Review comments

The paper describes an interesting study using soil CO2 profiles and a process-based soil C cycling model to calculate soil heterotrophic respiration fluxes. In my review I had the benefit of reading the reviewer #1 comments and the response by the authors with regard to the novelty of the data and the relationship of the current paper with previously published work. I do somewhat agree with ref #1 that there is a lot of duplication and the paper can be shortened considerably especially when describing methods. In my view the main new points of the paper are the application of two modeling approaches to long-term datasets. It seemed however that the application of the diffusion model to long-term data would have been a nice addition to the paper that is currently under review since according to the authors that paper only deals with short-term measurements. In addition, it would have made a lot of sense to include the RothC simulations to the Geoderma paper since now two very different approaches are presented in one paper and the two approaches are not merged in a very intuitive way. There may be a good reason why the two approaches are presented in one paper but this was not clear from reading the paper (especially the introduction). Similar to ref #1 I feel that the authors need to do a better job on describing the novel aspects of the study relative to previous work especially since the first part of the discussion is basically restating conclusions drawn from previously published work. A second major issue was the spelling/grammar. After a while I stopped marking up the manuscript since there were so many spelling errors, incomplete sentences, and other grammar issues that I think the manuscript requires a serious editing job. Not being a native English speaker myself I can relate to language issues but the current state of the manuscript is unacceptable and it was distracting me from focusing on the science. In addition to the grammar issues, the discussion contained many statements that were not supported by data or other references which again with better editing should have been caught. I suspect the senior author is relatively inexperienced and I'd suggest more involvement of the co-authors in the editing process. This also applies to the description of the methods which was confusing, repetitive and sometimes inconsistent so a thorough rewriting job is needed there to make sure the methods section flows better.

With respect to the detailed comments, I agree with most of the comments made by ref #1 so I will not reiterate those but instead add additional comments that I feel need to be addressed.

Introduction

I believe the introduction should be more focused on soil organic C especially in the beginning. In the second and third paragraph the authors discuss eddy-covariance and other flux-based techniques. These measurements focus on net ecosystem C exchange (NEE) which includes the net result of photosynthesis, autotrophic and heterotrophic respiration. Somewhere in the middle of the third paragraph the introduction appears to shift to soil fluxes only which include a subset of processes that contribute to NEE (heterotrophic respiration and belowground autotrophic respiration). I would focus the introduction on soil respiration or at least make a clear transition from discussing NEE to soil fluxes only. I agree with ref #1 that advances have been made in measuring NEE using eddy-covariance techniques in steep terrain so dismissing this technique is not entirely appropriate and would probably offend several people in the eddy-covariance community. In addition, the study site is very small with only a modest slope (according to previously published work) so eddy-covariance might actually work under these conditions. Consequently I would leave out any mention of eddy-covariance in the context of this paper. Also, the introduction would benefit from having a short description how the authors plan to address their objectives especially objective 1 related to the persistence of deep OC. I had a very hard time understanding why two modeling approaches were taken and how they were compared, i.e. which approach is better. It would have made much more sense to include the long-term CO2 profile simulations with the other paper that is currently under review and include the RothC modeling approach with the Geoderma paper. As it is now it is unclear why the two approaches are presented in one paper so some explicit text to this effect would greatly help. Only later in the methods it states that the RothC model was used for interpolating and extrapolating data but why this approach was used instead of some type of regression analysis was not clear.

Materials and Methods

As ref #1 suggested, more details are needed with respect to slope, elevation, land use, previous cropping regimes, etc. I realize some of that information is given in previous work but you could give a quick summary so people can read this paper without having to have previous papers at hand.

Page 13703, line 25: I do not understand what is meant here.

Page 13704, line 12: I am not sure you can conclusively state that 3 replicates are representative for the entire slope position so I would eliminate that statement or reword it. Incidentally, the first sentence of this paragraph is repeated verbatim at the start of the next page and on page 13707 (line 15). Once is enough.

I would rearrange 2.2 since at the start of page 13705 the authors come back to the CO2 and VWC measurements which were already mentioned on the previous page so I would consolidate this. It was confusing to read the way it is organized now. Also, it appears that several of these methods are described in detail in other papers so only a summary would probably be enough. For instance a figure showing the Vaisala probes with the membranes etc. is not needed here but can be referred to. Also the figure showing where exactly sensors are located is unnecessary but a better description in the text is needed as suggested by Ref #1.

