Our replies to the comments of Anonymous Referee #1 are given below *in Italics*.

1. While the subject of this manuscript is of significant interest the manuscript itself needs clarification of a number of things. I therefore suggest that the manuscript be accepted for publication in Biogeosciences after revisions. In particular, the authors needs to make connection between sections 2.1 and 2.2, which attempts to obtain litter decomposition rates from observation-based data, to section 2.3 which actually uses these data. Also, as a reader I wasn't able to follow what exactly was being done in sections 2.1 and 2.2.

Firstly, we thank the reviewer for very constructive comments. We will improve connections between sections in the revised manuscript. We will make sections 2.1 and 2.2 and Table 1 more coherent in terms of notations, by using terms k_{leaf10} (formerly: k_{10}) and k_{wood10} (formerly: k') for leaf and woody litter decomposition rate at 10°C, respectively. Section 2.3 will be rephrased to indicate that we firstly used overall k_{leaf10} averaged over all PFTs (k_{leaf10}^{avg}) and k_{wood10} averaged across all PFTs (k_{wood10}^{avg}) retaining the original LPJ temperature sensitivity description (our simulation W). We already reported these values (0.94 and 0.057 yr^{-1} for the leaf and woody litter at 10°C, respectively) at the page 8826, l.10-11. In the revised manuscript, we will add these averaged values to the footnote of the Table 1 as well. Secondly, we applied PFT-specific k_{leaf10}^{PFT} but still retaining the original temperature sensitivity from LPJ to leaf litter. We also replaced k_{wood10}^{avg} by a PFT-specific k_{wood10}^{PFT}, but kept the original temperature sensitivity from LPJ (noted as K). Finally, we replaced the average temperature sensitivity for woody litter by PFT-specific woody litter temperature sensitivity as derived in section 2.2 (noted as Q).

2. Abstract. Line 9. Please change "with except" to "with the exception".

Will be done

3. Abstract. Please also tell in the abstract the actual difference in CO2 concentration when observation-based wood and leaf litter decomposition rates are used.

As suggested, we will modify the abstract as follows:
*…"projects a strong increase in global litter stocks **by 35 GtC** and a concomitant small decrease in atmospheric CO2 **by 3 ppm** by the end of this century."*

4. Page 8820. Line 1 reads "After correction for environmental conditions (temperature), both lignin and nitrogen were highly significantly (P <0.001) related to k (Fig. 1)". I do not see any lignin and/or nitrogen concentrations in Figure 1, which actually plots measured versus predicted decomposition rates for leaf litter (I presume, because the figure caption doesn't say so explicitly). I also do not know what the measured and predicted rates actually mean in this context.

Figure 1 shows the decomposition rates as predicted based on lignin and leaf nitrogen concentrations only vs. the observation decomposition rate. We chose to show this pattern (instead of the plots of k_{leaf} vs. nitrogen and k_{leaf} vs. lignin), because particularly the combination of traits allowed for a good estimation of k_{leaf}. Moreover, the predicted vs. observed plots shows the overall power of traits to predict the variation in k, which is critical to the current paper. We will state explicitly in the legend of Figure 1 that the predicted decomposition rates are based on lignin and nitrogen concentrations combined. We can also add additional figures on k_{leaf} vs. leaf

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nitrogen and k_{leaf} vs. lignin as Supplementary Material to exemplify this point (see the figure in our reply to the comment 6 of the reviewer 2) in case reviewers think this is a necessary step.

5. Page 8820. Line 15. What does "reference litter site" means?

The 'reference litter site' should have read 'reference litter'. We also acknowledge that the description of the litter k_{leaf} values might have been too concise, whereas its understanding is of critical importance. Leaf litter decomposition is commonly quantified either by direct field incubations or through 'common garden' experiments. The latter procedure, in which litter samples of multiple species are all incubated simultaneously outdoors in a rather natural litter matrix, has as advantage that the decomposability of different species can be compared directly without confounding factors like differential access by detritivores or differences in microhabitat. Relative differences in k_{leaf} can thus be estimated in a robust unbiased manner. A disadvantage to the use of common gardens experiment is that the absolute k_{leaf} may be slightly deviate from that in the field. Therefore, we applied an approach combining the advantages of both methods: we compiled common garden experiments (Cornwell et al. 2008) that had incubated ≥ 6 species (and in 14 studies > 20 species) simultaneously to quantify the effects of litter quality on decomposability. Subsequently, we used litter with known litter quality (i.e. known litter N and litter lignin values) that had been incubated at the reference conditions ('reference litter'; see comment 8), to scale our k_{leaf} -values to true unbiased field level decomposition rates k_{leaf} at reference conditions. The combination of both is reflected in equation 1. This will be better phrased in the revised version. Additionally, we propose to rephrase equation 1 to identify both scaling and litter quality effects explicitly to:

