

Dear Editor and Referees,

We would like to thank the two Referees for their valuable comments on the content of our manuscript and their suggestions for improving the document. In this reply, we seek to clarify the issues, raised by the referees, point by point. Please find the detailed response (normal font) to the reviewers' comments (italic font).

### **Responses to comments from Referee 1**

*[General comment]: The authors use the La Tuille flux data set to extract 57 sites for which an investigation of winter respiration is presented. Besides purely descriptive sections, an attempt was made to look at the controlling factors of winter respiration, an aspect which I unfortunately found rather superficial and where I expected a much more systematic approach. The whole paper reads like a manuscript that was started by a small group, then sent to a large set of coauthors, but the feedback from them appears to only have led to additional secondary statements and notes, such that at the end the overall text probably contains a short touching of any of the relevant aspects but not in an organized manner. I unfortunately started to read the paper with a negative experience found in the abstract: these winter fluxes are so unrealistically high that I really wondered how the authors will justify so large numbers in the main text. They don't. It's just one of the plenty of technical issues or errors that I will mention at the bottom of my review. The topic is certainly of interest.*

**[Response]:** We apologize for the mistake made in the abstract and also other

technical errors inside the document of our previous version. We have made our best to correct all of them in the manuscript. Thanks to your comments, the relevant issues raised by you are answered and explored further in the following specific comments.

*[Comment 1]: Definition of winter. The authors use their own definition of winter as the primary approach, and the established climatological winter (December thru February) as the secondary, alternative approach, that is not consistently referred in all analyses. My view is that it should be the other way around, that is, the established convention is used as a reference and then a clear justification must be given for an alternative suggestion.*

**[Response]:** The main reason why we adopted °C threshold based winter definitions (a more ‘process-related’ definition) as the primary definition is because we would like to investigate carbon processes in the frozen period that are not explored in previous meta-data analyses (e.g. Yuan, et al., 2009; Migliavacca et al., 2011). The climatological winter definition D\_TM (December through February) has only been added for the readers convenience. The climatological winter definition assumes a constant winter duration, which makes it impossible to compare winter carbon losses to annual carbon balance between sites at high latitudes characterized by different frozen periods. Our revised MS considers other winter definitions based on different temperature thresholds (0 °C, -2 °C, -5 °C and -10 °C) and refers to D\_TM in all cases. We explain our rationale for the adopted definition of winter in L243-253 (new version), replacing 7006/9-15 (old version).

*[Comment 2]: Here I even see a flaw: by adding the length of the winter period as a covariate to the definition, you get a slightly higher statistical correlation in a few analyses, but then you find that winter length does not have additional explanatory power on the residuals (page 7013/5–6). This is uncommmented but is quite clear: if you already used a variable in a statistical analysis, then of course if you use the same variable a second time for explaining the residuals you must find a zero correlation. Here I wonder whether the authors really have understood their statistical analysis.*

**[Response]:** This is absolutely right. The winter length has already been taken into account in daily winter  $R_{eco}$  when we performed cross-site analysis of its correlation with drivers. The zero correlation found between daily winter  $R_{eco}$  and winter length can be definitely expected. In revised MS, we thus removed the residual analysis between winter  $R_{eco}$  and winter length.

*[Comment 3]: It is less clear to me what the reason for the zero correlation with winter precipitation is on the same lines (page 7013/5–6). Here I see an interesting point which is however not commented at all. The text leaves the reader with the impression that winter precipitation is not relevant, but by rethinking what I find in the paper and what is not commented, I think this deserves a clearer interpretation on the functional level (these controls which are mentioned at the beginning of the title): if winter precipitation does not lead to significant variations among sites, then it may well be that thanks to the snow cover soil moisture is buffered and hence as long as*

*there are no sites that never have a snow cover the importance of this variable is not seen in the variance, but there are good reasons to explain why such a variable is not necessarily irrelevant for the control of winter respiration. Here I would argue that this justifies why you do not need to use a soil moisture term in your temperature response model (because soil moisture is not limiting thanks to sufficient winter precipitation).*

**[Response]:** Thanks for your suggestions, which we have adopted on L516-535 after the residual analysis between winter  $R_{eco}$  and winter precipitation.

**[Comment 4]:** *There is a very surprising hypothesis given in the introduction on page 7002: “Part of this soil organic carbon (SOC) mass could be decomposed more actively than fresh input (e.g. litterfall) in response to future warming.” This is not elaborated in more detail and is not discussed at all later in the text. In fact, your statement on page 7009 directly contradicts your initial hypothesis (page 7009: “Across all the sites,  $R_{ref}$  significantly increases with  $\Delta LAI$  (Fig. 2a). This indicates that substrate availability and quality exerts a significant control on the spatial variation of  $R_{ref}$  across sites, and thus supports the conclusions of Grogan and Jonasson (2005) who found that  $R_{ref}$  was significantly reduced after removing plant and litter in a birch and heath tundra.”). Why are you not clearly structuring your paper in a way where you pick up your hypotheses presented in the introduction and then clearly show the evidence supporting vs. the evidence falsifying each hypothesis?*

**[Response]:** In our analysis, winter basal respiration ( $R_{ecoref}$ ) is found to be more

correlated with  $\Delta$ LAI (or GPP\_gs) than total soil carbon content across sites (Figure 2 in revised MS). This is not inconsistent with the statement (7002/17) presented in the introduction as you mentioned, because in frozen soils, SOC buried and accumulated beneath the active layer has found to be labile and respired in case of permafrost thawing. The decomposition of this old but labile SOC is of concern for future warming (decadal scale), although this process is masked by the faster C cycling of fresh litter (seasonal to inter-annual scale). In revised MS, in order to make it more precise, we replaced 7002/16-20 with L128-131 and added L382-394 after 7009/15.

*[Comment 5]: This brings me to another weak point that the clear structure is missing that includes establishing a testable hypothesis and then trying to reject the null-hypothesis with available data. Instead the authors use the “big is beautiful” approach, telling me about 256 site years (7004/25), but e.g. in Figure 2 I did not see the 256 points, and I did not see that the benefit of so many sites led to error bars for each site.*

**[Response]:** Following your suggestion, we formulated the hypothesis that interannual temperature sensitivity is lower than the spatial sensitivity in the revised MS and put it (L161-163) after 7003/23, and rephrased the 7002/16-20 (see the response to **Comment 4**). We have also added the error bar for each site in Figures (see Figure 2 and Figure 4 in the revised MS).

