

***Interactive comment on* “The impact of climate variation and disturbances on the carbon balance of forests in Hokkaido, Japan” by R. Hirata et al.**

R. Hirata et al.

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Received and published: 16 July 2014

Thank you for your comments. We hope that our responses and the resulting revisions will be satisfactory, but we will be happy to work with you further to resolve any remaining issues.

General comment

General comment 1:

Vague and subjective sentences are shown throughout the manuscript. Authors must use concrete representation. For example, when two results are compared, authors must specify not only which one was greater (or less), but also how much two results differed. If two results are regarded to be same, authors must show statistics. In terms

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of this point, authors must correct all vague and subjective sentences (e.g., p.2848 – l.14-15, p.2853, l.28 (gradually??), p.2856 –l.19-20, p.2858 –l.15-17, and p.2856 l.25 (how successfully?? use statistics!)). Add statistics (e.g., R2, p, and n) for concrete statements.

Reply:

We have added statistics for the comparison between simulation and observation in Table 3, together with a relevant explanation.

Following the reviewer's comments, we revised the sentences as follows:

p.2848 – l.14-15

We have added average values of R2 in Table 3.

p.2853, l.28

We have revised “gradually” to “linearly”. We now clearly show, by using values and years, how the CO2 concentration increased (P6L31 in the revised manuscript).

p.2856 –l.19-20

We have removed this sentence. In place of it, we now describe the comparison between simulation and observation in more detail by using the statistics in Table 3.

p.2858 –l.15-17

We now refer to Figure 4 in this sentence and provide values.

p.2856 l.25

To support our use of “successfully”, we have added many statistics (Table 3) and explanations, in accordance with the reviewer's suggestion.

General comment 2:

Authors used jargon without definition. Many statements are vague, resulting in that

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it is difficult to understand what authors mentioned. Authors need to use concrete and consistent terminology. E.g.; amplitude of NEP (p.2858 I.2; what timescale?), long- term trend (p. 2858 I.3; how defined long-term?), CO2 absorption (p.2858 1.11; GPP or NEP?), growth rate (p.2858 1.6; GPP or NEP?), several decade (p.2858 I.12; be concrete), interannual variation (p.2859 I.1; standard deviation of annual fluxes?), effect of CO2 (p.2859 I.2; to mean or variation?), productivity (p. 2859 I.2; GPP or NEP?), respiration (p. 2859 I.2; RE, RM, or RH??),. . . . ; Do not use ambiguous jargons.

Reply:

We have revised the sentences as follows:

p.2858 I.2

We have changed “amplitude” to “year-to-year variation”.

p. 2858 I.3

We have changed “long-term” to “52-year”.

We have also defined “long-term” as 52 years from 1958 to 2010.

p.2858 1.11

We have changed “CO2 absorption by plants” to “GPP (or NPP)”.

p.2858 1.6

We have changed “growth rate” to “GPP”.

p.2858 I.12

We have changed “several decades” to “52 years”.

p.2859 I.1

We have changed “interannual variation” to “standard deviation of NEP, which is an

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indicator of interannual variation in NEP”.

p.2859 I.2

We have changed “The effects of CO₂ on productivity” to “The effects of CO₂ on annual productivity”.

p. 2859 I.2

We have changed “productivity” to “productivity (NEP and GPP)”.

p. 2859 I.2

We have changed “respiration” to “respiration (RE and RH)”.

Specific comments

Specific comment 1:

p. 2848, I. 2: Climate condition in these sites is not boreal. Reply:

We have changed “temperate boreal” to “cool temperate”.

Specific comment 2:

p.2853, I.16-26. The spin-up must be done with historical climate and CO₂ concentration instead of recent data. Using recent data should bias modeled state of biogeochemical cycles, such as response to climate and CO₂.

Reply:

In response to the reviewer’s comment, we have now used long-term data (1948–2010) for the spin-up, and we have recalculated the simulated carbon fluxes in the mixed forest and young and middle-aged larch forests. Although Figures 2 and 3 and the relevant statistics have been changed slightly, there are no dramatic changes to the results. We have also changed the relevant sentences, as follows:

“The model was initialized by a spin-up run for 2000 years to create the dynamic equi-

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librium of soil organic matter and vegetation components using the reanalysis climate data corrected by in situ observational data from 1948 to 2010 for the Teshio and Tomakomai sites. CO₂ concentrations were constant at 312 ppm until 1948. After the spin-up run, a simulation was conducted for the observation period (11-year for Teshio and 3-year for Tomakomai sites) and for the long-term period for Tomakomai. The annual atmospheric CO₂ concentration was linearly increased from 312 ppm in 1948 to 404 ppm in 2012 for Teshio, from 312 ppm in 1948 to 380 ppm in 2003 for Tomakomai, and from 312 ppm in 1948 to 398 ppm in 2010 for 52-year simulation for Tomakomai.”

