Biogeosciences Discuss., 12, C7888–C7890, 2015
www.biogeosciences-discuss.net/12/C7888/2015/

Biogeosciences 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 #1

Received and published: 24 November 2015

General comments

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/ lit-

ter 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.

Specific comments

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?

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 re-distribution 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.

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.

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)?

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?

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

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?

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.

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