March 17 2015

New title: "Vertical partitioning and controlling factors of gradient-based soil carbon dioxide fluxes in two contrasted soil profiles along a loamy hillslope".

Previous title: ”Quantitative estimation and vertical partitioning of the soil carbon dioxide fluxes at the hillslope scale on a loess soil”.

Tracking #: bg-2014-405

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Dear Editor,

This manuscript is a resubmission of bg-2014-405. As suggested in the decision letter, we submit a shorter and more focused manuscript that takes into account all the comments and suggestions of the reviewers. The manuscript also benefited from a thorough editing for language.

Thank you very much for the review of our manuscript. We found the comments made by both you and the referees to be very constructive and believe that the manuscript, revised in light of them, is significantly better. We list below the specific responses to the individual points raised by the referees and detail the changes made in the manuscript. For easy reference, the original comments are presented in black, and our responses in bold italic red.

Our results have not been published elsewhere and are not under consideration for publication elsewhere. All authors have seen and agreed to the version submitted.

Sincerely,

François Wiaux, PhD

Biogeosciences Discuss., 11, C8695–C8696, 2015

www.biogeosciences-discuss.net/11/C8695/2015/

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.

1. Editor decision

Dear Dr. Wiaux,

Based on the detailed comments of three reviewers, publication of your manuscript in its present form is not recommended, and major revisions are being requested. Based on your response from February 5, 2015, I consider that revisions of this manuscript should be feasible and that you should be able to address most of the reviewers concerns. I therefore decided to re-open the publication process and allow submission of a revised manuscript.

In you re-submission, please explain how and where each point of the reviewers' comments has been addressed, and please supply a point-by-point response to all reviewer concerns. Should you disagree with any part of the reviews, please explain why.

Kind regards,

Daniel Obrist

As suggested in our response from February, we implemented the following main changes in the manuscript:

(i) we modified the introduction where we 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) we removed the modeling section 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) we rewrote the conclusions and abstract in light of the new focus of the paper.

This has resulted in a much more concise manuscript where the overlap with previously published work has been fully eliminated, except for the field site description.

In this new version, we now focus on two related and important issues: (i) the vertical partitioning of CO_2 fluxes and (ii) the storage of OC in deep layers of colluvial soils. Note that, as suggested, the sections describing the long-term CO_2 fluxes at the hillslope scale based on the RothC modeling are completely removed.

2. Anonymous Referee #1

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 CO_2 fluxes when not accounting for landscape differences. So my first impression was to approve this publication with minor edits.

We are very pleased with this positive comment on the relevance of our manuscript. The comments/suggestions raised by this reviewer were very constructive and helpful.

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 CO_2 fluxes than the summit position, and that the depositional footslope profile emits more CO_2 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 downslope 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 CO_2 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 CO_2 exchanges, and instead focus their discussion on depth patterns of diffusivity, diffusion gradients, and contributions to CO_2 fluxes.

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, as suggested by the editor, the discussions in relation to the hillslope scale carbon budget have been removed in this revised manuscript. By doing so, this paper now focuses more on the vertical partitioning of carbon fluxes. Note that the companion paper has been rejected by the editor and has not been submitted elsewhere so there is no overlap here neither.

This hillslope scale modeling and the yearly extrapolation exercise with the RothC-SOilCO2 model have been removed from the manuscript (please see editor comments and our answer). We now focus on the points highlighted by the reviewer.

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?

Wiaux et al. (2014c) has been rejected and will not be resubmitted elsewhere. Hence, there is no overlap anymore with this paper. As suggested by the editor, we merged the relevant parts of the Wiaux et al. (2014c) with the present paper.

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.

We have removed a substantial part of the material and methods and of the discussion sections, reducing the length to approximately 5200 words (excluding abstract and references). Furthermore, all tables (i.e. Table 1 and Table 2) and Figure 8 have been removed. Together, this now results in a short and focused paper.

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.

We agree with this assessment and we have prepared a substantially revised abstract. The abstract now reports the key results in relation to the vertical partitioning and the physical controls.

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 hillslope CO2 fluxes" and that they measure "at the scale of a hillslope". However, they only measure 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 hillslope CO2 fluxes 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.

We have removed the hillslope-scale assessment and now focus on the differences between 2 positions with different soil physical properties and hydrological regimes.

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

We apologize for this. The manuscript has been carefully edited by the more experienced co-authors. We hope that this new version will be easier to understand.

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

This has been corrected.

Title: should reflect that measurements were of CO_2 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.

We specified these are "gradient-based" fluxes

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.

This hillslope scale quantification issue has been abandoned (please see editor comments and our answer). Moreover, the abstract has been completely rewritten.

page 13700, line 5: the authors need to highlight what the importance of hillslope aggregate CO_2 fluxes are, what does the word "aggregated" actually mean in this sense, please clarify.

Idem

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

Done

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

Done

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?

We reworded this sentence: "We show that most of the time high water filled pore space downslope strongly limits the transfer of biotic CO_2 along the soil profile, contrary to the summit where gas can easily diffuse during all the year."

Is the CO2 production at depth limited by low O_2 content – it seems that authors refer to this without clearly saying it?

We cannot be explicit about the O_2 content, because we did not measure it. Given the high water content, O_2 limitations are likely.

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.

This hillslope scale quantification issue has been removed (please see editor comments and our answer).

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

This hillslope scale quantification issue has been removed (please see editor comments and our answer).

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.

This hillslope scale quantification issue has been removed (please see editor comments and our answer). Hence, this aggregation concept is not used anymore.

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.

This hillslope scale quantification issue has been removed (please see editor comments and our answer). Hence, we do not refer to Eddy Covariance technique any more.

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

Idem

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

This is thoroughly described in a previous study on the same site. We therefore cite the reference, and add a more explicit sentence to invite the reader to look at this publication for further information: “[…] and Wiaux et al., 2014a describing how much transfer and accumulation of OC has been observed along this studied hillslope”.

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.

These processes are related to combined effects of soil moisture, temperature and OC quality effects on soil microbial activity (Wiaux et al., 2014b). This more explicit description has been added in the manuscript.

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

We expanded this affirmation as here below: "Goffin et al., . (2014) showed that the 30 first centimeters of soil significantly contribute to the total surface CO2 flux (i.e. c. 80%, half of these coming from the 10 first centimeters)".

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.

We agree with this comment. This hillslope scale quantification issue has been abandoned (please see editor comments and our answer). Hence, this aggregation concept is not used anymore but we focus on a comparison between the summit and the hillslope.

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.

This issue has been addressed by refocusing the paper on the vertical gradients and CO_2 profiles.

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.

This information has been added:

"The slope percentage in the backslope area ranges between 8.5 and 16%, with a mean slope of 12%. The slope percentage in the convex shoulder area ranges between 4 to 8.5%, with an average of 6%. The field is plowed (0-30 cm soil surface layer) every year. Each year, manure and nitrate application are also carried out. The current crop rotation is winter wheat, maize and spring wheat. The study site is has been described in further details in Wiaux et al. (2014a,b)."

Page 13703, line 12: combine Figures 1 and 2 into one Figure.

Combining Figures 1 and 2 would result in a very complex figure with a large amount of information. Hence, we propose to keep them separated.

Statistics need to be added to clarify if and at what depth differences between the two locations are significant.

As requested by the reviewer, we tried to remove the overlap with previous papers. Statistical analyses have already been presented and described in Wiaux et al. (2014a) to discriminate OC content between slope positions. Hence, we propose to not display further details here.

Page 1374, 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.

The tubing method is an adaptation of the technique tested and presented by Young et al. (2009). These tubes were inserted vertically into the soil, after drilling holes with a diameter that equals the diameter of the PVC tubes. As a result, it was not necessary to backfill the holes around the tube.

We added this further description:

"This approach avoids the need to backfill the bore hole, which will disturb the soil structure and diffusion properties. Two rubber stoppers, one at 155 mm from the tube head, and another at the top of the tube, prevented atmospheric air from penetrating into the gas sampling volume. Petroleum jelly on these two rubber stoppers ensured a perfect air- and water-tightness and this was verified under laboratory conditions before using the probes. A nylon membrane was used to avoid soil particles entering the perforated tube and to limit further water infiltration."

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

We agree this was a bit confusing. We have rephrased this explanation as above:

"At each depth, the measurement of CO_2 concentration in triplicate allowed evaluating its spatial variability (Maier and Schack-Kirchner, 2014). The averaged CO_2 concentration for each depth interval was then representative of a measurement footprint of approximately 5 m^2, covering the same area as the IRGA chamber network located at the soil surface (Fig. 4)."

Page 13704, line 19: change "than" to "as"

Done

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

This information is presented further in this section, and is also visually represented in detail on Figure 4. We now describe the measurement design in more detail:

"At each of the 2 slope positions, we measured soil CO2 concentrations profiles at 4 soil depths using 3 replicates for each depth measurement (Fig. 4). For soil VWC profiles, at each of the 2 slope positions, 18 measurements were collected (6 soil depths, 3 replicates). Sampling depths for VWC were 10, 25, 35, 50, 70 and 95 cm (Fig. 4). For temperature, 4 soil depths (10, 25, 45, 85 cm) without replicates were measured (Fig. 4). The complete sampling design is schematically presented in Fig. 4."

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).

We have rephrased this as follows:

"The concentration ranges of the Vaisala CO_2 probe were adjusted for each soil depth and for each slope position. This allowed an optimal fit of the probes to the local concentrations. Each probe has

to characterize the entire range of values encountered across seasons while at the same time, it should have a sufficiently narrow measurement range to ensure the quality of observation in terms of precision."

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.

We used the Moldrup et al. (2000) model (Eq. 4), involving the Campbell soil water retention curve model. We chose this model because it was shown to provide the most accurate and precise results in the case of CO_2 fluxes calculation (Davidson et al., 2006; Goffin et al., 2014). This was also demonstrated in a methodological chapter of the PhD thesis of Wiaux, 2014. Including the soil water retention curve allows considering the impact of soil water content and tortuosity of the soil medium on diffusivity. Hence, a proxy for soil structure (slope and air entry value of the retention model) is integrated in the diffusivity model.

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 (~ 395 ppm)? That would probably be the correct way to assess the relevant concentration gradient to calculate surface CO2 fluxes.

