Response to anonymous Referee #1

We welcome this reviewer's thoughtful comments and suggestions and have provided responses to individual comments below. Our line-by-line responses describe changes that will be submitted in the revised manuscript.

General comments

This manuscript presents a unique and important dataset of ï¿ A˘D 14C-CO2 soil and ecosystem respiration from a high-latitude polygonal tundra site. The data are important because old permafrost C stocks could be released to the atmosphere. Overall the paper is well written although I think that it would be helpful to have more background about the ecosystem and context for why the different polygonal tundra features might affect ï¿ A˘D 14C-CO2. Specifically, what might explain some of the ï¿ A˘D 14C differences shown in Figure 1 and 2? If thaw depth does not explain the differences, is there anything else that might? Moisture, vegetation type, organic soil content, amount of cryotrubation?

At the reviewer's request, we have added a paragraph to the introduction providing further background on this ecosystem and its polygonal tundra features. In particular, we have added a brief discussion of how microtopography, hydrology, vegetation, and cryoturbation might influence carbon cycling rates in polygon tundra. In addition to this introductory material, we also added text to the discussion regarding other ecological properties that may explain the observed $\Delta^{14}C$ differences:

"At the scale of an individual soil profile, seasonal variations in $\Delta^{14}C_{Reco}$ correspond with changes in thaw depth. At the site scale, however, thaw depth may not be a useful predictor of spatial variations in $\Delta^{14}C_{Reco}$. Instead, the spatial distribution of carbon cycling rates may be more directly influenced by profile-specific properties that do not necessarily correlate with thaw depth. These factors—such as organic layer thickness, vegetation composition, productivity and rooting depth, the presence of cryoturbation, and oxygen availability to decomposers—vary according to polygon morphology (Newman et al., 2015; Ping et al., 2015; Sloan et al., 2014; Vaughn et al., 2016). Accordingly, these properties may underlie the differences in $\Delta^{14}C_{Reco}$ we observed in September 2014 between HC polygons and the other polygon types (Fig. 2)."

The respired and profile ï¿ A˘D 14C measurements are a very nice complement to each other because they really let us understand production vs. release mechanisms for soil C. One challenge with the profile data is that we don't know the time scale over which the old 14C has accumulated. There could be lots of really old 14C because of slow diffusion rates and high accumulation. For example (Lee et al., 2010) measure very high Co2 concentrations deep in the soil profile and attribute this to low diffusion rates rather than high production rates. Of course this is difficult to solve and perhaps under steady-state assumption the accumulated CO2 is constantly being produced and diffusing out of the soil profile, and the 14C reflects the decomposability of old C and therefore it's eventual release to the atmosphere. Perhaps the authors could add 1-2 sentences about this,

simply to point out some of the complexities with interpreting the data.

We agree with the reviewer that profile data are challenging to interpret because of unknown diffusion and accumulation rates and vertical mixing between depths. Because of these challenges, rates of old carbon release in soil surface emissions do not directly indicate when and how rapidly old carbon at depth is decomposing, nor do the depths of pore-space measurements necessarily correspond to the depths of CO_2 production. When interpreting soil pore-space data and linking it to co-located surface fluxes, it is important to consider both of these issues. Given the scope of our data, we were careful not to infer absolute rates or specific depths of old CO_2 production, but agree that it is worth addressing this point more clearly. We have added two passages discussing aspects of this issue in the manuscript's discussion:

"Detecting and characterizing the decomposition of older, deeper soil organic carbon requires direct measurements of soil pore-space CO_2. Such measurements provide a qualitative indicator of old carbon decomposition; as with surface CO_2 effluxes, proportional or absolute contributions from distinct carbon pools cannot be calculated without well-resolved vertical distributions of ^{14}C and ^{13}C source pools, as well as gas transport within the profile."

"Because of unknown diffusion rates within the soil profile, it is challenging to quantitatively use pore-space CO_2 measurements to link soil carbon cycling rates to soil surface $\Delta^{14}C_{Reco}$. Low diffusion rates can lead to the accumulation of high concentrations of ^{14}C-depleted, slow-cycling CO_2 deep in the soil profile (Lee et al., 2010), and vertical mixing can transport CO_2 away from the site of production prior to its collection as soil pore gas."

