Biogeosciences Discuss., 11, C1109–C1136, 2014
www.biogeosciences-discuss.net/11/C1109/2014/

Biogeosciences
Discussions
Open Access

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.

N. K. Ruehr et al.

nadine.ruehr@kit.edu

Received and published: 17 April 2014

General response

We thank the referees for their helpful comments, which improved our manuscript substantially. In the following we highlighted the major changes/reinterpretations we have made, followed by the detailed point-by-point answer to all referees' comments and the new/revised Tables and Figures at the end of the document.

1) All the referees commented on the mismatch between observed and simulated water fluxes. Thus, we re-evaluated the model output, and changed soil texture to

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

match the original measured values as well as set a fixed value for field capacity (not derived from Saxton equations of soil texture originally in the model). This resulted in an improved match of SWC (see Fig 4 supplementary) and ET with the observations. The effects on C dynamics were rather small. Furthermore, we added information on water fluxes to Table 3 and Table 4 (see Fig 2 supplementary). The net effect of underestimated ET on the water balance was close to zero, because of soil water drainage in the model that was about as large as the model vs observed differences in ET.

2) We revisited the interpretation of our field watering experiment. By adding information on tree hydraulics (see Fig 3 supplementary), we are now able to demonstrate that the field watering experiment kept SWC close to field capacity, while pre-dawn leaf water potentials declined (but substantially less than in the control treatment, see Fig 3). Simulating the irrigation experiment using the SPA model showed that SWC remained at field capacity and pre-dawn leaf water potentials at pre-drought conditions (–0.4 MPa, see Fig 5 supplementary). This indicated that the field watering experiment only partially reduced water limitation in trees, likely because the trees were partially rooting outside the watered area. The observations from the field watering experiment are nevertheless helpful to evaluate the model responses (especially the effects on heterotrophic respiration) and also provide an additional means to study the separate effects of soil water availability and VPD (see. Fig 3 b supplementary).

3) To provide further evidence for the limiting effects of VPD on GPP we run additional scenarios of reduced VPD (fixed at 1 kPa) under no soil drought. Reduced VPD caused GPP to remain at early summer levels (see Fig 4), with a similar treatment effect as in response to the irrigation scenario (no soil drought). Re-evaluating our results, we also changed our statement: “The 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.” to “The results from the field study and the simulation showed that soil water limitation has a pronounced effect on GPP, which nearly equaled that of VPD limitations during the

2010/11 summer”

4) We carefully revised our conclusions on –GPP/–NEE being affected more by heat than summer soil drought to: “In agreement, we found the decline in simulated –GPP and biomass in response to hotter summers to nearly double the effect of reduced precipitation. The larger decline in –NEE than –GPP in response to hotter temperatures was caused by heterotrophic respiration fluxes being apparently unaffected by summer heat, while soil organic matter decreased. This effect might dampen over the long-term with decreasing litter inputs along with reductions in soil C pools.”

5) We moved information on error estimates (observation and model) to a separate subsection to the end of the Methods section and added further descriptions of the field experiment and measurements to the Methods section.

The detailed responses are given in bold below the referees’ comments.

Referee 1

We thank the referee for his/her critical comments, which are addressed in detail below.

Method: Missing are how soil evaporation (E) was consider by the model.

We added the following information to the methods section: “Soil evaporation is determined by solving the soil surface energy balance; evaporation is linked to the thickness of the surface soil crust (i.e. dry soil), which is modeled dynamically.” More details and equations can be found in Williams et al. (2001).

Understory considered by the model as pine layer, have this evaluated? And does the annual GPP and T patterns of the pine similar to the below vegetation pattern? Information on the soil surface coverage by the canopy is missing.

Tree canopy coverage is about 12% calculated from stand density (162 tree ha^{-1}) and an average tree crown diameter of about 2.5 m. Accounting for shrubs results in a canopy coverage of about 20%. Following Raz-Yaseef et al. (2011;

semi-arid pine forest with about 50% of the precipitation occurring at our site) a semi-arid forest with canopy coverage of 20% should have a ratio of soil evaporation (Es) to precipitation (P) of about 50%. The ratio of Es/P derived from our simulation is 29% for 2010 and 37% for 2011, and thus is quite close to the findings from Raz-Yaseef et al. (2011), but indeed indicate that Es estimates from the SPA model might be to low. We added this reasoning to the Discussion section.

Setup of the irrigation water experiment is unclear: what was the irrigation frequency (ies)? was it added under the trees or evenly over the surface? and what was the logic in doubling precipitation in two months? It also unclear how NEE and GPP were measured/inferred in those plots? Likely that the sandy soil could not hold all that added water , was it considered? Anyhow, more details for this experimental part are needed in this section.

