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Interactive comment on "Quantitative estimation and vertical partitioning of the soil carbon dioxide fluxes at the hillslope scale on a loess soil" *by* F. Wiaux et al.

Anonymous Referee #1

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Overall comments: I first thought this was a great study, great experimental design and innovative measurement set-up. By and large, I like the methodology, the analysis performed, and I find this a comprehensive study. Maybe a shortcoming of the study was that it was done in an agricultural setting and the authors could have expanded more about the relevance of this site for other systems and for global fluxes, especially since they mention in the abstract about a 20% underestimation of CO2 fluxes when not accounting for landscape differences. So my first impression was to approve this publication with minor edits.

As I read this paper, I noticed that significant portions of this study were already published elsewhere (Wiaux et al. 2014 a-c), and the more I read to more referencing I

found where the authors mentioned that findings described here were in agreement with either of these other papers. I feel that the authors need to very clearly characterize what is really novel in this study as compared to three publications from this same site and measurement campaign (Wiaux et al. 2014, Geoderma; and Wiaux et al. 2014, Soil Biol. Biochem, and Wiaux et al, in review that the reviewer has not seen). For example, one main conclusion point of this paper as highlighted in the abstract is that the footslope site generates more CO2 fluxes than the summit position, and that the depositional footslope profile emits more CO2 than the summit, due to its high amount and quality of OC. This is the same conclusion as published in Wiaux et al., in Geoderma, where the authors report significant differences in respiration with 30% more at the downslloe and 50% more at the backslope relative to the uneroded summit position, and report higher amount of OC. I understand that there are differences in measurement methods (surface CO2 measurements compared to in-situ profile measurements), but it seems that the same result as published before is highlighted in this manuscript.

So I felt I should suggest to the authors to eliminate the modeling component to estimate annual surface CO2 exchanges, and instead focus their discussion on depth patterns of diffusivity, diffusion gradients, and contributions to CO2 fluxes. However, the authors then refer to a study (Wiaux et al., in review) where the authors apparently already presented CO2 vertical diffusion profiles of this same study. As I have not seen that paper, but if also CO2 profiles from this same study have been published, then I have serious doubts about the need for this publication as all major results have been already published in other journals?

Given the large amount of results that have already been published, this paper is very long and has many figures. Upon an approximate count, I found that the text is over 12,000 words, 2 tables, and 8 figures. This length greatly exceeds standard formats in any other journals (probably more than double), and makes reading this paper very exhausting. I would think the length could be greatly reduced given the material already published.

Further detailed comments: The abstract does not well represent and summarize observed patterns. For example, they focus on patterns at the footslope (i.e., high water content "disabling" vertical transfer), but don't mention any patterns of the hilltop location, nor any other seasonal or spatial aspects of observations. Such information should be included given the experimental design of this study using two contrasting measurement locations. It is unclear how the authors come to the estimated 20% underestimation of soil-atmosphere fluxes when not considering landscape dynamic processes.

This study measures CO2 gradients at two locations located along an agricultural hillslope (i.e., hilltop and the footslope positions). Throughout this manuscript, the authors mention and discuss that they measure and calculate "aggregated hillsope CO2 fluxes" and that they measure "at the scale of a hillslope". However, they only measure two the two end members of this hillslope (only two contrasting measurement sites), and therefore their claim of measuring across the hillslope seems inaccurate and highly overstated. They should re-phrase sections referring to aggregated hillslpoe CO2 fluxe and clarify that their measurements focus on contrasting end points of this gradient.

There are too many figures in this manuscript, and the overall length should be shortened: for example, Figures 1 and 2 can be removed as these figures have already been published in another paper by Wiaux et al. and Figures 3 and 4 should be combined into one figure. Note comments above about repeated publications from the same study, so unless new material is presented the figures can be eliminated and referred to.

The entire manuscript needs a careful edit, there a lot of small errors (e.g., prepositions of, to, from) and stylistic inaccuracies.

Title: Remove "the" of the ""the soil carbon dioxide fluxes"

Title: should reflect that measurements were of CO2 concentrations, and that fluxes were then inferred. The technique used is not a direct flux methods, but rather models fluxes based on observed vertical concentration profiles.

Abstract: page 13700, line 1-2: What do the authors mean with "large spatial scales" Their study assesses fluxes along a hillslope, which I might consider landscape scale, but certainly not large spatial scales.

page 13700, line 5: the authors need to highlight what the importance of hillslope aggregate CO2 fluxes are, what does the word "aggregated" actually mean in this sense, please clarify.

page 13700, line 8: change "contrasted" to "contrasting"?

page 13700, line 10-11: clarify what the "gradient method, i.e., that fluxes are calculated based on Fick's diffusion law.

page 13700: line 15: "disables" is too strong, I assume there is still some residual vertical transport during wet periods, just below the sensitivity of the system. And information should be given from the summit position, i.e., that no saturation was observed and that during no period diffusion was limited by high water content? Is the CO2 production at depth limited by low O2 content – it seems that authors refer to this without clearly saying it?

page 13700, lines 24-27: it needs to be clarified how not including landscape dynamic processes results in a 20% underestimation of soil-atmosphere fluxes.

Introduction: Page 13701, lines 1-5: the authors should give newer references on the global pool sizes.

