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Interactive comment on "Quantifying the biophysical climate change mitigation potential of Canada's forest sector" *by* C. E. Smyth et al.

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Dear Editors,

We are pleased to re-submit our manuscript entitled "Quantifying the biophysical climate change mitigation potential of Canada's forest sector" for consideration in the 9th International Carbon Dioxide Conference (ICDC9) special issue of Biogeosciences, Theme 3: Future of the carbon cycle: drivers, vulnerabilities, feedbacks and management options.

Our modelling approach is the only contemporary estimate of mitigation potential for Canada's managed forests based on detailed forest inventory and a range of mitigation activities. We considered seven forest management strategies and two harvested wood

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Interactive Discussion

Discussion Paper

product strategies, projecting harvest and natural disturbances forward to 2050. Our analysis is an important contribution to ongoing efforts to improve understanding of how mitigation strategies involving forest carbon could contribute to reducing emissions of greenhouse gases.

We thank the two anonymous reviewers for their thoughtful comments, and have responded to each of their comments as described below.

Thank-you for considering this manuscript for publication.

Sincerely,

Carolyn Smyth

Anonymous Referee #1

I reviewed the manuscript "Quantifying the biophysical climate change mitigation potential of Canada's forest sector" that I enjoyed reading. The paper presents a modelling analysis applying the CBM-CFS model to assess the mitigation potential of the entire Canadian forestry sector and associated substitution effects. The manuscript is very well structured and presents methods and results in a very clear manner. The discussion addresses all relevant points and clearly describes beneïňĄts and limitation of the method chosen. I would like to suggest some minor changes and corrections and have some suggestion for adding to the discussion.

Response: We thank Referee #1 for their constructive comments. We have incorporated the reviewer's comments in the revised manuscript, as described below.

Page 443, line 10: the reference Nabuurs et al. 2007 is not in the reference list. It appears again in line 18 and 21 on the same page. An alternative citation would be Obersteiner et al. 2010 (see reference list below).

Response: Thank-you for pointing out the Nabuurs reference is missing. We have

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corrected this omission in the revised manuscript. Thank-you also for providing us the Obersteiner et al. reference. We have included this citation in the revised manuscript on P443, L21 of the original manuscript (OM).

- Page 443, line 27: The reference Böttcher et al. 2012 referred to here (paper in GCB Bioenergy) according to the reference list is not appropriate. Böttcher et al. 2012 in CBM (see list below) should be cited instead if the authors want to refer to work on substitution effects in forestry.

Response: Thank-you for pointing out the Böttcher et al. 2012 reference is incorrect. We have updated the citation to refer to the correct article in the revised manuscript.

- Page 444, line 25: the authors do not specify here which pools they look at when they refer to forest management. An explicit list of the carbon pools considered would be helpful at this point. Response: We have added the following text after P445 L24 OM "The CBM-CFS3 tracks C stocks in ten biomass pools (hardwood and softwood versions of merchantable stemwood, foliage, coarse roots, fine roots, and 'other' which includes branches and non-merchantable-sized trees), 11 dead organic matter pools (which include woody litter, the soil organic horizon and mineral soil), and emissions of carbon dioxide (CO2), methane (CH4), carbon monoxide (CO) and an emissions factor for nitrous oxide (N2O) from slashburning and wildfires."

- Page 447, line 6: a prominent energy source in Canada would be oil from tar sands. Can this be considered? What is the magnitude of this source in the overall energy portfolio? Regarding emissions that could be replaced by biomass there should be signiïňĄcant potentials for mitigation.

Response: Yes, a prominent energy source is oil from the tar sands. It is estimated that more than 50 billion cubic meters can be recovered from the oil sands as technology improves (http://www.nrcan.gc.ca/publications/statistics-facts/1239). The oil sands accounted for 7.8% of Canada's GHG emissions (Environment Canada National Inventory Report: 1990-2011). In this study we assumed that bioenergy would displace

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domestic consumption, and there is mitigation potential found in the Boreal Plains ecozone. We did not make reference to any of the many sources of fossil fuel emissions for which forest sector mitigation offsets could be applied.