Indeed, we constrained the surface CO_2 concentrations with atmospheric CO_2 levels (about 400 ppm). This was already explained in the manuscript, but we now precise this here also. So yes, we account for the diffusion gradient between the top soil and the atmosphere.

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.

We appreciate this suggestion. We added a short section (3.2) that provides information on the variability of measured CO2 concentration profiles. In this new section, you will find information on both (i) the variability of daily extrapolated CO2 concentrations at the bottom of the soil profile and (ii) the variability of hourly measured CO2 concentrations at the 4 measurement depths. The latter has been summarized in a new Table (Table 1). These results provide a better idea of the quality of the data. We thank the reviewer for this suggestion.

"Providing unique values" sounds weird, maybe providing "an average value for each soil depth and location".

We agree and adopted your rephrasing.

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 CO2 fluxes based on surface measurements at these two slope positions (plus further measurement points in-between).

This hillslope scale quantification issue has been removed (please see editor comments and our answer). Hence, we do not extrapolate anymore.

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.

This hillslope scale quantification issue has been removed (please see editor comments and our answer). Hence, this modeling component is not used anymore.

Page 13712, lines 20 to 25: I assumed that among the papers published in this study, this one focused on the depth CO2 concentration profiles and contributions of different soil layers. The reviewer has not seen Wiaux et al., 2014 in press, but here the authors refer to published soil CO2 concentration profiles in a study in review.

Wiaux et al. (2014c) has been rejected and will not be resubmitted elsewhere. Hence, there is no overlap anymore with this paper. In addition, this paper now much more focus on the depth CO_2 ***concentration profiles and contributions of different soil layers rather than on the hillslope scale modeling issue.***

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 CO2 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 CO2 diffusion and concentration profiles is new?

Please see the previous comment.

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., 2014b 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.

This hillslope scale quantification issue and the modeling has been removed (please see editor comments and our answer). Hence, there is no risk of overlap with our previous publications comparing soil CO2 emissions at different slope positions. The results we present here are really novel and original, as well compared to our own papers as relatively to the literature. We indeed achieved a vertical partitioning of the soil CO2 fluxes along two contrasted soil profiles in a cropland, which was never done before in similar sites to our knowledge. Based on that, we highlight that downslope soil surface CO2 emissions do not reflect soil micro-organisms respiration but are much more governed by soil physical controls, and that soil OC at such a footslope is stored throughout the soil profile (below 10 cm) and submitted to a long-term stabilization.

Anonymous Referee #2

General comments

I find this article novel using an interesting approach to understand the role of landscape in the carbon cycle, and linking results of two different scales (landscape and soil profile). The authors a solid dataset with interesting results, and, in general, their discussion of results is solid and clear. I appreciate the methodological details, although I find that they are sometimes too detailed (the 'Material and Methods' part has almost the same number of words as the 'Results' and 'Discussion' combined). I suggest to accept publication of this article with minor revisions.

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

My general comments concern the involved mechanisms for both summit and footslope: soil micro-organism respiration seemed to control CO2 emissions in the summit soils, whereas soil respiration also increase the CO2 flux in the first 10 cm of the footslope soils (second §of 4.2, lines 3-16, page 13717). I understand that, in general, the CO2 flux at the footslope position is limited by the diffusivity, but the authors should state that microbial activity as a driver for CO2 emission is not specific to the summit soil only, as authors mentioned in the conclusion.

We now more carefully discuss the contribution of microbial respiration and the factors controlling diffusivity. See section 4.1 of the revised manuscript.

Moreover, comparisons with results from Goffin et al. (2014) were not discussed in term of types of soil or vegetation (for example, forested ecosystem in Goffin et al., 2014), and I would welcome more detailed comparison to the results provided therein (i.e., not be exclusively focus on the surface or deep layers).

The Goffin study reports on CO_2 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 have briefly clarified this in the discussion.

Moreover, I frequently read "in agreement with the recent findings of Wiaux et al. (2014)", "corroborates the results of Wiaux et al. (2014)", or "as described by Wiaux et al. (2014)". The authors have should better highlight the novelty of their current findings. In the current manuscript, it seemed that several results were already found in the previous studies, so I suggest that the authors focus on the novel aspects only.

As indicated in the responses above, we have more clearly identified the novel contributions of this study.

Furthermore, I do not understand the exact meaning of "hillslope aggregated CO2 flux" (abstract, as well as in the text). It will be nice to clarify the expression "aggregated" (spatial scale through the landscape, through the soil profile, temporal scale. . .?).

This hillslope scale quantification issue has been removed (please see editor comments and our answer). Hence, "hillslope aggregated CO2 flux" has been removed from the revised manuscript.

Finally, I find that figures are, in general, too small and it is difficult to read the captions, particularly for figures 5 and 6 where I cannot distinguish the different depths.

We have edited figures 5 and 6 to make it clearer and easier to read (and figure 8 has been removed). To that aim, we split Fig.5 into Fig.5A and Fig.5B and Fig.6 into Fig.6A, Fig.6B and Fig.6C. This will also allow a more flexible use of these figures for page setting, e.g. for the choice of disposition as well as to display these figures with a higher size to ensure a clear reading.

Also, the figure 4 is little bit complex: the caption need probably more explanation, like the difference between the foreground and the background.

We provide further explanation about the difference between the foreground and the background in the caption of figure 4.

The figures 1 and 2, rapidly described in line 11 (page 13703), are part of 'Results' (rather than 'Material and Methods').

Figures 1 and 2 are results of one of a previous publication (Wiaux et al., 2014a). We therefore suggest to keep it in the 'Material and Methods' section.

A final comment about the very frequent use of "ca." (circa). I suggest to replace this term with "about" or "approximatively" when necessary, and to remove it from many sections (e.g., when the authors write "and reach ca. 1811 g CO2-C m−2 year−1", it seems that this is quite a precise value and "ca." is not needed).

We now carefully avoid the frequent use of this term in the revised manuscript. In addition, we replaced "c." by the more explicit term "approximatively" throughout the manuscript.

<u>Minor comments</u>

Line 10: "for two periods of 6 months"

This has been corrected

Line 14: "from the first 10 cm"

This has been corrected

Lines 18-19: you have a problem in the unit: g CO2-C m−2 year−1

You are right, but this flux quantification issue has now been abandoned.

Introduction

Line 19 (page 13702): "that the first 30 cm of soil"

This has been corrected

Material and methods

Line 13 (page 13703): "for 48h"

This has been corrected

Line 7 (page 13704): I think that you mean Fig. 3 and not Fig. 4, right?

No, Fig. 4 illustrates that the measurement plots were covered with a synthetic permeable geotextile.

Line 26 (page 13707): the parenthesis is not closed

This has been corrected

Line 7 (page 13708): "by Wiaux et al. (2014c)"

This has been corrected

Results

Line 6 (page 13712): "from 4 to 28 ◦C at both, the submit and the footslope" (if it is the same temperature for the two locations)

There was indeed an error but the maximum temperature was not the same at the two locations. This has been corrected.

Lines 10. . . (page 13712): can you use the same units in both text and figure 5 (% or cm^3 cm^{-3})

We converted volumetric water content values in cm^3 cm^{-3} everywhere in the manuscript.

Line 12 (page 13712): I do not understand the "(respectively)". Did you mean from 38 (23+15) to 39 (34+5)? It is not clear.

Yes, this is what we mean. We removed the "respectively" to avoid misunderstanding.

Line 23 (page 13712): "at ca. 50 cm depth", right?

This has been corrected.

Line 8 (page 13713): "in the first 10 cm"

This has been corrected.

Line 10 (page 13713): "depending on"

This has been corrected.

Line 12 (page 13713): "of the first 10 cm"

This has been corrected.

Line 14 (page 13713): "the first 30 cm"

This has been corrected.

Line 23 (page 13713): "Table 1 and Fig. 8", right?

This section has been removed.

Line 15 (page 13714): "ca. 1.5 times more CO2-C"

This section has been removed.

Line 17 (page 13716): "the first 10 cm"

This has been corrected.

Line 20 (page 13716): change "who" to "which", it is "the study of Goffin et al. . ."

This has been corrected.

Line 22 (page 13716): "neither", did you mean "not" or "never"?

This has been corrected (we replaced "neither" by "not")

Conclusions

Line 11 (page 13721): "the first 10 cm"

This has been corrected.

Reviewer 3 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.

We are very pleased with this positive comment on the relevance of our manuscript. The comments/suggestions raised by this reviewer were very constructive and helpful.

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.

We fully agree with these statements, which have been highlighted by the three reviewers and the editor. We adapted the revised manuscript accordingly, as already explained (please see response to the editor and to the first reviewer).

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.

We apologize for the confusing English. The manuscript has been carefully edited by the more experienced co-authors. We hope that this new version will be easier to understand.

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.

All co-authors were strongly involved in revising this manuscript. We hope this has improved the overall quality of the paper.

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.

Based on these suggestions, we have completely rewritten the introduction; we removed the focus on NEE and eddy-covariance approaches. As already indicated above, the work related to soilRothC modeling has been removed and this addresses the comments related to the modeling approaches.

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.

We added more information with respect to slope, elevation etc but we limited this description to the main characteristics in order to avoid overlap with published work. In order to clearly present the main site characteristics without adding much more text, we also suggest to not remove Figures 1 and 2 (while already presented in published papers).

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.

We agree with your comment. We therefore merged the three similar statements, moved it to section 2.4. and rephrased it like this:

"At each depth and slope position, triplicate VWC and CO_2 concentrations data were averaged, providing good indicators of the mean CO_2 concentrations at each of these locations. This replication strategy at each depth also allow accounting for the spatial variability of VWC and CO_2 concentrations horizontally (Maier and Schnack-Kirchner, 2014), extending their measurement footprint to an area of i.e. 5 m^2."

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.

We have completely rearranged section 2.2. to make it more clear and to avoid repetitions.

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.

The paper Wiaux et al. (2014c) has been rejected and will not be submitted anymore. Hence, the figure showing the Vaisala probes with the membranes (Fig. 3) as well as the figure showing where exactly sensors are located (Fig. 4) do not appear any more in some of our publications. As suggested by the editor, who encouraged us to merge information from Wiaux et al. (2014c) with the present paper, we argue this is now really necessary to keep these two figures.