Specific comment 3:

Section 2.3: There is no description how authors treated disturbances in the model. Detailed descriptions about transferring carbon/nitrogen pools in each pool (overstory/understory leaf, wood, and root, litter, and soil pools) are necessary. How authors defined residue? If authors included coarse root as residue, then taking 100% is unrealistic (section 2.6). How clear cutting and conversion differed in the model application? Details in the disturbances treatments are necessary.

Reply:

We have added a description of the carbon flow when disturbances occurred, as follows:

“In the model, live biomass of foliage, stems, and roots is reduced to zero by clear-cutting, and residues are treated as carbon in litter compartments that are moved from the live biomass. Therefore, 55% of the live stem biomass was removed and treated as emission, and 45% of the live biomass was moved to the stem litter compartments. All live foliage and roots were moved to the foliage litter compartment and the root litter compartment, respectively. When a conversion event occurred, the parameters of the mixed forest simultaneously changed to those of larch forest.”

As the reviewer indicates, the treatment of foliage and roots was not realistic for the

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sensitivity test. However, we wanted to treat the emissions and residues simply. We now note this unrealistic assumption in the text, as follows: “As noted in section 2.3, only stems were removed and all foliage and roots remained on and in the forest floor, respectively, when clear-cutting was conducted. To simplify the test and the results, emission of tree biomass included foliage, stems, and roots. although this was unrealistic because only stem is exported and most of foliage and stems remain on the forest floor in the actual forest management as mentioned in section 2.3.”

Specific comment 4:

p.2854, l.20-23. I cannot understand what authors mentioned.

Reply:

We have revised the sentence as follows:

“We compared carbon fluxes calculated by using historical climate data with constant scenarios that had no interannual variation in each climate factor. For each scenario, a series of individual factors was replaced by an ensemble average for 1948–2010 and the same seasonal variation was retained during the simulation period. Constant scenarios started in 1958, when clear-cutting occurred and the plantation was initiated.”

Specific comment 5:

p.2855, l28. Sconst-precipitation would be better than Sconst-rain.

Reply:

We have revised the subscript as suggested by the reviewer.

Specific comment 6:

p.2855. add “respectively”.

Reply:

We have revised the sentence as suggested by the reviewer.

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Specific comment 7:

Section 2.6. Add details in transferring each pools as suggested in above comments.

Reply:

In response to the reviewer's comment we have added an explanation about the transfer of each pool, as follows:

“As noted in section 2.3, only stems were removed and all foliage and roots remained on and in the forest floor, respectively, when clear-cutting was conducted. To simplify the test and the results, emission of tree biomass included foliage, stems, and roots. although this was unrealistic because only stem is exported and most of foliage and stems remain on the forest floor in the actual forest management as mentioned in section 2.3.”

Specific comment 8:

Section 3.1. Add statistics for validating other timescale, such as annual and successional timescales, in addition to statistics from daily mean.

Reply:

In response to the reviewer's comment, we have added statistics for the daily, monthly, and yearly scales (Table 3). However, it was impossible to do this for a successional time scale, because we had only 1, 3, and 10 years of data for the three forest types. Instead, we have added statistics for average values for the whole observation period (Table 4).

We have added an explanation as follows:

“Table 3 shows statistics for comparisons of carbon fluxes calculated from all three forest types—the mature mixed forest (2002 in Teshio), young larch forest (2003–2012 in Teshio), and middle-aged larch forest (2001–2003 in Tomakomai)—with those estimated from field observations on daily, monthly, and yearly scales. However, in the

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case of yearly statistics, only NEP and GPP are shown for the young larch forest, because too few data were available or there were no significant relationships for other fluxes. Although daily statistics showed some biases (NEP of the three forest types, RE of mature mixed forest, for which the slopes were less than 0.8; Table 3) or weak relationships between simulated and observed carbon fluxes (NEP of the mature mixed forest and the young larch forest, for which the R^2 values were less than 0.5; Table 3), most of the monthly model output of carbon fluxes agreed well with observed values (the slopes were over 0.8 or less than 1.2 and the R^2 values were over 0.5; Table 3, Fig. 3). In this model, using monthly values appeared to be most suitable for evaluating seasonal variations in carbon fluxes, rather than using daily values. On the whole, the high correlation of the monthly data indicated that the model successfully simulated the seasonal patterns of observed NEP, GPP, and RE. On the yearly scale, model outputs of annual NEP and GPP of the young larch forest matched the observed values well (R^2 values were 0.82 and 0.62 for NEP and GPP, respectively; Table 3), although there was some bias in NEP (slope was 1.51 for NEP; Table 3). In contrast, we did not obtain significant correlations of annual NEP, GPP, or RE for mature mixed forest and middle-aged larch forest or for RE of young larch forest between the model estimates and the observations, because the differences among annual values were small (Table 4). However, the average of each annual carbon flux estimated by the model was similar to the observed value (Table 4). Overall, the model output was consistent with the observed fluxes from the seasonal to the yearly scale. We therefore used the model to evaluate the long-term effects of disturbance on carbon fluxes.”

