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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.

Anonymous Referee #1

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General comments:

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.

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.

Some specific comments:

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

(2) 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?

(3) 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.

(4) It is not clear to me how CO2 concentration trend was parameterized in the model?

(5) Figure 3: Do you have validation results for NEP, GPP, and ER?

Interactive comment on Biogeosciences Discuss., 11, 8789, 2014.