Dear Editor,

Thanks so much for sending us two reviews on our manuscript "**The effects of carbon turnover time on terrestrial ecosystem carbon storage**" (ID: bg-2017-183). We are very grateful to the reviewers for their constructive comments and suggestions. Their inputs have helped to improve the paper significantly. We have carefully studied the reviews, and revised our manuscript accordingly.

Here are our detailed responses to the reviews. Please note that the comments are in ***italics*** followed by our responses in **regular** text. In addition, we marked the changes or revision with the **red text** in the whole manuscript.

Yours Sincerely,

Xuhui

Xuhui Zhou
School of Ecological and Environmental Sciences,
East China Normal University,
500 Dongchuan Road, Shanghai 200062, China
E-mail: xhzhou@des.ecnu.edu.cn

Response letter to comments (ID: bg-2017-183)

Referee #1
General comments：

First, what exactly is the advance of this study over Carvalhais et al. (2014)? This wasn't clear to me.

[Response] Thanks so much for your comments. In *Carvalhais et al. (2014)*, global C turnover times and its covariation with climate were mainly examined. They also compared global C turnover time calculated by the model results from CIMP5 with those from observed data and showed their trend over latitude. Based on their work, we focused on the uncertainty from different observed data (HWSD vs. NCSCD), especially in high latitude. Litter data was updated compared to the study of *Carvalhais et al. (2014)*. More importantly, we examined ecosystem C storage over time from changes in C turnover time and/or NPP. In addition, we estimated the GPP-based the NPP-based and soil MTT to explore the difference among them. Therefore, our study advance the understanding of the uncertainty of global C turnover time (especially in high latitude) and ecosystem C storage from C turnover time with updated data. We revised the abstract and discussion to make it more clear for the advance in Lines 18~21, 309~318.

Second, this analysis mixes (I believe) spatial and temporal trends, assuming that they're equivalent, but this assumption is never explored or even really discussed.

[Response] Thanks for your suggestions. We assumed that the spatial correlation between temperature and MTT is identical to the temporal correlation between these variables. We added this assumption in Lines 207-208.

The steady-state assumption is also troubling. I understand why it may be necessary at a global scale, but the authors should at least estimate how much bias this might be introducing. For example, there are gridded disturbance and forest age maps available that could be incorporated into such a calculation.

[Response] Thanks so much for your comments. For an ecosystem, a steady state is defined as GPP equals total ecosystem respiration at a reasonable period of time and there is no net change in total standing crop of living and dead biomass. However, maintaining a steady state without change is rare for a long time.

As we know, disturbance and forest age will influence large-scale accumulation of biomass, the partitioning of C into pools with different turnover times, and thereby long-term C sequestration and turnover times. In the past decades, most of previous studies have considered the age-related decline in forest growth and simulated the current-age C flux to some degree (Sönke et al., 2006), which were involved in the gridded data. Therefore, the gridded disturbance and forest age maps can be used to simulate the current-age ecosystem turnover time using models to compare our results, although it has large uncertainty. However, the specific effects of disturbance and forest age on ecosystem turnover time are difficult to be examined, which was beyond our study. We thus added the discussion of the age effects on ecosystem turnover time in the discussion section (Lines 444-448).

The lack of any clear data availability statement is unacceptable. It's 2017, and I expect all code and data to be included as supplementary info, or (better) posted in a repository. It's not acceptable to produce results from a black box.

[Response] The original data including MOD 17, HWSD, vegetation C production of Gibbs et al (2006) and litter dataset from Holland et al., 2005, is open and shared. We provided the citations for data sources.

The figures should be improved. See comments below.

[Response] See the response as below.

Finally, while I appreciate the difficulties of writing in a foreign language, the current manuscript is riddled with spelling and grammar mistakes. This is doubly frustrating as I know that the senior author, at least, is fluent in English.

[Response] We carefully revised the manuscript, especially for the language editing. Meanwhile, we asked a native speaker: Shahla Hosseini Bai to carefully revise the manuscript. Hope our manuscript has been considerably improved.

Specific comments:

Line 24: Why "Thus"? Doesn't seem to be logically connected

L. 28: "difference"

[Response] Done as suggested.

L. 47: "validated" probably not the best word to use here

[Response] We revised "validated" to "evaluated".

L. 52: "amount of"

[Response] Done as suggested.

L. 62-63: Carvalhais et al. (2014) seems like a needed citation here

[Response] Done as suggested. We added the citation Carvalhais *et al* (2014) in Line 84-88. (Carvalhais et al. (2014) have estimated ecosystem turnover time as the ratio of carbon storage (soil and vegetation C) and influxes and the correlation to climate, which focused on the comparison of global C turnover time calculated by the model results from CIMP5 with those from observed data as well as their trend over latitude.)

L. 86-87: unclear

[Response] Done as suggested.