In addition, we have added relevant discussion.

“The correlations of daily and monthly NEP were lower than those of GPP and RE (Table 3), as reported in previous studies (Ichii et al., 2010; Ueyama et al., 2010; Ichii et al., 2013). NEP represents the small difference between large gross fluxes such as GPP and RE. Therefore, even a small variation in GPP and RE can have a large effect on the resultant NEP, causing low correlation of NEP. The low correlation of monthly

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NEP ($R^2=0.42$) and small slope for monthly RE (0.77) of the mature mixed forest might be attributable to the limitations imposed by a 1-year dataset. Therefore, in the scenario test (Fig. 4, Table 5), there might have been larger uncertainty in NEP than in GPP or RE, and the carbon fluxes of $S_{\text{non-conv}}$ might have included larger uncertainty than in other scenarios.”

Specific comment 9:

p. 2858, l.20. Why thinning decreased RH. Thinning seems to increase litter. Again, description in transferring each pool is necessary in the method section.

Reply:

We have added an explanation as follows:

“The reason of decreased RH is that soil carbon of S_{full} was smaller than that of $S_{\text{non-thin}}$. In the model, when thinning occurred, soil carbon increased by the moving of root biomass to the litter compartment. However, root litter is easily decomposable (decomposition rate 1 year after thinning were 36, 25, and 28% in 1970, 1985 and 2004, respectively). In addition, litter supply from live trees decreased by thinning. Therefore, soil carbon of S_{full} was smaller than that of $S_{\text{non-thin}}$ 3-year (1985) or 1-year (2004) after thinning”

Specific comment 10: p. 2859, l.5-9. I cannot understand what authors mentioned.

Reply:

We have revised the sentences as follows:

“The ecosystem shifted from a net carbon source to a sink 6 years (ECPNEP) after clear-cutting (Fig. 4a). It took 15 years for cumulative NEP to shift from negative to positive after the disturbance (i.e., ECPCNEP). It took 23 years for cumulative NECB (subtracting emission as logs from NEB) to shift from negative to positive after the disturbance (ECPCNECB).”

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Specific comment 11:

Discussion section. Authors review previous studies showing similar conclusion. But, I cannot understand how current results are new after the previous studies, and why results are same or different. Further careful discussion is necessary.

Reply:

We have added some discussions as follows.

“Whereas Janisch and Harmon (2002) and Thornton et al. (2002) simply examined the effect of disturbance without taking climate variations into account, we simulated carbon balances while accounting for both disturbance and climate variations. Despite the difference in simulation design and the type of model, the results of all three studies suggest the importance of residues to the carbon balance of forest ecosystems.”

“This could be one of the reasons why our results differed, and it suggests that treatment of residues affects carbon flux after thinning.”

“Both GPP and RE increase with increase in temperature. When increase in GPP exceeds increased in RE, NEP increases, and vice versa. Increased temperature affects many ecosystem processes (e.g., photosynthesis, plant respiration, soil respiration, biomass, phenology), and the sensitivities of all of these differ among plant species, soil types, and climate zones (Luo, 2007). Therefore, it is quite difficult to find a consistent pattern in the response to increased temperature (Luo, 2007), and further case studies should be conducted.”

“Although it is well known that drought affects soil respiration (Borken et al., 2006; Koppitke et al., 2013; Wang et al., 2014), reduced RH was not detected by our simulation under severe drought conditions. At the Tomakomai site, the relationship between soil respiration and soil water on the basis of chamber measurements was not clear (Liang et al., 2010). A much longer observation period is needed for detecting signals when severe drought occurs. Data based on a longer observation period are also useful for

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validating the VISIT model.”

“Yi et al. (2013) focused on the regional scale (boreal and arctic area), whereas we focused on the point scale in a cool temperate forest. The effect of disturbance depends on the area and strength of disturbance, because various types of disturbance occurred heterogeneously at the regional scale. The effect of climate might also differ among regions. In future research, we need to evaluate the spatial effect of disturbance and climate by gathering information on disturbance in Hokkaido or Japan.”

