This supplement includes responses to the reviewer's specific comments (which are called out by bolded font). We provide the re-written introduction between responses to General and Detailed comments.

## GENERAL COMMENTS

**"…the title implies that somehow the three basins studied here are representative of what would be observed across the western US, which is inaccurate, or at least, is not really proven in the manuscript."**
We agree that this title may imply a generality that our 3 catchments do not necessarily support. Instead of implying the broader implication of our results to "Western United States Mountain Catchments" in the title, we suggest modifying the title to read "**Three** Western United States Mountain Catchments"
A key contribution of this work is the geographic niche in which we are examining climate and soil controls on ET – mountain environments that receive a significant portion of their annual precipitation as snow in the winter. In the Western US, these basins are also water limited in the summer. A large body of work has examined controls of ET using empirical models globally. However little of this work, to our knowledge, can capture ET dynamics very well in western US mountain environments because these systems are both energy and water limited within a year. Eco-hydrologic behavior in these western mountain watersheds is best captured with a relatively fine-scale implementation of a process-based model. The complexity of using this process-based model however limits the number watersheds from which we might generalize our results. Thus we selected a few basins that have variable precipitation regimes – because water availability is the key limitation to forest ET - that are examples of different mountain climate patterns found in western US.

**"the authors perform sensitivity analyses to some of the driving variables of the RHESSys model in the three basins. However, the results are presented as a conceptual model of ET in response to climate and geologic characteristics of the basins. The conceptual model (section 2.1) is rather a discussion of the variables that the authors expect to be the principal controls of ET in the catchments. This is not really a model and should be clarified."**

We appreciate the critique in our language choice of a "conceptual model" for section 2.1 given our modeling-intensive methodology. We would modify our description of these variables to "conceptual framework" to provide clarity.
In addition to improving our word choice to reflect that this is a framework, we would shorten section 2.1. Reducing its length would call out and strengthen the text that justifies our use of these particular driving variables (currently page 2282, lines 10-end). Though precipitation and temperature are used to drive RHESSys, they are also the key climatic metrics influencing water availability through snowpack dynamics for western US mountain forests.

**The variables compared to ET include P, T, and $R_{75}$. There is no explanation in the text as of how these variables are calculated, are they averaged over the catchment for each water year? What are the ranges of the variables within these catchments? Large variations in these parameters should be expected in the catchments given the variations in topography, relief, soil properties, vegetation cover, and weather/climate.**

These variables are explained in section 2.3.1, page 2286 lines 7-26. However, this comment once again suggests that our manuscript would be strengthened by strategically editing our current text and better clarifying the goal of the paper. This editing to remove extraneous text would complement Reviewer 1's comment that portions of our manuscript were too detailed.
The ranges for these variables are presented in the figures and the ranges compared and discussed in the results sections 3.1-3.3.
We provide a spatial average of these parameters, which indeed have a large amount of variation, especially in mountainous catchments. The RHESSys model accounts for these variations using spatial maps (DEM, soil and vegetation cover) and algorithms developed for extrapolation of climate processes over spatially variable terrain (MTN-CLM). This explanation is provided in section 2.2, page 2283 lines 22-26.

**There is also little in the manuscript about the model implementation in the study basins. The authors only briefly discuss the model implementation in one of the catchments, and refer the readers to two other publications (one for which the reference is missing in the list). The model implementation and assumptions are important for the readers to understand the model's capability to accurately capture the processes that influence the variables analyzed, as well as the uncertainty expected in the model's output.**

We provided references as explanation of model implementation in two of our three watersheds in order to shorten an already-lengthy manuscript. It was a regrettable oversight that one these references was missing from the citations. We provide it here for completeness. We agree that description of implementation and assumptions are key in validating key aspects of a model's performance and we suggest that by providing reference to two very recent publications (2013) that we are able to not only describe implementation but provide extensive detail in these implementations' uncertainties.

Garcia, Elizabeth S., Christina L. Tague, and Janet S. Choate. "Influence of spatial temperature estimation method in ecohydrologic modeling in the Western Oregon Cascades." *Water Resources Research* 49.3 (2013): 1611-1624.