L. 90: this language is used frequently in the ms. Is ecosystem C storage really "driven" by MRT? I would say that MTT is an emergent property of changes in fluxes; it can't "drive" anything

[Response] Thanks for your comments. The ecosystem C storage is codetermined by C influx and C turnover time. For example, reduced soil C turnover time resulted in the insignificant net effect of increased atmospheric CO_2 on the equilibrium soil carbon storage (van Groenigen et al., 2014). Here, we referred the changes in ecosystem C storage due to the changes in C turnover time as the changes of ecosystem C storage driven by turnover time, compared to the changes in ecosystem C storage driven by C influx.

As suggested， we revised "driven" to "caused".

L. 116: by the definition above (pool/flux), it definitely would change

[Response] We agreed with your comments and have discussed how much the difference between ecosystem and soil turnover time in the discussion section (Lines 336-347).

L. 142: cite R correctly ("citation()"), including version numbers of all packages used

[Response] We adopted the method cited from Todd-Brown et al., 2013 and have added the citation in Line 158-161.

L. 166: at the biome level, do you mean?

[Response] We aggregated ecosystem mean C turnover time and mean annual temperature (MAT, ^{o}C), mean annual precipitation (MAP, mm) and aridity index (AI) within biomes.

L. 196-201: first, need to note that you're assuming that the current-day spatial correlation between temperature and MTT is identical to the temporal correlation between these variables. It's not at all obvious this would be true. Second, you're mixing models and remote sensing products; it would be good to document how much divergence these models have from MOD17 in 2011.

[Response] Thanks for your suggestions. We added this assumption in Line 207-208 and also discussed its limitation in the discussion section (Line 461~465). We assumed that the spatial correlation between temperature and MTT is identical to the temporal correlation between these variables. However, such assumption cannot reflect the processes like acclimation of microbial respiration to warming or shifts in plant species over time.

We used NPP in 2011 was from MODIS products and NPP in 1901 was from model for there was no MODIS GPP in 1901. Our previous paper (Yan et al., 2014) showed that the average NPP among modeled NPP is near to MODIS NPP and the difference between both was mostly less than 0.05 kg C m^{-2} yr^{-1}, so we used the average model NPP (CanESM2, CCSM4, IPSL-CM5A-LR, IPSL-CM5B-LR, MIROC-ESM, MIROC-ESM-CHEM, NorESM1-M and NorESM1-ME) for NPP in 1901. The detail information was described in Yan et al. (2014).

L. 211-221: are these really results? Aren't these just the GLC database num- bers?

[Response] The terrestrial C storage was calculated from the global datasets about plant biomass, soil and litter C, which described in the datasets section. GLC database was just used for plant functional types or biome class to aggregate all Carbon at biome scale.

L. 225-: be consistent in using long/short or high/low or large/small in referring to MTT

[Response] Done as suggested. We used long/short in referring to MTT.

L. 245: Q_{10} is 1.95 implies that MTT roughly doubles with a 10 ◦C increase? That seems nonsensical

[Response] The pervious researches reported that Q_{10} for soil or other C pool is near to or larger than 2. For example, Sanderman et al (2003) calculated a Q_{10} value of 2.9 for soil turnover time using eddy flux. Foereid et al (2013) used Q_{10} value of 1.5~2.27 for soil pool and 1.29~1.66 for litter pool due to pool properties. We thought it was reasonable.

L. 298-299: can you explain this more?

[Response] Done as suggested. We used the same method (the ratio of total C storage to GPP) as Carvalhais *et al* (2014) to calculate the GPP-based MTT, but two main factors resulted in the difference between both. Firstly, ecosystem C storage in this study was the sum of the soil, vegetation and litter C storage, while Carvalhais *et al* (2014) just considered the soil and vegetation C. Secondly, vegetation C came from the result of Gibbs (2006) while Carvalhais *et al* (2014) used remote sensing based carbon stock estimates for tropical and Northern Hemisphere vegetation. We added the more explanations in Lines 348~353.

L. 338-340: see comment 7 above re language and causality

[Response] In this study, we quantified the ecosystem C storage changes from 1901 to 2011 and separated it into three parts: caused by the changes in NPP, the changes in ecosystem MTT and the co-changes of both NPP and MTT (seeing equation 4). Our results indicated that the decrease in MTT increased ecosystem C loss over time while increased NPP enhanced ecosystem C uptake. We have revised it in Lines 400~404.

L. 365-366: is it possible to quantify, even in a back-of-the-envelope kind of way, how much error might be introduced by this assumption? That would be interest- ing

[Response] Thanks for your comments. It is sure that the large uncertainty will be introduced by the steady-state assumption. Currently, most studies still used this assumption to examine ecosystem C capacity and turnover time. For example, there are Carvalhais *et al* (2014), Zhou *et al.* (2012), and Barrett et al. (2006). However, it is very difficult to quantify the uncertainty. It is a big project. We did not have some good approaches to resolve this problem to date. We thus only discussed the limitation of this assumption in our discussion. (See the above response).

L. 389: but you're not measuring temporal variability (much), except for changes over time in the MOD17 product, right?

[Response] We used the regression between temperature and MTT to estimate MTT in 1901 and 2011 for the temporal variability of MTT. MOD17 product was for NPP changes over time.