We added the following information to the Methods section: "In brief, 2 x 2 m plots around each tree were irrigated automatically every other night, with 16 pressure-regulated drippers equally distributed every 0.25 m^2. The watering was adapted to keep soil moisture near saturation ($>$ 80% relative extractable water). In total, 436 mm of water were applied in 2010 (i.e., 1744 l per tree) and 582 mm in 2011 (i.e., 2328 l per tree)."In addition, we like to refer to the publication of Ruehr et al. (2012), where the set-up of the irrigation experiment and measurements are described in detail.

Why SWC level after adding 436 mm was lower that the winter-time (Fig. 4) values?

Regarding SWC in Fig 4 and Fig 1: The large SWC (above field capacity of 0.23 m^{3}m^{-3}) during winter results from frozen water in the upper soil layers, which infiltrates quickly to deeper soil layers after thawing. The following information was added to the caption of Figure 1 and 4: "Simulated winter-time SWC above field capacity ($>$ 0.23 m^{3}m^{-3}) results from surface layer freezing."

Results: Water fluxes (Page 563, line 21 onward and Figure 1): From May to August in both years, measured transpiration is lower than simulation, contrary ET measured for the same period is considerable higher than simulated. This requires explanation.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Because observed transpiration (Tobs) does not include understory it is lower than simulated T (Tsim) that also includes transpiration of shrubs. This is in detailed explained in the Results as follows: "This overestimation was intentional to allow for understory transpiration (about 15 % of LAI), and to improve the fit with the whole ecosystem ET and –GPP observations. Upscaling sap flow measurements from single trees to estimate study site transpiration is generally prone to larger errors. Here, we estimated the error of site-level $T_{tree-obs}$ to be $\pm 40\%$. In this context, and not having understory T measurements, a 60% bias between $T_{tree-obs}$ and T_{sim} (Fig. S3d) seems reasonable." ET was likely lower due to an underestimation of Es, as mentioned before. We added the following sentence to the results section, indicating that the overall effect of underestimation of ET (or Es) is not fundamental for the water balance: "The imbalance in modeled and observed annual ET (–67 mm) was largely outbalance by 58 mm of water lost via drainage from the model's 2 m soil column. Moreover, the good agreement between modeled and observed SWC (0–40 cm) indicates that the amount of water available for plants should have been similar between model and observation."

Measured T is about 1/3rd ET at least for the season peak activity (assessed from figure 1), likely that most ET is actually E, than it partially explain the low irrigation effect. The T/ET proportion is very low not common to most forest areas. Again the authors should explain this and implications. Tsim is higher by 40% than Tobs and the 15% Tsim increase cannot close this gap, it is thus unclear how GPP are the same by the model and observation? And if such WUE should deviated considerable between the two. . .

A study by Irvine et al. (2004) made in a young pine forest close-by with slightly higher stand density/LAI found that the contribution of Ttree to ET decreased from 70% to 30% between May and August. The contribution of observed Ttree to ET at our study site during July and August was 27±10%, thus well within the range of the findings by Irvine et al. This is in contrast to the SPA model.

The simulation showed highest contributions of T (tree + understory) to ET during the summer dry season (90% in August 2010 and 71% in August 2011), but also includes understory transpiration. Indeed, understory measurements of ET at a young pine-site close-by in July 2001 indicated that about 70% of ET originated from below 1.75 m height, where most of the leaf area is from shrubs and soil evaporation contributed only about 10% to ET. We added the following text to the Discussion: "Because the ratio of measured tree transpiration to ET in our study during summer (27±10%) agreed well with a study made in young pine-forest close-by (Irvine et al., 2004), and simulated transpiration fluxes (T/ET ratio of 70–90%, including over- and understory) were larger than measured tree transpiration, the underestimation of simulated ET maybe attributed to soil evaporation."

Similarly, with the irrigation/precipitation experiments, although T increases largely due to added water it has minimal effect on GPP (eg., Fig. 4). Need explanation.

Because of high temperatures and large evaporative demand during summer (average day-time VPD of 2.3 kPa) a small increase in stomatal conductance can result in a much larger transpiration flux. This is similar to what we have shown in Ruehr et al. (2012, see Fig. 1 supplementary): During the onset of the drought period, transpiration (light gray line) still increases due to large evaporative demand, but stomatal conductance (intermitted gray line) is already decreasing and causes GPP (black line) to decline. We added this reasoning to the Discussion as follows: "Stomatal conductance in young ponderosa pine is found to be tightly regulated when atmospheric water demand is large, even under non-limiting soil water conditions (Ruehr et al. 2012). This close regulation becomes apparent during the onset of the summer dry season, where we found transpiration to continue to increase due to large evaporative demand, while stomatal conductance decreased immediately and in turn –GPP declined."

