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Biogeosciences Discussions
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Interactive comment on "Quantifying legacies of clearcut on carbon fluxes and biomass carbon stock in northern temperate forests" *by* W. Wang et al.

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We have addressed Reviewer 1's comments for the revised manuscript point by point:

1. This study employed a well-established process-based forest model (PnET-CN) to evaluate the effect of clearcut on carbon (C) flux trajectory in two widespread plant functional types (deciduous broadleaf forest and evergreen needleleaf forest) in the upper Midwest region of Wisconsin and Michigan. The trajectory analysis of C flux after clearcut makes this study quite interesting. Results suggested that harvest have a big influence on early stage of forest succession, but only had little effects on late stage. The method used in this study is solid, and results met with expected recovery

trajectory in forest ecosystems. The manuscript is concise and well written, and the topic falls within the scope of BG.

Author response: Thanks for spending time to review the paper and for your constructive comments.

2. However, it is surprised to me that only one scenario was used. I understand that this study was designed to quantify the C flux trajectory following clearcut. Since no ecological model can exactly reproduce the natural system, it is maybe more interesting to compare how forest recovery trajectories vary after different management alternatives. But I realized that this will completely change the objective of this study. And also, PnET-CN may have limited ability to simulate different harvest regimes and forest regeneration, and I will leave this comment to authors for their future exploration.

Author response: We agree that it is interesting and useful to quantify the effects of different management operations (e.g., thinning and selective cutting) on the C fluxes and stocks using ecosystem models. Currently, very few biogeochemical models can directly include these processes. Rather, we focus on harvest intensity to represent partial and clear cutting with a sensitivity analysis. Our analysis shows how forest responds to clearcuts (100%) and two partial cutting (80% and 60% removal of living trees in terms of biomass) (section 3.3), although we are not able test the results of the alternative management scenarios against observations in the region. More field measurements in carbon fluxes and stocks in managed forest sites (e.g., forest thinning: Saunders et al., 2012, Dore et al., 2012) are expected to be useful for model testing in the future.

3. P8791 L23-26: See the latest debates on respiration, GPP, NPP/GPP trajectories during succession (Tang et al., 2014 PNAS).

Author response: Tang et al recently argue that the shaper decline in GPP than respiration might explain the age-related decline in NPP by using a global forest carbon database. We agree that the derived GPP and NPP changes with age, which is similar

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to traditional hypotheses. Therefore, we added the reference in the previous statement (Section 1).

4. The in-situ measurement data reflect real world condition, which was affected by changes in climate, atmospheric composition (e.g., CO2 rising, N deposition), and disturbance, while model simulation only included some of these factors (e.g., N deposition and disturbance). As I understand, climate data was used repeatedly from 1981 –2010. It is not clear how CO2 was parameterized in the PnET-CN. Does this influence your validation results?

Author response: For the period 1959-2010, we used the CO2 concentrations data from Mauna Loa. For the time period 1901–1958, we derived the time series of the historical atmospheric CO2 mixing ratio using a spline fit to the ice-core record of Etheridge et al. (Etheridge et al. 1996), as described by McGuire et al. (McGuire et al. 2001) and used by Xiao et al. (2009). We have described this in the revised manuscript (Section 2.3).

The simulations in this study ended in 2010, and the monthly CO2 concentrations are shown in the following figure (Fig. R1). CO2 fertilization has little effects on our testing sites during our simulation period. We did a sensitivity test for CO2 fertilization (see Supplementary figure S3 added in the revision). There was no noteworthy change in simulated trajectories in carbon fluxes and stocks.

5. For the sensitivity analysis, were dead wood removal fraction and soil removal fraction also changed, and how? Soil removal fraction may have a big influence (e.g., Peters et al., 2013, Ecosystems) on C flux and how these parameters was set deserved to be explained.

Author response: The other disturbance parameters (i.e., wood removal fraction, soil removal fraction) were kept unchanged in the sensitivity analysis. We only focused on the harvesting parameter (stand mortality) in this paper (Fig. 8). Harvested wood removal fraction was parameterized according to expert's experience, ranging from

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60%-90%.

We agree that soil carbon removal influence soil C fluxes and secondary forest growth. But several meta-analyses (Johnson and Curtis, 2001; Yanai et al., 2003; Nave et al., 2010) have showed that no significant change in soil carbon pools after harvest operations. Furthermore, the sensitivity analysis conducted by Peters et al, 2013 has shown the model sensitivity to soil removal. We have discussed this in the revision (Section 2.4, 3.3, 4.1 and 4.2). We have now added an analysis of sensitivity to soil removal fraction.

6. It is not clear to me how CO2 concentration trend was parameterized in the model?

Author response: We added sentences to describe the CO2 concentration data used in this study (Section 2.3). We also added a figure (Fig. S3) to show the model sensitivity to CO2 fertilization in the supplementary material.

7. Figure 3: Do you have validation results for NEP, GPP, and ER?

Author response: The figures 1 and 2 have shown the validation results for carbon fluxes. Since the model has been applied for major forest types in Midwestern USA (Peters et al., 2013; Ryu et al., 2012), most parameters were left unchanged in this study to facilitate the generalization (see Table S1).

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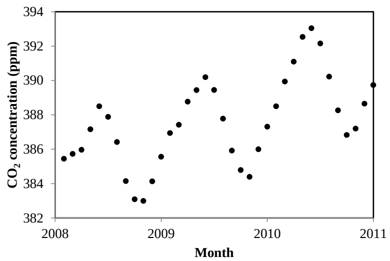

Fig R1. Monthly air CO_2 concentration measured in Mauna Loa, Hawaii, during 2008-2010.

Fig. 1.

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