L. 406-419: this is all duplicative and can be removed

[Response] Done as suggested.

L. 421-422: completely inadequate data availability statement. Elevation data?!?

[Response] Sorry for confusion. The original data including MOD 17, HWSD, vegetation C production of Gibbs et al (2006) and litter dataset from Holland et al., 2005, is open and shared. We provided the citations for data sources.

Figures generally: maps are pretty but have limited utility. At least of these might be more informative if gives as e.g. Latitude versus MTT plots

[Response] Thanks for suggested. We have rescaled color and also added the latitude pattern for Figure 7.

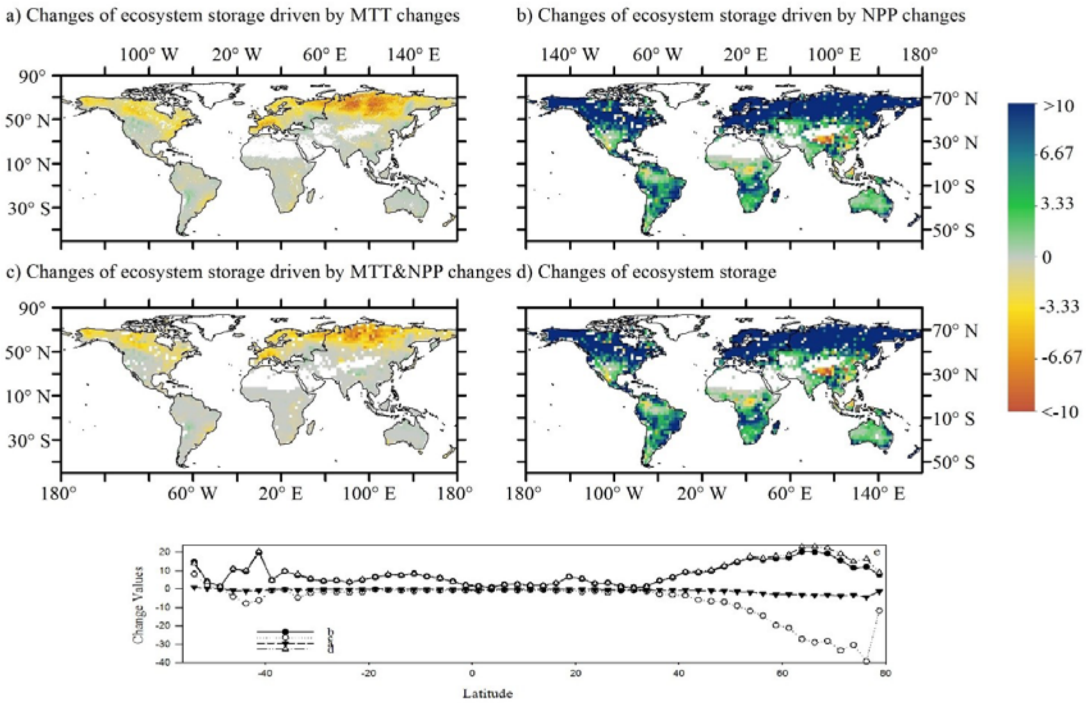


Figure 6: not at all useful in my opinion

. **[Response]** Done as suggested. We deleted it in the revised version.

Referee #2

General comments:

One major issue is that it isn't clear what the major new advance was in this analysis compared to previous, similar analyses. This analysis seems very similar to that of Carvalhais et al (2014), which is cited several times in the manuscript. In fact, Carvalhais et al was arguably more comprehensive than this analysis because it included direct comparisons to earth system model simulations. I think there were some new features in this analysis, such as the inclusion of litter estimates, comparing whole ecosystem vs. soil MTT, and looking at changes over the 20th century, but I think the paper could do a better job of highlighting which things are new and how they changed the results relative to previous, similar studies. If the litter estimates are new, then maybe there could be more discussion of how and why including that pool changed the results relative to previous analyses.

[Response] Thanks for your comments. In Carvalhais et al. (2014), global C turnover times and its covariation with climate were mainly examined. They also compared global C turnover time calculated by the model results from CIMP5 with those from observed data and their trend over latitude. Based on their work, we focused on the uncertainty from different observed data (HWSD vs. NCSCD), especially in high latitude. Litter data was updated compared to the study of Carvalhais et al. (2014). More importantly, we examined ecosystem C storage over time from changes in C turnover time and/or NPP. In addition, we estimated the GPP-based the NPP-based and soil MTT to explore the difference among them. Therefore, our study advance the understanding of the uncertainty of global C turnover time (especially in high latitude) and ecosystem C storage from C turnover time with updated data. We revised the abstract and discussion to make it more clear for the advance in Lines 18~21, 309~318.