*[Comment 6]: Moreover I only count 42 points, but the authors claim they used 57*

*sites (7001/18). This is indicative of a strong subjective selection bias that really could skew the statistical interpretation. Why are you only using 42 sites out of your selection of 57 sites (which already is a subset of the 255 available sites with 965 site years)? At least an objective explanation of how you removed even more sites must be given.*

**[Response]:** Actually 50 sites are plotted in Figure 2(a) of the previous MS since some of the sites are presented by overlapping symbols (deciduous broadleaf forest sites) in the graph. In the previous MS, we calculated site-specific  $R_{ecoref}$  using pooled data from all years available at each site, and two sites were excluded from the analysis because the inferred  $E_0$  from Arrhenius model goes beyond the acceptable range (0-450 kJ mol<sup>-1</sup>). Another five sites were also not considered because the MODIS-LAI data did not exist before the year 2000 (5 sites only have winter  $R_{eco}$  data before 2000). In the revised MS, site-year-specific  $R_{ecoref}$  is calculated first, and then averaged to obtain site-specific  $R_{ecoref}$ . This computation does not lead to the exclusion of the two sites, which was due to the fact that some specific site year within these two excluded sites in previous MS degraded the application of Arrhenius model when pooling data from site several years to infer  $R_{ecoref}$ . Based on this revised calculation, 52 sites are considered in the revised MS. To more easily locate each site, a corresponding index number is now marked in the graph for easy detection; secondly, the site selection criteria are explained in the materials and methods section. L273-289 was added after 7004/2 in the revised MS.

*[Comment 7]: In my view the US-Atq site that you still show in Fig. 2b but then removed from the regression analysis (page 7009) would be of more interest. Before having read your manuscript I was expecting that you will present a nice and clear assessment on how permafrost-controlled sites differ from sites without permafrost. That's what is the suggestive meaning of "mid- and high-latitudes" in the title (in my perception). But then you screen out possible indications of effects of permafrost from your analysis. Here I am really concerned if we really do not address more rigorously such key issues which are however heavily underrepresented in a data set like the La Thuile data set.*

**[Response]:** We agree with you concern that permafrost sites are heavily under-represented in the current La Thuile dataset, and winter  $R_{eco}$  controls in these regions can not be well understood based on only these two sites. Following your suggestion, we added L395-407 into revised MS where we discuss these issues.

*[Comment 8]: Overall the site selection bias is extremely strong in this paper and the interpretation further increases this skewness, such that in fact the statements relate mostly to forests. For example, 7007/11–12 states that wetlands are essentially wet tundra sites in the Arctic. But Table 1 shows that there are only 9 tundra site years vs. 7 non-tundra site years included in the analysis. Since Table 2 does not separate between different types of wetlands I do not agree with such statements. In my view "essentially" cannot possibly be synonymous to 56% (that is,  $9/(7+9)$ \_100%).*

**[Response]:** We agree with you point that the filtered sites in the La Thuile dataset for

investigating winter respiration are biased towards forest ecosystems. In revised MS, we separated the analysis of forest ecosystems from the one of croplands plus grasslands, and avoided to interpret the results regardless of distinction among vegetation types (we also explained this in the following responses). In addition, in the conclusion part, we pointed out that non-forest ecosystems are not well represented in our analysis, especially arctic ecosystems, which deserve further investigation.

We rephrased the sentence (7007/11–12) as “Both boreal and arctic wetlands have a smaller winter  $R_{eco}$  when using the definition D\_TM (151 days) compared to definition D\_AT0 (248 days)”. In the revised MS, based on the vegetation classification of the La Thuile dataset, boreal (FI-Kaa and CA-Mer) and arctic wetlands sites (US-Atq and US-Ivo) are distinguished in the Table (see Table 2 in the revised MS).

*[Comment 9]: Another critical point is 7005/13–16: “The seasonal amplitude (delta LAI) is defined as the difference between maximum and minimum of LAI and can be considered as a proxy for recent carbon inputs to soil, i.e. substrate available for sustaining winter respiration.” – do the cropland and grassland co-authors really agree with such a statement? If they do then some specific explanation should be given with respect to croplands (which are anyway heavily underrepresented) and grasslands. Currently my interpretation is that this definition automatically leads to the selection bias for forests, since delta LAI might be a weak indicator for winter*



*respiration conditions in croplands and grasslands.*

**[Response]:** Thank you for this suggestion. We checked the La Thuile ancillary dataset and found that both croplands and grasslands involved in this study are affected by human management. Meanwhile, our statistical analysis showed that: “Across sites,  $R_{ecoref}$  was found to increase with  $\Delta\text{LAI}$  (or  $\text{GPP}_{gs}$ ) both in the forests ( $\Delta\text{LAI}$ :  $r = 0.47$ ,  $p < 0.01$ ,  $n = 32$ , Fig. 2a;  $\text{GPP}_{gs}$ :  $r = 0.51$ ,  $p < 0.01$ ,  $n = 37$ , data not shown) and in GRA and CRO ( $\Delta\text{LAI}$ :  $r = 0.37$ ,  $p = 0.08$ ,  $n = 16$ , Fig. 2a;  $\text{GPP}_{gs}$ :  $r = 0.41$ ,  $p = 0.15$ ,  $n = 16$ , data not shown), respectively (Fig. 2a).”. Since a large fraction of the net primary production is exported from cropland and grassland sites,  $\Delta\text{LAI}$  (or  $\text{GPP}_{gs}$ ) may not be proportional to carbon inputs to the soil available for respiration, and a lower correlation of  $R_{ecoref}$  with  $\Delta\text{LAI}$  might thus be expected. In the revised MS, we have separated the analysis of croplands and grasslands from forests in both spatial (temperature dependency of winter  $R_{eco}$  across sites) and temporal scales (spatial vs. temporal temperature sensitivity comparison study).