**In summary, the article needs major revisions, the descriptions of the simulations and how the parameters are obtained needs to be expanded, and the conclusions from the analyses need to be stronger. After multiple readings, I gather that the authors show that ET response primarily to P, followed by T, and R75. The additional sensitivity analyses of the slope of the scatter plots between all these variables as a function of AWC complements these results. However, these do not represent a significant contribution to the current knowledge. Overall, as a**

**reader, I was left with enough questions about how the results are in reality representative of the processes in the watersheds.**
We hope that our above comments regarding model implementation and climate variables addresses concerns about sufficient description. We agree, especially in conjunction with all other reviewers' comments, that our conclusions should be stronger. We would also focus our introduction to highlight our work's contribution to the hydrologic literature; namely, that a soil-climate interaction influences estimates of forest ET estimates in the montane western US. Of broader importance, forests in these regions are sensitive to forest mortality events that are increasingly common worldwide. Identifying the physical instances (climatic or soil characterizations) in which our ability to estimate ET is most sensitive (or limited) by knowledge of soil characterization we hope will also help to prioritize regional data acquisition agendas. For example, our results show that the more strongly Mediterranean climates (OR-CAS and CA-SIER) are very sensitive to soil AWC.

## REVISED INTRODUCTION

"CLIMATE REGIME AND SOIL STORAGE CAPACITY INTERACT TO EFFECT EVAPOTRANSPIRATION IN THREE WESTERN UNITED STATES MOUNTAIN CATCHMENTS"

Western U.S. forests show substantial vulnerability to drought, with declines in productivity and increases in mortality and disturbance in drought years (Allen et al., 2010; Hicke et al., 2012; Williams et al., 2013). Understanding these ecosystems' responses to primary climate drivers is of particular concern given recent warming trends (Sterl et al., 2008) and multi-year droughts (Cook et al, 2004; Dai, 2004). The region is climatically characterized by warm summers that receive little precipitation and the general role of winter precipitation and snowmelt as a driver of forest water use and productivity has been well established (Boisvenue & Running, 2006, Hanson and Weltzin, 2000). Warmer temperatures are already shifting seasonal water availability in the western US through reductions in snowpack accumulation (Knowles et al, 2006) and earlier occurrence of peak snowpack (Mote et al, 2005) and this is evident in shifts in streamflow timing (Stewart et al, 2005). Increased temperatures also effect plant phenology, leading to earlier spring onset of plant water use and productivity (Cayan et al, 2001). However increases in early season water use, combined with higher atmospheric moisture demand, may lead to increased soil water deficit later in the season. These changes in water and energy demand are expected to intensify (Ashfaq et al., 2013).

Soil and snowpack store water for forests in much of the western US. Recently, field and modeling studies have shown that the years with greater snowpack accumulation can be a strong predictor of vegetation water use and productivity for sites in the California Sierra (Tague & Peng, 2013; Trujillo et al, 2012). Though this recent work emphasizes the relationship between snowpack storage and forest water use, there has been little analysis of how shifts in the storage of precipitation as snow will interact with soil moisture storage characteristics. Like snowpack, soil has the potential to act as a water reservoir, storing winter precipitation into the growing season (Geroy et al., 2011). The amount of water stored by a soil varies substantially in space with topography, soil properties, and antecedent moisture conditions (Famiglietti et al, 2008; McNamara et al, 2005). Soil characterization remains a key uncertainty in hydrologic modeling and, in the western US, soil moisture availability is a major limitation of forest water use and productivity (Hamlet et al, 2007; Zhao & Running, 2010). Subsurface drainage can provide moisture to points on the landscape (i.e., riparian zones and swales) during seasonal summer drought, contributing to vegetation presence and enhancing ET (Hamlet et al., 2007; Hwang et al., 2011; Voepel et al., 2011). However, field studies have also shown that shallow soils may not be able to capture all of the annual snowmelt (Kampf et al, 2014; Smith et al., 2011), limiting the role of soil in providing snowmelt for use during the summer. The relative and interacting roles of snowpack and soil storage will vary geographically with regional climate patterns and thus differ in supporting ET.

This manuscript's primary research objective is to address the interaction between soil characteristics and key climate metrics that influence forest water availability in Mediterranean environments that receive a significant amount of precipitation as snow. To our knowledge, little research has compared soil-climate interactions across multiple watersheds in this geographic niche due, in part, to the extensive fieldwork required to characterize these relationships and the difficulty in modeling these environments with empirical models. A spatially distributed, process-based model driven at a sub-seasonal time step can explore watershed response to these climate drivers and changes in their seasonality. We use a process-based model in three case study watersheds of differing precipitation regimes to investigate how climate and soil combine to control inter-annual variation in ET. This work has implications for better estimating a major component of the hydrologic budget.