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

This hillslope scale modeling and the yearly extrapolation issues have been removed (please see editor comments and our answer).

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

This section has been removed.

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

This section has been removed.

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

This section has been removed.

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.

This hillslope scale modeling and the yearly extrapolation issues have been removed (please see editor comments and our answer). This section has been removed.

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

This hillslope scale modeling and the yearly extrapolation issues have been removed (please see editor comments and our answer). This section has been removed.

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 C76192013 for part of the year or am I missing something?

This hillslope scale modeling and the yearly extrapolation issues have been removed (please see editor comments and our answer). This section has been removed.

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

This hillslope scale modeling and the yearly extrapolation issues have been removed (please see editor comments and our answer). This section has been removed.

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

This hillslope scale modeling and the yearly extrapolation issues have been removed (please see editor comments and our answer). This section has been removed.

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

This hillslope scale modeling and the yearly extrapolation issues have been removed (please see editor comments and our answer). This section has been removed.

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

This hillslope scale modeling and the yearly extrapolation issues have been removed (please see editor comments and our answer). This section has been removed.

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

This hillslope scale modeling and the yearly extrapolation issues have been removed (please see editor comments and our answer). This section has been removed.

Results

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

This has been corrected.

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.

This hillslope scale modeling and the yearly extrapolation issues have been removed and Table 1 has been removed (please see editor comments and our answer).

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

This hillslope scale modeling and the yearly extrapolation issues have been removed (please see editor comments and our answer). Section 3.2 has therefore been removed.

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.

This hillslope scale modeling and the yearly extrapolation issues have been removed (please see editor comments and our answer). Comparison with daily chamber-based flux measurements is therefore not carried out anymore.

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

We have edited figures 5 and 6 to make it clearer and easier to read (and figure 8 has been removed). To that aim, we split Fig.5 into Fig.5A and Fig.5B and Fig.6 into Fig.6A, Fig.6B and Fig.6C. This will also allow a more flexible use of these figures for page setting, e.g. for the choice of disposition as well as to display these figures with a higher size to ensure a clear reading.

Discussion

Page 13714-13715: How are differences in CO2 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 CO2 transport mechanisms? In contrast, during periods of high microbial activity, CO2 production may be much higher than diffusion causing CO2 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 CO2 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.

We agree with you that this potentially confusing. The first paragraph of section 4.1 has been expanded and we now provide a more detailed explanation. We also made some linkages with our previous study in relation to soil moisture and temperature controls on soil respiration (Wiaux et al, 2014b).

In the present paper, we observed that at the summit the surface soil CO_2 fluxes clearly follow the temperature variations (see section 4.1). Based on our findings about the controls of soil microbial respiration (Wiaux et al., 2014b), this suggests that observed soil CO_2 emissions at the summit are the result of CO_2 production by microbial respiration. On the contrary, at the footslope, the CO_2 emissions do not follow temperature variations. This strongly suggests that the high water content at this position negatively impacts CO_2 emissions. We argue that the specific dynamic of the CO2 emissions at the footslope is related to the VWC dynamics and that a high VWC: (i) strongly limits the transfer of biotic CO_2 along the soil profile, and (ii) reduces the production of CO2 in situ due to the lack of oxygen for the microbial community. In both cases, the lower CO2 emissions at the footslope relative to the summit are then due to gas diffusion limitations (even indirectly in the case of oxygen lack), contrary to the summit where gas can easily diffuse during all the year and along the entire soil profile (Fig. 6).

We further suggest that the factor controlling CO2 emissions at the footslope is not only VWC as such, but also the difference between the VWC and the water saturation level of the soil pore spaces. While the VWC at the footslope remained high throughout the year, we observed that the soil surface CO2 flux dramatically increased when the gas diffusivity exceeded a threshold value of approximately 0.1 cm2 d-1 (i.e. from day 255 to 305 of year 2013, Fig. 6).

This has been added to the discussion in section 4.1

Page 13715, line 9: Figure 5 shows temperature and moisture, not CO2.

This has been corrected.

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?

This hillslope scale modeling issues have been removed (please see editor comments and our answer). This paragraph and consequently been removed.

Page 13716, line 5-11: How do you come up with this conclusion? You present no information on CO2 production through microbial activity. Presumably this is presented in other papers but if so, you would have to mention this and discuss this.

This has been demonstrated in Wiaux et al. (2014b). This reference supports the control mechanisms which are specific for the summit soil profiles.

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 CO2 to be produced and if there were diffusion limitations you would expect CO2 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.

We apologize for this inconsistency: our results suggest that there is more probably no turnover occurring at the footslope. The evidence consists of (i) approximately 90 to 95 % of the surface CO2 fluxes originates from the 10 first centimeters of the soil profile at the footslope , and (ii) the total and labile soil OC pools remain important and homogeneously distributed along the entire soil profile at the footslope (Wiaux at al, 2014a). Hence, we removed the sentences suggesting any OC turnover and adapted the rest of this paragraph as follows:

"This absence of OC turnover along the footslope profile is also supported by the results of one of our previous publications (Wiaux et al., 2014a), showing that the total and labile soil OC pools remain important and homogeneously distributed along the entire soil profile at the footslope, while it exponentially decreases with depth at the summit (Fig. 1, as described by Wiaux et al., 2014a)."

What about contributions from vegetation over time? Could those be different between the two slope positions?

This issues has been discussed in detail in Wiaux et al. (2014a): there is no difference in plant contribution along the hillslope.

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.

This hillslope scale modeling issues have been removed (please see editor comments and our answer). This section has consequently been removed.

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.

idem

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.

idem

Vertical partitioning and controlling factors of gradient-based soil carbon dioxide fluxes in two contrasted soil profiles along a loamy hillslope.~~Quantitative estimation and vertical partitioning of the soil carbon dioxide fluxes at the hillslope scale on a loess soil~~

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Key words: C dynamic model; CO_2 flux; physical control; vertical partitioning; OC storage; Hillslope; cropland; loess soil.

Type of paper: Regular research paper

Abstract

We assessed soil CO_2 fluxes throughout two contrasted soil profiles along a hillslope in the central loess belt of Belgium. First, we measured time-series of soil temperature, soil moisture and CO_2 concentration at different depthsin the soil profiles for two periods of 6 months. Subsequently we calculated the CO_2 flux at different depths, using Fick's diffusion law and horizon specific diffusivity coefficients. The soil diffusivity coefficients were calibrated using profile specific surface CO_2 flux chamber measurements. The calculated fluxes allowed assessing the contribution of different soil layers to surface CO_2 fluxes and elucidating deep soil controlling factors on CO_2 emission.

The results show that approximatively 90 to 95 % of the surface CO2 fluxes originate from the first 10 centimeters of the soil profile at the footslope. This indicates that soil OC at such a footslope can be stored along the main part of the soil profile (below 10 cm) and submitted to a long-term stabilization. We also observe that time-series of soil CO_2 emissions at the summit are in accordance with the temporal dynamics of temperature. In contrast, at the footslope, we highlight that long periods of CO_2 accumulation alternate with peaks of important surface release due to the high water filled pore space that limits the transfer of CO_2 along the soil profile at this slope position.

~~Both modelling and experimental approaches have been applied to assess C exchange fluxes at large spatial scales. Yet, these approaches are subjected to substantial limitations and uncertainties. Here, we aim tohighlight two key mechanisms able to improve the estimation of the hillslope aggregated CO_2 fluxes: (i) the persistence of soil organic carbon (OC) in deep colluvium deposits; and (ii) the physical controls on CO_2 fluxes along soil profiles. Wassessed soil CO2 fluxes throughout two contrasted soil profileshillslope soil at different depthsin the soil profiles for two periods of 6 months Subsequently we the CO_2 flux using Fick's diffusion law and horizon specific diffusivity coefficients. The soil diffusivity coefficients were calibrated using profile specific surface CO_2 flux chamber measurements. The calculated fluxes allowed assessing of different soil layers to surface CO2 fluxes and elucidating deep soil controlling factors on CO2 emission. This study focuseson a sloping cropland in the central loess belt of Belgium. On two contrasted soil types along the studied hillslope, we recorded time-series of CO_2 concentration, water content and temperature along 1 meter long soil profiles during two~~

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periods of 6 months. Then, we calculated profiles of CO_2 fluxes using the gradient method. To extrapolate these fluxes to entire yearly periods (2011-2013), we performed simulation using the SOILCO2RothCmodel.

The vertical partitioning of the soil CO_2 fluxesresults shows that c. 90 to c. 95 % of the surface CO_2 fluxes originates from the 10 first centimeters of the soil profile at the footslope. We show that high water filled pore space at this slope position disables the transfer of biotic CO_2 along the soil profile. However, the total annual flux averaged along 3 years of simulation show that the top soil layer (0-10 cm) of the footslope generates CO_2 fluxes (870 ± 64CO_2-C m^{-2} $year^{-1}$)which exceed those observed at the summit position (583 ± 61CO_2-C m^{-2} $year^{-1}$). Hence, our results reconcile two seemingly contradictory hypotheses, i.e. (i) these support that soil OC at such a footslope is stored along the main part of the soil profile and submitted to a long-term stabilization, and (ii) at the same time these support that the depositional footslope profile emits more CO_2 than the summit, due to its high amount and quality of OC. Our results support the need to consider slopes when modeling soil-atmosphere C exchanges. If landscapes dynamic processes are not accounted for, we pointed out a risk to under-estimate annual soil-atmosphere CO_2-exchanges by c. 20 %.Walso at the summit the of temperature. In contrast, at , esurface at the footslope the while at CO_2through the profilefootslope are limited constraintscan be

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1. Introduction

Soils play a major role in the global C budget, as they contain 2 to 3 times more C than the atmosphere (Eswaran et al., 1993; Lal et al., 2003). There is now significant concern about the contribution of soil OC to future climate change where a climate change driven acceleration of soil OC decomposition could represent a positive feedback on climate. In addition to the role of soil mineralogy and microbial communities, recent studies highlight the importance of soil bio-physical conditions that may vary substantially with time and across landscapes (e.g. Dai et al., 2012). In addition to the combined effects of soil moisture, temperature and OC quality on soil microbial activity (e.g. Wiaux et al., 2014b), recent studies show the importance of physical controls on CO_2 fluxes such as gas diffusion barriers along soil profiles .(e.g. Ball, 2013; Maier et al., 2011). Furthermore, most process studies so far have focused on the soil surface layer while there is now increasing awareness that subsoil OC represents an important C store that interacts with the atmosphere (Rumpel and Kögel-Knabner, 2011). Recent studies (Rumpel and Kögel-Knabner, 2011) highlighted that deep soil OC is highly processed, and showed the need to consider C fluxes originating from deeper soil horizons. This is particularly relevant in landscapes with complex topography where buried OC in depositional areas contributes substantially to soil C emissions (e.g. Van Oost et al., 2012; Wang et al., 2014 and Wiaux et al., 2014a). Goffin et al. (2014) showed that the upper first 30 centimeters of a forest soil profile contribute substantially to the total surface CO_2 flux. However, to our knowledge, a vertical partitioning has not been evaluated in agro-ecosystems or in systems with contrasting soil physical and/or chemical properties.

