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Discussions

Interactive comment on "Prescribed-burning vs. wildfire: management implications for annual carbon emissions along a latitudinal gradient of *Calluna vulgaris*-dominated vegetation" *by* V. M. Santana et al.

Anonymous Referee #3

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Title: Prescribed-burning versus wildfire: management implications for annual carbon emissions along a latitudinal gradient of Calluna vulgaris-dominated vegetation

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Santana et al. simulate the effects of different rotation intervals of prescribed-burning and wildfires on biomass of Calluna and litter mass in sites in the British moorlands. They first develop biomass-accumulation curves from four Calluna-dominated ecosys-

tems along a latitudinal gradient, but it is actually more complex, and involves large variations in altitude, ambient air temperature, precipitation and soil types. They use previously published and field data to develop a matrix-model based on Markov Chains to calculate above-ground biomass for Calluna and litterfall, and then approximate net C accumulation under different prescribed-burning rotation intervals and an (unfortunately, an unrealistic) wildfire return interval.

Major comments:

The authors are correct in the Introduction that "fine-tuning" management practices where tradeoffs between management activities and carbon sequestration occur is a challenge, and needs to be understood to optimize these goals. However, the utility of the results of this exercise and how this would actually assist land managers is unclear, mostly because the scenarios that are simulated are not especially grounded in reality. The authors largely ignore the facts that fire managers are strategic in their placement of planned fires, and obviously consider landscape-scale fire breaks to reduce the size and intensity of wildfires. It is well understood that wildfire return intervals are stochastic, and not fixed, so that the simulated wildfire return interval is both temporally and spatially artificial. It would be more realistic to simulate a random wildfire interval, at least. Also, the authors need to include a wildfire-only scenario to actually evaluate the effects of prescribed burning (including feedbacks on reduced wildfire severity).

The authors do not provide information on the aerial extent of Calluna-dominated ecosystems, or information on the areas that are actually burned in prescribed fires, or on the return interval of wildfires in these ecosystems. Thus, the reader is left to guess that fire plays an important role in this ecosystem. Providing some information about annual acres burned and on estimated wildfire return intervals would be helpful. They are likely very different across the study sites.

The authors frame their analyses as "carbon emissions" when in fact they are estimating net accumulation of above-ground biomass of Calluna and litter only. If they really intended to simulate carbon emissions, they would need to include net ecosystem exchange (NEE) of CO2 pre- and post-burn to fully evaluate impacts on carbon emissions. A major concern is that annual changes in leaf area will affect the NEE of CO2, which has been documented in numerous studies. There for, throughout the manuscript, the authors should use "net" accumulation, because what they are really simulating is productivity – decomposition for vegetation and litter. How do the authors know what is consumed following wildfires, and whether or not biomass and litter accumulation curves are similar following prescribed fires vs. wildfires? It is likely that they are not identical, and not having wildfire data limits the analyses here.

Hypothesis 1 concerning latitudinal control of productivity is too simplistic, and needs to be reformulated or omitted. The latitudinal gradient is quite short, and is confounded with variation in altitude, ambient air temperature, precipitation depth, soil factors, and likely management histories which impact plant-soil feedbacks. Controls over GPP and decomposition are well known, and throughout the text the authors provide evidence that controls over net productivity and litter accumulation are more complex. So, why is such a simplistic still hypothesis entertained?

Without actual pre- and post-burn data to evaluate the amount of fuel consumption, the notion of "combustion completeness" is odd, and likely incorrect. In Fig. 5, the authors simulate a range of CC values up to 100%, but how can fires consume much above 90% of the biomass and litter? This would assume no char or ash production. Many prescribed fires consume somewhere between 40 and 70% of pre-burn fuel loading, and consumption is often a function of initial fuel loading. The approach the authors take here seems too simplistic, and it would be better (and more consistent with other studies) to use emission factors based on actual consumption data.

The authors run simulations over a 200 year interval, but projected climate change even at low emission scenarios will result in changes at each site. The authors should consider changing productivity – decomposition relationships to add an element of reality to their simulations.

Specific comments:

Page 17819: Schimel et al. 2001 and Pan et al. 2011 report on C dynamics of forests. It would be better to use more appropriate references that include fire as a disturbance in shrub-dominated ecosystems.

Page 17820: Authors should add "windy" to list of conditions driving wildfires.

Page 17821: The authors provide a lot information on prescribed burning policy in the Introduction, but the actual acreage burned in prescribed fires and wildfires should also be presented in the text, or preferably a table.

Page 17822, line 14: The selected wildfire return interval doesn't seem to reflect any realistic fire return interval for dry or wet sites. Where do these numbers come from?

Page 17823: Please provide the length of the transect (km) in the text.

Page 17823: The second paragraph provides enough information to indicate that the environmental variation along the selected transect is too complex to pose or evaluate Hypothesis 1.

Page 17824: Again, some of the information presented indicates that wildfire histories are known for at least some of the sites, and these should be presented somewhere in the text. A table including fire histories of the sites would be very helpful. Then, these fire return intervals should be used to guide simulations.

Pages 17824-17825: The authors use such different techniques for deriving biomass of Calluna and litter mass that they likely have little idea what the actual uncertainty of their estimates are. Can they provide some uncertainties to these measurements?

Throughout the text, the authors provide conflicting statements. For example, In Management Implications, the authors state that "...our results provide information to guide policies for future sustainable management of European heathers and moors in terms of C budgets", but elsewhere that "...this highlights the importance of studying site-specific biomass accumulation patterns with respect to environmental conditions for identifying suitable fire-rotation intervals to minimize C losses." The authors need to reconcile statements like this in the manuscript for their research to really be of value to land/fire managers. It seems that there probably are some generalizations that can be made here, but this leaves the reader thinking that every site would need to be quantified.

The use of ClossBP200 is an odd variable. It would be better to use net C accumulation, not loss. Also, these are minimum estimates for C loss (or net accumulation), because they do not include forest floor OM, peat or other vegetation. As the authors point out, at some sites vegetation other than Calluna forms approx. 20% of the biomass.

Why does litter mass at Moor house start at 8 t ha-1 following a fire and then never accumulate? Clearly the Calluna and other biomass is accumulating, and producing litter. This seems like an error in the field data.

How does the mass of Calluna accumulate linearly at the Holden site? If this was live- and dead aboveground mass, this would make sense. However, a linear increase seems unlikely. Perhaps this is an artifact of how the vegetation pools were developed from metadata?

Interactive comment on Biogeosciences Discuss., 12, 17817, 2015.