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Interactive comment on "Fire-regime variability impacts forest carbon dynamics for centuries to millennia" *by* Tara W. Hudiburg et al.

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Reviewer 1 comment

First of all, even though the authors refer to past published studies, they should present or document the reconstructed response of vegetation (changes or not) the site recorded at least with the same level of information as for the fire reconstruction they provide.

Response: We respectfully disagree with this comment, for two reasons. First, we did not use the vegetation record to drive DayCent, so in this respect we do not feel it needs the same level of information as the fire reconstruction. Second, the pollen record at this site indicates the dominance of subalpine forest taxa for the duration of the record presented here, which is consistent with other regional records. To support

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this statement, we provide the citation to the original paper with the pollen record, as well as other studies from the region: Caffrey and Doerner 2012, Dunnette et al. 2014, Higuera et al. 2014.

Secondly, and most importantly, I wonder why authors have used only the same fixed 30-year time series for climate data whatever the time frame simulated over the last 4500 years BP instead of using past climate simulations from GCM or ESM whose many have Holocene climate as well as Future climate runs. . .. whereas several studies have documented and discussed about the potential counter-effect of precipitation increase in compensating the effect of temperature increase on fire occurrences and spread. . ..

Response: We agree that using paleo and/or future climate scenarios would be very interesting and useful. However, in this paper we are purposefully isolating the potential impacts of fire-regime variability. Our intent is not to replicate the exact dynamics that occurred at Chickaree Lake; rather, we are using DayCent as a tool to test alternative hypotheses and using the fire history of Chickaree Lake as an example of realistic variability in fire activity. In DayCent, we thus prescribe when fire events occur, which automatically decouples the fire events from climate from a modeling point of view. Even if we had a perfect paleoclimate data, few (if any) models would be capable of replicating the Chickaree Lake record, which would turn the paper into a model development project. Additionally, we also prescribe the erosion events associated with fires, again decoupling them from precipitation events.

This would have prevent authors from saying that fires and climate are disconnected which is absolutely not true, or at least need to be tested for each ecosystem studied. Moreover, instead of just increasing the 30-year time series temperature by 2°C, they could have used the full climate time series for the 21st century simulated by the same climate or earth models that provided the Holocene runs. They even could have tested different IPCC scenarios and their impact of the NECB. The use of climate model data would have provided precipitation time series as well, whose changes could also have

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impacted soil nutrient (and C) leaching. Indeed, it is easy to show that fire regime change outweighs climate change when such climate change may be unrealistic or only taken into account through temperature increase whereas several studies have documented and discussed about the potential counter-effect of precipitation increase in compensating the effect of temperature increase on fire occurrences and spread.

Response: We certainly do not believe that climate and fire are disconnected, and much of our own work explores fire-climate relationships in these and other ecosystems. To clarify this, we added a note in the study area description, specifying the nature of fire-climate relationships regional subalpine forests and citing some key references. In DayCent, the only impact of using forced climate (with the forced fire and erosion events) would be the feedbacks to plant growth, which would increase or decrease the biomass available to burn given certain climate conditions. This is why we implemented the simple warming scenario: to see if/how our results would differ when biomass accumulation rates were higher (due to warmer temperatures). Our results indicate that the impacts of climate, as reflected by plant growth, is insignificant compared to the disturbance impacts. We believe this result is neither wrong nor insignificant, but it is a finding within the context of using DayCent experimentally. Finally, because the charcoal record indicates when fire events occur, incorporating a paleoclimate record at the daily timestep and for a single location in the Rocky Mountains would likely add significant uncertainty, in both the precipitation regime and certainly if fire was "dynamic" and occurred in response to simulated climate.

It is even more important in the studied system as authors suggested and used two types of high severity fires: those with and those without erosion. Stand-replacing fires (95% mortality) are not really severe fire if post-fire regeneration is occurring in the next following years from naturally adapted species. Fire severity would rather refer to the difficulty of post-regeneration encountered in special cases. Stand-replacing fires are usually very intense and fuel consumption includes all the litter and humus layers, leaving the mineral soil exposed. So, if erosion in the burned watershed occurs (towards

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the lacustrine receptacle), it is performed during (heavy) rainfall events. Therefore, this is another argument to show that it would have been valuable to use past simulated precipitation over the last 4500 years BP, in order to test if rainfall (even as mean annual rainfall) changes could have occurred contemporaneously to erosive events just after some fires as compared to others.

