

Interactive comment on “Modelling long-term impacts of mountain pine beetle outbreaks on merchantable biomass, ecosystem carbon, albedo, and radiative forcing” by Jean-Sébastien Landry et al.

Anonymous Referee #2

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In most respects this is an excellent study demonstrating how the magnitude of carbon and albedo radiative forcings from MPB outbreaks depends on a range of site-specific factors, particularly the degree to which non-host trees and lower canopy trees support post-disturbance growth and carbon accumulation. This study has many strengths. The introductory framing is great, outlining the state of knowledge and setting up the present study. The modeling design is excellent, presenting a range of scenarios for post-disturbance forest growth and exploring also the effects of disturbance severity (intensity) and return interval. Radiative forcing (RF) results are presented in a very instructive and useful way, showing both time traces and also time-averages over the

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240 year simulation period, and also providing results on a per hectare basis (with good discussion of scalability). Results on the radiative forcing from MPB outbreaks is compared to a fossil fuel (FF) pulse (or carbon sequestration event) by identifying the corresponding FF CO₂ RF that would be of similar magnitude, providing a very thoughtful and helpful frame of reference. The narrative is fairly open and honest about assumptions and limitations of the modeling assumptions, explaining their likely implications for the study's results (but see recommendations below). The graphics are of high quality and easy to interpret and understand. The writing is clear. The discussion is comprehensive but succinct, including some indications of a management context for the paper's findings. It is rare to find papers that are so well laid out and well thought out, and studies as complete.

I have four main recommendations for how I think the work could be improved, hopefully lending credibility or at least helping readers to understand more of what underlies the results. They revolve around displaying, justifying and/or evaluating the model parameter or structural treatments or the associated output.

1) It would help if you would present the CO₂-only, albedo-only, and combined components of RF for at least some of the cases if not all.

2) Among a number of important findings, this work suggests that MPB-induced albedo RF is much weaker than its CO₂ RF, contrary to some past work, particularly O'Halloran et al.'s study. This seems to be attributed to perceived weaknesses in other studies. However, the present study does not provide a quantitative evaluation of its modeled post-MPB carbon stock changes or albedo changes, nor does it present the data and results that would be needed for others to be able to do so. Furthermore, it would be helpful to have a table or figure that provides quantitative comparisons to works by others. For example, how does this study's delta albedos in absolute units (not percentage changes) compare to those found by O'Halloran et al., Vanderhoof et al., Bright et al and/or other studies. Similarly for NEP, NPP, or carbon stock changes on a per area (e.g. g C m⁻² y⁻¹) basis compared to whatever is in the works of Hicke et

al., Romme et al., Kashian et al., Ghimire et al., Kurz et al. etc. This could be done with tables or figures but either way would provide an opportunity for readers to (a) see more of what is behind this study's results, and (b) have a better sense for how and why its findings differ from those reported previously. There are hints in the discussion but expansion in this way would help.

3) This studies most important finding is that carbon dynamics dominate the RF of MPB outbreaks in this region and that carbon dynamics can vary enormously from a new warming from a reduction in stocks to net cooling from an increase in carbon stocks if sequestration by surviving individuals outpaces that prior to the outbreak. This result hinges very importantly on the model's assumptions about the capacity for the lower canopy, for surviving trees, and for non-host PFTs to experience a release and experience vigorous regeneration and growth following MPB damage. Some of this modeled response is even stimulated by overestimation of heat storage in the model's treatment of dead standing trees (P11, L13), which seems odd. I am fine with the inevitability of a model treatment of such dynamics that is a necessary simplification of reality. However, there is no presentation of the rate of post-disturbance growth for the various PFTs, and thus no way for readers to judge if the model's characterization of this post-D growth is plausible. The paper should show temporal trajectories of stand level biomass and NPP with time since disturbance for each PFT and for each case of the disturbance scenario by climate setting design. Ideally the authors would present data from forest inventory plots alongside this and any data on NPP with time since disturbance to provide a context for evaluation. It would also help if you would do more to explain and demonstrate what is behind the NPP increase that we see post-MPB outbreak for the LC unconstrained and AllPFT cases. Is there any observational or experimental evidence in this region and these forest types to support the model's outcome that stand level NPP is higher in stands that have a mix of PFTs than for NE-only stands, with all else being equal (climate, topographic setting, soil type and fertility)? It also seems like a significant weakness that the model's default PFT growth parameters were used for this study (P12, L16) without adjustment to be representative of produc-

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tivity, allocation, turnover, and growth dynamics in this region's particular forest types. This should be avoided for regional applications, or at least the model's parameters and emergent growth dynamics should be evaluated against data.

4) Since this is a model, it should be possible to unambiguously explain precisely why the results turn out to be what they are, albeit with some additional simple point-scale model experiments. In a few cases key model outcomes remain incompletely understood and each should be documented more fully with further testing or demonstration.

a) It is unclear exactly why the presence of dead standing trees leads to elevated productivity, or at least enhanced carbon uptake, for surviving trees either in the lower canopy or for non-host trees such as the broadleaf deciduous PFT. b) What explains the smaller post-disturbance productivity (and carbon stock) increase and release in the southern compared to central and northern settings remains unclear. The authors conjecture that this is due to soil moisture stress in the southern setting but precipitation is not much lower there and temperatures are not much hotter there (Table 1). Is there observational support for such a strong gradient in productivity and such strong soil moisture limitation in the southern region? c) It is unclear why climate effects are unimportant for the needleleaf evergreen PFT (only) but so pronounced when the lower canopy can respond in an unconstrained way and when all PFTs are present, particularly the broadleaf deciduous PFT. This should be explained with quantitative demonstration.

Other comments:

Application of the model's results with maps of MPB severity, pre-outbreak lodgepole and non-host density, and climate regimes would be a fantastic extension to translate the heuristic model based findings to an estimate of the landscape and region-wide carbon, albedo, and net RF implications of the outbreak. Not for this study of course.

The abstract should have some quantitative results (numbers) for example on the RF for a 1 ha outbreak of a certain kind (severity, model assumption) for CO₂ only and

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combined with albedo RF.

The paper has many abbreviations, some of which are less common. I recommend spelling it out in most cases, particularly for DST, maybe even NE and BD. It does not take much to do so and it is so much easier for readers.

P10, L5: It would help if you put the present result of 818 Tg C over 50 years into terms that are more comparable to those of the Kurz et al. 2008 study by making at least the time frame consistent (20 or 21 years only).

P13, L7: Conclusions: It would be helpful if you also noted that this is equivalent to about 4 years of Canada's emissions. The global context is a bit unfair as a way of judging the importance of a regional episode.

P4, L10+: Either here or in the discussion it might be helpful if you were to compare some of these model treatments/assumptions to what has been done in work by others who sought to model similar dynamics (e.g. Kurz et al., Ghimire et al, Arora et al, Edburg et al.).

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