$$^{10}logk_{leaf10}{}^{PFT} = {}^{10}logk_{leaf10}{}^{ref} * (a*{}^{10}log(lig^{PFT}) + b*{}^{10}log(N^{PFT}))/(a*{}^{10}log(lig^{ref}) + b*{}^{10}log(N^{ref}))$$

6. Page 8820. Line 17. I do not understand what does "Arithmetic means of litter chemistry by PFT" actually means.

This phrasing indeed was somewhat cryptic. We derived equation 1 and 'a' and 'b' (see above) based on the full database of Cornwell et al. 2008, containing trait values for each species-site combination. For the application in model experiments (as described in section 2.3), we only have an average k_{leaf10} per PFT. That $k_{leaf10}{}^{PFT}$ was calculated based on equation 1, giving lig^{PFT} and N^{PFT} the values of the averages for each PFT as represented in the database. A remark along those lines seems more suitable to section 2.3 (than its current position) to avoid confusion.

7. In sections 2.1 and 2.2 both leaf and wood litter decomposition rates are referred to as k. Please, at least, use k_leaf and k_cwd to keep the distinction in the text as is done in some figures.

We fully agree that a more consistent formulation of k is needed. We suggest a solution for this notation in our reply to comment 1.

8. In its current form, it is little unclear what sections 2.1 and 2.2 are exactly doing. In section 2.1, it appears that observation-based decomposition rates are regressed against leaf litter lignin and nitrogen concentrations to obtain the parameters a and b, which are then used together with lignin and nitrogen concentrations averaged over a

large number of samples from a single PFT to determine an average PFT dependent de- composition rate. However, I do not understand why the PFT dependent and reference site decomposition rates are in the numerator and denominator of equation 1.

Equation 1 needs a more extensive explanation. As indicated in our reply to comment 5, common garden incubations provide a proper estimate on the relative impact of litter quality (as identified through lignin and leaf N concentrations) on decomposability. This relative impact is central to our phrasing of equation 1 with the numerator and denominator. To convert the relative impact to an absolute measure of decomposition, an unbiased estimate of k_{leaf} at reference conditions with known litter quality is needed. For this reference litter, we took data from those sites in our database that had Mean Annual Temperature between 8 and 12 °C, which were not obviously constrained by low precipitation and in which incubation conditions strongly resembled that of litter in situ. These studies included temperate rainforest in New Zealand, temperate forest in Poland and temperate peatland and woodlands in The Netherlands. The Grand Mean of leaf litter k_{leaf} across these studies, after accounting for $a*^{10}log(lignin) + b*^{10}log(N)$ as measured for the reference litter, was taken to represent reference litter decomposition at MAT of 10 °C. As explained in our reply to comment 5, this procedure is the most optimal for estimating k_{leaf10}^{PFT} in an unbiased way.

9. I am also unable to follow the logic of equations 2 and 3. What does the reference k-value mean in equation 2? Is the primary purpose of these equations to find the betas and the Q10 parameters? If yes, how are these betas and Q10s used to find PFT-dependent coarse woody debris decomposition rates in LPJ.

We will rewrite the text to make clear that only two sets of parameters are actually passed on to LPJ: "Two sets of PFT-specific parameters, k_{wood10}^{PFT} and Q_{10}^{PFT}, were used in LPJ. k_{wood10}^{PFT} is the baseline decomposition rate for a given PFT under conditions 1) where the mean annual temperature T (°C) is at a reference temperature T_{ref} (set to 10° C) and 2) where all covariates attain a value of zero. This refers to the mean values of ap (1416 mm, i.e. non limiting conditions) and d (10.2 cm) and the situation of a downed log in contact with the soil surface (pos = 0). Sensitivities to covariates other than temperature, i.e. the β^{PFT}'s, were not used in LPJ. However, estimating them was necessary in order to extract comparable baseline rates. "

Regarding notations, please see also our reply to the comment 1.