Another issue is the potential for bias in some of the results due to the datasets used for GPP and NPP. While MODIS-derived GPP is constrained by satellite observations, it also depends on assumptions about climatic and environmental factors that affect plant growth and photosynthesis. For example, the efficiency parameter that converts absorbed light into GPP varies with VPD and temperature. MODIS NPP includes maintenance respiration that is calculated based on estimates of plant biomass and a temperature-dependent Q10 relationship. This raises questions about the temperature and moisture relationships shown in Figures 4, 5, and 6, as well as the related estimates *of changes in MTT over time. It is difficult to tell how much these relationships are affected by the underlying assumptions of the MODIS NPP algorithm. Since the estimates are not completely measurement based, it is harder to be confident about their meaning.*

[Response] Thank for your comments. MTT was calculated as the ratio of C storage and C influx (e.g., GPP or NPP), so the relationships should be affected by the relationships of GPP or NPP with temperature and VPD. The MODIS NPP algorithm would affect the estimates, discussed in the previous paper (Zhao et al., 2005; Zhao, M. and Running, S. W. 2010) but the uncertainty was within the allowable range. We thus thought that the uncertainty from the underlying assumptions of MODIS NPP algorithm was not considered in this study.

The MDOIS NPP algorithm is expressed as: $\mathrm{NPP} = \sum_{i=1}^{365} PsnNet - (R_{mo} + R_g)$, where PsnNet is net photosynthesis ($\mathrm{PsnNet} = \mathrm{GPP} - \mathrm{R}_{ml} - \mathrm{R}_{mr}$). R_{ml} and R_{mr} are maintenance respiration by leaves and fine roots, respectively. R_{mo} is maintenance respiration by all other living parts except leaves and fine roots (e.g., livewood), and R_g is growth respiration. GPP was calculated as: $\mathrm{GPP} = \varepsilon * \mathrm{FPAR} * \mathrm{PAR}$, where ε is the radiation use efficiency of the vegetation determined by maximum ε in each biome and temperature and soil moisture. All the parameters were abstained from the MOD17 Biome Parameter Look-Up Table (BPLUT). Therefore, the performance of the algorithm can be largely influenced by algorithm itself as well as the uncertainties from upstream inputs, such as land cover, FPAR/LAI, the meteorological data. For C5 MOD17, the BPLUT and the upstream inputs were be improved, so the MOD17 NPP is comparable to ecosystem Model-Data is comparable to the Ecosystem Model–Data Intercomparison (EMDI) NPP data set, and global total MODIS GPP and NPP are inversely related to the observed atmospheric CO_2 growth rates, and MEI index, indicating MOD17 are reliable products (Zhao et al., 2005). For example, direct comparison of MODIS annual GPP (MOD17A3) with observations for 37 site-years has resulted in a higher correlation and lower bias (r^2=0.6993, relative error=19%, unpublished data) than MODIS annual GPP calculated using tower meteorology (r^2=0.595, relative error=2%).

The estimates of changes in MTT over the 20th century are also problematic because NPP in 1901 was modeled rather than measurement-based. This means that all the *changes in NPP from 1901 to 2011 are based on a comparison between average output from several models (1901) to a measurement-based (but partially modeled) estimate (MODIS in 2011). How much of the difference was due to climatic factors that changed over that time period and how much was due to differences between the different sets of NPP estimates? I wonder whether the results in Figure 7b (difference between models in 1901 and MODIS NPP in 2011) would look compared to the hange in NPP from the model ensemble between 1901 and 2011. In the end, if models of NPP are being compared, what is the advantage of this MTT approach compared to just analyzing the change in carbon stocks from the actual model output over time?*

[Response] Thank for your comments. We used NPP in 2011 was from MODIS products and NPP in 1901 was from models, which were no MODIS GPP in 1901. Our previous paper (Yan et al., 2014) showed that the average NPP among modeled NPP is near to MODIS NPP and the difference between both was mostly less than 0.05 kg C m^{-2} yr^{-1}. We thus used the average model NPP (CanESM2, CCSM4, IPSL-CM5A-LR, IPSL-CM5B-LR, MIROC-ESM, MIROC-ESM-CHEM, NorESM1-M and NorESM1-ME) for NPP in 1901 and assumed the average model NPP was similar to MODIS NPP in 1901. The details described in Yan et al., 2014, so we did not add the comparison between difference between models in 1901 and MODIS NPP in 2011 and that in NPP from the model ensemble between 1901 and 2011.

The analysis depends on a space-for-time substitution (developing temperature and precipitation relationships based on spatial patterns and assuming they also apply to changes over time). What is the potential for bias in this assumption? Processes like acclimation of microbial respiration to warming or shifts in plant species ranges could make changes over time quite different from those that would be expected from observed spatial patterns.

[Response] Thank for your comments. We assumed that the current-day spatial correlation between temperature and MTT is identical to the temporal correlation between these variables in the revised MS, because there is no time series of data between MTT and temperature at the global scale. However, such assumption cannot reflect the processes like acclimation of microbial respiration to warming or shifts in plant species ranges, which could make changes over time. In the revised MS, we added the limitation for this assumption in the discussion section (Lines 461~465).

Comparing GPP and NPP as separate and independent metrics doesn't make much sense since both are derived from the same MODIS product. The difference between GPP and NPP is entirely determined by the assumptions of the MODIS NPP algorithm, so I'm not sure I would expect that distinction to provide much useful information in this type of analysis.