A few additional comments below point out a few places where more consistent data presentation would make the manuscript more reader friendly and reduce some confusion that I encountered. Beyond that, I think this manuscript documents an important and interesting data set and should be published.

Data and code could be accessed with the DOI and links provided!

Specific comments

Overall data presentation

Year/month is inconsistent. Sometimes month is reported with year, sometimes without. For example, figure 1 ignores years, while figure 4 explicitly represents years. That's confusing. How important do the authors think that year is? Can year be left out?

Our choice to include year in the Figure 4 legend was not intended to emphasize the importance of year, but rather to clearly define each radiocarbon depth profile displayed in the figure. Based on this comment and the reviewer's specific suggestions below, we have modified Figure 4 substantially, now grouping soil profiles by polygon type and position. With this new formatting, year and specific core name are no longer included in

the legend. Additionally, we have modified Figure 1 substantially, based on comments from Reviewer #2. The new version includes day of year on the x-axis in place of sampling month. We have also edited the caption to clarify the fact that data in Figure 1 were compiled across the 3 sampling years.

Table and Figure comments

Can a seasonal Reco flux rate figure be added?

Because Reco was measured on only a subset of sampling dates, we have chosen not to include a seasonal Reco flux rate figure. From different soil profiles at the same site, two published studies (Vaughn et al., 2016 and Wainwright et al., 2015) both show a clear seasonal decrease in Reco rate from July/August into October. Although we have not added a seasonal Reco figure in this manuscript, we will instead include a reference to these two datasets in the revised manuscript. With this reference, we will briefly discuss how these seasonal Reco trends support our interpretation that slow-cycling carbon sources contribute large proportions of total ecosystem respiration only during times of low overall Reco.

Tables: The tables are tough to read. Could some of the environmental data be summarized in a figure and the tables moved to the supplement? As I understand it all the CO2 flux data is shown in figures so the tables aren't critical for the reader to understand the patterns.

Based on this suggestion, we will move Table 1 to the supplement and add two additional figures: (1) soil temperature by month, and (2) a third panel in Figure 2 showing thaw depth in September 2014. We chose to leave Table 2 in the main document because it shows the results of the mean age of respired carbon calculation, and we felt this table was not as unwieldy as Table 1.

Figure 2: Is the data the same as in Figure 1, September? It looks different: : :.. Flat has 14C<0 in Figure 1 and >0 in Figure 2

Yes, these figures represent different data. Figure 1 includes all sampling years (which for September, includes 2013 and 2014), whereas figure 2 includes only September 2014, which we highlight as a balanced sub-dataset that clearly demonstrates the influence of microtopography on $\Delta^{14}C_{Reco}$. We have clarified this in the Figure 1 caption, and by adding the following sentences to the main document: "In September 2014, we measured $\Delta^{14}C_{Reco}$ and ecosystem respiration rates from the centers of three polygons of each type (Fig. 2). The influence of microtopography on old carbon emissions is particularly apparent in this complete and evenly distributed measurement set."

Figure 3: Can the month in the legend be written as a month name (ie: July, September)? That would be much easier to read.

The figure legend has been changed as suggested.

Figure 4: Can the legend be Flat2-Center-August 2012, Flat4 Center July 2013, etc? Would be easier to read. Even if there is no overall temporal and spatial pattern could the lines in the figure be systematically grouped? One colour for each location, and a different symbol+line type for early/late months? It might be conceptually helpful to have a horizontal line at 0cm to indicate the soil surface, and perhaps put the chamber flux data at +2cm?

Based on this suggestion and the comment above, we have reformatted Figure 4. Figure 4 now groups $\Delta^{14}C_{CO2p}$ profiles by position within polygon and polygon type and no longer lists the specific profile ID and measurement month/year. We have also added a horizontal line at 0 representing the soil surface and moved the chamber flux data to +2 cm depth.

Line-by-line minor comments

Page 1: Line 29-30: Cite Bond-Lamberty soil respiration database paper?

We have added this reference.