Then, in Figure 2 observed versus predicted decomposition rates are compared. What does "observed" mean in this context and how is "observed" related to Equation 2. What is "observed" – k or k'? And what is "predicted". Predicted rates, as the names suggests, are likely from a model – which model - LPJ implemented in the ESM or is equation 2 the model that is being referred to here.

We now state explicitly that the predicted values refer to the empirical model, not to LPJ predictions. In the new version we will write: "Comparison of observed versus predicted decomposition rates of coarse woody debris of <u>the empirical model</u> is shown in Figure 2 and modelled sensitivity of coarse woody debris decomposition rates to mean annual temperature is presented in Figure 3".

10. In Section 2.3, it is not described clearly how the information obtained in sections 2.1 and 2.2 is actually used. Also, please describe the simulations clearly. Please explicitly say how many simulations were performed, name them and then clearly say what they do and for what duration were they ran.

After reading section 2.3, I am unclear how long were the 1750 pre-industrial simulations ran, what was the difference between two transient simulations (line 18, page 8824) and how long were these transient simulations. I am guessing the difference between transient simulations is that they were run 1) with LPJ's default leaf and wood litter decomposition rates and 2) in the WKQ configuration. If yes, please say this explicitly.

We will include the following table into the text with footnotes explaining differences among the simulation setups:

Simulation acronym	*Separate k_{leaf10} and k_{wood10} values*	*PFT-specific k_{leaf10} and k_{wood10}*	*PFT-specific Q_{10} values for $k_{wood}(T)$*
Equilibrium simulations (1,000 yrs with pre-industrial CO_2 level of 280 ppm)			
CTL^1	-	-	-
W^2	+	-	-
WK	+	+	-
WKQ	+	+	+
Transient simulations (historical CO_2 emissions + SRES A2 scenario, yrs 1750-2100)			
CTL-T	-	-	-
CTL-WKQ	+	+	+

11. In section 2.3, line 14 reads, " . . . to represent land surface processes at high resolution". What resolution was CLIMBER ESM run at?

CLIMBER is run at spatial resolution of 51°lon x 10°lat. The LPJ is run at the 0.5°x0.5° resolution. This information will be added to the methods section.

12. The last sentence of Section 2.3 reads "In the transient simulations, CLIMBER2-LPJ was driven by SRES A2 scenario of fossil fuel and land use emissions . . . starting from pre-industrial equilibrium at year 1750". This means there was a historical 1750-2000 simulation also performed. Isn't it necessary to describe how the historical simulation was done? The authors also appear to assume that the reader knows that transient simulations with A2 emissions are for the 2001-2100 period.

The historical (1750-2000) part of transient simulations was done with historical CO_2 emissions. We will provide a reference for this experimental setup, and also refer to the table with simulations setup (see our reply to the comment 10 above).

13. Page 8825,line 2. " . . comparison with data for woody litter . . .".Replace "data" with "observation-based estimates"

agree

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[1] k_{leaf10}=k_{wood10}=0.3 yr^{-1}

[2] Averaged trait-dataset values, k_{leaf10}=0.94 yr^{-1} and k_{wood10}=0.057 yr^{-1}

and give a reference.

The reference is Table 2.

This sentence refers to the CTL simulation, but the CTL simulation does not make the distinction between leaf and woody litter so it is not possible to make this comparison. Except, of course, the assumption that would go here is that most of the litter biomass is made of woody litter.

Indeed, the original formulation of LPJ has only one litter pool. However, the model calculates fluxes going into leaf and woody part of the litter. Knowing these fluxes, we allocated them into separated leaf and woody litter storages and found out these storages in the CTL simulation. We included these results into the Table 2. Since this approach seems to cause a confusion, we will discuss this "diagnostic separation" of storages in the CTL simulations in the Methods section.

14. There should be some more discussion in the manuscript about the higher simulated litter mass (~180 to ~190 Pg C) compared to other estimates. What is also surprising is that while simulated litter mass in the CTL and WKQ simulations are generally lower than or similar to their observation-based estimates in Figure 5, the simulated global litter mass is higher than observation-based estimates. The caveat here, of course, is that observation-based estimates are from different sources. A comment on this would be useful for the reader.