[Response] Thanks for your comments. Thompson and Randerson et al (1999) has indicated that there were two types of mean C turnover times for terrestrial ecosystems: the GPP-based or the NPP-based mean turnover time according to the terrestrial C models for some models use NPP as their C input and others use just GPP from atmosphere (i.e., NPP is GPP minus autotrophic respiration). However, there was no clear distinction in most pervious researches, so we calculated the both two for comparison and NPP-based MT is more availed in comparison with soil MTT than GPP-based MTT. The difference between GPP-based and NPP-based NPP was determined by the ratio of the ratio of GPP and NPP which entirely determined by the assumptions of the MODIS NPP algorithm. We added their difference in the discussion sections (Line 359-361).

In general, I think the Discussion doesn't say enough about why this analysis is useful compared to existing models and previous analyses. The suggestions given for incorporating these results into earth system models and land models are not very useful because most of these factors (e.g., temperature dependence of turnover rates) are already included in all existing models. I do think that there are some useful outcomes from this type of analysis, but I think the Discussion needs some more interpretation of the specific results in the context of ecological factors rather than general statements about how models should take these results into account.

[Response] Thanks for your comments. In discussion, we tried to add the context of ecological factors in the revised MS, such as Lines 335-347.

The manuscript also could use some proofreading for English usage.

[Response] We carefully revised the manuscript, especially for the language editing. Meanwhile, we asked a native speaker: Shahla Hosseini Bai to carefully revise the manuscript. Hope our manuscript has been considerably improved.

Specific comments*:*

Line 56-57: This analysis generally discusses NPP and mean C turnover time as independent, but they could also be related. For example, faster plant growth could accelerate soil C turnover via priming effects, or there could be correlations between plant growth rates and the longevity of vegetation.

[Response] Thanks for comments. The transient C storage is determined by the MTT and NPP. If climate increases C influx (NPP) into an ecosystem but does not change C transient times (MTT), the C sequestration rate of the ecosystem increases due to fact that more C strays in the ecosystem for the same length of time, which could be correlated between growth C plant growth rates and the longevity of vegetation. Certainty, climate would increases C influx and also accelerate soil C turnover, the C sequestration rate of the ecosystem increases is determined by both the amounts of C influx and their MTT. Therefore, in this study, we firstly separated the C storage changes into three parts: caused by the changes in NPP, the changes in ecosystem MTT and the co-changes of both NPP and MTT (seeing equation 4), and secondly, the NPP and MTT in 1901 and 2011 were used to estimate the changes in ecosystem C storage from 1901 to 2011, and finally we discussed the spatial pattern of ecosystem C storage changes and the possible reasons.

Line 62-63: It seems like Carvalhais et al (2014), which this analysis largely follows, did do a pretty good job of quantifying this spatial variation at global scales.

[Response] Thanks for your comments. Based on their works (Carvalhais et al (2014)), we mainly aimed to compare different versions of MTT and quantify the spatial variation in carbon storage changes caused by turnover time or C flux.

Line 66-68: Another recent radioisotope paper to cite is He et al (2016)

[Response] Thanks you for providing the new reference.

Line 78-82: This suggests that the main contribution of this paper is comparing *different versions of MTT calculations. But it's not really clear later on if that is meant to be the focus or not. The paper is also about changes in MTT over time, but doesn't really connect these two parts together.*

[Response] We focused on comparing different versions of MTT calculations and its effects to climate and quantifying the ecosystem C storage changes driven by ecosystem MTT. The changes in MTT over time was used to estimate the ecosystem C storage changes caused by MTT (equ. 4), which was calculated by the relationship between MTT and climate.

Line 165-166: "interpret the quantity as an emergent diagnostic at the ecosystem level": What does this emergent diagnostic actually tell us? There isn't any discussion of how it should be interpreted or what kind of bias would occur as a result of the steady state assumption being violated.

[Response] If the ecosystem is not at the steady state, the C turnover time cannot be calculate by the ratio of C storage and C flux. We thus followed the assumption of Carvalhais et al. (2014) and have discussed the limitation of steady-state in MS (Lines 431-436, 444-448)

Line 180: The equation for MTT looks like it's fitting a ratio of MAT/MAP, but I think this is actually meant to say either MAT or MAP. It's very confusing the way it's currently written.

[Response] Done as suggested. We revised that to $\mathrm{MTT} = \mathrm{a}e^{-bMAT\ or\ MAP}$.

Line 214-216: If most of the carbon was in soil, then total ecosystem MTT would be largely determined by soil MTT. What are the implications of this when comparing those two estimates?

[Response] $\mathrm{MTT}_{EC} = \frac{Cpool}{GPP} = \frac{C_{soil+}C_{veg+}C_{litter}}{NPP/\varepsilon} = \varepsilon * MTT_{soil} + \varepsilon * \frac{C_{veg+}C_{litter}}{NPP}$ （$\varepsilon = \frac{NPP}{GPP}, MTT_{soil} = \frac{C_{soil}}{NPP}$）.