**[Comment 10]:** *The main conclusion (“First, winter RECO temperature sensitivity obtained on space and temporal scales should be treated differently, since the RECO sensitivity to warming obtained from spatial gradients will definitely be exaggerated when extrapolated to future warming”) is based on very weak evidence presented in the paper. One problem I have is that the authors do not really present a clear analysis of spatial and temporal scales; they refer to these terms implicitly by assuming that the reader e.g. automatically sorts the sites according to their latitude*

*etc. I am strongly objecting to such implicit argumentation: if this is the main conclusion we need to see serious and careful analysis of (a) the spatial gradient and (here I have one more question mark) (b) temporal gradient.*

**[Response]:** In the revised MS, we perform further analysis of winter  $R_{eco}$  spatial vs. temporal temperature sensitivity according to different winter definitions and different vegetation types. The inferred difference between the two temperature sensitivities was found for all winter definitions and for all vegetation types except the wetlands (Figure 2 in revised MS). This might be expected since the small samples ( $n = 4$ ) in wetlands might not be enough for a robust statistical analysis. To figure out the possible reason why there exist differences between spatial and temporal temperature sensitivities, we fitted daily winter  $R_{eco}$  divided by site-specific  $R_{ecoref}$  (i.e. normalized  $R_{eco}$ ; see Sect. 3.2.2 in revised MS) against air or soil temperature using the Arrhenius function. The inferred activation energy ( $E_0$ ) is smaller than without normalization, a result robust to different winter definitions. This supports our hypothesis that the spatial temperature sensitivity not only accounts for gradients of climate affecting decomposition, but also reflects gradients in ecosystem state (e.g. soil C pools) in space. Simply projecting winter  $R_{eco}$  in response to near future warming based on spatial relationships can therefore overestimate CO<sub>2</sub> losses by ecosystems, and future climate feedbacks. In response to your question, we replaced the paragraph 7011/6-15 with L439-454 and added L290-297 and L459-476 after 7006/2 and 7011/20, respectively.

*[Comment 11]: Your conclusions are further weakened by the fact that you consider the two parameters in the Lloyd and Taylor model as independent from each other and hence discuss them separately without considering the probably high correlation between them (7009). This appears quite misleading unless you can document a low correlation between the two parameters (which you most likely can not, given Fig. 2). In my view the overall effect with respect to anticipated climate change is not at all as clear as you try to phrase your conclusions you draw from this analysis.*

**[Response]:** We analyzed the covariance between the two parameters ( $E_0$  and  $R_{ecoref}$ ), and found that they are weakly negatively correlated in D\_TM ( $r = 0.30, p < 0.01, n = 57$ ), D\_AT0 ( $r = 0.22, p < 0.05, n = 57$ ) and D\_AT-2 ( $r = 0.20, p < 0.05, n = 54$ ) and not significantly correlated in both D\_AT-5 ( $r = 0.14, p = 0.46, n = 43$ ) and D\_AT-10 ( $r = 0.14, p = 0.33, n = 27$ ). Meanwhile, in our new analysis, we found that the weak negative  $E_0$ -temperature correlation was not consistent among different winter definitions (see the response to **Comment 25**). Thus we have removed the conclusion in the previous MS that an increase of winter  $R_{eco}$  with temperature might be dampened in previous MS, which is drawn from a negative  $E_0$ -temperature relationship, although low correlation is observed between  $E_0$  and  $R_{ecoref}$ .

*[Comment 12]: 6998: affiliation 16 is next door from my own office, but the associated co-author as not been seen here in the past few years. Maybe there are other such errors, please check your names and affiliation*

**[Response]:** Thanks for your reminding. The names and affiliations of coauthors were

checked in revised MS.

*[Comment 13]: Abstract, 7001: after having read the whole paper I saw that you multiplied your numbers by ten (never ever do something like this!) in Table 2 and that's why these winter fluxes are way too high, in fact one order of magnitude too high.*

**[Response]:** We apologize for this typing error which has been corrected in the revised manuscript.

*[Comment 14]: Abstract, 7001:25–28: what do you want to express with a phrase like “The increase in winter RECO with a 1°C warming based calculated from the spatial analysis was almost that double that calculated from the temporal analysis.”? This is not intelligible to the general reader (see guidelines).*

**[Response]:** In revised MS, we have a further investigation into this issue (see the response to *Comment 10*). We replaced the sentence (P7001/25-28) with L108-112.

*[Comment 15]: Abstract, 7001/8: ratios never have units; here you wanted to write “rates”.*

**[Response]:** We changed the term ‘ratios’ to ‘rates’.

*[Comment 16]: Use SI units (e.g. <http://physics.nist.gov/cuu/Units>, link SI units and link prefixes. See also the special note on degree Celsius). That means that K must be*

*used at many places where you use °C (e.g. 7003/16). That also means that at places where you use K but wanted to use the prefix k for kilo you need to correct the error (e.g. 7001/21, 7005/22 and more). Please also check the definition of temperatures in the SI document referred to above; you seem to mix up the freezing point of water (273.15 K) with the tripple point of water (273.16 K). This must be corrected (e.g. 7005/22). Use the approximate sign ( $\approx$ ) in place of the proportionality sign ( $\sim$ )*

**[Response]:** Thanks a lot for your detailed suggestions. We corrected the prefix k for kilo, and changed the triple point of water (273.16 K) to the freezing point of water (273.15 K). We checked the papers published in both BG and BGD, approximate sign ( $\sim$ ) instead of ( $\approx$ ) is used. In addition, for convenience, we kept the SI-derived unit (°C) for temperature. Following the link you suggested (<http://physics.nist.gov/cuu/Units>), we found also that: ‘...temperature differences or temperature intervals may be expressed in either the degree Celsius or the kelvin using the same numerical value’.