We apply our model at a daily time step to three watersheds located in the western Oregon Cascades (OR-CAS), central Colorado Rocky Mountains (CO-ROC) and central California Sierras (CA-SIER). These watersheds receive a substantial fraction of precipitation as snowfall, but vary in their precipitation and temperature regimes and magnitude of snowpack relative to precipitation. We use these case studies to provide a range of precipitation/temperature conditions and use these to examine how soil moisture storage can interact with a combination of inter-annual variation in precipitation timing and magnitude and shifts in snowpack storage. We first focus on how differences in three primary climate metrics that strongly influence seasonal snowpack--precipitation, temperature, and soil moisture recharge—vary in their correlation with annual ET. We then explore the interaction effects between climate and soil in changing ET, showing that spatial heterogeneity in soils varies the strength of climatic drivers within and across watersheds.

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## DETAILED COMMENTS

**In general the use of the term “snowpack” is odd. Please review throughout the text. I included a few examples in the detailed comments but there are many others in the text.**
Noted

**The normalization of some variables needs to be explained. Normalized by what? (I deduct the mean from the text, but this needs to be explained)**
We normalized by the mean.

**A conclusion section should be added**
In order to clarify our research’s contribution to the literature we could add a short and concise conclusion section.

**p. 2278. ll. 11-12. “As we expect” could be eliminated.** Noted
**p. 2279 l. 17. “Snowpack magnitude” does not seems correct. Perhaps, “maximum snow accumulations” or “Peak SWE” would be more descriptive.**
**l. 23. Again, “peak snowpack” does not refer to a quantity.**
**p. 2281 l. 15. Same as above: “magnitude of snowpack”. Review throughout the manuscript.**
Thank you, noted.

**p. 2282 l. 13. “all” is repeated.** Noted.
**ll. 14-15. By which mechanism does climate alter water demand? Explain, the end of the line leaves this still unclear.**
Increased temperature increases the duration of water demand in western US snow-dominated basins by melting out snowpack earlier, making this water reservoir unavailable for forest water use earlier in the summer months when rains occur infrequently.
These mechanisms are detailed in the introduction on p. 2279, line 21 through p 2280 line 5.
This comment suggests that key pieces of the introduction could be removed and used to strengthen our text in the methodology.

**l. 16. Which geologic and topographic properties? Expand.**
We focus on soil properties such as hydraulic conductivity, pore size index and air entry pressure, which we synthesize into our soil available water capacity (AWC) metric. At this point in the text we could note that we expand on these properties in a later section.

**l. 20. “spring” or “winter and spring” rather than just winter?**
Yes, ‘winter and spring’ would be more descriptive.

**l. 26. Clarify if “winter precipitation” is referring to snow,**

We were precise in using ‘precipitation’ because watershed OR-CAS receives a significant portion of its winter precipitation as rain rather than snow, as compared to CA-SIER.

**l.28. “then soil will have little availability to extend water availability” reads odd to me.**
The text should read “little *ability* to extend water availability”, and might be improved by in readability by not rhyming, i.e., “less capable of extending water availability”.

**ll. 10- p. 2283. l.9. There are no references in this paragraph, however, there are a lot of processed mentioned without support.**
We appreciate this comment because it indicates two sections that should be merged in the manuscript, which would shorten the document and focus our message. References for these statements are included throughout the introduction (page 2279 l. 21 through page 2281 line 6). We summarized those references here as justification for the controls we explored.

**p. 2283. l. 3. “uncontroversial” sounds odd here.** Noted

**p. 2285. l. 9. What statistics were used?**

Two streamflow statistics: daily bias and Nash-Sutcliffe efficiency. Page 2285 lines 10-12:

Streamflow statistics were set such that good soil parameters resulted in daily flow magnitude errors less than 15 % and Nash–Sutcliffe efficiencies (a measure of hydrograph shape) greater than 0.65 (Nash and Sutcliffe, 1970).

**p. 2286**

**ll.1-2. The statement about the future research is unnecessary.** Noted

**l. 7. What “statistical analysis”? The term is too vague.**

In lines 8 and 9, we explain: “We performed linear regressions…” and line 12 contains our threshold p-value “greater than 0.05 is not significant”.

