Response to Interactive comment on "Carbon and greenhouse gas balances in an age-sequence of temperate pine plantations" by M. Peichl et al.

We thank the reviewer for the constructive comments. Our responses to each of the comment follow below in bold font. Page and line numbers in our responses refer to the MS Word .doc/pdf version of the resubmitted manuscript.

Anonymous Referee #1

The work of M. Peichl and co-authors compiles results from intensive research on ecosystem C cycling and GHG efflux from a pine plantation age-sequence. The intensity of studied parameters (biometric, eddy flux, GHG fluxes. . ..) at the four sites is more than impressive and together, the unique dataset was used to get more detailed insights in the stand age effects on C cycling and the potential role of non-CO2 GHG in the overall GHG balance. On first sight, the fact that younger forest stands grew under more favorable site conditions than older stands might be seen as a limitation. However authors found a way to cope with this and finally came up with conclusions about age and site quality effects on C cycling and the GHG balance. After some modifications, the study should be published in Biogeosciences.

General comments:
Global warming potentials (GWP) for CH4 and N2O are not up to date. Please use the recent IPCC values (28 or 34 for CH4; 265 or 298 for N2O) in the text and for all calculations. You find the values at:
http://www.climatechange2013.org/images/report/WG1AR5_Chapter08_FINAL.pdf
We have recomputed the GWP of CH_4 with the new value of 34 (including climate-carbon feedbacks), respectively, and corrected the text in the manuscript accordingly (Introduction Page 3 Ln 30 and Section 2.4. Page 9 Ln 2). We had already used the new value of 298 for N_2O in the previous version of the manuscript. However, despite the very small changes in GWP of CH_4, the new total GWP values are different only for the second or third digits which after rounding to the precisions presented in this manuscript did not change the presented GWP values. Thus, the recomputed GWP values did not affect our main findings. We also cite now the newest IPCC 2013 report in the revised manuscript.

I would avoid the term "cooling effect" throughout the manuscript. You measured uptake of CO2 or C sequestration. Cooling was actually not directly assessed as for instance the effects of changing albedo from agricultural land to forest were not considered; and so forth. . .
We have incorporated the reviewer's suggestion and removed the term 'cooling effect' from the manuscript.

I have real problems to understand how the fine root respiration (RA) can be considered as more or less the same (quantitatively) throughout the age sequence (P8238 and Fig 3) if the fine root mass (F) is 15 times higher in the old stand than in the young stand (or even 30 times higher in the middle aged stand.
There could be a few reasons for the apparent mismatch between the patterns of fine root biomass and root respiration across the age-sequence as expected by the reviewer. The first one might result from the fact that fine root biomass was determined in 2004 (Peichl and

Arain, 2006), at which time seedling trees at the young site (TP02) were only 2 years old. In the subsequent years (2004-2008), during which root respiration was estimated, the seedling trees grew vigorously (about up to 70 to 100 cm height growth each year). In addition, a dense cover of herbs and weeds established during these subsequent years at this site. Thus, the fine root biomass was likely considerably higher during the years which contributed to root respiration measurements at the youngest site. Another reason could be the different contribution from tree and herb/understorey grass fine root contribution at the four sites which might further mask the root respiration pattern that one might expect from the patterns of tree size and root biomass, since herbaceous fine roots might respire differently (e.g. faster) per gram biomass compared to fine roots from older trees. Finally, root respiration is not only the result of fine root biomass, but also affected by aboveground photosynthesis and subsequently the transport of assimilates to the roots. Since the younger sites were more productive than the older sites, this might have further masked a clear root biomass –root respiration relationship. We have included these reasons in the discussion of the revised manuscript (Section 4.1. Page 12 Ln 2-10).