**[Comment 17]:** *I am not happy with the mixture of symbolic writing with abbreviations. The two established ways to refer to ecosystem respiration is either as a symbol (with subscript),  $R_{eco}$  or as an abbreviation TER where each letter represents a word (Terrestrial Ecosystem Respiration). Your RECO is a mixture of both which I am not in favor of. Use one of the established versions instead.*

**[Response]:** We have changed “RECO” to “ $R_{eco}$ ” in the revised MS.

*[Comment 18]: The p-ratio is an utterly confusing concept because it is easily mixed up with the statistical p-value. This is most pronounced when you sloppily write of the p-ratio value (7007/22) and moreover express it a percentage instead of ratios. A better and nonconfusing symbol and wording must be found. The same applies to the p0-ratio (7007). Here I was even unable to really grasp its definition. Should it be **mean** winter Reco rates to annual mean Reco? The confusion exists, since your p-ratio is defined based on cumulative winter Reco. Please clarify this. In my view the two ratios only differ by the inclusion of the winter duration variable, hence it is associated with your problematic definition of winter DI.*

**[Response]:** In revised MS, the p-ratio (the Ratio of Winter Cumulated  $R_{eco}$  to annual cumulative  $R_{eco}$ ) is changed into RWCR and the p'-ratio (the Ratio of Winter  $R_{eco}$  Rate to annual  $R_{eco}$  rate) is converted to RWRR. The introduction of the RWRR in our analysis stems from the idea that it might represent the overall soil microbial activity for decomposition during the winter season relative to the annual level. Though RWCR only differ from RWRR by the inclusion of winter duration, providing RWCR value for each ecosystem type can indicate the contribution of winter season to the annual carbon budget. In higher latitudes, microbial activity might be greatly constrained by low temperature or other conditions (low RWRR), but this sustained small respiration rate can account for a significant share of the annual carbon release due to the prolonged winter duration (high RWCR). We have put L256-263 as a new section (Sect. 2.3.1).

*[Comment 19]: With respect to the winter definition I of course agree that there is need for critical consideration of how to lump data for the analysis. The climatological definition has the advantage that it has a clear duration and hence the variable of true length of winter would be a second (independent) variable. But here I do not see the value of your definition because in reality for plants and ecosystem the length for each specific year appears to be important, not the mean duration or mean temperature (7006/13) of a few to a few more years (max. 10 in your study). Here I see a serious flaw in your concept that in my view does not really advance our understanding.*

**[Response]:** In the previous MS, given large year-to-year climate variability at some sites, adoption of mean winter duration provides a simpler metrics to compare respiration across sites and across years. In revised MS, following your suggestion, we however calculate winter duration each year, for to different temperature thresholds (-10 °C, -5 °C, -2 °C and 0 °C). We acknowledge that such a choice is coherent with our will to focus on a process-driven definition of winter. See details in the response to *Comment 1*. Based on this alternative definition, 218 rather than 256 site years are included in the revised MS.

*[Comment 20]: Moreover the definition of D1 is entirely unclear with respect to which data granulation was used. 7004/7–8 mentions that you used half-hourly and daily values; does this mean that you have two different definitions of D1, one derived from half-hourly data (where a single 30-minute value above your threshold might*

*exclude a full 10-day period from being considered winter) and one for daily aggregated data (where a short warm period during peak daytime may still not elevate the daily mean above your threshold)?*

**[Response]:** Sorry that we did not make it clear. Winter season definition throughout the whole paper is based on daily values rather than half-hourly ones. Since daytime  $R_{eco}$  is separated from NEE based on the temperature sensitivity of nighttime NEE in La Thuile dataset, we use nighttime NEE and temperature to investigate temperature dependency of winter  $R_{eco}$  within each site year. The referred winter season is defined according to daily temperature values. We clarified it in the revised MS on L283-284.

*[Comment 21]: Equations should be presented in journal style mode, not FORTRAN source code, same for numbers (e.g. 7011/9–11). Moreover, the use of brackets in equations must be homogenized and corrected.*

**[Response]:** We corrected them in revised MS.

*[Comment 22]: Numbers should be rounded to their significant digits. In this example 6E-18 is probably just the internal digital resolution of the variable type used in the analysis. Standard computers typically use 16 significant digits for floating point variables, so such a number in a statistical analysis is rather unlikely to be significantly different from zero. Specifying the standard error of parameter estimates would probably directly have helped to see the uncertainty of these estimates. My suggestion is to add the standard errors to all parameter estimates and more carefully*



*inspect the statistical output before copying the information to a manuscript.*

**[Response]:** Thanks for your suggestion. In the revised MS, we rounded the numbers in figures, tables and main text to their significant digits and also added the standard errors to the parameter estimates (e.g. Fig. 4c and 4d in the revised MS).

*[Comment 23]: Language, 7008: “It should be noted that the use of the open-path gas analyzers for eddy covariance estimates of small fluxes relying on the WPL correction (Webb et al., 1998) can introduce the errors (e.g. Kondo and Tsukamoto, 2007), and CO<sub>2</sub> releases can be systematic underestimated” – please improve*

**[Response]:** We have added a new paragraph addressing the issue of self-heating for open-path gas analyzers in L200-218.

*[Comment 24]: 7009/18: what if US-Atq is included? Would it still be significant or would it tell you that you have to consider permafrost vs. non-permafrost sites as one of the key controls for winter respiration? On 7012/4 you even exclude yet another elucidating site, US-Ivo –would you not agree with me that these sites clearly indicate that permafrost vs. nonpermafrost conditions would be a key issue to address in a paper that claims to focus on controls on winter respiration?*

**[Response]:** When using D\_AT0 (in revised MS) or D\_AT0\_p (characterized by constant winter duration for each site as done in the previous MS),  $E_0$  and soil temperature would not be significantly correlated if US-Atq was included into the regression analysis. We agree with you that two arctic permafrost sites (US-Atq and

US-Ivo) can convey some information of winter  $R_{eco}$  controls in permafrost regions. Compared to mostly upland ecosystems, permafrost sites are under both deep freezing and anaerobic conditions (Davidson et al., 2006) during the winter season and low apparent activation energy is thus inferred. Our understanding of winter C processes in permafrost regions is however restricted, given the sample of only two such sites in La Thuile dataset. The dominant winter C controls might vary with permafrost type (e.g. continuous, discontinuous, and sporadic), vegetation type on the permafrost (e.g. Eugster et al., 2005), and in particular the different responses to freezing of oxic and anoxic systems underlain by permafrost and these should deserve further study. We added this discussion in the revised MS but could not make an extensive analysis with only two permafrost sites (See details in the response to **Comment 7**).