**ll.7-16. I would like to see the analysis, e.g., the scatter plots. I think the reader would wonder about how these relationships look like. Also, What temperatures are being used here? From single stations? At what elevations? Are they representative of the whole basin? Generally speaking, this is is hardly the case, for which you should provide more information to understand the limitations of the data used here.**
Out of concern for the length of the document, we chose not to include the exploratory plots showing 1 and 3 month averaged temperatures with annual ET. We agree with that including the temperature’s data source would be valuable information. We would add text to the manuscript noting: “All precipitation and temperature data is derived from climate stations within the basins and extrapolated across the terrain using MT-CLIM algorithms”. Elevation data for the climate stations could be included in Table 2.

**ll.12-13. Include the correlation values obtained.**
Noted.

**l. 18. Which water limited basins? Clarify. Also, I don’t see how the authors have tested the water limitation in this basins, how is this known? No support is included.**
Our text notes these are “seasonally” water limited basins and a citation is included in the new introduction:
Hanson, Paul J., and Jake F. Weltzin. "Drought disturbance from climate change: response of United States forests." *Science of the Total Environment*262.3 (2000): 205-220.

**l. 23. I don’t have this clear. Do you need to know the total recharge to know what the 75% would be? How is the total recharge estimated? And, from reading this, it seems like the assumption is that all water that reaches the soil infiltrates, which is not correct depending on many factors such as infiltration rates, soil properties, saturation, water fluxes reaching the surface, etc.**
Lines 24-26 note that “we do not differentiate between water that, upon reaching the soil surface becomes runoff, and water that infiltrates into the soil.” We treat this variable as a temporal marker of potential water availability– one that denotes the lateness in the water year that water that has potential to infiltrate the soil. Though the factors you noted may certainly change whether or not this water is absorbed, we do not account for whether the water enters the soil column.

**l. 24. A comma is missing after “rain”.**
“Rain throughfall” refers to the liquid precipitation that is not intercepted by the canopy and evaporated away.

**ll. 24-26. What is the justification for this assumption? It should be included in the text.**
Please see our response to line 23 above.
**p. 2287.**
**ll. 15-17. Does this mean that there is only one soil type or an average per watershed?**
The watersheds are represented by one (OR-CAS), two (CA-SIER), and five (CO-ROC) soil types. Number of soil types was determined based on best available soil mapping for each watershed. Though explanation of soil map data sources are included in watershed implementation we realize based on earlier comments that referring the reader to another journal article is not desirable. Therefore, this text would benefit from our plainly stating number of soil types and that our AWC metric represents an average value for all soil types in the basin.

**l. 21. 25-45 yrs refers to what period? Include**
Included in Table 2, page 2308, line: “Climate record”

**p. 2288.**
**l. 19. Can the Colorado basin really be considered Mediterranean? Also, an explanation on what a Mediterranean climate is pertinent here.**
We consider our CO basin to be Mediterranean because its ET estimates do not meet its potential ET during the summer/fall growing season. We would include a better definition of Mediterranean climate in the introduction.

**l. 24. The word “loss” does not seem appropriate here. And how is this known? How about soil recharge, etc?**

Text that further explains that Figure 2 shows average daily streamflow over our 45 year record and that peak streamflow occurs in December, as compared to CO and CA’s peak streamflow occurrence in May. We assume these streamflow peaks are winter precipitation pulses.

**l. 28. It may be more clear to expand “Carbone” and “Nitrogen” in this line. p. 2289.**

Noted

**ll. 4-5. Garcia et al. (2013) is missing in the reference list. Tague et al. (2013) is Tague and Peng (2013)?**

Noted

**l. 12. No need for the word “downloaded”. Also, opening “(“ is missing before the link.**

Thanks, noted.

**l. 14. Is the date of access needed? I’m missing the point of some of the information in these lines.**

It is frequently included in other peer-reviewed scientific literature to detail the data’s provenance.

**l.15. “so” can be eliminated**

**l. 20-21. A comma is needed after “rock”. Same comment as above (l. 14).**

**ll. 26-28. According to these lines, only two points (Stations?) are used to estimate the spatial distribution of temperature and precipitation that drives the model for this 350 km^2 basin. This must lead to a lot of uncertainty in your results.**

The station data is spatially interpolated using climate algorithms developed for mountain environments (MTN-CLM). Nonetheless, some uncertainty in our results undoubtedly exists due to error in climate inputs. Our validation against daily streamflow records (page 2285 line 11) is an important step in supporting model estimates.