In the discussion section we will emphasize again the need for modelling all relevant covariance effects to make observations coming from different locations under different environmental constraints and measured with different methods comparable. In the new version this will be explained as follows:

"The databases that we used to derive the k values reported in Table 1 are currently the largest global databases in its kind, derived from studies that measured litter k_{leaf}. Extracting generic and representative PFT-specific parameter values from a heterogeneous dataset of world-wide observations required a sophisticated pre-processing. In order extract PFT-specific estimates of baseline decomposition rates and temperature sensitivities, a wide range of chemical, structural and environmental variables had to be controlled for (Kattge et al. 2011). The value of the databases at hand is specifically due to the availability of such covariates. In the case of leaf litter, k_{leaf} values were linked to litter chemistry to predict the variation in k within and between PFTs. Subsequently, we predicted k from the average litter lignin and litter N contents of that PFT. For woody litter we accounted for log dimensions, degree of soil contact, and precipitation in order to improve our estimates of baseline rates and temperature sensitivities. This led to highly robust estimates of k (see Figures 1 and 2 for illustration of variation in k_{leaf} and k_{wood}, respectively). "

15. On Page 8825 the following two sentences essentially convey the same message. Please consider combining these two sentences into one.

"At the northern boundary of boreal forests, the model overestimates the living biomass stocks by a factor of two, in particular because of the absence of a permafrost parameterization which otherwise would limit tree growth substantially, and this is reflected in the high litter stocks."
"However, because of a bias in living biomass stocks, a mean litter stock in boreal needleleaved forests is overestimated by ca. 80% (Table 2).

We will omit the 2nd sentence and re-write the first sentence as follows: "At the northern boundary of boreal forests, the model overestimates the living biomass stocks by a factor of two, in particular because of the absence of a permafrost parameterization which otherwise would limit tree growth substantially, and this is reflected in the high litter stocks (overestimated by ca. 80%, see Table 2)."

In Table 4 the transient CTL and WKQ simulations are referred to as CTL-T and WKQ- T simulations but this terminology is mentioned in the text in a very subtle way. Like I suggested earlier, it would be really useful to introduce all the simulations names early on, what they do, and how long they ran. Authors can also consider using a table.

We will add a table, see our response to the comment 10.

Page 8827, last paragraph. The discussion about the effect of litter mass on fire seems somewhat unnecessary. Fire behaviour can be very complex and in my opinion this discussion requires more than few lines. Fire CO2 emissions also depend on area burned. Did the area burned change between the CTL-T and WKQ-T simulations? I suggest that this discussion be left out completely.

Indeed, changes in litter affect not only emissions, but burned area as well. We prefer to keep the fire discussion in because of litter significance for the fire models. Litter storage is an important parameter, which can limit or promote a fire by controlling fire spread rates (Rothermel, 1972, Thonicke et al., 2010). We will emphasize in our discussion that other factors will likely contribute too.

Page 8828, line 24. Replace "is" by "are".#

We will re-write it as follows: "there are other factors…"

Figure 3 caption. I wouldn't call these curves "modelled sensitivity of cwd decomposition rates to temperature" because these are not modelled per se. I would call these "Temperature sensitivity of decomposition rates used in the WTQ simulation" or some- thing along these lines. I would also suggest to show the curve based on the standard Q10 value used in the LPJ model. Also, I don't think the caption needs to include the words "mean annual". This is just the sensitivity to temperature.

With respect to Figure 3, the predicted values do refer to the empirical model, not to LPJ predictions. See our response to the comment 9.

References

Rothermel, R. C.: A Mathematical Model for Predicting Fire Spread in Wildland Fuels, Intermountain Forest and Range Experiment Station, Forest Service, US Dept. of Agriculture, Ogden, UtahUSDA Forest Service General Technical Report INT-115, 1972.

K. Thonicke, A. Spessa, I. C. Prentice, S. P. Harrison, L. Dong, and C. Carmona-Moreno, The influence of vegetation, fire spread and fire behaviour on biomass burning and trace gas emissions: results from a process-based model, Biogeosciences, 7, 1991–2011, 2010.