Discussions: Indeed, percentage of canopy soil coverage effect on soil evaporation is well documented (eg., Raz Yeseef 2010). Likely that this effect is missing and can

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

explain part of the T to ET large deviation (mentioned above).

Thank your for pointing us to this, we added the following text to the relevant Discussion section. "Indeed, a study in a semi-arid pine forest in Israel, shows that soil evaporation can vary substantially between sun-exposed and shaded areas, and indicates that at a canopy cover of about 25%, half of the annual precipitation evaporates from the soil (Raz-Yaseef et al. 2010). Applying this ratio to annual precipitation measured at our study site, suggests that soil evaporation estimates from SPA might be 30–40% too low. This indicates that the differences found in ET between model and simulation could be largely attributed to an underestimation of soil evaporation, probably due to large spatial variability not accounted for in the modeling."

The weak correlation between added water (irrigation plots), the considerable higher T at those plots (likely upper by 100% at August then in the non-irrigated trees, fig. 4 c&d), but with low effect on GPP requires explanation. Known in hot semi-arid environments that when conditions easy and some water exist, plants activate at time of the day when VPD is relatively relaxed, such as early mornings and late afternoons (VPD likely to be below 3500 Pa then, see references in articles already mentioned in the text). Have the trees activity on those hours checked?

C uptake in the SPA model is maximized within the limits of the hydraulic system. Thus, if critical leaf water potential (defined by the user, here -1.9 MPa) is reached, stomata will close to maintain leaf water potential above that threshold. Simulated transpiration increased by about 50% in response to the full irrigation treatment (= soil water availability at field capacity), while stomatal conductance (Gs) increased only by about 30%, resulting in 20% larger –GPP. The smaller increase in Gs compared to T can be explained by limiting effects of VPD. The effect of VPD on Gs and GPP becomes also clear when running the same simulation but under low VPD conditions (0.5 kPa), here we found a 46% increase in T to result in a 45% increase in –GPP, because Gs responded strongly (+90%). In addition, also our observations showed strong effects of VPD on stomatal con-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

ductance, which caused large diurnal and day-to-day variations. We added a new Figure 3 (Fig 3) to the revised manuscript, showing stomatal conductance (Gs) to decrease with declining soil water potential as well as strong responses of Gs to low VPD conditions ($<$ 1.7 kPa).

Regarding the model future simulations; Even without fertilization effect, higher atmospheric CO_2 means less water losses per carbon uptake, thus for the same stomata closure possible more carbon will absorb and will increase the GPP. Does the model consider that?

As we mentioned throughout the manuscript, we did not incorporate elevated [CO_2] in the future scenarios, as we wanted to concentrate our study on temperature and drought effects. We are aware that increases in CO_2 may increase GPP, through the effect you described above. We have discussed this in detail in the Discussion section 4.3

Two sentences following page 572, line 6 are unclear.

We re-phrased the sentences to: "Second, the response of decomposition may be buffered and lagged by large soil C pools and litter inputs downstream of GPP. Thus, if litter and soil C pools continue to decline, the relatively large differences in Rh and GPP could diminish over time (Shi et al. 2013)" and hope this makes it clearer.

Figure 1. VPD relevant for trees activity is of the day hours, better to present that and not for the whole day.

Thank you for pointing us to this mistake. Indeed, we presented VPD for daytime hours only (PAR $>$ 20 μmol m^{-2}s^{-1}), but we did not mention it. We added this information to the caption of Fig. 1.

Referee 2

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.

We like to thank the referee for the overall positive evaluation of our work, and hope that our improvements to the revised manuscript and answers given below address all of the referee's concerns.

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...".

Thank you for pointing us to this. We have partly re-written the section in the revised version of the manuscript: "This is also indicated by the dynamics of observed and modeled T agreeing well, but absolute flux rates were overestimated by the model (Figs. 1g–h). This overestimation was intentional to allow for understory transpiration (about 15 % of LAI), and to improve the fit with the whole ecosystem ET and –GPP observations. Upscaling sap flow measurements from single trees to estimate study site transpiration is generally prone to larger errors. Here, we estimated the error of site-level $T_{tree-obs}$ to be $\pm 40\%$. In this context, and not having understory T measurements, a 60% bias between $T_{tree-obs}$ and T_{sim} (Fig. S3d) seems reasonable. "

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.