Response: In western North America, subalpine forests like our study area are classified as "high-severity fire regimes," where "severity" refers to the immediate impacts of a fire on the ecosystem, often measured (directly or indirectly) by the amount of vegetation killed. In most cases, post-fire regeneration in subalpine forests does indeed start in the year immediately following fire, but we consider this an ecosystem response. While we appreciate the shortcomings of the concept of "fire severity," this is the standard terminology used, and we have added some references to support this use (i.e., Keeley 2009, Int. Journal of Wildland Fire). We simulated consumption of litter and humus layers in DayCent. In fact, the fires were parameterized to consume (combust) the forest biomass pools given known combustion coefficients for these types of forests (which includes 99% removal of the litter layer). With respect to climate forcing, again, we are forcing the erosion events to occur regardless of precipitation, based on the reconstructed fire history record. It would be ideal to test if the erosion events occurred with large precipitation events/years, but this is beyond the scope of this study.

Moreover, authors provide no information on the vegetation compartment modeled except the Net Ecosystem Production for outputs, so we have no idea about which plant types are used for this site nor why 30cm deep was chosen as the targeted depth to analyze the site response. Finally, in the current version, except from NEP, we have not idea about the effect of vegetation change in terms of composition nor structure through time, we cannot see the direct as well as indirect effects of climate change on vegetation nor climate on fire as climate dataset was fixed and repeated along the 4500 years BP, even though fire ignition and fire spread conditions may have been more or less favorable.

Response: Again, our purpose in this study is not to predict the effects of climate (or fire) on vegetation change over time (or the effects of CO2 or nitrogen deposition, etc). The study site description includes a description of the known vegetation cover and based on the previously published pollen record from this site and others, we are confident that this general forest type did not change over the duration of our record (as noted above). DayCent (and most biogeochemical models) can only model soil C dynamics to a depth of 30 cm, primarily because this is the most active zone.

For all these reasons I see two options that require to modify the manuscript: Option 1: to do the modelling experiment exercise once again but using climate data that represent the studied Holocene period for the first part and the 21st century for the second part. Even though climate data come from GCM and are not perfect, they will still be better than present-day ones applied to past and/or future periods, especially if climate is tested and its relative impact compared to that of fire regime variability. In parallel to temperature and precipitation datasets, authors should explain how they deal with air CO2 concentration as it should have been modified from 280 ppmv until 1750 to the historical recorded concentration until nowadays, and for the Future, at least a mean CO2 increase should be used if authors do not want to test several RCP scenarios. By keeping the CO2 at a fixed concentration could still be acceptable but once more, as they are tracking C pools, I think that the atmospheric C input should be taken into account.

Response: This is beyond the scope of this study and we are concerned that this activity would introduce large amounts of uncertainty (given modeling limitations) rather than actually clarifying our results. Again, our purpose here was not replicate the exact Holocene dynamics of this site.

Option 2: keep the modelling experiment in the current version but authors need at least to remove the third objective as climate has not been properly taken into account as compared to the fire regime factor. In such case, they should explicitly present this study as a first-step modelling approach integrating only the fire regime information

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and therefore only testing it. All sentences related to climate effect should be modified in order to rather present or discuss limit of non-using proper climate data. This would better fit with the balanced way results must be discussed. In such a case, the first two objectives are still OK. Results and conclusions should be fairly presented without omitting that the climate data used may be a limit to the interpretations done.

Response: We agree the climate objective should not be a 'main focus' or main objective of the paper. We disagree, however, that the results of the 2 degree warming scenario are 'not correct' in terms of the feedbacks that can be simulated within this framework. We have modified the text to clarify how climate was and was not addressed in this study.

Otherwise, I found pertinent the improvements suggested in the M.W.I. Schmidt's comment posted for improvement definitions, more detailed explanations and improvement in figure quality so I encourage the authors to take them into account. They will facilitate the reading of the manuscript for people not fully familiar with model requirements and functioning such as the need of a spinup period, the use of several pools or compartments... If supplementary material is allowed I suggest to add such information there, even with a scheme presenting how the DayCent model works.

Response: We have addressed and utilized many of the comments from Schmidt. DayCent has excellent documentation online (powerpoints, step by step instructions, publication lists; http://www.nrel.colostate.edu/projects/daycent-downloads.html). If allowed we will include the link in the manuscript. We will also post our model input and output on the Dryad repository (not allowed until manuscript is published).

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