Review comments

This review represents the second time I have seen this paper and in general I am pleased with the way the authors addressed the criticisms of the previous version. Eliminating the modeling section greatly helped focus the manuscript and for me it was easier to understand the manuscript. A few issues remained that I think need to be addressed before it can be published but these are mainly editorial. I think the methods section could be rearranged I a bit since it jumps back and forth between different methods. I also may have identified a few new issues that I did not comment on in the previous version, partly because in my previous review I focused on some 'bigger-picture' issues. My apologies for that. Below I have outlined my comments.

Introduction

I would suggest including Schmidt et al 2011 in either the introduction or discussion. This paper argues that low decomposition rates of organic matter in soils may be because of physical conditions (high moisture/low O2) or other means of protection rather than chemical composition. This would fit well with the objectives and results of this manuscript.

Line 41: spell out OC the first time

Line 41-42: whether or not climate change represents a positive feedback would depend on the current conditions and the type of change that occurs. Warming may have a positive effect in mesic, temperate environments but not in arid conditions. You may want to expand on this a little bit more and be explicit about the types of changes (temperature vs. precipitation changes) you expect to happen and how they may affect decomposition.

Line 50-51: If the OC is highly processed you would not expect large contributions of this OC to the total decomposition flux. However, Schmidt et al argue that physical conditions may prevent decomposition of 'deep' OC even if this OC would be easily decomposable under optimal conditions.

Line 55-57: Would you expect ag soils to be different from forest soils in terms of the contribution of deep vs. shallow OC to the total CO2 efflux? I am missing the development of clear hypotheses.

Materials and Methods

Line 75-76: The soil classifications are based on the FAO system so I would reference that.

Line 79-83: Was porosity mentioned in previous papers? If so, than I would also add a few lines about how OC and labile OC were measured. In other words, why describe how porosity was measured but do not describe how OC and labile OC were measured if all of these were reported in previous papers?

Line 84-86: How were the SWR's determined? Either describe this in more detail or refer to another paper and see previous comment.

Line 88: replace purpose-built with custom-built.

Line 90-91: I am not sure I understand this sentence.

Line 113: I would move the section from line 192 describing the LI-COR surface flux measurements to line 113. In line 123 you mention the surface flux measurements but you have not described these yet.

Line 129-135: I would move this to the section after line 103 and perhaps eliminate the specific ranges at the various depths.

Line 141-146: I would also move this to the section describing the CO2 measurements.

Line 147-150: Perhaps move this to after line 123.

Line 162: add that 'z' is the depth

Line 191: Not sure that 'punctual' is the right word here. Perhaps 'instantaneous' is more appropriate.

Line 199: How were the calculated fluxes corrected? Was this done by adjusting specific parameters such as diffusivity or were the calculated fluxes simply decreased/increased by 5% or 22% to make the calculated fluxes match the measured fluxes? This is unclear.

Line 201-208: This section is still a little bit unclear to me. Goffin et al. and Maier and Schack-Kirchner calculated CO2 production from each soil slice. However, the calculations used by the previous papers seems to be different from the one presented in the current manuscript. Given that this is a critical part of the paper I would recommend expanding this section and mention the differences/similarities used between the approach taken in this manuscript vs. the approaches taken in the other papers mentioned in this section. In addition, Eq 2 does not give you the flux but rather the concentration profile so some additional steps are needed to go from Eq 2 to the actual fluxes at the various depth intervals.

Line 207: What do the authors mean by a 'semi-seasonal' timescale? Can you be more specific?

Results

Line 224: I don't see in the graphs that VWC in the subsoil of the summit profiles reached 0.5 cm3/cm3 (which incidentally is higher than the total porosity). Is this a typo?

Line 234-235: Can you expand on this a little bit more? What time periods are you talking about?

Line 228: The authors start with discussing fig 6C rather than 6A. I would change the order of the panels to make it align with the text. Please note that different units are being used for the CO2 fluxes between the different panels. Please make sure to use the proper units consistently. This also applies to the CO2 concentrations throughout the text. Sometimes the authors use ppm, mumol/m3, or %.

Line 246: Replace 'between' by 'from'.

Line 257: Again, the order in which the results are discussed is different from the order in the figure. Start with discussing the summit rather than the footslope profile.

Line 267: Is this negative uptake significant given the high standard deviations? You probably want to come back to this since this could be an artifact of the calculation method perhaps in combination with measurement uncertainty.

Discussion

Line 278-280: I think I understand what the authors are trying to say here but they may want to expand on this a little bit more. You are basically talking about the relative amounts of water- vs. air-filled pore space, correct?

Line 299-301: Yes but for CO2 to accumulate it has to be produced as well which is likely related to the labile OC present. The fact that it stays in the profile is due to the low diffusivity.

Line 301-302: What is the difference between 'CO2 efflux' and 'instantaneous soil respiration'? I am not sure I understand what you are trying to say here.

Line 321-322: I would expand on this a little more. While I agree that ag soils and forest soils are very different can the authors compare/contrast the studies in terms of OC quality, diffusivity, etc.? In general, I think the authors can emphasize the differences in amounts of total and labile OC between the two profiles and while the footslope profile contains more labile OC in the subsoil, there is a less of a contribution from the subsoil to the overall respiration fluxes due to physical limitations (low diffusivity and lack of O2). This information is somewhat implied in the discussion but I think the authors can be more explicit about this. In addition, the implications of all of this is that if hydrologic regimes change and footslope soils become drier, there is a large amount of potentially easily decomposable OC stored at depth that can suddenly decompose if moisture conditions become more favorable.

Line 344: This corroborates the notion by Schmidt et al suggesting that deep organic matter may be protected because of unfavorable physical conditions rather than substrate limitations.

Table 1: What does 'NI' mean?

Figure 5 and 6: I would consider splitting these up in several separate figures so rather than have 5(A)(a)(b) and 5(B)(a)(b) make two separate figures for temperature (5(a) and 5(b)) and moisture (6(a) and 6(b)). Similarly, I would split up figure 6 into three figures and make sure that the figure order matches the order in which the figures are discussed in the text (see below).

Figure 6: Make sure the order of the panels is the same as discussed in the text.