If most of the carbon was in soil, the ratio of NPP to GPP is the key to determine the difference between ecosystem MTT and soil MTT and NPP-based MTT is similar to soil MTT. We have discussed the difference versions of MTT in Lines 335-347.

Line 220: I would expect permafrost soils to have much larger C stocks in places with very deep organic soils. It's not unusual for deep permafrost to have >100 kgC/m2 (Schuur et al., 2015). Could that lead to bias in these results?

[Response] In this study, ecosystem MTT was calculated as the ratio of C storage and influx ($MTT_{EC} = \frac{Cpool}{GPP} = \frac{C_{soil+}C_{veg+}C_{litter}}{GPP}$). When the deep permafrost is considered, the ecosystem MTT will become longer. If we assumed that soil C in deep permafrost is 100 kg C/m^2 and GPP is 0.2 kg C m^2 yr^{-1}, the MTT is 500 years.

Line 224-225: He et al (2016) used radiocarbon analysis to estimate a mean soil C residence time of about 3000 years, which they found to be consistent with several other published estimates. What explains the 2 order of magnitude difference from the estimates here? Turnover time for tundra also seems very short, given that permafrost soils are known to have been steadily accumulating carbon for thousands of years.

[Response] In our MS or Carvalhais et al. (2014), we assumed that ecosystem was in the steady state and calculated MTT as the ratio of C pool and C flux. Here, we did not separate C pool into slow, fast or passive, which could largely underestimate the ecosystem MTT. Another factor is that the current soil dataset such as HWSD underestimate the soil C storage, especially for permafrost soils. In the discussion section, we discussed the limitation of the assumption of the steady-state and the difference soil datasets effects on the estimate of ecosystem turnover time (Lines 431-436, 444-448, Line 362~368).

Line 256: It doesn't seem like the increase in R2 was really that significant.

[Response] R^2 for the regression function of soil MTT with MAT was 0.76 when AI>1, while R^2 was 0.52 when AI<1 (Fig. 5e & h)

Line 261-262: It would be nice to include a map of temperature changes along with MTT and NPP changes so all driving factors could be compared. Also, why was only temperature and not precipitation included in this part of the analysis, even though both looked like they had significant relationships with MTT?

[Response] Done as suggested. We added a map of temperature changes in Figure 7 (Figure 6 in the revised MS, seeing the below). There is no change in R^2 when MAP was incorporated into the regression function of ecosystem MTT with MAT, so we just considered the temperature changes included in this part of the analysis.

Line 268 and 271: I think these units should be PgC, not PgC/year

[Response] This unit is PgC/year for the change of C storage over years (averaging in one year).

Lines 270-275: This might be a good place to discuss whether the whole ecosystem patterns differed from the soil C patterns if there were any interesting patterns there

[Response] C patterns can determined by NPP and MTT, so the difference between the whole ecosystem and the soil C patterns was determined by the difference between ecosystem and soil MTT. In our study, MTT in 1901 and 2011 was calculated using the relationship between MTT and temperature, so the difference of temperature function determined the difference of both MTT and then the C storage patterns. Therefore, we added the limitation of MTT calculation in the discussion (Lines 458-461). When the relationship between soil MTT and temperature was used ($MTT_{soil} = 58.40e^{-0.08MAT}$), the changes on ecosystem C storage caused by MTT could decrease to 161.42 Pg C and that driven by NPP could be 1125.6 Pg C, with the similar spatial pattern as the ecosystem.

Line 293-297: I think a lot more could be said about the ecology behind these results. What features of dominant plant species and soil contributed to these differences? Differences in plant lifetime? Tissue lifetime? Susceptibility to decomposition?

[Response] Thanks for suggested. The difference between NPP-based ecosystem and soil MTT was determined by the turnover time of vegetation and litter, a trait related to plant functional types (PFTs). We have added some ecological information behind these results in Line 336-347**.**

Line 299: Since the ratio of GPP to NPP is entirely determined by the assumptions of the MODIS NPP algorithm, I don't think this result has a lot of real-world meaning.

[Response] Thompson and Randerson et al (1999) has indicated that there were two types of mean C turnover times for terrestrial ecosystems: the GPP-based or the NPP-based mean turnover time according to the terrestrial C models for some models use NPP as their C input and others use just GPP from atmosphere (i.e., NPP is GPP minus autotrophic respiration). However, there was no clear distinction in most pervious researches, so we calculated the both two for comparison and NPP-based MT is more availed in comparison with soil MTT than GPP-based MTT. The difference between GPP-based and NPP-based NPP was determined by the ratio of the ratio of GPP and NPP which entirely determined by the assumptions of the MODIS NPP algorithm. (Line 359-361).

Line 377-379: Why would this reduce the uncertainties?

[Response] The ratio of the pool to flux could reduce the uncertainties derived by the same sources.

Line 381-382: Doesn't aggregating everything to the biome level violate the assumptions behind calculating change in MTT over time? This would suggest that MTT could only change if the spatial extent of different biomes was shifting.