- Page 447: An overview table of the substitution options and factors would help to provide a better overview of the methodology applied for calculating substitution effects.

Response: Displacement factors have been added to the table in the revised version of the supplementary information.

- Page 448 bottom: is soil carbon included in the assessment? The treatment of harvest residues affects soil carbon and such effects should be considered in options that foresee to remove harvest residues for energy production (see work by Vanhala, Repo et al.)

Response: Yes, soil carbon is included in the assessment, and we account for the reduction in emissions from heterotrophic respiration when residues are used for bioenergy. We assume that removal of residues does not affect tree growth, although we acknowledge that that reduced tree growth could arise over successive rotations.

- Page 464, line 10ff: the authors treat HWP use and bioenergy use as alternatives. If wood from HWPs is recovered and used for energy, the cascade is complete and pressure on residue use could be lowered (see Böttcher ta al. 2012 CBM). Could this be considered in the analysis? At least as a theoretical option the effects could be calculated rather easily, I guess. At least there should be a discussion of the option of recovering wood after its HWP life.

Response: We do recover 10% of retired HWP for bioenergy (P446 L12 OM), and assume that this energy displaces another energy source. In most scenarios the energy produced from retired products is the same as in the BASE CASE and the displaced emissions cancel out. In some scenarios there is a small influence of displaced emissions, and these results are included in Figure 5 OM (Figure 6 in revised manuscript).

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References: - Böttcher H, Freibauer A, Scholz Y, Gitz V, Ciais P, Mund M, Wutzler T, Schulze E- D (2012) Setting priorities for land management to mitigate climate change. Carbon C78Balance and Management, 7:5.

- Obersteiner, M., H. Böttcher and Y. Yamagata (2010) Terrestrial ecosystem management for climate change mitigation. Current Opinion in Environmental Sustainability. 2: 271-276.

- Pekka Vanhala, Anna Repo, Jari Liski (2013) Forest bioenergy at the cost of carbon sequestration? Review Article. Current Opinion in Environmental Sustainability, Volume 5, Issue 1.Interactive comment on Biogeosciences Discuss., 11, 441, 2014. C79

Anonymous Referee #2

This manuscript analyzed carbon mitigation potential of Canada's forest sectors under various management scenarios. The topic is of signiïňĄcance and scenario based results are meaningful, but some problems in logic and structure degrade its overall quality.

Major comments: The title is bit confusing. Forests affect climate through biophysical and biogeochemical processes. Biophysical processes often refer to albedo, evapotranspiration (as in your discussion). . . and biogeochemical processes mainly refer to carbon (and other GHGs). The study is actually dealing with how forest management affects CO2 (biogeochemical effect), so the term "biophysical climate change potential" may cause some misunderstanding (at least from my perspective). I would expect something related to albedo stuffs comes with the current title. My suggestion is to change the title like "Quantifying the climate change mitigation potential of forest management on Canada's forest sector". From the texts, biophysical potential is relative to the technical potential. I think that is what the author actually means here. So "Quanti-

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fying the biophysical potential of carbon mitigation for Canada's forest sector" is also a better choice. Just try not using words like "biophysical climate change potential". That is too big and the current paper is about carbon.

Response: We agree that the terms biophysical and biogeophysical are not consistently defined in the literature. However, the term "biophysical" is used in the context that we use it, including by the IPCC (Nabuurs, 2007), so we prefer to retain this term and distinguish the biophysical mitigation potential from the technical or economic mitigation potential. The first line in the abstract clarifies that we are speaking about biophysical capacity to mitigate GHG emissions (by increasing sinks and reducing sources), rather than mitigation of climate change which as you rightly point out involves wider scope of factors influencing Earth's energy balance that we do not consider in this paper.

In the introduction, the author needs to review related work and give more emphasis on contribution and signiïňĄcance of this research. The current introduction provides very little information on this.

Response: Text providing this information was moved from the Discussion to the Introduction, where you rightly point out (below) that it was mis-placed and better to have in the Introduction.

