Biogeosciences Discuss., 10, C1844–C1846, 2013
www.biogeosciences-discuss.net/10/C1844/2013/

Biogeosciences
Discussions
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Interactive comment on "Carbon balance of a partially-harvested mixed conifer forest following mountain pine beetle attack and its comparison to a clearcut" *by* A. Mathys et al.

Anonymous Referee #2

Received and published: 14 May 2013

This manuscript documents aspects of the carbon balance and evapotranspiration from two sites in northern British Columbia representing sub-boreal forest subjected to mountain pine beetle infestation. The study, which lasted two years, examined a clear-cut and a partial cut site. The results demonstrate that partial removal of infested trees is more beneficial in terms of the recovering carbon balance than complete removal of the overstory.

The measurements of fluxes and the basic meteorological variables were well done and carefully described. An open-path infrared gas analyzer (LI-7500, LI-COR) was used for carbon dioxide and vapour fluxes. The authors note that this instrument has proven problematic for winter measurements because of a possible self-heating error.

They did not attempt any sort of correction. Rather they decided to reject all wintertime flux values when the net ecosystem exchange was <0 and the wind speed was <4 metres per second. The descriptions of methods are adequate with a couple of exceptions. There is a rather vague commentary about instrument calibration being performed in the lab prior to field deployment. Very scant or no further explanation is given and so correcting for calibration changes is implied rather than clearly demonstrated. Secondly, there is insufficient reason given for measuring precipitation at a height of 5 m in a canopy opening. I suspect that this height was chosen to allow for sufficient field of view above the gauge, but this is not explained.

The authors correctly point out that increasing air temperature is a primary reason why the mountain pine beetle has been an increasingly serious problem in the pine forests in northern British Columbia. Where this is discussed (page 4930, lines 4 to 6) they note an increase in air temperature from 1895 to 1995 as the leading cause. However, it is the increase in winter air temperature in particular that is critical. It would be more useful to include winter temperature increases in the argument.

The seasonal carbon and evapotranspiration characteristics of the two sites over the two years of measurement are exhaustively described with abundant numerical values of fluxes and good graphical displays. I found only one suspect description: NEP for the clear-cut site (MPB-09C) in June 2010 is given as -20 g C per square meter per month (page 4943, line 2), but the value is more like -15 (see Figure 10).

Table 1 documents the characteristics of the partial harvest site. It is unclear what the stated standard deviation of stand age means. It is more likely to be simply an estimate of the error in the estimate. Also there is no standard deviation in the thickness of the LFH horizon. Does this mean there was only one sample taken? It would be useful to know the number of samples for determination of mineral soil carbon and fine mineral soil bulk density.

It is not clear to me why the site map (Figure 1) designates different patches with

different trail spacing. So far as I can tell the different trail spacing played no role in the argument.

The notion of the trees becoming more windfirm, and thus having fewer trees wind thrown, is interesting (Page 4946, line 29 to page 4947 line 2). It is not clear how “a significant fraction of trees in the stand were located close to the stand edge” and thus became more windfirm. This statement implies a very uneven spatial distribution of trees, something that was not made clear in the discussion.

Editorial Corrections

Page 4930, line 2: change “conditions” to “concentrations” Page 4946, line 3: change “10 yr” to “for 10 yr”

Interactive comment on Biogeosciences Discuss., 10, 4927, 2013.