Thank you for this helpful comment. We re-evaluated the model output, and changed soil texture to match the original measured values as well as set a fixed value for field capacity (not derived from Saxton equations of soil texture, as originally in the model). This resulted in an improved match of SWC (see Fig 4 supplementary) and ET with the observations. The effects on C dynamics were rather small. In addition, we added information on water fluxes to Table 3 and Table 4 (see Fig 2 supplementary). Looking in more detail in the water balance of the model indicated large drainage, which nearly outbalance the differences in ET between model and observation. We have added the following sentence to the results section: "the imbalance in modeled and observed annual ET (–67 mm) was largely outbalance by 58 mm of water lost via drainage from the model's 2 m soil column" Thus, we think that the mismatch in ET between observation and simulation should not influence the model's ability to capture drought and warming responses of the pine ecosystem. In addition, concerning the watering experiment, after re-running the model with improved soil parameterization, SWC of observation and model matched very well during the irrigation period. However, responses of the pines' were different, but this was because the irrigation experiment could not totally eliminate soil water limitation (small area watered). We have added a Figure on observed Tree hydraulics in relation to SWP to the Results section (see Fig 3 supplementary) and a Figure showing SWP during summer 2011 for the observations (control and watered treatment) as well as for the simulation to the supplementary (see Fig 5 supplementary).

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?

We added more information on these points to the Methods section as follows: "A variety of ecosystem measurements were carried out, including net ecosystem exchange of carbon dioxide (CO_2) and water (H_2O) using the eddy covariance approach, tree transpiration derived from sap flow estimates (heat-dissipation technique), soil and heterotrophic respiration, biomass inventories (4 x 17 m radius plots), leaf area index (in two transects with the LAI-2000,Li-Cor, Lincoln, NB, USA following Law et al. 2001) and phenological observations in 2010 and 2011. A detailed description of these measurements, data processing, and gap-filling of eddy covariance data can be found in Ruehr et al. (2012ab). To test the effects of water availability we conducted a two yr watering experiment during the summer drought season (July–August) and compared responses of watered trees and soil to control plots ($n = 5$ trees per treatment). In brief, 2 x 2 m plots around each tree were irrigated automatically every other night, with 16 pressure-regulated drippers equally distributed every 0.25 m^2. The watering was adapted to keep soil moisture near saturation ($>$ 80% relative extractable water). In total, 436 mm of water were applied in 2010 (i.e., 1744 l per tree) and 582 mm in 2011 (i.e., 2328 l per tree). In addition, to account for effects on heterotrophic respiration, we established soil plots in existing gaps within the forest at least 5 m away from the nearest tree (0.5 x 1.5 m, n = 3 per treatment). Soil CO_2 efflux from these plots reflects mainly heterotrophic respiration (see Ruehr et al. 2012). Soil CO_2 efflux was measured half-hourly in automated mode (Li-8100 with Li-8150 multiplexer, Li-Cor, Lincoln, NB, USA) in 6 plots and bi-weekly in manual mode in all plots (Li-6400, Li-Cor, Lincoln, NB, USA). To derive half-hourly heterotrophic respiration (Rh) the rate of automated measured soil CO_2 efflux (Rs) was multiplied by the ratio of Rh/Rs derived from manual measurements. More details on the irrigation set-up and soil CO_2 efflux measurements can be found in Ruehr et al. (2012)." and "Dry weight of root biomass was assessed by sequential coring

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

to 30 cm soil depth in June, August and September, and visually sorted in fine (< 2 mm) and dead roots (brittle, dark color; see Ruehr % Buchmann 2010) Maximum rooting depth was estimated at 1.1 m after excavation of a soil pit, with 80 % of the fine root mass in the 0–60 cm soil layer. Soil C stock estimates were derived from measurements of soil C and bulk density (1.28 g cm^{-3}; 0–10 cm). We measured N and C concentrations of first and second year needles sampled in September 2010 (p = 0.30; control: $1.24\% \pm 0.04$; watered $1.30\% \pm 0.03$). To upscale needle C and N to the study site we multiplied the concentrations with specific leaf area and LAI." We did not want to provide to much details on methods that have already been described in Ruehr et al. (2012) in detail, as our manuscript is of considerable length, and the Methods section alone 8 pages long.

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.

At first, we want to make clear that there was no 35% and 120% watering treatment. There was only one field watering treatment with the purpose to reduce soil drought, but to find the best match with the results from the field experiment we run several simulation scenarios with water added relative to the field experiment (w100$_{sim}$ = w100$_{obs}$, w50$_{sim}$, w40$_{sim}$, w30$_{sim}$ and w20$_{sim}$). We hope this makes it clearer.