Line 34: something is missing in the end of the sentence, the grammar/tense is wrong: 'heterotrophic decomposition of soil carbon that cycles on broad range of timescales'

To improve readability, we have changed this sentence to the following: "CO_2 emitted from the soil surface includes autotrophic respiration of rapidly cycling carbon as well as heterotrophic decomposition of carbon that cycles on broad range of timescales."

Page 2: Line 5: 'thaw depth' is not an obvious variable here without introducing permafrost? To some extent thaw depth is captured by soil temperature. Perhaps 'soil C pool' would be useful to add? Or maybe 'permafrost state'?

Following the reviewer's suggestion, we have removed the words "thaw depth" and explicitly mentioned carbon pools earlier in this sentence.

Line 6: what does 'such variations' refer to?

For clarity, we have changed the sentence to read, "variations in the radiocarbon abundance of respired CO_2…"

Line 7: the jump from environmental controls to use or availability of soil C substrate pools is a bit unclear. I think clarifying whether 'such variations' refers to 14C or variation in environmental factors would help. I suggest explicitly naming the variation that is meant, rather than 'such variations'.

We believe that the added mention of carbon pools in line 5 and language clarification in line 6 have addressed this comment.

Line 11: parameters of what?

We have changed this to "carbon pool-specific respiration rate parameters."

Line 17: consider also citing (Elberling et al., 2013; Schädel et al., 2014)

We have added a reference to Elberling et al., 2013.

Line 21-22: something is missing from this sentence

To improve readability, we have changed this sentence to read, "to quantify in situ decomposition rates, field radiocarbon measurements can be used to differentiate between slow-cycling and fast-cycling carbon and link decomposition dynamics to environmental controls.

Line 24: (Nowinski et al., 2010; Schuur et al., 2009)?

Both references have been added.

Line 26: It would be helpful to explain, in a few sentences, what polygonal tundra is, why it's important, and what unique features it has (eg: drained vs saturated microsites, C accumulation, temperature regimes). (Ping et al., 2015) might be a useful reference. This is mentioned in the methods, and I think it would be worth a brief mention in the introduction too.

We agree with this suggestion and have added a brief paragraph to the introduction describing polygon tundra and its relevance to carbon cycling. To the methods section, we have also added and estimate of the spatial distribution of the three polygon types across the study region.

Page 3:
Line 17: chamber height? Or volume?
Line 19-20: Oh, I see. I would move this sentence one earlier.

As suggested, we moved the chamber height description one sentence earlier.

Page 5: Line 3: what is the mean 13C value of these samples? Is it possible that this 13C value largely represents autotrophic respiration, rather than soil respiration? My guess would be that the chambers with rapid CO2 accumulation and the highest Co2 concentrations have high plant respiration.

Page 5 line 3: We have added the Reco $\delta^{13}C$ end-member values to the text (-24.6, -26.5, and -26.2 ‰ for low-centered, flat-centered, and high-centered polygons respectively). We believe the reviewer is correct that these $\delta^{13}C$ values may be largely influenced by autotrophic respiration from aboveground vegetation and roots. Because we are using this background atmosphere correction to determine $\Delta^{14}C$ of ecosystem respiration,

which includes autotrophic respiration, a strong autotrophic signal in end-member $\delta^{13}C$ values should not invalidate our atmospheric contamination correction.

Line 10: This is a good idea for dual filtering criteria. I like it.

Page 7:
Line 25: is the data in figure 2 a subset of figure 1? The patterns between polygons in September look different in the two figures, and I can't understand why.

As discussed above, figure 1 includes 2012, 2013, and 2014, whereas figure 2 is a detail of just September 2014 data. We chose to highlight this September 2014 data subset because it was balanced and complete for both Reco and $\Delta^{14}C$, and it clearly demonstrates spatial patterns. We have added clarifying sentences in the text where Figure 2 is introduced and have clarified the Figure 1 caption.

Line 30: profiles of what?

We have changed this to read, "across soil profiles…"

Line 32: I feel this needs a little more elaboration: 'At the scale of individual profiles seasonal variations in ï¸ A˘D 14C Reco correspond with changes in thaw', that's inferred from seasonal pattern of ï¸ A˘D 14C Reco decreasing as thaw exposes deeper parts of the soil profile? In contrast, across sites, there is no correlation between thaw and ï¸ A˘D 14C Reco.