Presentation and discussion of C cycling is well done but the N2O and CH4 chapter might profit from a bit more detailed literature survey. For instance, you claim that N2O emissions during the cold season can be neglected as they don't add much to the annual budget. However, actually, cold season N2O emission peaks were often observed to account for major parts of the annual N2O efflux from forest soil. E.g.: http://www.biogeosciences.net/9/1741/2012/bg-9-1741-2012.pdf.
We are aware that freeze-thaw cycles can result in high N_2O emissions during the non-growing season in the temperate climate region, however, at the Canadian forest chronosequence sites presented in this study, cold temperatures and a substantial snow cover remain during the entire winter from about December until the end of March. Thus, the number of freeze-thaw cycle are fewer and resulting N_2O emissions most likely less frequent at our study sites as compared to the German Höglwald site (in the study of Luo et al. 2012 noted by the reviewer) and other similar sites. Nevertheless, we have included this reference and one more study (Teepe et al. 2001) suggesting the potential for considerable winter emissions. We have clarified in the revised manuscript that freeze-thaw events are likely less frequent at our study sites (Section 4.2. Page 14 Ln 12-20).

Also more literature survey regarding the effects of site quality on NEP would be helpful – for instance http://www.nature.com/nclimate/journal/v4/n6/full/nclimate2177.html
We had already included the reference to this article by Fernández-Martínez et al. (2014) in the previous version of this manuscript (Introduction Page 4 Ln10-11). In the revised manuscript, we have also cited the study by Vicca et al. (2012) which discusses site fertility effects on forest growth and C allocation (Section 4.1. Page 13 Ln 24-25).

There might also be more literature regarding the contribution of N2O and CH4 to the complete GHG balance as the few ones cited.
We have added some more references including a review on N effects (Liu and Greaver, 2009) and modelling studies of management and global change effects (Metsaranta et al, 2011; Ximenes et al. 2012) on the GHG balance in forests (Section 4.1 Page 14 Ln 12-20). To the best of our knowledge, however, we could not find any further suitable articles in

the literature presenting measured data on the contribution of N_2O and CH_4 to the complete GHG balance specifically on the whole-stand level for temperate forest ecosystems.

Specific comments:
Abstract L9-10: "The total ecosystem C pool increased with age from 9 to 160 t ha-1. . ..." – there is something wrong with this numbers. Even the soil has approx 30 tha-1 in all four age classes (Fig 3). Please check.
We accidently presented the numbers for vegetation C pool only. We have corrected these values by adding the soil C to present the total ecosystem C pools in the abstract and result section.

Introduction:
P8230 L11-12: Soil C stocks do not tell much about the current C sink potential. . ..
We reworded this sentence.

P8232 L22 onwards: here, more literature, e.g. from the Höglwald experiment could be cited
We have included the suggested references for the Höglwald experiment (Luo et al., 2012).

Methods:
P8236: N2O cold season issue.
As described above, at our Canadian chronosequence sites, a substantial snow cover remains on the ground during the entire winter from about December until the end of March. Thus, the number of freeze-thaw cycle is much lower and resulting N_2O emissions most likely less frequent at our study sites as compared to other temperate sites with disrupted snow cover and winter temperature crossing freeze/thaw temperature thresholds. We clarify this aspect in the revised manuscript (Section 4.2. Page 14 Ln 12-20).

Results:
P8238: fine root issue. . ..
As described in detail above, there could be a few reasons for the apparent mismatch between the patterns of fine root biomass and root respiration across the age-sequence as expected by the reviewer. We list these in the revised manuscript (Section 4.1. Page 12 Ln 2-10).

Discussion:
P8240 first chapter: You are talking only about needle litter decomposition? Here however it seems you talk about litter decomposition in general which would include root litter – please clarify.
We mean needle litter and have clarified this in the revised manuscript.

P8242 chapter 4.2: The first part of this chapter is mentioned in the intro already. The chapter could be improved a bit by more literature research.
It is correct that here in this discussion part we refer again to some of the few studies available on the forest stand level GHG balance that are mentioned in the introduction

part. However, in contrast to the Introduction section, we use these studies here to compare our own findings with their results in the Discussion section. We therefore don't see a redundant overlap here. Nevertheless we have surveyed the literature once more and added few more references (Liu and Greaver, 2009; Metsaranta et al., 2011; Ximenes et al., 2012) to this part of the discussion. With these additional references we now list 76 references in the revised manuscript which we believe is a sufficient review of the literature.

Tables and figures are nice (I must confess, I did not check all the numbers in Fig 3)
We have double checked the numbers in Fig 3.