*[Comment 25]: 7010/4–7: “Extrapolating the relationship in Fig. 2 from space to time would imply that the future warming trends reduce the activation energy of winter soil C decomposition, hence dampening the potential increase of RECO with temperature.” This would be important to do if you want to support your conclusion, but why do you not show this analysis/extrapolation to convince the reader that there is evidence in your data set to accept (not reject) your hypothesis?*

**[Response]:** Consistent with previous analysis, significant but weak correlation between  $E_0$  and soil temperature is obtained using D\_AT0 ( $r = 0.22$ ,  $p < 0.05$ ,  $n = 57$ ;  $r = 0.03$ ,  $p = 0.81$ ,  $n = 56$ ) only if we exclude the permafrost site (US-Atq) from the analysis. However, this significant correlation disappears using other winter

definitions (D\_TM:  $r = 0.24$ ,  $p < 0.05$ ,  $n = 57$ ; D\_AT-2:  $r = 0.10$ ,  $p = 0.38$ ,  $n = 54$ , D\_AT-5:  $r = 0.04$ ,  $p = 0.23$ ,  $n = 43$ , and D\_AT-10:  $r = 0.20$ ,  $p = 0.67$ ,  $n = 27$ ) even if US-Atq site is excluded from the analysis.  $E_0$ -soil temperature relation is not consistent across different winter definitions, and the statement based on their negative correlation in previous MS was removed from the revised MS.

*[Comment 26]: 7010/20: Here comes snow cover as a quick side aspect. But why did you not use the albedo measurements in the La Thuile data set for sites that have them, and combine this with albedo values from a similar data product as you used for  $\Delta LAI$ ? In my view this would have strengthened your interpretation. The structure of the manuscript needs improvement.*

**[Response]:** The reasons why we did not use albedo values to investigate snow effects on winter Reco are listed as follows. Firstly, the snow information which can be obtained from albedo measurements is the start and end of snow cover duration (or snow phenology). However, the acquisitions of snow phenology from continuous albedo values of La Thuile dataset or satellite-derived albedo products are questionable in evergreen forest ecosystems. Since under-canopy snow is more sheltered from wind speed and solar radiation (e.g. Davis et al., 1997; Pomeroy et al., 2008; Rutter et al., 2009), snow melting on the forest floor is largely delayed in comparison to the snow that remains on the leaves and branches. Both incoming and outgoing shortwave radiation are measured above the canopy in the La Thuile dataset (sometimes with gaps) and this can result in biased snow start and end dates on the

ground floor with which we are more concerned. Satellite-derived albedo products, besides the above-mentioned problem, also represent a large area (e.g. 1 km by 1 km for MODIS data) than the footprint of eddy covariance towers, and the tower albedo differs from the site albedo function of site heterogeneity.

Secondly, the transmission of air temperature extremes to the soil can be dampened in the presence of snow cover, and the difference between air and soil temperature depends on snow thickness. Even the defensible start and end date of snow cover can be obtained, snow effects on winter  $R_{eco}$  are more evident through snow depth and conductivity than through dates of snow accumulation and snow melt (as inferred from albedo). Many snow manipulation and field studies documented the importance of snow depth in regulation of winter soil respiration (e.g. Schimel et al., 2003; Monson et al., 2006; Nobrega and Grogan, 2007; Morgner et al., 2010). Unfortunately in-situ snow depth measurement at site level is not available in this study. Snow cover duration (as inferred from albedo) is not well related to soil decomposition processes. Recent studies showed that its importance might be more seen in the regulation of growing season carbon cycle (e.g. Vaganov et al., 1999; Kirdeyanov et al., 2003; Aurela, 2004; Groendahl et al., 2007). We added L577-584 in the conclusion to state poor representation of snow behaviors in this study.

*[Comment 27]: As an example I put a big question mark on 7009/12, where you very surprisingly write “This indicates that substrate availability and quality exerts a significant control”. This statement was purely based on the fact that Lloyd and*

*Taylor mentioned  $R_{ref}$  to be a function of substrate availability and quality. But you cannot draw the inverse conclusion that if you see an effect in  $R_{ref}$ , then this must be both factors. So far you have only provided a variable on substrate availability ( $\Delta LAI$ ) but none on quality. This is not the correct deductive approach for interpreting statistical results.*

**[Response]:** Thanks for drawing our attention to this issue in our initial manuscript. We searched through the La Thuile ancillary database for the litter quality index (for example, litter nitrogen); however, the number of samples present in the dataset ( $n = 4$ ) is not enough for robust analysis between  $R_{coref}$  and litter quality. Accordingly we have removed the words ‘substrate quality’ in the revised MS.

***[Comment 28]:** In general your wording should be more careful with statistical analyses. If you make a variance analysis and you do not find variance, then this does not mean that a variable (like snow cover) is not important, it just means that it does not express its importance in the variance among sites. There is no causal relationship in statistical analysis, the causal relationships (“controls”) must be established on physical and biological considerations. In your paper I have the impression that you tried to deduce all controls from statistical considerations alone.*

**[Response]:** Thanks for your suggestion. In the revised MS, we paid more attention to the wording after the statistical analysis and also proposed a null hypothesis to test before the analysis thanks to **Comment 5**.

*[Comment 29]: Another statistical problem that you do not address with one single word is the significance of ratios. A simple t-test is not a correct measure for ratios, unless you make a couple of assumptions. Since it has been shown that the t-test is rather robust against many violations about its assumptions, this is not a big deal, but as a reviewer I would have gotten more confidence that you actually understood your statistical analyses if you had critically discussed the limitations of your approach or warned the reader about the shortcomings that such an analysis has.*

**[Response]:** Thanks for your advice. In the revised MS, we test the normality of the data using a one-sample Kolmogorov-Smirnov test (K-S test) without the Dallal-Wilkinson-Lilliefors corrected  $p$  value and found that the pooled data from all of the site years are not significantly different from normal distribution except winter  $R_{eco}$  rates ( $\text{g C m}^{-2} \text{ d}^{-1}$ ) in D\_TM (D\_AT0: winter cumulative  $R_{eco}$ :  $p = 0.130$ ; winter  $R_{eco}$  rates:  $p = 0.062$ ; RWCR:  $p = 0.059$ ; RWRR:  $p = 0.140$ ,  $n = 218$ ; D\_TM: winter cumulative  $R_{eco}$ :  $p = 0.068$ ; winter  $R_{eco}$  rates:  $p = 0.013$ ; RWCR:  $p = 0.171$ ; RWRR:  $p = 0.091$ ,  $n = 218$ ). However, if the K-S test with  $p$  value correction is used, the data in all the cases did not conform to the normal distribution. In the revised MS, we still keep the parametric ANOVA analysis and possible violation of t-test assumptions is discussed. We have added L846-849 in the footnote of Table 2 in the revised MS.