**l. 27. How is this shift determined at this elevation? And what does climatic regime refers to?**

This shift was determined based on conversations with regional field scientists (which should be noted in the text as ‘personal communication’) and would modify our language – instead of ‘climatic regime’ we might write precipitation patterns (seasonality and magnitude).

**pp. 2289-2290.**

**P. 2290. Here, only the model of CO-ROC is described, yet the manuscript uses the model results for three basins. I understand that this is done because the models for the other two basins are presented in the two references in the paragraph before, but what it really means is that I (and the readers) have to read two more papers to really get an idea of what the other model implementations required. The article should be comprehensive, even if the support for the models of the other two basins is included in the other articles. At least a summary of the implementation of the**

**model for the other two basins should be included here; I was lost trying to figure out why only one basin was included. Also, the first reference of the other two basins is missing in the reference list.**
We would include additional text that refers to climate data sources and reiterate, as noted on page 2284 line 25 – page 2285 lines 17, the commonalities in soil calibration. However, description of all basins' data layers may be beyond the scope of this manuscript's methodology.

**l. 8. Opening parenthesis missing.**
Noted

**l. 20. This is a writing style comment, but statements such as "not surprisingly" in this kind of sentence seems unnecessary.**

**l. 21. Significant as in statistically significant? Or how is the significance evaluated? A correlation value and p-value?**
Thank you, this would be a good place to reiterate our p-value significance thresholed of 0.05 and provide a first reference to Table 4, where al p-values are provided.

**l. 26. I suggest using "dryer years" instead of "dry years".** Thank you.

**p. 2291.**
**l. 1. Include the p-value or other statistics used to evaluate the "significance" of the relationships. Check this throughout the test and reword when appropriate.**
Table 4 provides p-values – our text would be amended to explicitly state this.

**ll.5-6. The water-limitation of the basin can not simply be assessed by the percentage of precipitation during the growing season. This should include more rigorous support.**
We agree that a measure such a PET-AET would be more rigorous.

**l.7. "in all watersheds". In is missing in text.**
Thank you.

**p. 2292.**
**l. 7. "lost" again, see comment above. Also, I don't see any justification for this statement. If the meltout is earlier, how is the infiltration and net recharged reduced? For example, if the same amount of snow is melted, how does that lead to an increase in runnoff and a decrease in recharge just from having earlier melt?**
We appreciate that this comment highlights an assumption we do not explicitly state in our text which is that soils are more likely to be saturated in the spring months, after receiving winter precipitation. We assume that later in the growing season, soils would not be saturated so throughfall/snowmelt would enter the soil matrix and be available for plant water use.
**p. 2292. l. 25. Expand on the methodology by Muggeo.**
**p. 2293. ll. 15-18. Include TWI values.**

Currently included in Table 2.

**l. 22. Normalized by?**

Thank you, text should note that we normalize by the mean AWC.

**p. 2295-2296 ll. 25-28. This sentence is too long and hard to follow, rephrase.**

**p. 2297. ll. 7-10. The sentence needs rewriting, and "isn't" should not be in a scientific text.**

Noted.

**l. 13. "Snowpack acts as a storage mechanism" is odd. Reword. Also, using the active voice for an inanimate object may not be the best way to describe this. But again, this is a writing style comment.**

**ll. 13-17. This sentence does not really present much information. For example, how is the timing of soil moisture recharge and snowpack dynamics "important"?**

We should strengthen the sentence to denote that timing of recharge and snowpack dynamics are primary/secondary explanatory variables of ET.

**l. 18. Again, "important"**

**ll. 20. "rather than" not "then"**

**l. 24. "modest" is vague, what does it represent?**

**The location of the basins should go in one figure. The insets are too small to clearly see the location.**

**Figure 2. Insets and text is hard to read. Same as lat. and long. Coordinates. Where are these coming from? Are they at a single station or are the averaged over gridcells or patches of the basin? How is the data displayed obtained?**

**The captions should present/describe what is in the figures (e.g., fig. 4). Also, you can leave the main message or statement that the figure conveys in the text, not needed in the figure. Also, in general, you can use smaller markers to reduce overlap.**

We thank the reviewer for all detailed stylistic comments, including those that refer to figure legibility, and will take them into consideration in future edits.