Based on the reviewer's suggestion, we have reworded the end of this paragraph to clarify the distinction between patterns across time in an individual soil profile and patterns across space at a single time point. This section now reads, "Interestingly, soil thaw at this time was deepest in FC polygons; from this set of September 2014 measurements, we saw no correlation across soil profiles between thaw depth and $\Delta^{14}C_{Reco}$. In contrast, repeated measurements from individual soil profiles indicate that $\Delta^{14}C_{Reco}$ tends to decrease as thaw depth increases and exposes deeper soil layers to unfrozen conditions. These findings suggest that the relationship between the depth of thaw and old carbon mineralization depends on the spatial and temporal scales of observation. At the scale of an individual soil profile, seasonal variations in $\Delta^{14}C_{Reco}$ correspond with changes in thaw depth. At the site scale, however, thaw depth may not be a useful predictor of spatial variations in $\Delta^{14}C_{Reco}$."

Page 8: Line 6: I think this should be reworded to something like: 'As a result, old, slow-cycling C from deep Reco comprises a large percentage of the total C flux only when autototrophic and surface soil (or fast-cycling) contributions are low. I think that might be a more accurate generalization, rather than old soil contributions being high when Reco rates are low, because there could be a number of reasons for low Reco rates like overall low plant&microbial activity, which might not affect the ï¸ A˘D 14C.

We agree with the reviewer's consideration and have reworded this sentence to incorporate their suggestion. The changed text now reads, "As a result, old, slow-cycling carbon from deep soil respiration comprises a large percentage of the total carbon flux only when respiration rates are low from autotrophic and shallow (fast-cycling) soil sources."

Line 18 -24: That's really interesting! Line 19: Should this be 'Figure 4'??

Yes, this was a typo and should be Fig. 4. Good catch! This has been changed in the text.

Line 24: The reason why cryoturbation may explain the more positive ï¿ A˚D 14C at depth may only be obvious to people familiar with permafrost dynamics? One sentence would be sufficient to say that cryoturbation can transport large chunks of surface/organic material deeper into the profile.

Following the reviewer's suggestion, we have added a brief definition of cryoturbation.

Page 10: Line 5: But these slow cycling contributions might be missed when measuring surface ï¿ A˚D 14C fluxes alone?

This is a good point that was not sufficiently expressed in our original manuscript. We have restructured this paragraph to emphasize that soil pore-space CO_2 measurements demonstrate that old, slow-cycling carbon is being decomposed, even when it does not contribute a substantial portion of the surface respiration flux. This paragraph now begins, "Measurements of radiocarbon in late-season ecosystem respiration indicate that carbon that cycles on millennial timescales contributes substantially to soil respiration. When thaw depth approached its maximum in September and October, highly depleted ^{14}C in respiration indicated that carbon older than 1000 years was a major source of heterotrophic respiration. Decomposition of old, slow-cycling soil carbon, however, may be missed when measuring surface $\Delta^{14}C_{Reco}$ alone. In the soil pore-space, $\Delta^{14}C_{CO2p}$ declined steeply with depth…"

Line 14: the distinction between newly thaw and historical annual thaw might be very difficult for people without an Arctic/permafrost background to understand. Perhaps elaborate a little what this means and why it matters. Newly thawed does not refer to new C, it is newly exposed old C, I think that's a very permafrost-specific concept.

We have added a sentence to clarify this concept for readers less familiar with Arctic soils. This section now reads, "As climate change alters these environmental controls and soils warm and thaw, a key question is how decomposition rates will change. Where permafrost degradation occurs, either through gradual deepening of the active layer or rapid thaw events, old soil carbon that has remained frozen for years to millennia is exposed to thawed conditions. A particularly important—but unknown—factor is the decomposition rate of this carbon released from thawing permafrost (Hicks Pries et al., 2013; Koven et al., 2015; Kuhry et al., 2013). Our measurements cannot differentiate

between such newly thawed soil organic matter and carbon that has historically experienced an annual thaw."

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