In this study, we aim to elucidate the role of physical controls on soil-atmosphere CO_2 fluxes and its variation with soil depth. To that aim, we present a comparative analysis between two contrasting soil profiles along an eroded and cultivated hillslope. The objectives of this study are: (i) to quantify the relative contribution of soil surface and subsoil OC to CO_2 fluxes through a vertical partitioning of these fluxes; and (ii) to identify the role of soil physical properties using time-series of soil moisture measurements and gas diffusivity at different depths. The selected study site is characterized by two contrasting soils in terms of soil hydrological regimes and structure.

Soils play a major role in the global C budget, as they contain 2 to 3 times more C than the atmosphere (ref) and c. 3 times more C than the aboveground biomass. In addition, the size of soil C pool corresponds approximately to a third of the geological reservoir present as fossil fuels (Eswaran et al., 1993; Lal et al., 2003).. There is now significant concern about the contribution of SOC to future climate change where a climate change driven acceleration of SOC decomposition could represent a positive feedback on climate. However, the uncertainty associated with predictions based on state-of-the-art SOC dynamic models is large and this points to substantial deficiencies in the parameterization of current models (ref). In addition to the role of soil mineralogy and microbial communities, recent studies highlight the importance of soil

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Current large scale estimations of the exchange of C between the soil and the atmosphere are associated with large uncertainties (Houghton et al., 2003, 2007; Peters et al., 2010). Both modelling and experimental approaches have been applied to assess C exchange fluxes at large spatial scales. Yet these approaches are subjected to substantial limitations: (i) the current technical possibilities to measure directly hillslope aggregated CO_2 fluxes are limited (e.g. Baldocchi, 2003), and (ii) the complexity of processes at the scale of a whole catchment is not fully considered in current models of C at the soil-atmosphere interface (Chaopricha and Marín-Spiotta, 2014; Trumbore and Czimczik, 2008).

In situ measurements of the hillslope aggregated CO_2 fluxes has been largely achieved using the Eddy-Covariance technique (e.g. Goulden et al., 1996; Eugster et al., 2010), but this technique is not appropriate for sloping landscapes, providing an uncertainty on the CO_2 fluxes ranging from 100 to 200 g C m-2 yr-1 at such non-ideal sites (Baldocchi, 2003). At the local scale, more precise technologies such as survey chambers with infra-red gas analyzers (IRGA) (e.g. Davidson et al., 2002) or such as the non-dispersive infra-red (NDIR) probes (e.g. Young et al., 2009) can be used. However the support scale and spatial resolution of these devices are often too small to make robust large scale assessment of C exchanges across the soil atmosphere interface.

Alternatively, soil modeling of OC dynamics allows assessing the heterotrophic soil respiration (e.g. Herbst et al., 2008). Such models simulations have already been used to calculate the hillslope aggregated CO2 fluxes (e.g. Dai et al., 2012). However, the predictive capabilities of the models are limited because they do not account for the varying topography and Biobio physical conditions that may vary substantially with time and across the landscapes (e.g. Dai et al., 2012) In addition to the combined effects of soil moisture, temperature and OC quality on soil microbial activity (e.g. Wiaux et al., 2014b), and because some key mechanisms controlling the soil CO_2 efflux are not sufficiently implemented in current OC dynamic models: (i), these studies show the importance of physical controls on CO_2 fluxes, e.gsuch as . gas diffusion barriers along soil profiles, . (e.g. Ball, 2013; Maier et al., 2011; Wiaux et al, 2014 c). Furthermore, most process studies so far have focused on the soil surface layer while there is now increasing awareness that subsoil OC represents an important C store that interacts with the atmosphere (ref rumpel; . and (ii) the contribution of buried OC at downslope depositional areas to soil C emissions (e.g. Van Oost et al., 2012; Wang et al., 2014; Wiaux et al., 2014a).

While the transfer of soil OC by erosion has been recognized (e.g. Quinton et al., 2010; Stallard, 1998), its impact on both local and global C budgets is poorly understood (Lal 2003), and consequently not implemented in OC dynamics models. Once brought at the bottom of the slope, sediments deposits enriched in soil OC accumulate and are progressively buried deeper and deeper along the soil profiles, forming colluvic soils at the depositional site, with an increasing soil OC stock (Van Oost et al. 2005a, Van Oost et al. 2005b; Van Oostet al., 2012; Wiaux et al., 2014 a). However, a series of complex and interacting processes are acting in these depositional sites, able to decrease as well to enhance mineralization (Lal 2003; Wiaux et al., 2014b). Recent studies (Rumpel and Kögel-Knabner, 2011) highlighted that deep soil OC is highly processed, and showed the need to study more in details theconsider C fluxes coming originating from deeper soil horizons. This is particularly relevant in landscapes with complex topography where buried OC in depositional areas contributes substantially to soil C emissions (e.g. Van Oost et al., 2012; Wang et al., 2014 and Wiaux et al., 2014a). Recently, through a vertical partitioning of CO_2 fluxes along soil profiles, some authors

Commentaire [Kv1]: Deep soil organic matter—a key but poorly understood component of terrestrial C cycle Rumpel et al Plant & Soil 2011 338: 143-158.

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(Takahashi et al., 2004; Davidson et al., 2006; Goffin et al., 2014) showed that the upper 30 first centimeters of a forest soil profile significantly contribute substantially to the total surface CO_2 flux. However, to our knowledge, such aa vertical partitioning has never been carried out neithernot been evaluated in larger scale agro-ecosystems nor in downslope colluvium with buried OC in deep soil layersor in systems with contrasting soil physical and/or chemical properties.

In this study, we aim to quantify the soil-atmosphere C flux at the scale of a hillslope. More specifically, we aim to evaluate atmospheretwo key mechanisms able to improve this estimation of the hillslope aggregated CO_2 fluxesand its variation with soil depthpresentcontrastingand cultivated . The objectives of this study are: (i) to quantify the persistence of OC in deep colluvium depositsofsoil and subsoilOC to through a vertical partitioning of soil CO_2 fluxes; and (ii) to identify the role of soil physical properties using the physical controls on CO_2 fluxes along soil profiles. The selected study site is characterized by two contrasting soils in terms of soil hydrological regimes and structure. : tThe soil profiles at the bottom of the hillslope is a are colluvial deposits. Although this profile ey shares the same parent material as the non-eroded soils at the summit of the hillslope, it has they havehas different structural properties and areis characterized by specific hydrological conditions (Tran et al., 2015). This allows us to identify the role of soil physical properties on soil-atmosphere CO_2 fluxes.

2. Material and methods

2.1. Study site description

The study was carried out in the Belgian loam belt along a cultivated hillslope of 150 meters length (50.6669°N, 4.6331° W). The site has a maritime temperate climate, with an average annual temperature of 9.7°C and an average annual precipitation of 805 mm. The slope percentage in the backslope area ranges between 8.5 and 16%, with a mean slope of 12%. The slope percentage in the convex shoulder area ranges between 4 to 8.5%, with an average of 6%. The field iswas plowed (0-30 cm soil surface layer) every year. Each year, manure and nitrate applicationfertilization awere also carried out. The current previous crop rotation iwas winter wheat, maize and spring wheat. The study site is has been described in further details in Wiaux et al. (2014a,b). For this study, Wwe selected

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~~2~~two measurement stations along the hillslope: one at the summit and one at the footslope position. The soil is a Dystric Luvisol type at the summit and a Colluvic Regosol in the depositional area at the footslope (Wiaux et al., 2014a,b). The soil properties of these two soil profiles have been characterized by Wiaux et al. (2014a,b): soil total OC, labile ~~pool~~ OC and porosity profiles are illustrated ~~on~~ in Fig. 1 and 2, respectively.

We measured ~~The~~ the total porosity (Ø) ~~was measured~~ in the laboratory by weigh~~t~~ing 100 cm^3 undisturbed soil cores both at saturation and after oven drying at 105°C ~~during~~ for 48h. ~~The~~ We deducd Ø ~~was then deduced~~ from the mass of water needed to fill sample pores. ~~The~~ We calculated the air-filled porosity (ε) ~~was calculated~~ as the difference between Ø and volumetric water content (VWC). We calculated ~~Average~~ average and standard deviation values ~~were calculated~~ on triplicate samples for each depth.

We characterized ~~Soil~~ soil water retention (SWR) curves ~~were characterized~~ using undisturbed soil cores at 10, 25, 35, 50, 70 and 95 cm depth, with 3 replicates at each depth. We obtained the ε_{100} and b parameters ~~were obtained by fitting~~ of the Campbell (1974) SWR model by fitting the model ~~of SWR curve~~ to the SWR observations (Moldrup et al. 2000).