Following the comment of the referee, we have re-written large parts of the Results section 3.2 to clearly point the reader to the limitations of the field watering experiment (SWC was close to field capacity (23%), but resulted in a SWP of watered trees that suggested a much lower SWC). However, we also made it clear

that adding the same amount of water in the model (w100$_{sim}$), maintained pre-drought SWP, thus eliminating soil water limitation. To study the limiting effects of VPD, we included a simulation of low VPD (1 kPa) to the full irrigation scenario. The results show that –GPP rates are largely maintained at pre-drought conditions (see Fig 4 supplementary), and that the effects of soil drought and VPD were about similar in magnitude. The revised Results section is as follows:

“To quantify the effects of soil water limitation on the pine’s physiology we conducted a summer irrigation experiment with soil water content close to field capacity (0.23 m^{3}m^{-3}). Nevertheless, pre-dawn leaf water potentials (i.e., soil water potential) of watered trees declined from a pre-drought maximum of –0.4 MPa to –0.7 MPa counterintuitive to the large soil water content (see Fig. S1). Along with the decline in SWP, tree transpiration decreased by 20% and tree conductivity by 40% (Fig. 3). This indicates that watered trees experienced a mild water limitation, despite the large amount of water added. Thus, water available to the trees must have been less than indicated by SWC measurements. A likely explanation is that the area watered (4 m^2 around each tree) was too small to cover horizontal root distribution of the trees. Nevertheless, the watering clearly affected the trees’ hydraulics and resulted in T being larger by 15% in watered compared to control trees. Isotope analysis of δH^2 in tree source water confirm that the additional water available for transpiration was from the irrigation (21 ± 7% on 4 August and 20 ± 3% on 22 August 2011). In addition, our observations also show that changes in stomatal conductance (Gs) depend on available water, but with variations of VPD superimposed, for example Gs increased by 100–400% under low compared to high VPD conditions (Fig. 3b).

To further study the effects of reduced drought on ecosystem fluxes and because our field experiment only partially removed water limitation, we simulated the irrigation treatment using the SPA model ($w100_{sim}$). Additionally, to quantify the effects of atmospheric drought on GPP, we run the simulations under low

VPD conditions. To test if the observations from the field experiment could be reproduced by the model, the amount of water added was step-wise reduced in the simulations (e.g., $w40_{sim}$ corresponds to 40% of water added).

Soil drought in the simulation ($w100_{sim}$) was eliminated when applying the same amount of water as in the field experiment (Fig. 4). SWP$_{sim}$ remained at –0.4 MPa (Fig. S4), resulting in larger T_{sim} and –GPP$_{sim}$ fluxes compared to the summer baseline conditions (June–September, Tab. 2). Nevertheless, –GPP$_{sim}$ continued to decrease during the summer season, while T$_{sim}$ remained relatively constant (Fig. 4c–f). The increase in VPD during the summer season at relative constant T reduces Gs which causes –GPP to decline. The dominant effect of VPD was confirmed by the low VPD scenario ($w100\_vpd_{sim}$), with –GPP$_{sim}$ rates maintaining at early summer conditions (Fig. 4e–f).

Comparing observations with the $w100_{sim}$, showed that heterotrophic respiration agreed well (not affected by small plot size), while effects on tree hydraulics were, as expected, larger under simulated than field conditions. The best agreement between observed and simulated SWP was found for the 30% irrigation scenario ($w30_{sim}$ +174 mm, see Fig. S4) in 2011. This was confirmed by relative increases in T_{obs} ($+23$ %) and T_{sim} ($+28$ %) during July–August, and further indicated by the percentage of irrigation water taken up (20% under field conditions derived from isotope labeling, and 22% in the $w30_{sim}$ scenario)."

In this context, part of the Discussion was revised as follows: "The mismatch between observed and modeled tree responses to the experimental water additions was caused by a shortcoming of the field experiment. The watered trees experienced mild soil water limitations (about 40% loss in tree conductivity), while soil water content was close to field capacity. This was likely caused by the trees rooting partially outside the watered area, and thus some of the rooting system was exposed to dry soil. In controlled split-root experiments, even though part of a root system is well watered, it is not uncommon to observe drought stress-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

related decreases in stomatal conductance (e.g., Sobeih et al., 2004).". and "Watering trees to maintain SWC close to field capacity during seasonal summer drought provided us with the opportunity to study the effects of soil water limitation on the pines' physiology in more detail, and further allowed us to evaluate the effects of eliminated soil drought predicted by the SPA model. Despite non-limiting soil water conditions and large transpiration rates, –GPP continued to decline during July and August, which points to a strong VPD control on gas exchange. Indeed, driving the model under low VPD conditions increased stomatal conductance and largely maintained GPP rates at pre-drought levels."