*[Comment 30]: 7012/15: typo on units of  $\Delta\text{LAI}$*

**[Response]:** We corrected it.

*[Comment 31]: 7012: use subscripts for indices of parameters both in equation and text (no source code!)*

**[Response]:** We corrected them in the revised MS.

*[Comment 32]: Table 1: inconsistent rounding of numbers. Recall that the convention is that the mean and SD or SE must have the same number of significant digits, and if only the mean is given, then the last shown digit must be significant (and all significant digits must be shown!)*

**[Response]:** Thanks for your suggestion. We rectified them in the revised MS.

*[Comment 33]: Table 1: why is there a footnote sign (1) for Type, but the footnote sign is not found in the footnotes?*

**[Response]:** We corrected it in the revised MS.

*[Comment 34]: Table 1: what does the asterisk denote?*

**[Response]:** The asterisk denotes the open-path instrument installed for the eddy covariance site, which was missing ahead of its explaining footnote in Table 1 of previous MS. We have already added it in the revised MS.

*[Comment 35]: Table 2: you surprised yourself with multiplying numbers by 10. This must be avoided. If necessary it would be smart to use an SI prefix (but in this case it would be quite exotic to use deca-grams)*

**[Response]:** In revised MS, we kept the original daily winter  $R_{eco}$  values instead of using any prefix.

*[Comment 36]: Table 2: since I did not grasp your definition of p0-ratios I wonder how the big differences between p-ratios and p0-ratios should be interpreted. An assessment would only be possible with a clear definition.*

**[Response]:** We clarified it in the revised MS. See details in the response to **Comment 18**.

*[Comment 37]: Figure 1: misleading display of histograms: (i) Histogram bars have gaps between them; (ii) It is not clear whether the histogram bars are placed in the correct interval; (iii) The x-axis range is 0–30 for D1 but 0–20 for D2 which does not allow a comparison; (iv) In the same panels the intervals are chosen differently for D1 and D2 which also does not allow for a comparison.*

**[Response]:** Following your suggestion, we redrew the Figure 1 in the revised MS.

*[Comment 38]: Figure 2: leading zeros before decimal points missing; panel labels at unorthodox locations; caption unclear, not standalone (points seem to be a further selection and an average of years without range bars)*

**[Response]:** We redefined Figure 2 in the revised MS. In this new figure, we added the leading zeros before decimal points and rephrased the caption. The error bar is also added, and the explanation in data selection can be found in our response to



*Comment 6.*

**Responses to Referee 2 comments:**

*[General comment]: The topic of winter time CO<sub>2</sub> exchange is in my opinion one of the most interesting unknowns of the high northern latitudes, which is certainly not fully understood at this point. The ms use the La Thuile dataset from the fluxnet database with 57 sites in the Arctic, Boreal and Temperate region, covered with eddy covariance data to analyse winter-time exchange of CO<sub>2</sub>, which seems like an obviously good approach to illustrate this problem. Unfortunate, the manuscript in my opinion suffers from a number of weaknesses in the analysis, which together with awkward definitions of key constrains, makes the manuscript unsuited for publication at the present stage. The ms uses the classical Arrhenius type equations in the interpretation of the ecosystem respiration during the winter, which seem as a natural point of departure, but does not really add to the present state knowledge, dating back to Lloyd and Taylor from 1994.*

**[Response]:** We followed the path of previous studies to use Arrhenius model in the documentation of  $R_{eco}$ -temperature relationship, but site productivity variable is also added to the classic Arrhenius empirical model. This model's applicability was not tested against Fluxnet data specifically for winter respiration before our study. From another perspective, our work demonstrates the applicability and the extension of this classic model to the frozen season, which has widely been tested in the growing

season and the laboratory incubation studies using frozen soil (e.g. Mikan et al., 2002). Other issues from the application of the model are explored in the revised MS, e.g. the comparison between two different spatial temperature sensitivities using the Arrhenius model. We thus think that our study contributes to the advancement in this field.

*[Comment 1]: Abstract: Respiratory fluxes during winter of more than 10 g C m<sup>-2</sup> per day is simply not realistic and shows a lack of experience within this field of research. One must assume that the long list of very experienced never saw this part of the ms and I would recommend that the first author involve co-authors more actively in a new version of the manuscript.*

**[Response]:** We corrected this error of typo in the revised MS. In addition, this revised MS has also benefited from the active involvement of some of our co-authors.

*[Comment 2]: Introduction: In order to compare the winter fluxes across a wide range of latitudes one needs a proper definition a winter season as a term, I'll agree to that. However, it seems to me that the latitudinal range of 34° and a temperature gradient of 24° C may be too much to illustrate the issue of cold season respiration, in a proper way, from just a few environmental parameters. This could be one of the reasons why the correspondence between RECO and e.g. temperatures in most cases is not better than the original version of the Lloyd and Taylor equation. I would suggest that a narrower range in both temperature and latitude was used to investigate this issue, as well as focus on the northernmost end of the latitudes to*

*distinguish this study from the original Lloyd and Taylor work and many of those who followed. I do realize that this will decrease the number of data, but would assume that a more focused approach could also give more qualified explanations.*

**[Response]:** Following your suggestion, we considered five winter season definitions based on four air temperature thresholds (0 °C [D\_AT0], -2 °C [D\_AT-2], -5 °C [D\_AT-5] and -10 °C [D\_AT-10]) and one temporal duration (D-TM, December through February). We also distinguish forests from croplands-and-grasslands. Our results show that the predictive power of temperature (air and soil) in explaining winter  $R_{eco}$  decreased as winter became colder (see Figure 4 in revised MS), for example, in forest ecosystems the coefficient of determination decreased from 0.64-0.68 (D\_AT0) to 0.50-0.54 (D\_AT-10). The relationship between winter  $R_{eco}$  and temperature is worse for croplands and grasslands compared to forest, e.g. D\_AT0 using air temperature:  $r^2 = 0.17$ ,  $n = 16$ ; D\_AT0 using soil temperature:  $r^2 = 0.30$ ,  $n = 16$ ).