2.2. Monitoring of soil CO_2, water and temperature

We measured soil CO_2 concentrations ~~by means of specifically designed~~using purpose-built soil CO_2 probes. The CO_2 sensor in the probe is based on the CARBOCAP® Single-Beam Dual Wavelength non-dispersive infra-red (NDIR) technology (GMM221, Vaisala corp., Vantaa, Finland). The ~~A~~analytical precision is 1.5% of the measurement range added to 2% of the observed value. The sampling head of the CO_2 probe is a cylinder of 18.5 mm diameter and 40 mm long, covered with a PTFE (polytetrafluoroethylene) membrane, enabling gas exchange and protection against water infiltration. Since the GMM221 sensors were not designed for wet soil conditions, the sensors were encapsulated into an additional perforated PVC tube, providing an additional protection against water (Fig. 1). ~~The sensor was covered with nylon and PTFE (polytetrafluoroethylene) membranes and~~

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encapsulated in a tube to avoid soil particles entering the sensor and to limit water infiltration.This tubing method is an adaptation of the technique presented by Young et al. (2009). We inserted These these tubes were inserted vertically into the soil, after augering creating bore holes with a diameter that equals the diameter of the PVC tubes. This allowedapproach avoids the need –to not backfill the augeringbore hole, which will would have disturb the soil structure and diffusion processperties. Two rubber stoppers, one at 155 mm from the tube head, and another at the top of the tube, prevented atmospheric air from penetrating into the gas sampling volume. Petroleum jelly on these two rubber stoppers ensured a perfect air- and water-tightness and this waswe verified this under (tested in laboratory) conditions before using the probes. We used aA nylon membrane was used to avoid soil particles entering the perforated tube and to limit further water infiltration.

We monitored Ssoil temperature was monitored using a thermistor probe (Therm107, Campbell Scientific Lt., UK). Analytical precision is 0.4°C. We monitored sSoil volumetric water content (VWC) was monitored using Time Domain Reflectrometry (TDR) probes. We used Topp's equation (Topp et al., 1980) was used to determine VWC, from the measured apparent dielectric constant measured by TDR probes. We used Tthe parameters of the Topp's equation were thoseas identified by Beff et al. (2013). TheyIn this study latter study, calibrated the Topp's equation was calibrated for an experimental field in the close vicinity of our fieldstudy site, using the method of Heimovaara (1993) and following the protocol described by Garré et al. (2008). We recorded Wwater, temperature and CO_2 concentration profiles measurements were recorded with an automatic data logger (CR1000, Campbell Scientific Lt., UK), connected to a multiplexer (AM16/32, Campbell Scientific, Campbell Scientific Lt., UK).

In order Tto obtain an equilibrated soil environment around the soil concentrationsoil VWC, temperature and CO_2 concentrations probes, we started measurements 1 month after the installation of the probes installation. We covered The the measurement plots were covered with a synthetic permeable geotextile during the complete measurement period (Fig. 4). This avoided vegetation growth and any autotrophic contribution to the soil respiration.

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At each of the 2 slope positions, we measured soil CO_2 concentrations profiles at 4 soil depths with 3 replicates on each depth (Fig. 4). Triplicate CO_2 concentrations data were averaged, providing unique values for each depth, representative of the entire slope position.

The sampling design is depicted in Fig. 4. At each of the 2 slope positions, we measured soil VWC and CO_2 concentrations profiles with 3 replicates on each measurement depth (Fig. 4). For soilWe collected 18 VWC profiles (6 soil depths, 3 replicates), at each of the 2 slope positions, 18 measurements were collected (6 soil depths, 3 replicates). Sampling depths forWe measured -VWC wereat a depth of 10, 25, 35, 50, 70 and 95 cm depths (Fig. 4). ForWe measured temperature, at 4 soil depths (10, 25, 45, 85 cm) without replicates were measured (Fig. 4). We measured CO_2 concentrations profiles were characterized byat a depth of 4 measurements points (110, 25, 45 and 85 cm depth). The complete sampling design, showing how these probes were collocating with each others, is described in Fig. 4.

Continuous CO_2 concentrations profiles were generated by fitting a decreasing double sigmoidal model to the observations (section 2.3).

As a reference, wWe also performed surface CO_2 fluxes measurements with an infra-red gas analyzer (IRGA) linked to a survey chamber at 16 dates (profile and surface sampling time data matched in time, with a maximum time lapse difference of was within a 30 minutes time interval between each other). Note that The replicates of CO_2 concentrationalong soil profiles allowed catching its spatial variability at the different depths (Maier and Schack-Kirchner, 2014), Tthe averaged values of CO_2 concentration atfor each observation depth extending the measurement footprint to the same area (i.e. 5 m^2) cover than thenthe same area as the IRGA chamber network located at the soil surface (Fig. 4). These reference surface CO_2 fluxes allowed calibrating parameters of the soil gas diffusion modelof Eqs. 1 and 4 to, ensure ensuring the accuracy of profile CO_2 fluxes (section 2.3).

Soil temperature was monitored using a thermistor probe (Therm107, Campbell Scientific Lt., UK). Analytical precision is 0.4°C. Soil volumetric water content (VWC) was monitored using Time Domain Reflectrometry (TDR) probes. Topp's equation (Topp et al., 1980) was used to determine

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~~VWC, from the apparent dielectric constant measured by TDR probes, as further described in Wiaux et al. (2014c).~~

~~Water, temperature and CO_2 concentration profiles measurements were recorded with an automatic data logger (CR1000, Campbell Scientific Lt., UK), connected to a multiplexer (AM16/32, Campbell Scientific, Campbell Scientific Lt., UK).~~

~~At each of the 2 slope positions, we measured soil CO_2concentrations profiles at 4 soil depths with 3 replicates at each depth (Fig. 4).~~

We adjusted ~~T~~the concentration~~s~~ ranges of the ~~Vaisala~~ CO_2 probe ~~concentrations ranges were adapted adjusted to~~for each soil depth and ~~to~~ for each slope position. This allowed an optimal fit of the probes to the local concentrations. ~~to~~ Each probe has to characterize the entire range of values encountered ~~across~~during the ~~,~~seasons ~~and at the same time to~~while at the same time, it should have a sufficiently narrow measurement range ~~without impacting~~to ensure measurement precision~~the quality of observation in terms of precision.~~ At the summit position, measurements ranged between 0-2 % at 12, 25, 45 cm depth and between 0-5 % at 85 cm depth. At the footslope position, measurements ranged between 0-5 % at 12 cm depth, between 0-10 % at 25 and 45 cm depth and between 0-20 % at 85 cm depth.

~~For soil VWC profiles, at each of the 2 slope positions, 18 measurements were collected (6 soil depths, 3 replicates). Sampling depths for VWC were 10, 25, 35, 50, 70 and 95 cm (Fig. 4). For temperature, 4 soil depths (10, 25, 45, 85 cm) without replicates were measured (Fig. 4).~~

We recorded ~~Hourly~~ hourly time-series ~~measurements~~ of VWC, temperature and CO_2 concentration ~~along soil profile~~s ~~were performed recorded in 2012~~ from 12 May to 13 December 2012 and from 14 May to 22 November 2013 at the footslope position and ~~at the footslope and~~ from the 2~~nd~~ ~~of~~ June ~~until~~ to ~~the~~ 13 December 2012 and from the 14 June to 22 November 2013 ~~at the summit,, and in~~~~In 2013, measurements were taken from 14 May at the summit and 14 June 2013 at the footslope until 22 November 2013 at the two stations~~at the summit position. In 2012, important parts of ~~some~~ CO_2

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To increase the quality of the soil concentration data time-series, we removed observations where the battery voltage was lower than 11.5 V. We also corrected soil profile CO_2 concentrations measurements for temperature variations using the empirical formulas described by Tang et al. (2003). This allowed removing the impact of temperature on the CO_2 reading of the CO_2 probe, since the CARBOCAP® technology is temperature dependent. The probe manufacturer (Vaisala corp., Vantaa, Finland) provided probe specific parameters values for the correction formulas.

We averaged triplicate VWC and CO_2 concentrations data, providing an average value for each soil depth and slope position. Note that averaging strategy allows to account for the spatial variability of VWC and CO_2 concentrations (Maier and Schack-Kirchner, 2014), by extending the measurement footprint to an area of c. 5 m^2.

We calculated soil temperature and VWC profiles using a linear interpolation between the depth specific values within the profile. We kept the values constant between the sampling point at the top of the profile and the soil surface. We calculated the CO_2 concentrations profiles by fitting Eq. 2 to the observations. We evaluated the performance of thisfitting by means of the regression coefficient (R^2). When the R^2 values were lower than a threshold value of 95%, we considered the CO_2concentration profile as unreliable and we did not retain the resulting CO_2 fluxes in final analysis.

2.3. Calculation of the CO_2 fluxes

We calculated ~~The~~ the CO_2 flux ~~of CO_2 was calculated~~ using Fick's first law of diffusion according to the gradient method (Eq. 1, e.g. Maier and Schack-Kirchner, 2014):~~.~~

$$F_{CO_2} = -D_s \frac{\partial CO_2}{\partial z} \qquad \text{(Eq. 1)}$$

~~Where~~where F_{CO_2} is the soil CO_2 flux [µmol m^{-2} s^{-1}], D_s the diffusivity of CO_2 in soil [m^2 s^{-1}], CO_2 the soil CO_2 concentration [µmol m^{-3}], and $\frac{\partial CO_2}{\partial z}$ the vertical soil CO_2 gradient.

In order to calculate the vertical soil CO_2 gradient, we used a double sigmoidal equation (Eq. 2), which allows accounting for some curve concavity variations: ~~(Wiaux *et al.*, 2014 c; Maier and Schack-Kirchner, 2014):~~

$$CO_2(z) = 0.04 + A\left(\left(\frac{1}{1+e^{-\gamma_1 z}}\right)+\left(\frac{1}{1+e^{-\gamma_2 (z-d)}}\right)-\left(\frac{1}{2}+\frac{1}{e^{\gamma_2 d}+1}\right)\right) \qquad \text{(Eq. 2)}$$

where *z* is the soil depth [cm], d is the soil depth [cm] at which the sharpness of the curve changes due to a diffusion barrier, γ_1 and γ_2 [cm^{-1}] are fitted parameters which characterize the sharpness of the curve, respectively above and below the soil depth d, and *A* [%] is a reference value used to define the fitted asymptotic value of the CO_2 concentration at infinite depth. ~~The~~ We ~~fitting~~ fitted the ~~parameters~~ *A*, d, γ_1 and γ_2 parameters ~~were evaluated~~ for each CO_2-profile ~~observation in time~~ using ~~a~~ the trust-region-reflective optimization algorithm in Matlab ©. The derivative of ~~this function~~ (Eq. 2~~)~~ provided the CO2 gradient ($\frac{\partial CO_2}{\partial z}$) used in Eq. 1.