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.

We added a detailed water balance to Table 3, which shows massive drainage at the w100$_{sim}$ scenarios (40–55 % of the water added was lost via drainage). We really think that the mismatch the referee is referring to is caused by soil evaporation being much higher under field conditions because of the small area watered, resulting in higher soil temperatures (about 1.5 degree C) than in the model and because of neighboring shrubs taking up the water. We also think that lateral diffusion of water to dry soil surrounding might be an explanation. In a study at a young pine plantation close-by with similar soil properties, after a large irrigation event most of the surface water was evaporated within 3 three

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

days at a rate of 4 mm (Kelliher et al. 2004).

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.

We added a mass balance for each simulation treatment to Table 3 (see Fig. 2)

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.

Thank you for pointing us to this. We agree with the referee and added the following sentence to the discussion section 4.3: "Thus, we may speculate that more favorable early season growing conditions will not facilitate the trees survival of extreme stress, but that the imbalance of needle to root biomass as indicated in the simulation may increase the risk for hydraulic failure, ultimately leading to tree death (e.g., McDowell et al. 2013)."

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 referee likely refers to Fig. 5 and Table. 5. The 100% reduction in sum-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

mer precipitation (about –50 mm) decreased GPP by –9% compared to a –17% decrease in response to +4.5 degree C summer temperature (along with 40% increase in VPD). Thus, increased summer temperature and VPD reduced –GPP more than a reduction of summer precipitation. To acknowledge the referee's concern we changed the sentence he/she is referring to: "In summary, this largely confirms our first hypothesis that GPP in isohydric pine is affected more by changes in atmospheric demand than reductions in summer precipitation, but this may largely depend on the severity of summer drought conditions and antecedent soil water content."

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.

We agree with the referee. We have revised our interpretations of the field watering experiment and did additional simulation runs with decreased VPD. Please see the detailed answer to point 9) below.

9) In the conclusions, the statement "the 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 reevaluated and revised.

We think the names assigned to the simulated watering experiments were

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

slightly misleading. The 100% experiment did not mean doubling summer precipitation, but reflected the field irrigation experiment where 436 mm were added in 2010 and 582 mm in 2011, which was about 800–1000% of normal summer precipitation. This large watering was chosen to sustain SWC at pre-drought levels (i.e., close to field capacity). Because of the discrepancy between field experiment and model (largely due to the experimental design) we reduced stepwise the added water (50%, 40%, 30% and 20%) to find the best match with the field experiment. To make this clearer we revised the treatment names: w100$_{obs}$ is assigned to the field irrigation experiment, w100$_{sim}$ is the simulated full irrigation experiment, w50$_{sim}$ is the simulated irrigation experiment with 50% water added, w40$_{sim}$ is the simulated irrigation experiment with 40% water added and w30$_{sim}$ is the simulated irrigation experiment with 30% water added. In addition, we added new simulation runs were we decreased atmospheric drought by running the simulation at low VPD (1 kPa) under water-limiting (ctrl$_{vpd1\_sim}$) and non-water-limiting conditions (w100$_{vpd1\_sim}$). Reduced atmospheric drought resulted in 8% larger annual –GPP (ctrl$_{vpd1\_sim}$), while reducing atmospheric drought under non-water-limiting conditions (w100$_{vpd1\_sim}$) doubled the increase in –GPP (18 to 26%) compared to the w100$_{sim}$ treatment. We think this clearly shows that the effects of atmospheric drought were equally important than soil drought during the summer 2010 and 2011. We changed the statement the referee is referring to: “The results from the field study and the simulation showed that soil water limitation has a pronounced effect on GPP, which nearly equaled that of VPD limitations during the 2010/11 summer ”

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 reevaluated and revised.

We agree with the referee that there is a misinterpretation in the Conclusions. –NEE declined 4 times stronger in response to the 2080 summer conditions, while the decline in –GPP and biomass nearly doubled that of reduced precipitation. We changed the paragraph he/she is referring to: "In agreement, we found the decline in simulated –GPP and biomass in response to hotter summers to nearly double the effect of reduced precipitation. The larger decline in –NEE than –GPP in response to hotter temperatures was caused by heterotrophic respiration fluxes maintaining, while soil organic matter decreased. This effect might dampen over the long-term with decreasing litter inputs along with reductions in soil C pools."

Referee 3

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 attention paid on error analysis and model evaluation in this manuscript.

We like to thank the referee for acknowledging the quality of our study, and hope we were able to address all the referee's comments in the answers given below.

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.

