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Interactive comment on "Moderate forest disturbance as a stringent test for gap and big-leaf models" *by* B. Bond-Lamberty et al.

Anonymous Referee #1

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Bond-Lamberty and co-authors simulated the effects of prescribed moderate disturbance on forest carbon dynamics using three ecosystem models (one biogeochemical model and two gap models). The three models were used to test the proposed hypotheses (e.g., Gough et al. 2013) supporting the possible sustained carbon uptake after the disturbance. This study is interesting and an important step for quantifying the effects of "not stand-replacing" disturbances on forest carbon dynamics. The manuscript was well written. However, I'm not convinced that the model that has poor performance in the control site/pre-treatment site could represent the reality of ecosystem resilience. Especially, Biome-BGC seriously underestimated carbon fluxes (e.g., NEP and NPP) and the biomass accumulation for the control site, which could induce the unexpected model response (e.g., LAI in Fig 2) to the girdling in 2008. I'm hoping to see a better model performance in the control site and/or reasonable arguments in the revision.

Specific comments: 1. The authors presented each model and its parameterization in section 2.3 and 2.4. Although the text is clear, it would be better to have a table showing the differences and similarities among the three models. A parameter table for ED could be useful too. Then I don't need to check the reference and to assume what parameter values being used. To be honest, I don't find parameters for the ED model in Hurtt et al. 2002 and Fisk et al. 2013. I am also not sure if ZELIG include the processes of GPP, Ra, and Rh.

2. In my opinion, it is inappropriate to compare modeling results that driven by different climate data sources. Specially, climate normals were used in ED model simulations. For example, the largest difference in NEP between the control and the treatment sites occurred in warmer 2010 (Fig 4 in Gouph et al, 2013). Losing climate variability could miss the model response to the prescribed disturbance. The "sustained" carbon uptake in the disturbance site could be induced by climate events.

3. Page 11218 Lines 20-23 and Page 11220 Lines 11-24: To my knowledge, most ecosystem models, such as Biom-BGC, ecosys, ED, PNET-CN, TRIPLEX, simulated much better in carbon dynamics for mature forests. Three fourth of models don't simulate stand-replacing disturbed site in NACP site synthesis studies (e.g Schaefer, K et al., 2012).

Schaefer, K., et al. (2012), A model-data comparison of gross primary productivity: Results from the North American Carbon Program site synthesis, J. Geophys. Res., 117, G03010, doi:10.1029/2012JG001960.

Most ecosystem models still have troubles to simulate successional trajectories of carbon fluxes after stand-replacing disturbances such as harvests and fires. Biome-BGC has been tested several times against carbon storage and fluxes in chronosequence eddy covariance sites (e.g., Law et al., 2003 and Bond-Lambert etal 2007), but there is still room for model improvement.

Technique comments:

4. If the authors could organize their text following the same order during the methods and results as possible as they can, it would make the paper easy to understand. For example, I was expecting Biome-BGC rather than ED in the first paragraph of section 2.4.

5. Page 11224 Line 28 to Page 11225 Line 5: Is this a data-model assimilation method? I might miss something. Please identify what the search domain is? From the simulations results, I don't see the expected model performance.

6. Page 11226 Lines 6-10: I assume that the harvested biomass was left on-site for decomposing. Please clarify.

7. Page 11227 Lines 10-15: I'm not sure how to estimate total NPP in this study. The two previous publications (Nave et al. 2011 and Gough et al. 2013) just showed aboveground wood NPP.

8. Page 11228 Lines 21-22: Could the authors please give me a clue why there is no difference in the two carbon pools (leafc_storage and leafc_transfer) before and after the treatment. This could be the reason why annual leaf productions are similar. See my comment 17.

9. Page 11229 Lines 14-17: Peters et al. (2012) said "At long-return intervals (200 years), increasing harvest intensity from a selective to clear-cut resulted in 11 and 10% lower mean NPP in black spruce (Figure 4) and jack pine stands (data not shown), respectively." However, in their figure 4, selective cutting (white bars) resulted in higher mean NPP compared to clearcuts in black spruce stands. I would not recommend the reference.

10. Page 11230 Line 4: the authors may want to say ". . . in their assumptions, parameters, and processes, . . ."

11. Page 11230 Lines 7-11: I am surprised that LAI ("more or less leaf area") doesn't have effects on GPP in the three models. I probably misunderstood. But if it is true,

how the models simulate the forest carbon dynamics? Could light use efficiency be changed or just be constant in the three models? Does nitrogen availability matter in Biome-BGC? How the other two gap models do? A model comparison table might help. See my comment 1.

12. Page 11231 Line 2: I don't see the Appendix 1 in the manuscript. Does it mean Table 1?

13. Page 11231 Lines 8-15: Please clarify the difference between ZELIG and ZELIG-TROP? It helps little in the discussion to compare modeling results from the same model (versions).

14. Table 1: Please change the values of maximum tree height column or the unit in the table note. Tree height of 30 m is more reasonable.

15. Table 3: The authors used the same data source for Biome-BGC and ZELIG, but in table 3 the values are different. Please clarify.

16. Figure 1: Please check the figure. For example, the LAI of treatment site in 2007 don't show in Fig 1 (b). Does the treatment site have the same AGB with the control site in 2007?

17. Figure 2: I found that simulated LAI by Biome-BGC didn't gradually decrease. In the model experiment, the 13-14% biomass removal annually was assumed to represent the prescribed disturbance through 2008-2010. Please explain in more details?

18. If the models could be tested against derived annual GPP, ER (Gough et al. 2013) and soil organic carbon, it might help find why and how NEP changes after the disturbance.

19. Page 11228 Lines 10-14 and Figure 3: it is not clear either in the text or in the figure caption that how to evaluate the model performance. Were all predicted changes for AGB, LAI, NEP, and NPP during the period 2008-2012 used? The sampling size is 20 (= 4 variables * 5 yrs) for each model? The authors may forget the third statistics

(NRMSE?).

20. Figure 4: Please clarify the retranslocated N? Is it leaf N, or absorbed N by plants, or N availability in soil, or N released from dead trees? If possible, please show more related N components? Theoretically, relative N availability after abnormal tree mortality events should be enhanced, as N demand might decrease and N mineralization might be improved.

Interactive comment on Biogeosciences Discuss., 11, 11217, 2014.