Page 13705, line 13-19: But the RothC simulations include 2011. Please check this.

Page 13707, line 18-19: I don't understand this sentence.

Page 13707, line 19-21: This is repetitive, either remove it here or remove it from the previous page.

Page 13707, line 26-27: I would move this to page 13704 where you describe your field methods.

Page 13708, line 1-7: So the modeled fluxes under- or overestimated (this is not clear) measured fluxes? Why was that and what conclusions were drawn from this? One could argue that the profile method doesn't work.

Page 13708, line 12: Vertical or horizontal space (I assume the former). Please come up with a better term.

Page 13708, line 23-24: This is the first time it becomes clear why you use the RothC model. Why use this to interpolate fluxes and how do you know if this approach is valid? From Figure 8 it appears you only did this in 2013 for part of the year or am I missing something?

Page 13709, line 1: But on page 13705 you said you measured for two years.

Page 13710, line 2-3: Why was the RPM pool assumed to be zero? Were harvest residues absent? What was the cropping history?

Page 13711, line 5-21: I was not sure what was going on here. Please make this understandable for non-modelers.

Page 13711, line 9: 'sensitive analysis'???

Page 13711, line 14: what 5 initial concentrations are meant here? 5 sites, depths, other?

Results

Page 13712: Please describe the results in the same order as shown in Figure 6.

Page 13713, line 24-25: What is actually compared here? In the footnotes of Table 1 it says that the model was validated by a small number of instantaneous observations during 2011 and 2012 and simulated fluxes using the profile method in 2013. So during two years only a (very) small number of observations is used whereas in 2013 on model is validated using another model? How confident are the authors using this approach? This needs more discussion.

Page 13713, line 28: How would soil alkalinity contribute to CO_2 emissions? Degassing from carbonate precipitation? Please provide more explanation.

Page 13714, line 1-2: How were instantaneous chamber-based flux measurements converted to daily measurements? This is not described anywhere as far as I could tell.

Several of the figures were pretty much unreadable because of the small font size so evaluating the results section was really difficult.

Discussion

Page 13714-13715: How are differences in CO_2 production rates from microbial respiration accounted for? When soils are waterlogged microbial activity is likely to be low as well so how can this effect be separated from the CO_2 transport mechanisms? In contrast, during periods of high microbial activity, CO_2 production may be much higher than diffusion causing CO_2 to build up. Perhaps this is implied in this part of the discussion but there is no mention of the production here. As a result I don't know how you draw conclusions about the contribution of deep OC since you present no information about the relative decomposability of this OC. There are likely to be differences in diffusion patterns as a result of differences in soil properties between the two profiles but not knowing what the differences in CO_2 production within the profiles is makes it in my view difficult to interpret the results. You could say something about this since this apparently was the topic of previous papers.

Page 13715, line 9: Figure 5 shows temperature and moisture, not CO_2.

Page 13715, line 23-26: Leading up to this statement there is very little discussion on how well the modeling approaches worked in terms of simulating measured fluxes. Consequently, how do you know that you improved the RothC model?

Page 13716, line 5-11: How do you come up with this conclusion? You present no information on CO_2 production through microbial activity. Presumably this is presented in other papers but if so, you would have to mention this and discuss this.

Page 13717, line 21-24: What evidence do you have that turnover actually occurs? If you had turnover happening deep in the soil you would expect CO_2 to be produced and if there were diffusion limitations you would expect CO_2 to build up. Is this what you mean? I also do not understand why this explains the differences in distribution of stable and labile pools between the two soils. What about contributions from vegetation over time? Could those be different between the two slope positions?

Page 13718, line 24-30: How can you say that the model was better than the EC measurements based on the model error? The uncertainty in the EC measurements may be related to spatial variability in the landscape whereas the modeling is based on two specific points in the landscape using average values based on a relatively small amount of replication and probably represents a mathematical error rather than an error based on spatial differences. This needs better explanation.

Page 13719, line 11-14: Except that your analysis does not account for potential contributions of root respiration since you had no vegetation at the site. Vegetation density/type is likely to vary with position on a hillslope and as a result root respiration may be very different as well which could explain differences in soil CO_2 emissions between different points along a slope.

Page 13719-13720: I think it is difficult to compare your results with other studies since a multitude of factors could explain differences between studies such as amounts and quality of organic matter, climate etc. in addition to the factors you mention in line 3-8 on page 13720. I’d take this out.