[Response] The original data including MOD 17, HWSD, vegetation C production of Gibbs et al (2006) and litter dataset from Holland et al., 2005 were created based on the plant functional types by the assumptions of algorithm, so we aggregated MTT to the plant functional types to estimate the change in MTT over time for data match.

Line 390-391: This would be a good place to discuss alternative soil databases like Hengl et al (2014) - available at soilgrids.org

[Response] Thanks for your suggestions. We have discussed the uncertainty caused by the different datasets (in Line 362~368) and also added the soil databases of Hengl et al (2014). (If SoilGrids (Hengl et al., 2014) was used to estimate C MTT, the MTT in the top 1 m could increase to 30.3 years for GPP-based, 66.9 years for NPP-based and 45.7 years for soil.)

Line 392-393: This is arguably the primary purpose of all land surface models. They *all already consider this.*

[Response] Thanks for your comments. We have deleted it.

Line 397-398: All land surface models already include temperature functions that affect pool turnover times.

[Response] Thanks for your comments. It is sure that all land surface models already included temperature functions that affected C pools and fluxes via plant photosynthesis and respiration. These effects probably directly affected turnover times of C pools to some degree. Carvalhais *et al* (2014) examined the covariation of climate with turnover times. In this study, we emphasized the effects of moisture or precipitation on soil decomposition, especially in high-latitude zones with greater warming and increased precipitation.

Line 401-404: Land surface models already include these processes. In general, this whole section about improvements to land models isn't supported by any comparison between this study and actual land model output. Carvalhais et al (2014) did explicitly compare their MTT results to earth system model simulations, and I don't think it makes sense to discuss these model-related suggestions without doing a similar comparison here.

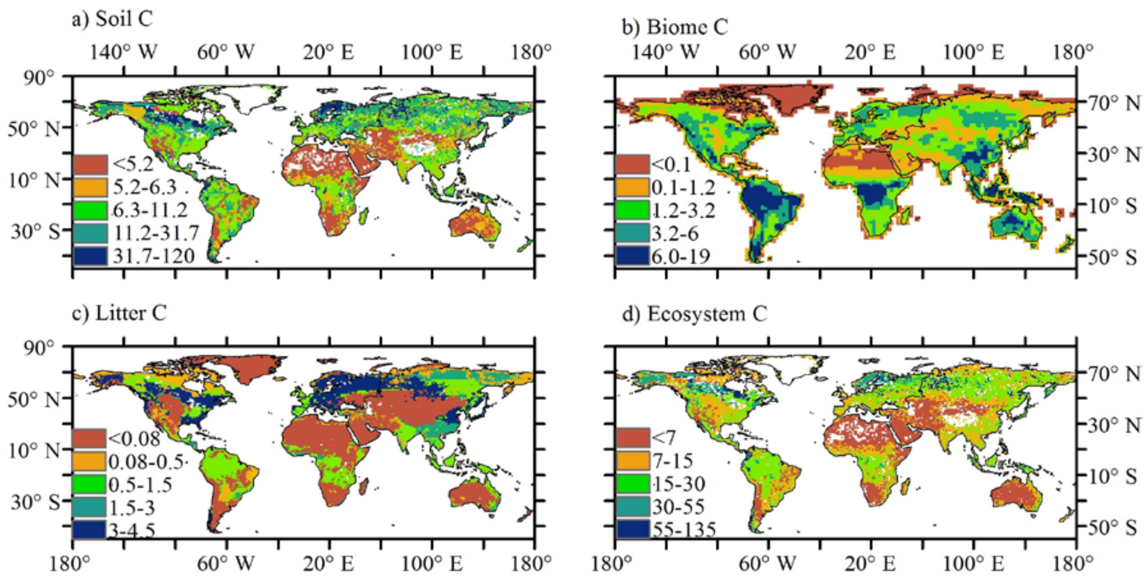
[Response] Thanks for your comments. Compared with Carvalhais et al (2014), we mainly discussed the difference of the climate effects between on ecosystem MTT and soil MTT, especially for moisture. Our results also showed that the temperature sensitivity of ecosystem turnover time was lower than that of soil C pool (Q_{10}: 1.95 vs. 2.23), while moisture stress on soil MTT was significant, especially under low aridity conditions. Current land surface models have considered moisture stress on vegetation, but the incorporation of moisture or precipitation stress into soil decomposition should be strengthened, especially in high-latitude zones with greater warming and increased precipitation.   To make it clear, we have rewrote these sentences (Lines 472~480).

Line 421-422: Data availability would require putting all the MTT data somewhere that readers can access it.

[Response] The original data including MOD 17, HWSD, vegetation C production of Gibbs et al (2006) and litter dataset from Holland et al., 2005, is open and shared. We provided the citations for data sources

Figure 1: The colors need to be rescaled, especially for soil C. It's really hard to see anything in that map. Also, the soil C has some obvious artifacts, like the sharp change in soil C on the border between Alaska and Canada. What does this mean for the results? It would also be nice to have a map of NPP here s ons.

[Response] Done as suggested. We have rescaled the colors for map (Figure 1).