Following your suggestion, we also investigated the temperature dependency (e.g. using air temperature) of winter  $R_{eco}$  in three different latitude bands (30-45° N, 45-53° N, and >53° N) (Figure 1a and 1c in the responses). Since data splitting by latitude can still not avoid the overlap of temperature between latitude bands, we further divided the data into four bands according to both latitude and temperature. However, as shown in Figure 1b and 1d (in the responses), this distinction did not make the correspondence between winter  $R_{eco}$  and temperature much better than when all the data are used together. One of the possibilities is that the uncertainties in winter

C fluxes measurement can become larger when the true fluxes are smaller (Berger et al., 2001). This distinction is thus not added into the manuscript. In the revised MS, winter  $R_{eco}$ -temperature analysis based on different winter definitions and the distinction between forests and human-managed croplands and grasslands are presented.

*[Comment 3]: I assume that the dataset used here are not gap-filled- it doesn't say so anywhere. Using criteria of less than 30% gaps in the annual dataset could still allow substantial gaps during winter, and I would suggest that the 30% limited was applied to the winter season only.*

**[Response]:** The quality-assessed La Thuile dataset is already gap-filled using the techniques described in Papale et al. (2006), Reichstein et al. (2005), Moffat et al. (2007) and Papale and Valentini (2003), and the diagram showing the data processing can be found in the link:

[http://www.fluxdata.org/DataInfo/Dataset%20Doc%20Lib/data\\_proc\\_scheme.pdf](http://www.fluxdata.org/DataInfo/Dataset%20Doc%20Lib/data_proc_scheme.pdf). However, there are still data gaps in the dataset due to missing meteorological data induced by instrumental failure. We only restricted the analysis to eddy covariance sites where both annual data gap and winter data gaps are within 30%. We clarify our approach in the revised MS on L191-195.

*[Comment 4]: Modis LAI data might be the best choice, but there is a scaling issue which is not dealt with, as it should be. I would suppose that significant variability in*

*LAI exist a most sites and a spatial resolution of 1 km<sup>2</sup> cannot resolve that, at the eddy covariance scale, this should be discussed .*

**[Response]:** Thanks for your suggestion! We found that the agreement ( $r^2$ ) between satellite and in-situ LAI measurements is 0.48, which prompted us to use site level GPP<sub>gs</sub> (growing season gross primary productivity) as another proxy of recent carbon input to the soil. We added L231-241 after 7005/16 in our revised MS.

*[Comment 5]: I find the use of two different definitions of winter confusing, especially since none of them really takes into account that winter last longer at higher latitudes, than at lower.*

**[Response]:** In revised MS, we consider five types of winter definitions: D\_TM: climatological winter season (December, January, and February) and air-temperature based winter season (four temperature thresholds are chosen: 0 °C [D\_AT0], -2 °C [D\_AT-2], -5 °C [D\_AT-5] and -10 °C [D\_AT-10]). Our regression analysis showed that the winter duration increases with increasing latitude, when using any of the temperature-based definition (**D\_AT0**:  $WLEN = 3.2LAT - 33.9$ ,  $r^2 = 0.26$ ,  $p < 0.01$ ,  $n = 57$ ; **D\_AT-2**:  $WLEN = 3.4LAT - 60.7$ ,  $r^2 = 0.30$ ,  $p < 0.01$ ,  $n = 54$ ; **D\_AT-5**:  $WLEN = 3.8LAT - 95.9$ ,  $r^2 = 0.36$ ,  $p < 0.01$ ,  $n = 47$ ; **D\_AT-10**:  $WLEN = 3.5LAT - 100.4$ ,  $r^2 = 0.49$ ,  $p < 0.01$ ,  $n = 27$ ). *WLEN* and *LAT* represent winter duration and latitude respectively.

*[Comment 6]: The use of the 10 times (though it says 0.1 in the table) multiplication*

*in table 2 for daily values is confusing, and seems to confuse yourself as it leads to the erratic values in the daily RECO in the abstract.*

**[Response]:** Sorry for this mistake. We corrected this in the abstract of our revised MS.

*[Comment 7]: Use of the term p-ratio is confusing, because it is easily mixes up with the p-value used in statistics. Further, it is not clear to me exactly what you want to illustrate with this ratio, please explain more thoroughly.*

**[Response]:** We changed the term p-ratio into RWCR in revised MS. The reason why we introduce RWCR is based on the idea that providing this ratio for each ecosystem type is a measure of the role of winter carbon losses in annual respiration losses, allowing to compare sites between each other, for instance. Following your suggestion, we added L256-263 as a new section (Sect. 2.3.1) in the revised MS.

*[Comment 8]: On page 7007 you argue that Arctic wet tundra comes out with the lowest RECO, due to the low soil temperatures at the sites. I do agree that this is part of the reason, but more importantly the high standing water table of wetlands makes most of the decomposition anaerobic, and thus less efficient.*

**[Response]:** Thanks to your suggestion, we added L325-328 after 7007/15.

*[Comment 9]: Fig. 3 though trend lines are presumably significant, the explained variances are all very weak, and can in my opinion on not make basis for any*

*conclusions.*

**[Response]:** We agree on that the explained variance is very weak, which might not be sufficient for the conclusion we made in the previous version of the MS. So in the revised MS, the implications drawn from this relationship were not considered as one of the main conclusions. In response to your question, we removed Figure 3 of the previous MS, the sentence “*The interannual variability of winter RECO is better explained by soil temperature than by air temperature*” from the Abstract and both 7013/26-27 and 7014/1-6 in the conclusion part. In addition, we also emphasize the weakness of this relationship and pay more attention to the wording from this statistical analysis in section 3.2.2.

**[Comment 10]:** *On Page 7011 the authors conclude that the sensitivity of RECO to temperature is different when looking at spatial gradient from interannual variability. Basically, I don't see any evidence of that in the material presented here or in the discussion just above this statement.*

**[Response]:** In the revised MS, a detailed analysis between the two temperature sensitivities (for different winter definitions and different PFTs) is presented. Details can be seen in revised MS or in the response to **Comment 10** by reviewer #1.