The diffusivity of CO_2 in soil, ~~(defined as~~ D_s ~~in~~ (Eq. 1) is a function of the diffusivity of CO_2 in free air (varying with temperature T and pressure, e.g. Davidson *et al.*, 2006) and of the gas tortuosity factor (ξ) (Eq. 3~~).~~):

$$D_s = \xi\, 1.47\, 10^{-5}\left(\frac{T+273}{273}\right)^{1.75} \qquad \text{(Eq. 3)}$$

~~Where~~ where ξ depends on soil physical and hydrological properties. We used the Moldrup *et al.* (2000) model (Eq. 4) which was shown to provide the most accurate and precise results (Davidson et al., 2006; Goffin et al., 2014~~; Wiaux *et al.*, 2014 c)~~); ~~. We verified that this diffusion model was suitable for our specific and contrasted soil profiles by comparing the calculated gradient based CO_2 fluxes with directly measured IRGA CO_2 fluxes. This analysis indicated a good prediction with and (R^2 of =92% for all soil types together, data not shown).~~

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$$\xi = (2\varepsilon_{100}{}^{3} + 0.04\varepsilon_{100})\left(\frac{\varepsilon}{\varepsilon_{100}}\right)^{2+3/b} \qquad \text{(Eq. 4)}$$

~~Where~~ where ξ is the gas tortuosity factor, ε [m^3 m^{-3}] is the soil air-filled porosity, b [-] is the slope of the Campbell (1974) soil water retention curve model between -100 and -500 cm H_2O water suction, and ε_{100} [m^3 m^{-3}] is the soil air-filled porosity at a soil water potential of -100 cm H_2O.

CO_2 fluxes, as assessed by the gradient based method, were calculated on an hourly time-scale, and then integrated on a daily basis. Temperature, VWC, diffusivity and CO_2 concentration values were also averaged on a daily basis.

In contrast to other studies (e.g. Pingintha et al., 2010; Turcu et al., 2005), we did not aggregate the soil ~~CO_2~~ diffusivity coefficient ~~was not aggregated~~ for the entire soil profile or for an entire soil layer. We considered the ~~, but its~~ vertical distribution ~~was~~ explicitly, and integrated ~~accounted for. To this end,~~ Eq. 4~~. was inserted~~ in the finite difference numerical solution of ~~into~~ Eq. 1. In this numerical integration, we ~~, and Eq. 1 was then numerically~~ used ~~evaluated using~~ a depth increment of 0.1cm and ~~by constraining~~ constrained the surface CO_2 concentrations with atmospheric CO_2 levels (i.e. 400 ppm). ~~The surface CO_2 flux that we obtained with using the gradient-based method was then considered as being~~to be ~~the top of the calculated CO_2 flux profile using Eq. 1.~~

We calibrated the diffusion model by adjusting the parameters related to the gas diffusion coefficient (i.e. b and ε_{100}) such that calculated fluxes fit punctual CO_2 fluxes observations at 16 dates spread along the measurement period. We obtained these observations by means of a portable infra-red gas analyzer with an automated closed dynamic chamber (LI-8100A system, LI-COR, United-States), following Davidson et al. (2002). The sampling design of these surface chamber CO_2 fluxes measurements on the same study site has been described in Wiaux et al. (2014 b). Comparing the gradient-based CO_2 fluxes with directly measured IRGA CO_2 fluxes, we obtained a good prediction with a R^2 of 92% for all soil types together. This ensures the consistency (and consequently the

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Mis en forme : Police :(Par défaut) Times New Roman, 11 pt precision) of the calculated fluxes. The slope of the fit (i.e. 1.05 and 1.22, respectively in 2012 and 2013) was used to correct the calculated fluxes and to ensure accuracy.

We verified that the diffusion model was suitable for our soil profiles by comparing the gradient-based CO_2 fluxes with surface CO_2 fluxes as made with the IRGA flux chamber. We calibrated the diffusion model by adjusting the parameters related to the gas diffusion coefficient (i.e. b and ε_{100}) such that calculated fluxes fit punctual CO_2 fluxes observations. We obtained these observations by means of a portable infra-red gas analyzer with an automated closed dynamic chamber (LI-8100A system, LI-COR, United States), following Davidson et al. (2002). The sampling design of these surface chamber CO_2 fluxes measurements on the same study site has been described in Wiaux et al. (2014 b). The regression coefficients of the relationship between both measured surface chamber and calculated CO_2 fluxes ensure the consistency (and consequently the precision) of the calculated fluxes (i.e. R^2=92% both in 2012 and 2013). We used the slope of the fit (i.e. 1.05 and 1.22, respectively in 2012 and 2013) to correct the calculated fluxes and to ensure accuracy.

2.4. Data treatments and adjustments

To optimize increase the quality of the soil concentration data time series, observations corresponding where tohe battery voltage was lower than 11.5 V were removed. Soil profile CO_2 concentrations measurements were a posteriori also corrected for temperature variations using the empirical formulas described by Tang et al. (2003). This allowed removing the impact of temperature on the CO_2 reading of the CO_2 probe, since the CARBOCAP® technology is temperature dependent. Probe specific parameters values for these correction formulas were provided by the probe manufacturer (Vaisala corp., Vantaa, Finland).

Triplicate VWC and CO_2 concentrations data were averaged, providing an average value for each soil depth and locationslope positionunique values for each depth, representative of the entire slope position. AtFor each depth and slope position, the triplicate CO_2 concentrations data were averaged, providing good indicators of the mean CO_2 concentrations at each of these locationsslope positions. Note that Tthis replication strategy at each depth also allows accountingto account for the spatial variability of VWC and CO_2 concentrations horizontally (Maier and Schack-Kirchner, 2014), by extending the measurement footprint to an area of i.ec.. 5 m^2.

Soil temperature and VWC profiles were calculated using a linear interpolation between the depth specific values. Surface values were not extrapolated, and were considered as being equal to the closest observations in the profiles. CO_2 concentrations profiles were generated by fitting a decreasing double sigmoidal model to the observations as described in the previous sub-section. The performance of this model (Eq. 2) was evaluated using the regression coefficient (R^2). When R^2 values of the fitted CO_2 profiles were lower than a threshold value of 95%, the gradient of CO_2concentration was considered as unreliable and the resulting CO_2 fluxes were not calculated retained in the final analysisat that time.

/isn't the following section a repetition of what you describe above? We calibrated the diffusion model by adjusting the parameters related to the gas diffusion coefficient (i. e. b and ε_{100}) such that calculated fluxes fit punctual CO_2 fluxes observations. These observations were obtained by means of a portable infra-red gas analyzer with an automated closed dynamic chamber (LI-8100A system, LI-COR, United States), following Davidson et al. (2002). The sampling design of these surface chamber CO_2 fluxes measurements on the same study site has been described in Wiaux et al. (2014 b). The regression coefficients of the relationship between both measured surface chamber and calculated CO_2 fluxes ensure the consistency (and consequently the precision) of the calculated fluxes (i.e. R^2=92% both in 2012 and 2013). The slope of the fit (i.e. 1.05 and 1.22, respectively in 2012 and 2013) was used to correct the calculated fluxes and to ensure accuracy, as explained in Wiaux et al. (2014 c)..

CO_2 fluxes, as assessed by the gradient based method, were calculated on an hourly time scale, and then integrated on a daily basis. Temperature, VWC, diffusivity and CO_2 concentration values were also averaged on a daily basis.

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2.~~5~~4. Vertical partitioning of CO_2 fluxes

We partitioned ~~The~~ the ~~space~~ continuous CO_2 flux~~es~~ profiles obtained using Eq.2 ~~were partitioned~~ into 10 slides of 10 centimeters along the soil profile. For each soil slide, we calculated the difference between the top and bottom fluxes. We -divided ~~This~~ this difference ~~was then divided~~ by the total CO_2 flux (e.g. the value at the soil surface). This provides the relative contribution in terms of both CO_2 production and transfer (in %) of each soil slide to the surface CO_2 flux (e.g. Goffin et al., 2014; Maier and Schack-Kirchner, 2014).

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In order to allow an easy representation of the temporal dynamic of this vertical partitioning, we averaged -values ~~were averaged~~ on a semi-seasonal time-scale. Standard deviation values reflect the variability ~~in~~ over time during each semi-season.

~~2.6. Interpolation and aggregation of CO_2 fluxes in time and space~~

~~Field measurements were carried out during limited time periods, and hence would not allow assessing the C budget at the whole year scale. In order to obtain continuous time series covering the entire yearly periods, an OC dynamics model was used as a tool to interpolate and extrapolate measured data of VWC, temperature and CO_2 fluxes for the period of 3 years (2011-2013). Then, we integrated the daily simulated CO_2 fluxes for each of the three studied years. These yearly CO_2 fluxes were averaged over the three studied years and compared between slope positions. The mean yearly CO_2 flux obtained at the summit position was considered to be representative of a non-sloping landscape. To calculate mean yearly CO_2 fluxes representative of a hilly landscape, we calculated a weighted average CO_2 flux of the summit and the footslope, thereby considering the fact that the footslope colluvium represents c. 35% of the surface area of the studied hillslope.~~

~~2.6.1. Description of the SoilCO$_2$-RothC model~~

~~The SoilCO$_2$-RothC model has been described in detail by Herbst et al. (2008). The model combines the coupling of a one dimensional water, heat and CO_2 flux model (SOILCO2) with a pool concept of hillslope aggregated CO2 fluxesbon turnover (RothC) for the prediction of soil respiration. The~~

performance of this model was previously evaluated by Herbst et al. (2008) based on a 8 years data set of CO_2 fluxes measurements, and its predictions were judged to be acceptable (with a difference of 0.007 g C m^{-2} d^{-1} between measured and simulated mean daily respiration rates).

This model was run on a daily time step for a period of three years (2011-2013), both for the summit and the footslope positions. Other temporal resolutions (i.e. hourly and weekly time steps) were evaluated but provided poor results.

