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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.

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We would like to thank Referee 3 for the provided comments and thoughts because they can be very helpful to improve this manuscript. Questions and comments are addressed below:

RC1: 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).

AC1: We agree to some extent with Referee #3. It is true that wildfire return intervals are too simplistic and stochastic simulations would be optimum. However, for comparison between sites we think that the fixed return intervals used in our paper are preferable to allow contrasts between different intervals. Using our approach we can understand the tradeoffs between carbon sequestration and fixed fire intervals, being fundamental for fire planning. It can give a clearer idea of carbon losses on increasing return intervals, and not changing the final results and conclusions obtained. In addition, we think including more complex model parameters such as random wildfire intervals, spatial variability of wildfires and wildfire severity feedbacks are out of the scope of our paper. Of course including these parameters will result in more realistic model outputs, but we think they will not change the essentials of results and management implications outlined in our manuscript. On the other hand, following the answer to Referee #2, including C losses produced by the wildfire only scenario can be easily solved. In our modelling, the amount of biomass in situations without prescribed burning corresponds to that found in equilibrium 50 years after fire, and shown in figure 2. For scenarios with 50 years wildfire return interval the carbon released is four times these values, for 100 years twice the values, and for 200 years once the value. These values can be easily added to Table 2 to solve the lack of these values. Burning for firebreaks is an obvious strategic plan, but we suspect that he/she is thinking of another ecosystem. In studied ecosystems, prescribed fires are performed following a patchy strategy for rejuvenating vegetation. We challenge the referee to identify fire breaks as a strategic aid in this photo – taken from Douglas et al. (2015) (Figure 1 at the end of this file).

RC2: 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.

AC2: Yes, we agree with Referee #3 in the fact that more information about the area burned annually by prescribed fires and wildfires in Calluna-dominated ecosystems is needed. Information about this is still very scarce and confusing for these ecosystems. It is difficult to obtain this information because the singular pattern of prescribed fires distribution; however, the known references and data can be easily included in the manuscript in order to reduce this lack of information. Douglas et al. (2015) assessed that, historically, prescribed burning in Great Britain has occurred across 8551 1-km squares. In addition, the annual number of burns from 2001 to 2011 increased significantly overall across Scotland, England and Wales at a rate of 11% per year. Similarly, previous studies had estimated this area in approximately 6600-17000 km2 (Bunce and Barr 1988, Grant et al. 2012, Hudson 1992). This represents about 20-60% of Calluna dominated moorland, although not all of this may be subject to rotational burning (Ball et al. 1983, Brown and Baindbridge 1995). From the total burnable area, the area burned annually varies widely throughout Great Britain. For example Allen et al. (2016) found for the Howden site an annual area burned 0.9% from the total. Hester and Sydes (1992) found an annual average between 1% and 2% of Calluna in Scotland. In contrast, Yallop et al. (2006) found for England that in the year 2000 17% of the area of this habitat had been burned within the previous 4 years, equivalent to

114 km2 year-1 . Very little is known about wildfire return interval in Great Britain. It is unpredictable, and particularly ignition often results from human involvement in the UK (accidental or arson; Albertson et al. 2009). Further research is needed in order to assess this parameter. However, our modelled values straddle the approximate natural background fire incidence of 125 years between 1000 and 3000 years ago, which were derived from peat cores on nearby Robinson's Moss (K. Halsall, unpublished data). Therefore, we think our modelling assumptions can be a valid approximation to assess effects of increasing/decreasing wildfire return intervals on ecosystems studied.

RC3: 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.

AC3: In this paper we are calculating two different parameters. Firstly, as the Referee comments, we calculate net accumulation above-ground biomass. However, as a second step, we calculate carbon losses by crossing this biomass accumulation with CC in prescribed and wildfires. This finally results in carbon losses by the effect exclusively of fire. Obviously, including other factors such as ecosystem respiration or annual variations in leaf area will improve the simulation. However we think this is out of the scope of this paper, and modelling so complex processes together does not always give an improvement of outputs but an increase of the complexity of results. On the other hand, we agree with the referee that biomass accumulation patterns may slightly differ after prescribed fire and wildfires. However, we assume these possible differences by simplicity, and because the difficulty to obtain these patterns in the reality. We think the general results and conclusion obtained are not affected by this.

RC4: 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?

