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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.

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Dear Editor,

Thank you for reviewing our paper. With this letter, we describe how we intend to improve the manuscript. In general, the key issues raised by the reviewers will be addressed as follows:

(i) manuscript length: as suggested by reviewer #1 and by the editor, we will remove the modeling component, several figures and some parts of the discussion (see below). We also intend to remove some parts of the "Material and methods" section, as suggested by reviewer #2. This will shorten our manuscript by approximately 40%;

(ii) Overlap with other publications: we propose to focus on two related and important issues which are the vertical partitioning of CO2 fluxes and the storage of OC in deep layers of colluvial soils. At the contrary, we plan to eliminate modeling components for long-term CO2 fluxes based on the RothC modeling and will remove the discussion related to the CO2 fluxes quantification at the hillslope scale. This will help to identify a clear focus and reduce the overlap with our Wiaux et al 2014a and 2014b studies. With respect to the companion paper: we are caught between a rock and a hard place: We will not include the content of the companion paper (rejected for BGD) in this manuscript, as it is not compatible with your request to reduce the manuscript length and the requested focus on the physical and environmental controls. We think that a new section on methodological issues in relation to the derivation of soil CO2 fluxes is outside the scope of this study and is not needed to understand the approach taken here.

To this end, we propose to (i) modify the introduction where we will focus on soil organic C cycling and soil fluxes only and remove the links to net ecosystem C exchange based on eddy-covariance techniques, (ii) remove the modeling sections in the methods (i.e. section 2.6) as well as the parts of the results and of the discussion sections related to SoilCO2-RothC modeling and hillslope C budgets (i.e. section 3.2., section 4.3 and parts of section 4.1 and 4.2) and (iii) rewrite the conclusions and abstract in the light of the new focus of the paper.

Below, we further detail this approach by providing direct responses to the main issues raised by the three reviewers. Most of the minor comments will be addressed by a careful editing of the paper and a more detailed response to all the minor suggestions will be provided when we can submit a fully revised version.

Reviewer #1:

Significant portions were published elsewhere:

Although we accept that there is a need to identify more clearly the novel components of our study, we do not agree with the assertion that ‘significant’ portions were published elsewhere. We present new methods and data but discuss our results in a broader context using results from other, but also our own studies that were conducted on the same site. These studies focused on the spatial and vertical patterns of soil organic carbon quality (Wiaux et al 2014a) and soil surface respiration (Wiaux et al 2014b). The use of this information in the discussion part of our paper seems appropriate to us. However, the discussions in relation to the hillslope scale carbon budget will be removed in a revised manuscript and this will reduce the overlap with these two studies. The companion paper has been rejected and has not been submitted elsewhere so there will be no overlap.

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)

As indicated above, we propose to focus the paper on the vertical partitioning and the controlling biophysical factors and remove the discussion related to hillslope scale carbon budget. This will remove some of the references to our previous work and will allow us to clearly characterize the novel contributions of this particular study. This implies that we will rewrite the key conclusions identified in the abstract.

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

In response to this comment, we propose to remove the modeling component, which was used to provide continuous time series for 3 years. In a revised manuscript, we will focus on the direct observations.

This paper is long and has many figures. We are surprised to see that the reviewer reports a word count of 12000 words. In fact, our paper counts 7368 words (excluding

abstract and references) and 9900 words when all references, figure and table captions acknowledgements are considered. As such, our paper is not exceptionally long. As indicated above, we propose to remove a substantial part of the material and methods and of the discussion sections and this will reduce the length by approximately 2500 words. Furthermore, all tables (i.e. Table 1 and Table 2) and Figure 8 will be removed. Together, this will result in a short and focused paper.

Detailed comments:

Abstract does not well represent the key results: We agree with this assessment and will improve the abstract by reporting the key results in relation to the vertical partitioning and the physical controls.

Measuring of end-members and hillslope-scale fluxes:

We propose to remove the hillslope-scale assessment and will focus on the differences between 2 position with different soil physical properties and hydrological regimes.

Too many figures: See above

Reviewer #2:

We thank the reviewer for the positive assessment of our manuscript.

mechanisms for both summit and foots-lope: soil micro-organism respiration We propose to more carefully discuss the contribution of microbial respiration and the factors controlling diffusivity.

Discussion of Goffin et al 2014 paper does not consider differences in soil type/vegetation. The Goffin study reports on CO2 production in forest soils and we agree that a direct quantitative comparison is not straightforward, while similarities exist in the physical controls and the method used to calculate the vertical partitioning. We propose to briefly clarify this in the discussion.

Novelty of the findings. As indicated in the responses above, we will more clearly identify the novel contributions of this study and more carefully discuss this using results from other studies.

Aggregation at the hillslope scale: As requested by the editor and reviewers #1 and #3, we propose to remove this from the manuscript.

Figures: We will improve the overall quality of the figures.

Reviewer #3:

Paper length and description of methods: We will reduce the length of the methods section since the model description will be removed. As suggested, we will carefully edit this section to provide a logical and consistent description of our methods.

Novel aspects: See above.

Publication strategy: We propose to remove the SoilRothC part of the paper. We will also rewrite the introduction to reflect the new focus on the vertical partitioning.

Language/references: We think that most of these issues can be tackled by a careful editing by the senior authors of the paper.

Introduction: We agree that the introduction can be improved and as suggested, we will remove the focus on NEE and eddy-covariance approaches. As already indicated above, the work related to soilRothC modeling will be removed and this will address the comments related to the modeling approaches.

Materials & Methods. We will add more information with respect to slope, elevation etc but we can only present the main characteristics in order to avoid overlap with published work. In order to clearly present the main site characteristics without adding more text, we also suggest to not remove Figures 1 and 2 (while already presented in published papers).

Discussion The reviewer proposes several suggestions that will improve the discussion and may assist in the interpretation of our results. Note that there was no vegetation

on the plots and that the decomposability of OC is given in Figure 1.

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