**[Comment 11]:** *I do not see the logic behind the statements made, that you do not find any relation between soil carbon content (concentration) but still choose to use LAI as a proxy for substrate. Please, explain the hypothesis behind those apparently*

*contradicting statements.*

**[Response]:** In our previous MS, due to a lack of distinction among different ecosystems, we found that no correlation existed between  $R_{ecoref}$  and total soil carbon content. Total soil carbon content reflects the fraction slow and passive compounds which do not contribute much to  $R_{eco}$ . On the other hand, LAI was chosen as a proxy of fresh litter and more easily decomposable organic carbon that determines cross sites reference  $R_{eco}$  rates. In the revised MS, given that croplands and grasslands are affected by management, we separate the forests from non-forest ecosystems.  $R_{ecoref}$  is found to marginally correlate with total soil carbon concentration in the forest ecosystems ( $r = 0.52$ ,  $p = 0.08$ ,  $n = 15$ , Figure 2b in revised MS) if the site US-Ha1 with the largest standard error is not considered. We did not perform the same analysis for croplands and grasslands due to limited sample size ( $n = 5$ ). Meanwhile,  $\Delta\text{LAI}$  ( $r = 0.47$ ,  $p < 0.01$ ) was better correlated with  $R_{ecoref}$  than total soil carbon content, which might support Grogan et al. (2001) who found that winter soil respiration is to a larger extent from more easily decomposable carbon (e.g. litter) than bulk soil organic matter. In response to your question, we added L382-394 after 7009/15.

**[Comment 12]:** *The differences in explained variance going from temperatures alone to equations (2) and (3) seem to be only marginally better than for temperatures alone.*

**[Response]:** In the revised MS, we presented the fitting statistics for the Arrhenius model with and without  $\Delta\text{LAI}$  (or  $\text{GPP}_{gs}$ ) as a predictor. Our results showed that the



addition of  $\Delta\text{LAI}$  or  $\text{GPP}_{\text{gs}}$  can only make winter  $R_{\text{eco}}$  prediction marginally better especially in D\_TM, D\_AT0 and D\_AT2 in the forest ecosystems (Figure 4c and 4d in revised MS) as you mentioned. This might be related to the possibility that aboveground respiration can account for winter  $R_{\text{eco}}$ , which reduce the dependency of winter  $R_{\text{eco}}$  on substrates, directly feeding soil microbes. It is true that temperature (air and soil) make a disproportionate large contribution to the explained variance across sites in winter  $R_{\text{eco}}$  compared to  $\Delta\text{LAI}$  (or  $\text{GPP}_{\text{gs}}$ ). However, including substrate availability (indexed by  $\Delta\text{LAI}$  or  $\text{GPP}_{\text{gs}}$ ) into the Arrhenius model is based on the biological process that substrate availability can affect soil basal respiration, which was not only found by our cross-site analysis but also supported by many laboratory and field studies (e.g. Grogan and Jonasson, 2005; Elberling. 2007). Thus, we added L484-488 and L536-549 in the revised MS.

*[Comment 13]: Technical issues: The authors comments (p7008) on the influence of the “Burba correction”, which is not a fully resolved issue at this point, but do not distinguish between observations from open versus closed path instruments or whether the correction was applied or not. In my opinion this could be done and may potentially explain some of unexplained variance found in the data*

**[Response]:** See reply to **Comment 23** by reviewer #1.

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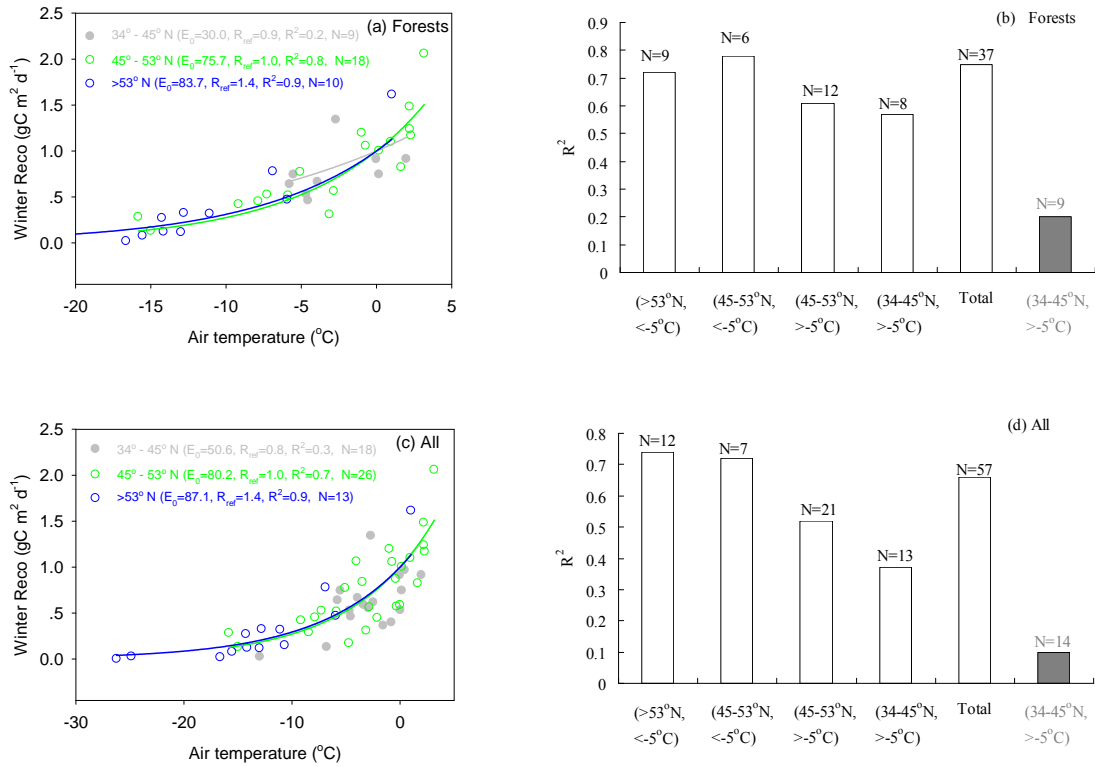
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**Figure 1.** The relationship between winter  $R_{eco}$  and air temperature depicted by Arrhenius function in three latitude bands ( $30^{\circ}$ - $45^{\circ}\text{N}$ ,  $45$ - $53^{\circ}\text{N}$ , and  $>53^{\circ}\text{N}$ ) were shown in the forest sites (a) and all the sites (c); The comparison of goodness-of-fit ( $R^2$ ) among four bands categorized by both latitude and temperature using the forest sites (b) and all sites (d) are also shown.