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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 1 for the provided comments and thoughts because they can be very helpful to improve this manuscript. Questions and comments are adressed below:

RC1: This study models an optimal regime for prescribed burning intervals with increased wildfire frequency for emission reduction benefits in Calluna dominated ecosystem of British moorlands. The study is some extension of already published work (Allen et al. 2013) done by same of two authors. Same modelling approach, same ecosystem and even the same wildfire intervals are applied in this study. Authors added new bits on biomass/ litter accumulation curves along latitudinal gradient which were derived from published literature; no new empirical data was added. The study doesn't not present any novel ideas, concept or data; The idea of wildfire vs prescribed burn for carbon emission benefits is well established in the literature (e.g. Bradstock et al 2012); concept used in the study is recycled from Allen et al 2013 and the data was derived from already published studies (e.g. again Allen et al 2013, Miller 1979, Alday et al 2015 and Chapman et al 1975). Modelling is very simplified but conclusions are grand. Level of uncertainty is not even discussed.

AC1: We disagree with referee 1 in the fact that nothing new is presented in this work. Specifically, the novel part of this work is the modelling work developed in hypothesis 2 and 3, where we disentangle the carbon loss patterns as a function of the prescribed burning interval and superimposed wildfires. Additionally, this was done considering a climatic gradient. To our knowledge, nothing is known about carbon loss patterns as a function of the prescribed burning interval and wildfires in relation to variations in heathlands/moorlands biomass accumulation patterns produced by different conditions, despite its relevance for the mitigation of carbon losses to the atmosphere. The concept of wildfire vs prescribed burning was established by Bradstock et al. (2012), but for specific ecosystems dominated by Eucalyptus in Australia. Hence, it has not been tested on heathlands/moorlands. There is a lack of studies taking into account C losses by integrating prescribed burning and wildfires for the different ecosystems and biomes worldwide. We think our work is an essential contribution for designing management plans of prescribed burnings in heathlands/moorlands dominated by Calluna from boreo-temperate ecosystems.

RC2: How was emission estimated? Authors don't mention any emission factors, vital in any emission estimates. How does latitudinal gradient or burn season affect emission factors and thus total emission?

AC2: Here, we work in terms of total C losses to the atmosphere by means of biomass combustion. We calculated this C by accounting the biomass consumed by fires and multiplying it by its C concentration. This is explained in Page 28, lines 10-13 of the manuscript. We do not work with emission factors for greenhouse gases (CO2, CO CH4) within these total C losses. This is out of the scope of this paper. However, we think that the calculation of total C losses to the atmosphere is of vital importance and interest, although we do not discern between gases and the variability of them within the gradient.

RC3: Authors modelled the effect of combustion completeness (CC) on carbon emission and went as far as applying CC of 100%. The question is, is it ecologically /physically possible to get CC of 100% in wildfire/ prescribed burn?? Does it mean that trees will evaporate without a trace (even of mineral ash)? Authors do not account for carbon left in dead trees or converted to charcoal. Body of literature exists on carbon redistribution from live to dead pools or production of black/pyrogenic carbon during fires (e.g. see Santin et al 2015, Volkova et al 2014, Bennett et al 2014), all of this will affect emission estimate.

AC3: We think the referee did not completely understand the systems under study. As we stated clearly since the beginning, we study heathlands/moorlands dominated by the dwarf shrub Calluna vulgaris and not trees. Indeed definition of a heath according to Gimingham (1972) is “a treeless tract”. Vegetation is clearly described in Pages 23 and 24. In addition, it is worth noting that extreme wildfire events can reach consumptions of 100% of aerial biomass and, even, reach the underneath peat (>100%). See for example Maltby et al. 1990. On the other hand, it is worth noting that we took into account carbon redistribution from live to dead pools after fire. We used for this study a typical CC calculated by the own research group in prescribed fires performed at Howden (71.4_2.6% for Calluna and 54.5_2.8% for litter).How this calculations were performed are described in Allen et al. (2013) as we indicate in our manuscript, and we would like to highlight that transferences from live to dead and to the inorganic black

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carbon component was also took into account in these CC.

RC4: I can't see any real/practical management implications authors claim in the study (5, p 20), surely any planning for prescribed burning requires taking into account site specific conditions; authors advise avoid intermediate burning intervals of 20 years for warm/dry sites, but advocate for short 8-10 and long 30-50 years rotational intervals? It's unclear how burning every 10 years will reduce emission compare with burning every 20 years? And what about climate change? In 30-50 years, a cold and wet site can be warm and dry.