Methodology part is too long and hard to follow. It should be more concise and better organized. Adding necessary ïňĄgures to illustrate relationship between management strategies, scenario designs and its parameters can make things clearer. Parameters can be listed in a table with their reference and short description but doesn't need to explain all of them in the text. If possible, the methodology should be made shortened and more condensed.

Response: We have condensed the methods and added a new figure (Figure 1 in the revised manuscript) that graphically depicts the strategies.

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Discussion is bit too long. It seems the author want to include many things but this make the discussion lack of focus. Also, the order/logic of the discussion is obscure (perhaps it can be improved by using sub sections). My suggestions are: consistent comparisons with similar work can be moved to strengthen introduction (e.g. P457 L19-), inconsistencies and the reasons are more suitable in discussion; key uncertainties related to results are essential but some technical uncertainties about methodology and design are less important, so it doesn't need to go too much detail; some contents are not directly relevant to the subject and can be shortened; some contents are repeated; the practical implications of different management strategies are very important and should be emphasized (such as effect of strategy portfolio; trade-off between long-term and short term mitigation goal).

Response: Length and logical order issues were addressed by moving some content to strengthen the introduction, removing some text to avoid having too much detail, and by adjusting the order of other text to provide a more logical flow of content and provide greater emphasis on practical implications.

Minor comments: P443 L10: reference mismatch. Is Nabuurs et al., 2007 refers to G. J. Nabuurs et al., in Climate Change 2007: Mitigation, B. Metz et al., Eds. (Cambridge Univ. Press, Cambridge, 2007)?

Response: Thank-you for pointing out the Nabuurs reference is missing. We have corrected this omission in the revised manuscript

P445 L19: How to separate managed forest from natural forest? So natural forest has been excluded from the analysis?

Response: Natural forests are included in the 230 Mha of managed forest. "Managed" for the purpose of GHG accounting includes all forest subject to some form of management including reserves, protected areas, parks, etc..

P448 L4-9 There are several concurrent activities for Better Utilization in the descrip-

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tion, but why not all of them appear in Table 1? For example, stopping burning of harvest residue in situ is not included in the changed parameters. And what are the differences between Residue recovered (

Response: All concurrent activities have been added to Table 1. The description of residue recovered has been clarified to “percent of harvest residues recovered for bioenergy”.

P451 L1: Are there overlaps between F and P at given time for? Because tress would have F before it is harvested, and there is regrowth as well.

Response: Yes, we do account for forest emissions/removals before harvest, as a result of harvest, and also consider the subsequent regrowth. Harvested C removed from the forest is tracked with various commodity half-lives and end-of-use options following IPCC guidelines for HWP C calculations.

P451 Sec 2.5: If any sensitivity tests related to the assumed parameters had been taken? Or if the results are sensitive to the choice of parameter as there are so many assumptions.

Response: We have not considered the sensitivity analyses for parameter values, only changes in fire regimes. These questions are addressed elsewhere, and are beyond the scope of the current manuscript.

When discussing the results, it is better to reference some mitigation goal of Canada or current emission, so the readers can have a better awareness to what the numbers actually means.

Response: Thank-you for this suggestion. We have added the national GHG emissions for 2011 and the 2020 target (P455L2 OM).

P455 L14-15: How to determine the portion of HWP consumption for domestic and foreign markets?

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Response: Production and export of Canada's wood product commodities were based data from the FAO online forest products database, which in turn are based on Canadian statistics.

Figure 5. It is better to use symbols that are more distinguishable. Foreign energy cannot be seen from the chart.

Response: We could switch to a symbol for foreign energy in Figure 5 (Figure 6 in the revised manuscript), but it is so small (15 Tg CO2e), that it is not visible at the scales shown. In our scenarios, we assume that bioenergy is consumed domestically.

Forest also releases other GHGs (N2O, CH4). Are these included in the model? If not, it is better to mention them in the text because they are also important for mitigation.

Response: Thank-you for this suggestion. Yes, we do account for the release of other GHG's in the case of fires. We have mentioned the other gases in the revised manuscript (P445 L24 OM).

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