Soil C storage in Alaska is near 30 kg C, while that in Canada is less than 10 kg C, forming the sharp change in soil MTT on the border between Alaska and Canada. Here, soil MTT in Alaska ranges among 70~95 years and that in Canada on the border is less than 20 years.

Figure 1 showed the C storage in different C pools (soil, vegetation, litter and ecosystem), so NPP was not mapped in Figure 1.

Figure 2: Since all three of these look about the same, I don't really see the point in including all of them as separate metrics

[Response]Thanks for your comments. The colors have been rescaled to strengthen the difference among three of these.

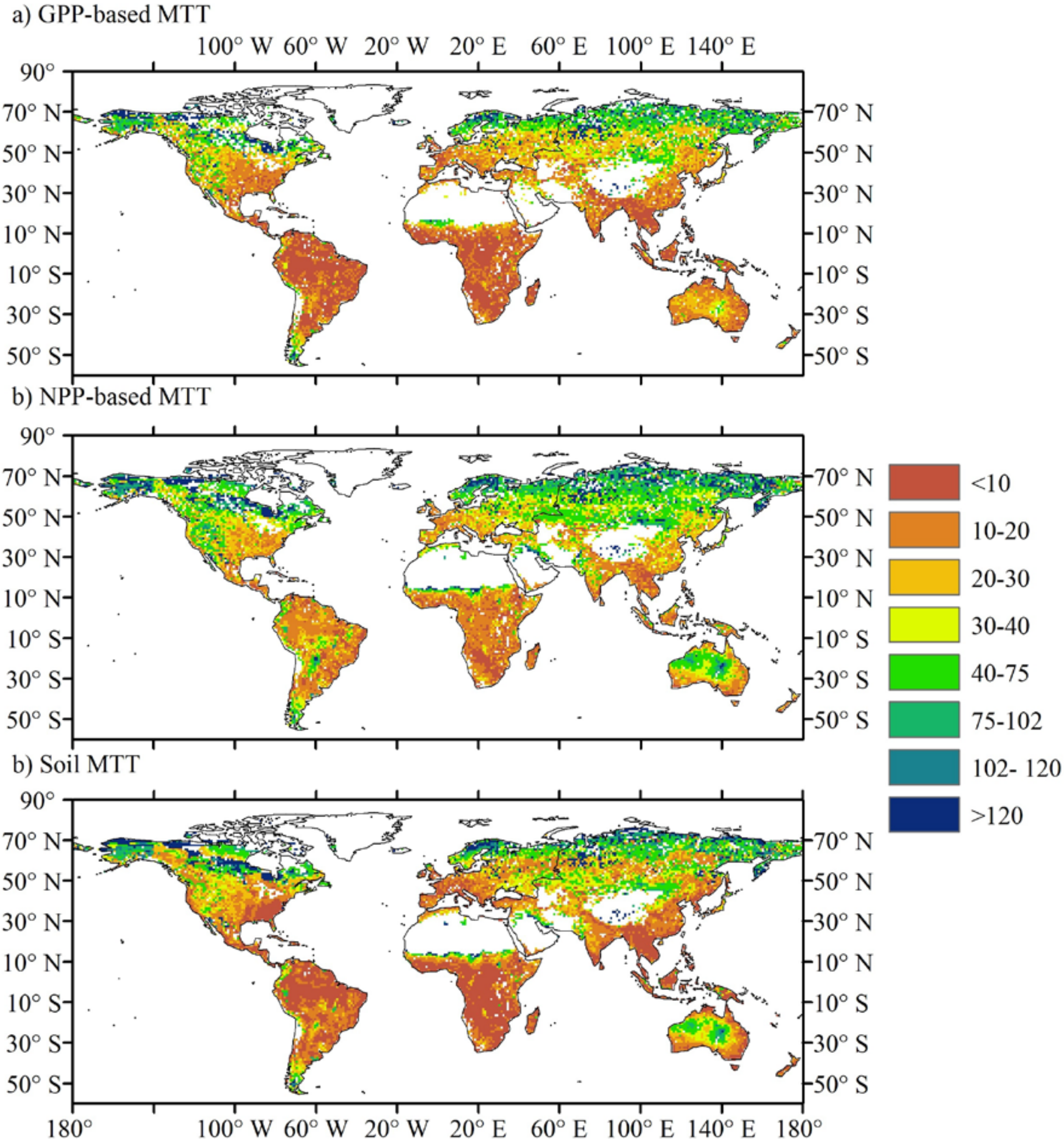


Figure 4: Panel a: The regression looks like it underestimates the slope of the curve by a lot. Panel d: The exponential fit does not do very well at the high precipitation end. What does this mean for the results?

[Response] The curve fit does not do very well at the high precipitation end at Panel a or Panel b**.** If the high precipitation (>2000mm) was neglected, the exponential fit would do better. For example, R^2 would increase to 0.86 at Panel d.

Figure 7: The titles on the figure say from 1991 to 2011, but the text says it goes from 1901 to 2011.

[Response] We have revised the titles on the figure from 1901 to 2011.