The unsaturated soil water flux is described by the Richards equation, and both the soil water capacity and unsaturated hydraulic conductivity function are calculated according to Van Genuchten (1980). Heat transport is implemented according to Simunek and Suarez (1993). Transport of soil CO_2 is simulated considering diffusion and convection in the gas phase, as well as dispersion and convection in the liquid phase. For CO_2 diffusion in the gas phase, we implemented the new Moldrup et al. (2000) model (Eq. 4) which was shown to be appropriate for calculating CO_2 fluxes (Davidson et al., 2006; Goffin et al., 2014; Wiaux et al., 2014c).

For the production of CO_2, we considered two different OC pools, i.e. decomposable plant material pool (i.e. DPM, with a high decomposition rate constant, representing the labile OC pool as defined in chapter 3) and a humus pool (i.e. HUM, with a low decomposition rate constant, representing the stable OC pool as defined in chapter 3). Reduction factors functions are used to simulate the effect of CO_2 concentration, water pressure head, and temperature on the CO_2 production according to the original version of the SOILCO2 model, as described by Simunek and Suarez (1993).

For the boundary conditions for the soil hydrological balance, we used meteorological data, i.e. precipitation and evapo-transpirationat at the top of the soil profiles, and a free drainage concept at the bottom of the soil profiles. Precipitations were directly measured in a meteorological station close to our study site (c. 2 km). At the summit, we considered a run-off production once input water flux exceeds the infiltration capacity, while at the footslope we specified that water can accumulate at the soil surface. Daily evapo-transpiration was calculated according to the Penmann-Monteith equation, based on measured meteorological data. The boundary conditions of soil heat flow were defined using directly measured soil temperature both at the top and at the bottom of the soil profile.

2.6.2. Model parametrisation and calibration

In this study, 5 soil depth increments were considered for the two studied soil profiles, i.e. 0-18 cm, 19-30 cm, 31-45 cm, 46-70 cm, and 71-100 cm depth. These increments were chosen to consider the depths where measurements probes were installed and the soil structural properties (Wiaux et al., 2014a). The soil hydrodynamic parameters of the van Genuchten – Mualem function (Mualem,1976; van Genuchten,1980), as well as parameters related to the gas diffusion coefficients (i.e. b and ε_{100}) are specified for each soil material. The initial concentration of the labile and stable OC pools were specified for each soil material, as presented in Wiaux et al. (2014a).

For identifying the value of input parameters, we calibrated the model using the global inversion model PEST (e.g. Gallagher and Doherty, 2007). We used measured soil VWC, temperature and CO_2 concentration measurements, as well as calculated CO_2 fluxes within the profile to invert the model.

We carried out a simple sensitive analysis of the SOILCO2-RothC model to identify key parameters. Among the more sensitive parameters, which could significantly impact the outputs of the model, we firstly inverted the 9 soil Mualem - van Genuchten parameters, related to VWC, both at the summit and at the footslope. In a second step, we kept them fixed and inverted parameters related to soil CO_2 fluxes, both at the summit and the footslope: (i) the 5 initial concentrations of the labile OC pool, (ii)the decomposition rate of the labile OC pool, (iii) the activation energy reflecting temperature sensitivity, as well as (iv) the HB1 coefficient (i.e. the value of the pressure head at which CO2 production by soil micro-organisms is at the optimal level). Initial concentrations of the labile OC pool were inverted inside a realistic range of values (i.e. average ± 3 times the standard deviation) as compared to the previous measurements done by Wiaux et al. (2014 a). At the footslope, we additionally inverted the 5 ε_{100} parameters related to the gas diffusion.

3. Results

3.1. Spatio-temporal analysis of measured soil variables

Fig. 5 shows the spatio-temporal variation of soil temperature~~,~~ and moisture~~ and gas diffusion~~, ~~and~~ while Fig. 6 shows the spatio-temporal variation of CO_2 fluxes, concentrations and ~~and~~ diffusion~~fluxes~~. All these values correspond to in-situ measurements during a 6 month period ~~of c. 6 months~~ in 2013. Similar measurements have been carried out in 2012 and display similar spatio-temporal trends (data not shown).

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~~The~~ During the observation period, the soil temperature (Fig. 5A) ~~does~~ did not significantly differ between the summit and the footslope, ~~except~~ although higher temperatures were observed at the summit profile for some shorter periods ~~during July~~ (e.g. day of year 180 to 220~~)~~ where temperatures ~~are~~ are ~~c.~~approximately ~~c.~~ 2 to 3 ~~degrees~~ °C higher~~higher at the summit while they follow exactly the same temporal dynamic~~). The ~~surface~~ mean daily surface temperatures ~~vary~~ range ~~all along the soil profiles from~~between 4°C to 28°C at the summit, and ~~from~~ between 4°C to ~~28~~25°C at the footslope ~~(for the period of measurements)~~.

The space-time dynamics of the soil volumetric water content (VWC, Fig. 5B) differ ~~completely~~ substantially between the summit and the footslope profiles. At the footslope, the observed soil VWC ~~values~~ at different soil depths ~~remained inside~~varied in a narrow ~~interval~~ range (0.36 to 0.39~~%)~~ cm^3 cm^{-3}) ~~all along the soil profile during the considered period~~. In contrast, ~~At the summit,~~ soil VWC at the summit varie~~s~~d ~~from~~ between 0.23 to 0.34~~%~~ cm^3 cm^{-3} ~~in~~ for the plow layer (0-30cm depth) and ~~then increases by an absolute value of 0.15 to~~higher values (~~c.~~approcimately 0.5 cm^3 cm^{-3}) were observed ~~0.5% cm^3 cm^{-3} (respectively) in~~for the rest of the soil profile. ~~-~~The soil at the summit position ~~is~~ was the wettest during the early spring and the late autumn and driest in the summer. At the footslope, soil VWC ~~reaches~~ reached the saturation level in the early summer after an important rainfall event and then slowly ~~decreases~~ decreased until the early autumn and ~~reaches~~ reached saturation again in the late autumn. ~~Similarly~~

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~~Inversely~~In contrast to the VWC, ~~in the summer,~~ the soil gas diffusivity (Fig. 6C) ~~reaches~~ reached its maximum value in the summer at the summit while it ~~reaches its lowest value~~was low at the footslope. Soil gas diffusivity ~~is c.~~was approximatively 10 times lower at the footslope ~~relative~~ than at ~~to~~ the summit.

~~Soil CO_2 concentrations (Fig. 6) are c. 3 times lower at the summit relative to the footslope. Along soil profiles, soil CO_2 concentrations increases with depth, following a double exponential trend as described in Wiaux et al. (2014c).This second exponential curve begins at c. 50 depth, and is especially pronounced at the footslope, reflecting a shift of c. 4% CO_2between 44 and 100 cm depth.~~ The ~~time course of~~ soil CO_2concentrations at both the summit and the footslope ~~increases~~ increased gradually from spring to late summer. Thereafter, concentrations dropped again and ~~and then decreases to reach its~~ lowest values ~~in~~were observed in the late autumn.

~~The CO_2 fluxes (Fig. 6) were calculated based on both CO_2 concentrations and diffusivity following the method described in Wiaux et al. (2014c). These~~ The ~~calculated~~ ranges of CO_2 fluxes ~~vary in the same range of values when comparing the~~obtained for the footslope and ~~the~~ summit profiles were very similar. However, their temporal distribution was different: -the ~~peaks~~ periods characterized by high CO_2 fluxes ~~do~~ did not occur at the same ~~period from a slope position to another~~time and had a different duration.~~,~~ ~~with maximum CO_2 fluxes being emitted respectively during summer and autumn for the summit and the footslope. In addition, the duration of these maximum peaks differ between the summit and the footslope. Along the~~For all soil profiles, CO_2 fluxes decreased with depth and reached null values at ~~c.~~approximately~~.~~ 30 cm depth at the summit and at ~~c.~~approximately ~~c.~~ 15 cm depth at the footslope.

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3.2. Shape and variability of CO_2 concentrations and fluxes profiles

~~Along soil profiles,~~ The observed soil CO_2 concentrations (Fig. 6Bb) ~~increases~~ increased with soil depth, ~~ranging~~ from the atmospheric value of 0.04 % ~~until~~at the surface to concentrations which were ~~2~~two orders of magnitude higher ~~concentrations~~ at 100 cm depth ($CO_{2,}$(z) in Eq.2) (Fig. 6Bb). For the measurement period of 6 months considered here, $CO_{2,}$ concentration values at 100 cm depth ~~are~~were

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3three to 4 four times higher at the footslope position than at the summit position. In 2013, these values ranged frombetween 0.86 to 3.46 % at the summit position and frombetween 3.68 to 9.12 % at the footslope position.

These observed CO_2 concentration profiles followed a double exponential trend (Eq. 2). This particular model fitsfitters our observations quiterelatively well, with regression coefficients ranging between 97 to 100% depending on the date and on the slope position. This The second exponential curve begins starts approximately at about 50 cm depththe middle of the profile, and is especially particularly pronounced at the footslope, reflecting a shift of approximatively c.nearly 4% CO_2 between 44 and 100 cm depth.

Standard deviations around averaged values of observed hourly CO_2 concentrations at each depth are given in Table 1. Thise small-scale spatial variability is quite low and remains reasonable relative to the mean values of triplicated CO_2 concentrations, the only exception beginbeing the footslope at excepted at 25 cm depth at the footslope where the maximum standard deviation exceeded the maximum mean value.

The CO_2 fluxes (Fig. 6A) were calculated based on both CO_2 concentrations and diffusivity. For all soil profiles, CO_2 fluxes decreased with depth and reached null values at c.30 cm depth at the summit and at c. 15 cm depth at the footslope.