AC4: We do not understand what the doubt of referee 1 is. As can be clearly observed in Figure 4, annual C loss follows a hump shaped curve in warm/dry sites, with maximum carbon losses at burning intervals of 20 years. Therefore, in order to reduce carbon losses we suggest that prescribed burning on these sites may be performed at short or long intervals. We think our results have a direct interest for managers and stakeholders when designing prescribed fire management policies. On the other hand, it is true that future scenarios can lead to modifications of climatic conditions in specific sites and, thus, alter patterns of biomass production and C losses. Therefore, considering the simulation approach using different fire scenarios and time included here, we think our paper is interesting and fundamental to understand these processes and the possible consequences. We see this as a strength of our article and not as a weakness.

RC5: How did authors derive Closs PB200 in Table 2? It's higher that available carbon in Fig 2, e.g. Table 2: Kerloch site, C loss PB200=103 t/ha (or tC/ha?), while Litter (ca 20t/ha) + Biomass (ca 23 t/ha) in Fig 2 gives a total of 43 t/ha (at 50 years since burning)?

AC5: As we explain in P17828, L23-29, ClossPB200 makes reference to Carbon losses over a period of 200 years. These losses include prescribed burning and superimposed wildfires at different return interval (50, 100 and 200 years). Therefore, in addition to C

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losses by prescribed burning on these 200 years, we have to include 4 fires where all biomass was burned in the 50 years return interval, 2 in the 100 years, and 1 fire in the 200 years. Carbon losses over 200 years can easily overcome the carbon accumulated after fire showed in Figure 2 because it only refers to the biomass accumulated after fire in the subsequent 50 years.

RC6: The novelty as I see was in the developing biomass and litter accumulation curves. But I can't understand why biomass accumulation patterns did not follow north to south gradient but litter did, with the correlation between the two of R2=0.809?

AC6: We disagree with Referee 1 on this point. Firstly, we think the novelty of this work is not primarily the biomass and litter curves fitted. They are one of the most important parts of the study because the modelling process is based on these curves but, undoubtedly, the novelty of the work relies on the different patterns of carbon losses derived from these curves when different prescribed burning intervals are applied. In fact, these biomass and litter accumulation curves are derived from other studies. Secondly, the Litter-Calluna correlation was performed in base of each 1 x 1 square of biomass sampled in the different sites. Therefore, the above-ground biomass of Calluna of each individual square was highly correlated with the underneath litter accumulation in the same square. A different point is the global patterns of biomass accumulation of Calluna and litter between sites and along time (50 years), where we found variability. We agree with Referee 1 on the interest of the different patterns along the studied gradient between Calluna and litter, in fact, we dedicated a complete paragraph to this point in the Discussion (P17833, L9-26) describing the possible causes and implications.

RC7: Reference Bradstock et al 2012 in 5, page 3 is not appropriate.

AC7: The reference can be removed without alter the meaning of the text.

RC8: It's very frustrating to read throughout the paper that more research is needed (e.g. 5 page 19; 15, page 20) don't we all know this?

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AC8: To our knowledge, one of the main points of the Discussion in scientific papers is to detect the weak parts of the study, as well as to suggest new lines of research for reducing this uncertainty. Although these works are focused on the scientific community, it is possible that researchers highly specialized in ecosystem modelling or management don't have this so clear. In addition, we expect this work to be of interest and to have impact outside of academia, given the recommendations to practitioners we make. In particular, the gaps in the evidence base surrounding the use of prescribed fire need to be highlighted to policymakers and statutory agencies, who at present base their guidelines on thin evidence at best.

RC9: Because authors do not take into account carbon re-distribution, char production, no mentioning of emission factors, apply unrealistic CC of 100%, their recommendations on emission saving benefits are unreliable and I do not recommend this study for publication in BGD.

AC9: We completely disagree with the Referee because, as we explain above, (1) we take into account carbon redistribution, char production and black carbon production; (2) we don't work with emissions factors but carbon losses; (3) CC of 100% are not common but are completely realistic in the ecosystem studied (Maltby et al. 1990); and (4) our results give direct recommendations for designing management plans. Therefore, this study is of great interest.

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