Thank you very much. We agree that we have not presented enough observational data in the manuscript and we provided too much detail on model data comparison. Therefore, we largely restructured the Results section concerning the field experiment, report more of the observational results and provide a Figure on observed tree hydraulics (Fig. 3). Please find more details in the response given to answer 4 of referee 2.

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.

We understand the referee's concern, but models are widely used to test hypothesis that can't be or are very difficult to test under field and/or controlled conditions, particularly related to feedbacks which are not intuitive from the equations used. Models like SPA synthesize our current understanding of the complex hydraulic-carbon pathway and thus can be used to investigate complex and dynamic processes such as drought responses of forests (e.g. Williams et al. 2001). For example, in a recent Tansley review similar models like SPA were used to test the carbon starvation versus hydraulic failure hypothesis in triggering tree mortality (McDowell et al. 2013).

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

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

to follow, please combine all the information on the same topic (e.g. general model overview) to a same paragraph and remove repetition.

We try to make this clearer in the Results section of the revised manuscript by presenting observation and model results in separate paragraphs, and by adding sim or obs to the variables. We also shortened the text and carefully removed repetitions.

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?).

We added more information on that to the Methods section, please see reply to referee 2, comment 3. Âă

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.

Soil water potential was used for model parameterization (see Fig. S1) and for comparing the results from the field watering with the model (Fig. S1 and S4, now revised, see Fig 5 supplementary). In the revised version of the manuscript we present a new Figure, showing the effects of transpiration, stomatal conductance and loss in conductivity in relation to SWP for the control and watered treatment (Fig. 3). See reply to comment 4 of referee 2.

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.

We largely agree with the referee and have reduced the emphasis on model-data comparison to about 3 paragraphs in the Results section, and also moved Fig. 3 to the supplementary and instead presenting data on tree hydraulics from the watering experiment.

We found that earlier spring partially compensates for strong C losses during summer. It is difficult to assess the certainty of this result, comparing a –5% decrease in –NEE (all season warmer scenario) to a –38% decrease (summer warming only) and considering a 10–20% error of modeled annual NEE derived from sensitivity analysis, we can conclude that warmer spring reduces negative effects on –NEE outside of the simulation error. However, we also found that leaf biomass increased while root biomass decreased in response to earlier spring, but assessing the uncertainty thereof would need further research on the effects of extremes superimposed on changes in trends. Global predictions are unfortunately not the scope of the study. We also think it rather bold to upscale from a site-level study to the globe.

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?

As mentioned before, we have moved Fig. 3 from the manuscript to the supplementary, compressed some text (Abstract, Result and Discussion section) and have reduced the amount of details given if appropriate.

Specific comments: Abstract page 552 lines 14-17:Please revise the sentences starting “Dramatic in- creases in summer water availability...“ and “This clearly demon-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

strates...“. These don’t seem to have logical connection.

Revised.

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?

We found this compensating effect at our study site (Ruehr et al. 2012, see Fig. 1 above, in answer to referee 1). The response of soil respiration to drought was similar to that of transpiration, at first both fluxes increased due to increasing temperatures as long as water was not really limiting. Soil respiration fluxes declined below drought levels at relative extractable water content of 40% (see Fig 1 above). References are added to the Introduction.

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

Yes, we measured water potentials of the fascicles (to avoid cutting the resin channels in the needles) with a pressure chamber equipped with a compression gland sealing system, which makes it easy to insert the fascicles. We added the following information to the Methods section: “Pre-dawn and midday needle water potentials were measured on two south-facing fascicles of 5 trees per treatment on 10 occasions between end of June and mid September 2011 using a Scholander-type pressure chamber (PMS, Albany, Oregon).”

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?

The following information was added to the Methods section: “Dry weight of root

biomass was assessed by sequential coring to 30 cm soil depth in June, August and September. Before drying the roots were visually sorted in fine ($<$ 2 mm) and dead roots (dark color, brittle; see Ruehr & Buchmann 2010 for details). Maximum rooting depth was estimated at 1.1 m after excavation of a soil pit, with 80% of the fine root mass in the 0–60 cm soil layer."

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.

We think there is a misunderstanding. Of course we derived error estimates from variability between trees/plots, but there was some data that were not measured on a plot scale or measurements were repeated but combined to one number. Rephrasing the sentence hopefully increased clarity: "We derived observational errors from standard deviations between plots/trees and/or measurement campaigns if suitable"

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.

The CV is the ratio of SD divided by the sample mean. If negative numbers are included the mean will be smaller than the SD and thus implicating a larger CV. More information can be found in a standard statistics book.