Page 13701, lines 10-11: please clarify what is meant with "hillslope aggregated CO2 fluxes", I think they mean CO2 fluxes scaled/average across a gradient from hilltop to the footslope of a watershed, or something like that. Please clarify and define.

Page 13701, lines 17-19: it is not correct that EC technique is not appropriate for sloping landscapes, there are attempts to doing this. But I agree that it is difficult and subject to higher measurement uncertainties, this statement should be more careful rephrased.

Page 13701, lines 22-25: clarify what is meant with “support scale”?

Page 13702, lines 10-12: please add some quantitative data on how much transfer and accumulation of OC has been observed along hillslopes.

Page 13702, lines 13-14: clarify and give examples what the “series of complex and interacting processes” are that are acting on these deposition sites.

Page 13702, line 19-10: expand on the percentage contributions of the top 30 cm as compared to deeper soil layers.

Page 13702, lines 23 -28: add information about the experimental setup to address the goals of this study, i.e., measurements at two points (hilltop and hillslope) of a hill (how large/steep)? Further, the contrasting two measurements at the hilltop and hillslope only in may view does not allow to calculate “aggregated hillsope CO2 fluxes” nor measures “at the scale of a hillslope”, but rather presents a contrasting view on two end members of this hillslope. This should be clarified here and throughout the text. Most importantly, clarify what is new in this study compared to Wiaux et al. 2014 a,b,c, and focus the paper only on the new aspects.

Materials and Methods: Page 13703, lines 2-5: mention the slope angle of the hill; what is the cultivation regime at this site? It also needs to be clarified if the cultivation is the same on the hill as in the footslope.

Page 13703, line 12: combine Figures 1 and 2 into one Figure. Statistics need to be added to clarify if and at what depth differences between the two locations are significant.

Page 13703, line 22: change to “specifically designed soil CO2 probes”

Page 1370, line 4-5: vertically inserting probes into the soils may cause diffusion along the vertical walls of the tubes; please clarify how soils were backfilled after insertion (if at all), and how the authors can exclude the possibility that their measurements were affected by vertical diffusion or advection.

Page 13704, line 17-20: please rephrase, this sounds confusing.

Page 13704, line 19: change “than” to “as”

Page 13704, line 22-25: clarify the number of soil temperature and soil moisture probes, were these collocated with each individual CO2 measurements?

Page 13705, lines 2-8: please rephrase how concentration ranges of probes were adapted to best fit their placement, this was confusing to read (I had to read several times to understand what they did).

Page 13706, lines 19-21: can the authors please clarify why they included the soil water retention curve model into the tortuosity factor? Since they directly measured soil water content using TDR probes, I would assume they can directly use measured soil water content to adjust for changes in tortuosity based on water content, and don’t need the steps to use retention curves? Please clarify and explain.

Page 13707, line 5-6: so does that mean to calculate surface CO2 fluxe, they used the top 0.1 cm based on their 0.1 cm increments – or the top gradient measured with the first probes at 10 cm? Did they account for the diffusion gradient between the top soil and the atmosphere, e.g., by constraining the surface CO2 concentrations with atmospheric CO2 levels ($\sim$ 395 ppm)? That would probably be the correct way to assess the relevant concentration gradient to calculate surface CO2 fluxes.

Page 13707, lines 15-17: it would be nice to have some information and discussion on the variability of measured CO2 concentration profiles. This would add a nice discussion on smaller-scale spatial variability. “Providing unique values” sounds weird, maybe providing “an average value for each soil depth and location”.

Page 13708, lines 22 to page 13709, line 7: the authors should give reason to extrapolate fluxes to yearly fluxes in lieu of previously published difference in surface CO_2 fluxes based on surface measurements at these two slope positions (plus further measurement points in-between).

Sections 2.6.1 and 2.6.2. It is entirely unclear to the reader what the purpose of the modeling component is, this aspect so far has not been measured in the introduction nor in the abstract. The authors need to clarify in the abstract what the purpose of this modeling component is.

Page 13712, lines 20 to 25: I assumed that among the papers published in this study, this one focused on the depth CO_2 concentration profiles and contributions of different soil layers. The reviewer has not seen Wiazu et al., 2014 in press, but here the authors refer to published soil CO_2 concentration profiles in a study in review.

Discussion: page 13714 to 13715: I have now real troubles believing in the need and novelty of this study. The authors discuss that the diffusion limitation for CO_2 emissions at the footslope site, and mention that this corrobotes diffucitivity profiles from Wiaux et al., 2014c and that is in support with reporting gas diffusion barriers in Wiaux et al., 2014. So what is new here if not even the depth patterns of CO_2 diffusion and concentration profiles is new?

Discussion: page 13718, lines 6 -19. The modeling shows a flux averaged along 3 years of simulation of ca. 1.5 times higher at the footlsope relative to the summit. This is apparently in agreement with Wiaux et al., 2014 c that shows fluxes 1.3 times higher at the footslope relative to the summit. I really don't understand the need for this modeling component since all they do is to compare it to a measurement-based approach that is already in publication. So what is new here, and why do the authors publish their results from the same site in multiple journals. They even state reasons for this as published.

I could continue to review and critique further aspects in the discussion and conclusion

sections, but not knowing what is really novel and new kind of makes this effort useless.

Interactive comment on Biogeosciences Discuss., 11, 13699, 2014.