a model. The model will represent our best current understanding of the processes, but ultimately it will not test whether the processes are described with correct equations and interactions even if the best available data was used for the parametrizations. This problem of mixing model and experimental results is apparent also in many places of the text. Therefore, I ask the authors to pay attention which of their own results as well as of the studies they refer to are experimental and which are based on modeling, and communicating this to the readers. The text also contains a lot of repetition, and the discussion reads as an extended results section. To make it easier to the readers to follow, please combine all the information on the same topic (e.g. general model overview) to a same paragraph and remove repetition.

As to the methods, they are fairly unevenly explained. For example, very detailed information on the A/Ci curves is given but no information is given on e.g. how heterotrophic respiration and leaf area index are measured. It remains unclear how many treatment plots were used and how they were distributed, and how much irrigation was given to each plot (how does that relate to soil saturation or field capacity?), and it becomes clear to the reader only after reading the results that there was only one irrigation treatment and all the other water reductions were model runs (did I get this correct?). It also remains unclear how the soil water potential curves were used in the model as in all the results only soil water content is presented.

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The results seem to be focused on model performance against the data on the test period rather than the results of the simulations to the future, which makes the reader to ask what is new here. How is this model better then other similar models. If one of the main results is the prediction that earlier start in the spring will compensate for carbon losses during a warm autumn and winter, I would have liked to see some kind of an evaluation of how certain is this result, based on the model structures and equations used, and the other climate scenarios available. An estimate on how important this aspect of the semi-arid ponderosa pine forests could be globally, would also be interesting. The manuscript is generally quite lengthy with all the detailed values and percentages given in the text. It would help the readers if the authors could compress the text and reconsider the current figures and tables presenting only what is essential and maybe adding some of the current information to the supplementary materials. For example, are all the figures 1, 2 and 3 really needed for convincing the reader of the performance of the model?

Specific comments:

Abstract page 552 lines 14-17:Please revise the sentences starting "Dramatic increases in summer water availability..." and "This clearly demonstrates...". These don't seem to have logical connection.

Introduction page 553 lines 17-19: This statement about soil moisture constraints on heterotrophic respiration being compensated by temperature should have a reference. How long could that statement apply? If it gets dry enough no temperature increase will keep respiration up. How does this compare with your site and the drought your trees experience?

Methods: page 556 line 22: Did you really measure needle water potentials, not shoots? Generally needles are very hard to measure alone as they tend to shoot out of the stopper used for holding them in the pressure chamber. Please also give the model and manufacturer of the pressure chamber. How often did you do these

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measurements? What was the driest condition you observed? How many fascicles did you measure per tree?

Page 557 lines 8-10: How did you separate the fine roots from dead roots? How about bigger roots? were they considered at all? How did you determine rooting depth?

Page 557 lines 11-12: If I understood this correct, the variability in the data was estimated either based on variability between plots or measurement dates. Why did you not use variability between trees each time measurements were done? How many trees per measurement campaign were measured on each plot? Averaging over time (measurement campaigns) sounds very strange as there could be natural seasonal variation that the model tries to capture.

Page 557 line 15: I don't understand why defining errors by coefficient of variation is not suitable for NEE. One can easily take a percentage error from negative values as well. Please explain. Also, in this paragraph the basis of the error estimates could be explained better.

Page 558 line 17: The model uses fixed allocations to study drought effects. Does drought affect allocation anyhow? There should be at least some literature available on this topic. As to the allocation functions in general, it looks to me that this model assumes that plants increase allocation to the roots under drought (see figure 6). I don't think this is necessarily true, especially for an isohydric species like ponderosa pine. Generally pine trees tend to lose connection with the soil some time after stomatal closure during drought (see Plaut et al 2012 Plant, Cell and Environment), and if the drought is severe enough, roots can't grow in any case as the soil gets so hard.

Page 559 lines 14-24: This whole discussion of photosynthetic capacity possibly changing with seasons is a bit strange as it is a well known fact that photosynthyetic capacity changes (see e.g. Schwartz et al 1997 Tree Physiology, Lloyd et al 2002 Tellus, Suni et al 2003 Global Change Biology, Sevanto et al 2006 Tree Physiology and many others). If the model is somehow deficient in describing this, it cannot be reliably

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used for estimating the seasonal fluxes of carbon and water. How was this part really modeled here?

Results page 563 lines 3-5: I cannot see this from figure 1. Figure 1 is 5 day moving averages. Please correct the text or make a figure that shows this.

Page 565 lines 12-23: In figure 4 it is impossible to see which line is which. The gray shades are not distinguishable even online. It is also still unclear here whether the irrigation treatments were different or whether the irrigation reductions were only modeled.

Page 567 line 8: Please be consistent with what is negative and positive in NEE and what is an increase and a decrease. This applies to many places in the manuscript. Literally speaking a decrease in NEE daytime would be an increase in carbon uptake as NEE becomes more negative.

Page 570 lines 7-8: Did watering of the trees change VPD in your experiment? How did you do the watering?

Page 571 lines 14-23: Is this really important to say here? How is this related to your study?

Discussion page 573 line 19: How about the second hypothesis? How did your results relate to that? The first and the third are only discussed here.

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