AC4: Yes we agree with the Referee #3 in the fact that Hypothesis 1 is too simplistic and the latitudinal gradient also includes other confusing variables such as altitude, climate and soils. We initially included this hypothesis in order to make easier to the reader the understanding of the initial and descriptive part of the paper, where we talk about the environmental conditions of the different study sites. We think that including this part within a subsection with a defined easy question may help to draw a dividing line throughout the text for the reader.

RC5: 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.

AC5: We disagree with the referee in this part. For the whole calculations, we used CC values based on real data sampled in real prescribed fires (70% for Calluna and 54% for litter; Allen et al. 2013; See page 17828, lines 17-18). Only in calculations shown in Fig. 5, we simulate different CC scenarios which range from 20% to 100%. It is true that CC scenarios of 100% are not common on these ecosystems, but they are not impossible. In fact, severe fires where all aboveground biomass is consumed and the underneath peat is affected can occur sometimes (Maltby et al. 1990). In addition, we assume that within the ongoing climate change there will be an increasing trend of CC as consequence of warmer and drier conditions. Therefore, we think CC simulations included in Fig. 5 are not only useful but realistic.

RC6: 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.

AC6: We think this is a strong part of our paper; considering that sites that behave nowadays in a specific way can behave as others in a near future. We agree in the fact that changes in site conditions can occur in these 200 of simulation, but our result can be useful to set possible future carbon losses trends and design management practices. Reproducing so complex and realistic modelling is out of the scope of our paper.

RC7: 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.

AC7: We can include some reference referring to shrublands such as Fernandes et al. (2013)

RC8: Page 17820: Authors should add “windy” to list of conditions driving wildfires.

AC8: It can be included.

RC9: 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.

AC9: See response above (AC2).

RC10: 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?

AC10: See response above (AC2).

RC11: Page 17823: Please provide the length of the transect (km) in the text.

AC11: It can be included, approximately 700 km.

RC12: 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.

AC12: As explained above, hypothesis 1 tries to describe easily for the reader the environmental conditions of the different sites. We acknowledge it is too simplistic, but we think it is an elegant way to set the initial conditions where the subsequent modelling is based on.

RC13: 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.

AC13: As explained above, there is little information about this, but this lacking can be complemented with some data and references.

RC14: 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?

AC14: The curves used were derived from replicated data and SEs are available. At the same time, the uncertainties of model parameters were evaluated, as well as the CI of predicted values to include them in the Leslie Matrix modelling. Therefore, the uncertainties of these measurements and models are known.

RC15: 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.

AC15: this statement refinement can be done easily as suggested by the reviewer.

RC16: 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.

AC16: We disagree with Referee #3. ClossPB200 is defining the C loss produced by consumption of the biomass estimated. For this we use a biomass CC of 70% for Calluna and 54% for litter (see page 17828, lines 17-18). We are not referring to net C accumulation. On the other hand, we only include in our modelling aboveground biomass (litter and Calluna). Peat is excluded because its burning follows a different mechanism in time and space and it is out of the scope of our paper (see answer to referee #2 for more information). In the case of bryophytes, it is true that in some cases they can reach until the 20% of aboveground biomass (here, only the case of Moor House). However, because their consumption is completely null in case of fire as consequence of their high levels of moisture, they are not accounted in our modelling. Finally, the presence of other plant species is negligible in all study sites and, for simplicity, they are not accounted in our modelling. These issues are clearly described in Page 17826, lines 14-24.

RC17: 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.

AC17: Prescribed fires on these systems are performed in winter, when soils are completely wet and frozen and it is possible that no effect on litter is observed. This can be especially possible in Moor House, where environmental conditions are perhaps the least suitable in the UK for prescribed burning. It is the wettest site and with the highest altitude, and the performing active prescribed fires affecting the litter lying on the soil is difficult. In addition, this is the study site with lower accumulation of Calluna biomass along time and, therefore, litter production and deposition rates can be so low that can be not significant and detected by these samplings. Briefly, we think this odd trend of litter in Moor House occurs in reality.

RC18: How does the mass of Calluna accumulate linearly at the Howden 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?

AC18: It doesn't, there is slight curvature – it described as a log-log relationship because this was the best statistical fit. Professor Marrs has tested the top end of these data independently.

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Source: Esri, DigitalGlobe, GeoEye, i-cubed, Earthstar Geographics, CNES/Airbus DS, USDA, USGS, AEX, Getmapping, Aerogrid, IGN, IGP, swisstopo, and the GIS User Community

Fig. 1. Example image showing moorland strip burning. Extracted from Douglas et al. 2015.