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

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

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.
In agreement with the referee and as stated in the Methods section, the carbon allocation of daily GPP into plant respiration and biomass (fine roots and woody matter) is assumed to be fixed in the model, with the exception that allocation of GPP to plant respiration was slightly increased during winter and that C allocation to needles was only activated at bud break (determined by growing degree sums) and ended after 60 days. Please see Table 2 of the manuscript for all allocation parameters.

Regarding results from Fig 6, needle, root and woody biomass declined in response to hotter and drier summer conditions. The positive effects of warmer winter and spring (earlier start of the growing season) was positively affecting needle biomass, but not root or woody biomass, due to changes in early season allocation (earlier start of allocation to needles). We discuss the effects of allocation changes in detail in section 4.3.

Considering the effects of soil hardiness on root growth with increasing drought: this is not the case for the soil at our study site, which has a sand content of 82% and becomes more lose with drying.
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 used for estimating the seasonal fluxes of carbon and water. How was this part really modeled here?
We strongly agree with the referee, this is why we allowed Vcmax and Jmax to change seasonally, by including a factor for daylength control, as described in

the Methods section: "By adding a factor for daylength control (df) to photosynthetic capacity (Vcmax and Jmax with 25 deg C temperature optimum) df= daylenght^2/maxdaylength^2 as found in the CLM4 Community Land Model."

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.

We agree and removed the reference to Figure 1.

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.

We modified Figure 4. Differentiating between lines and simulation and observational data should be much clearer now (see Fig. 4 supplementary).

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.

We agree with the referee and changed NEE and GPP to –NEE and –GPP accordingly.

Page 570 lines 7-8: Did watering of the trees change VPD in your experiment? How did you do the watering? A detailed description of the methods has been added to the text. Please see also see the answers to referee 1.

We could not observe any changes in VPD of watered trees, this is also unlikely due to the high coupling of the leaf surface with the atmosphere, indicated by mean daytime leaf boundary layer conductance averaging 160 times stomatal conductance, and thus VPDair should equal VPDleaf.

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

Because we did not include elevated CO$_2$ in the model scenarios, we wanted to make this transparent to the reader and briefly discuss the implications of rising

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

atmospheric CO_2 on photosynthesis.
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.
Because there was more than one answer to our second hypothesis, we had added it to Table 5. However, to make the hypothesis and related results clearer to the reader, we added the following sentence to the last paragraph of the Discussion section 4.2: "This partially confirms our second hypothesis that GPP responded more than Rh to increasing summer temperatures, while both were affected similar by declining summer precipitation."

List of References

Irvine, J., Law, B., Kurpius, M., Anthoni, P., Moore, D., and Schwarz, P. (2004). Age- related changes in ecosystem structure and function and effects on water and carbon exchange in ponderosa pine. Tree Physiology, 24(7):753–763.

Kelliher, F., Ross, D., Law, B., Baldocchi, D., and Rodda, N. (2004). Limitations to carbon mineralization in litter and mineral soil of young and old ponderosa pine forests. Forest Ecology and Management, 191(1-3):201?213.

McDowell, N. G., Fisher, R. A., Xu, C., Domec, J. C., Hölttä, T., Mackay, D. S., Sperry, J. S., Boutz, A., Dickman, L., Gehres, N., Limousin, J. M., Macalady, A., Martinez-Vilalta, J., Mencuccini, M., Plaut, J. A., Ogee, J., Pangle, R. E., Rasse, D. P., Ryan, M. G., Sevanto, S., Waring, R. H., Williams, A. P., Yepez, E. A., and Pockman, W. T. (2013). Evaluating theories of drought-induced vegetation mortality using a multimodel–experiment framework. New Phytologist, 200(2):304–321.

Raz-Yaseef, N., Rotenberg, E., and Yakir, D. (2010). Effects of spatial variations in soil evaporation caused by tree shading on water flux partitioning in a semi-arid pine forest. Agricultural and Forest Meteorology, 150(3):454–462.

Ruehr, N. K., Martin, J. G., and Law, B. E. (2012). Effects of water availability on car- bon and water exchange in a young ponderosa pine forest: Above- and belowground responses. Agricultural and Forest Meteorology, 164(0):136–148.

Ruehr, N. K. and Buchmann, N. (2010). Soil respiration fluxes in a temper- ate mixed forest: seasonality and temperature sensitivities differ among microbial and root-rhizosphere respiration. Tree Physiology, 30(2):165?176.

Williams, M., Law, B., Anthoni, P., and Unsworth, M. (2001). Use of a simulation model and ecosystem flux data to examine carbon-water interactions in ponderosa pine. Tree Physiology, 21(5):287–298.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C1109/2014/bgd-11-C1109-2014-supplement.pdf

---

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