In addition, we have rewritten section 4.2. We have moved the passage beginning “By applying” in P2862L15–24 (original manuscript) to the end of this section. To clarify the effects of temperature and precipitation, we have added new text regarding the effect of temperature (P15L11 in the revised manuscript) and precipitation (P15L21 in the revised manuscript), in separate paragraphs.

Specific comment 12:

p.2862. l.8. Use “suggested” rather than “supported”.

Reply:

We have revised the sentence as suggested by the reviewer.

Specific comment 13:

p.2862. l.10. The current analysis cannot discuss effect of global warming, because effect of global warming would differ in season. Please discard the sentences.

Reply:

In response to the reviewer’s comment we have deleted this sentence. In place of it, we have added the following text:

“Both GPP and RE increase with increase in temperature. When increase in GPP exceeds increased in RE, NEP increases, and vice versa. Increased temperature affects

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many ecosystem processes (e.g., photosynthesis, plant respiration, soil respiration, biomass, phenology), and the sensitivities of all of these differ among plant species, soil types, and climate zones (Luo, 2007). Therefore, it is quite difficult to find a consistent pattern in the response to increased temperature (Luo, 2007), and further case studies should be conducted.”

Specific comment 14:

p.2862. l.22-23. Please specify what timescale they mentioned.

Reply:

We have added “from 2000 to 2010”.

Specific comment 15: p.2863, l.8. Use “suggested” rather than “indicated”, because validation about drought effect was not conducted in this study.

Reply:

We have revised the sentence as suggested by the reviewer.

Specific comment 16:

p.2863, l.24. Show definition of photosynthetic capacity, and how much photosynthetic capacity is greater than other trees.

Reply:

We have added an explanation, as follows:

“The maximum GPP at light saturation at the TMK site was about $45 \mu\text{mol m}^{-2} \text{s}^{-1}$; this was higher than in a larch forest in Siberia (about $5 \mu\text{mol m}^{-2} \text{s}^{-1}$), an evergreen needle-leaf forest in Japan (about $20 \mu\text{mol m}^{-2} \text{s}^{-1}$), and an evergreen broad-leaf forest in Thailand (about $35 \mu\text{mol m}^{-2} \text{s}^{-1}$) (Hirata et al., 2008).”

Specific comment 17:

p.2863, l.24-25. Quantify the role of understory. Model would easily simulate the role of understory.

Reply:

We have added the following explanation:

“According to the long-term simulation of the TMK site (Fig. 4), the GPP of the understory accounted for 75% of the total GPP in the first decade after disturbance (13 and 10 t C ha⁻¹ year⁻¹ for total GPP and understory GPP, respectively).”

Specific comment 18:

p.2864, l.16-24. some studies (e.g., Gough et al., 2007, Latty et al., 2004) suggested that repeated disturbance reduced carbon storage because disturbances reduced site quality. Please discuss this point in terms of modeled nitrogen cycle.

Reply:

Repeated disturbance reduced nitrogen in the ecosystem. Therefore, the recovery period may be long when disturbances are repeated several times, because NPP might become smaller after repeated disturbance.

Our sites are located in Hokkaido, where people began to inhabit this area only about 130 years ago. Therefore, we assumed that repeated disturbance had not occurred.

However, this point is not mentioned in the revised manuscript because the other reviewer suggested that this part be removed.

Specific comment 19:

Conclusion section. Authors only mentioned the residue effect in conclusion, but other results, such as role of disturbance and climate, were excluded in conclusion. Restructuring is necessary throughout the manuscript.

Reply: In response to the reviewer's comment, we now describe the effects of distur-

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bance and climate and have restructured the Conclusion as follows.

“We evaluated the long-term effects of disturbances and climate on the carbon balance of forest ecosystems in a cool temperate region by using the process-based model VISIT. Clear-cutting strongly affected the carbon balance, not only just after clear-cutting but also 52 years after the disturbance. The effect of clear-cutting was larger than that of climate, even 52 years later. Disturbance controlled the long-term trend in carbon balance, whereas climate factors controlled the yearly variation. Among the climate factors, increased temperature and severe drought played vital roles in interannual variation of the carbon balance. The three ecosystem compensation points are useful indices for evaluating the recovery of a forest ecosystem from disturbance with respect to the carbon balance.”

Specific comment 12:

p.2865, l.22. Use overstory instead of canopy.

Reply:

We have revised the sentence as suggested by the reviewer.

Specific comment 13:

p.2869. 2869. As suggested by other reviewer, details in each parameter (e.g., references or how parameters were optimized) are necessary. Add these information in Table A1.

Reply:

In previous studies (Ito, 2008; Ichii et al., 2010, 2013), most of the parameters were determined manually by using the trial-and-error method. We have added an explanation.

Interactive comment on Biogeosciences Discuss., 11, 2847, 2014.

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