3.3. Vertical partitioning of CO_2 fluxes

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The distribution of the soil CO_2 fluxes in the profile is illustrated in Fig. 7. At the footslope, c. 90 to c. 95 % of the surface CO_2 fluxes is was generated in the 10 first 10ten centimeters of the soil profile. The soil layer between 10 and 20 cm contributed for only 5 to 10 % depending of the period, and the deeper layers dot did not significantly contribute to the surface fluxes. At the summit, the relative contribution of the different soil layers is was more dynamic in time, with a contribution of the 10 first 10ten centimeters of the soil profile ranging from c. 80 % at the late spring, decreasing to c. 60 % in the early summer, and reaching ~~c.~~ 40 % from late summer to the late autumn. At the summit, the ~~30~~ first 30 centimeters of the soil profile significantly contributed to surface fluxes. This contribution ~~decreases~~ decreased with depth in the late spring and the early summer, but is homogeneously distributed with depth for the rest of the time. At the summit, soil layers deeper than 30 cm depth sometimes contributed for up to 20% of the total flux, especially in the autumn. Between 40 to 50 cm depth, and 80 to 90 cm depth, some negative contribution (i.e. CO_2 uptake) up to -20% is also observed.

~~3.2. Modeling of surface CO_2 fluxes~~

~~Results of the daily aggregated simulation with the SoilCO2-RothC model are summarized in Table 1 and Fig. 6. The simulated soil temperatures and VWC represent adequately the observations (Table 1). The simulated CO_2 fluxes fit well the CO_2 fluxes in 2013, both for the summit and the footslope. This fit is less good at the footslope (R^2=42%, Table 1) but it remains acceptable given the quite local shift between observations and model simulations (Fig. 8). This shift (model underestimation) may be explained by the contribution of soil alkalinity to soil CO_2 fluxes during specific dry events in summer (e.g. Laponis et al., 2008). Punctual surface CO_2 fluxes measurements (Licor chamber system) extended to daily values corroborate the goodness of fit (GOF) of the simulations in 2012 (Table 1 and Figures 6 and 7).~~

~~Simulated surface CO_2 fluxes (Fig. 8) remains more or less in the same range of values (from 0 to 6 g C m^{-2} day^{-1} in 2011 and 2013, and up to 8 g C m^{-2} day^{-1} in 2012). However, the temporal dynamic differs between slope positions and between years of simulation, with a clear alternation of peaks of year 2011 and 2013. At the summit, CO_2 fluxes increases from the winter to reach their maximum during the summer and then decreases again (Fig. 8), similarly to the temporal dynamic of soil temperature (see Fig. 5 as an exemple). At the footslope, the lowest CO_2 fluxes occur in the middle of the summer of each year, while a very high CO_2 fluxes can be observed from the late summer until the early autumn in 2011 and 2013 as well as in spring of year 2011 (Fig. 8).~~

The time integrated CO_2 fluxes are presented in Table 2. For the considered a simulation period of 3 years, the footslope emits c. 1.5 more CO_2-C than the summit ($p<0.01$), which represents an additional flux of 287 ± 106g CO_2-C $m^{-2}year^{-1}$. The uncertainty on model simulations (given by ME values in Table 2) remains lower than the difference between slope positions for each year (Table 2). Once integrated at the hillslope scale, this means that such a loamy hillslope emit c. 1.2 times more CO_2-C relative to a flat landscape ($p<0.1$).

4. Discussion

4.1. Soil physical control on CO_2 emissions

The observed differencess of thein the temporal dynamicss of surface soil CO_2 fluxes between the footslope and summit positionssoil profiles (, as illustrated in Fig. 6A,) indicates that the limiting controlling factors on flux emissions are not the same all along the hillslope. At the summit, on one hand, the dynamic of surface soil CO_2 fluxes (Fig. 6A) clearly follows the temperature variations (Fig. 5A, maximum during the summer). At the footslope, on the other hand, the surface soil surface CO_2 fluxes (Fig. 6) are remainwas small even when temperature increasesd and isremained relatively small the lower during thethroughout the summer period (Fig. 6A). This canis most likely related be due to the particularyvery high VWC values observed at the footslope (Fig. 5B), knowingas it is well known that VWC negatively impacts soil CO_2 emissions (e.g. Webster et al., 2008b; Perrin et al., 2012; Wiaux et al., 2014b). when the soil is close to water saturation. More precisely, we suggest that the factor controlling of the CO_2 emissions at thisthe footslope is not only VWC but also the degree of proximity ofdifference tbetween the VWC fromand the water saturation level of the soil pore spaces. Indeed, w While the VWC at the footslope the VWC remainsed high duringthroughout all the year, we observed here that the soil surface soil CO_2 flux dramatically increasesd when the gas diffusivity exceedsed a threshold value of approximativelyc. 0.1 cm^2 d^{-1} (i.e. from day 255 to 305 of year 2013, Fig. 6A). Hence, -we argue that the specific dynamic of the CO_2 emissions This at thisthe footslope profile (compared to the summit) isare related to the fact that a high water filled pore spaceVWC both: (i) strongly limits the transfer of biotic CO_2 along the soil profile, and (ii) reduces the production of

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CO_2 in itself due to the lack of oxygen for the microbial community. In both cases, the lower CO_2 emissions at the footslope, profile relative to the summit, are due to gas diffusion limitations (even indirectly in the case of oxygen lack). This is in sharp contrast to , contrary to the summit profile where gas can easily diffuse during allthroughout the year and along the entire soil profile (Fig. 6C). disables the transfer of biotic CO_2along the soil profile (Fig.1). The surface soil CO_2 flux dramatically increases when the gas diffusivity exceeds a threshold value of c. 0.1 cm^2 d^{-1}(i.e. from day 255 to 305 of year 2013, Fig. 6).

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In the period preceding this the important CO_2 emissions (i.e. from day 255 to 305 of year 2013, Fig. 6A), the soil CO_2 cannot move along the soil profile and accumulates within soil pores. This results in a CO_2 concentration increase both in the early and the late summer, especially below c. 50 cm depth (Fig. 56). This phenomenon is particularly evident below the compacted soil layer between c. 40 cm and c. 50 cm depth. This is consistent withBased on the porosity profile described by Wiaux et al. (2014c) and illustrated in Fig. 21., tThis also corroborates the results of Wiaux et al. (2014 c), suggesting suggests that in downslopefor our footlslope soil profile , which is a Colluvic Regosols, gas diffusion barriers (explained by a compacted soil layers) strongly impact the CO_2 concentration profile, and hence the temporal dynamics of resulting soil surface CO_2 fluxes at the soil surface. This supports is in agreement with recent studies (e.g. Ball, 2013) that showed that soil pore continuity and size are key to understand the mechanisms regulating the soil gases emissions.

As a consequence, the significantly higher CO_2 concentrations observed at the footslope relative to the summit, especially in for deeper soil layers, is are probably not explained related by to the large amount of soil labile OC along the footslope soil profile that was found at this position (shown in Wiaux et al., 2014 a,b), but is more likely the result of thefrom the accumulation of CO2 along time (during periods too with a very low diffusivity periods). Maier et al. (2011) showed that the CO_2 efflux can deviate from the instantaneous soil respiration due to CO_2 storage into soil pore spaces. Hence, we suggest that at the footslope, soil physical variables properties are dominating the dominant control of on surface CO_2 fluxes at the footslope. This supports the conceptual improvement of the SOILCO2

RothC model realized in this study, and the use of soil specific hydrodynamic parameters to characterize the gas diffusion (Moldrup et al., 2000).

Due to the interactions between variables and the complexity of the above-described system, simple correlation analyses will often not provide satisfactory results when studying soil respiration (Maier et al., 2011; Wiaux et al., 2014b), especially across sloping landscapes. This highlights the importance to use process based C dynamic models considering both production and transfer terms (i.e. $SoilCO_2$-RothC) when studying the soil CO_2 efflux.

In summary, we highlight that the mechanisms that which govern soil surface CO_2 emissions are highly variable in both space and timevary throughout the landscape. On a well-drained soil at the summit of a hillslope, the observed soil CO_2 emissions are were directly related to the soil microbial respiration and CO_2 production through the phenomenon of soil micro-organisms respiration (demonstrated in Wiaux et al., 2014b). However, on a wet at the footslope of the hillslope, which is characterized by a different hydrological regime, we observed that the temporal dynamic of soil CO_2 emissions much more reflectswere more closely related to the physical transfer mechanisms: long periods of CO_2 production and accumulation alternate with periods of important release at the soil surface.

4.2. Soil organic carbon storage in downslope deposits

The Ssoil respiration rate can be interpreted as an indicator of soil OC persistence (e.g. Gregorich et al., 1994). However, a further analysis of what occurs along the soil profile is needed to thoroughly answer the question of the persistence of OC. The vertical partitioning of the soil CO_2 fluxes, as illustrated in Fig. 7, shows that during the observation period, c. 90 to c. 95 % of the surface CO_2 fluxes originates originated from the 10 first 10ten centimeters of the soil profile at the footslope. Given the important amount of OC until up to 100 cm depth in our study site (Fig. 1, Wiaux et al., 2014 a), this observation is not in agreement with the study of Goffin et al. (2014), who whichwho suggestsed that the relative contribution of the a soil layers to the surface CO_2 fluxes is related to OC distribution along the soil profile. However, Wwhile similarities exist in the physical controls and the method used to calculate the vertical partitioning, the ~~sudty~~study of Goffin et al. (2014) reports on CO_2 production in forest soils, preventing ~~from~~ any direct quantitative comparison.

In addition, ~~T~~~~this higher~~the substantial contribution of the upper soil layers found here ~~seems to be~~was ~~neither~~ not related to higher temperatures ~~values~~ (Fig. 5A), contrary to what was suggested by Takahashi et al. (2004). According to the CO_2 concentration and diffusivity profiles (Fig. 6C), the relative contribution of the soil layers to the surface CO_2 flux~~es~~ is more likely governed by soil physical controls (Ball, 2013) rather than by biological production depending on thermal energy and OC substrate. Here, soil gas diffusivity strongly decreases from 10 to 40 cm depth (where diffusivity is null) at the two slope positions, and the profile of CO_2 concentration displays no gradient between 10 and 40 cm depth, ~~specifically~~ particularly at the footslope (Fig. 6A).

Here, we show that despite the fact that the footslope profiles generates ~~overall~~ CO_2 fluxes which exceed those observed at the summit position (demonstrated in Wiaux et al., 2014b), the ~~low~~ contribution of soil layers ~~deeper~~ below ~~than~~ 10 cm depth ~~in~~is very ~~low~~small (Fig. 7~~), ~~). ~~In other words, t~~The ~~surfacing soil~~ OC in the top layer of the soil profile (~~top soil layer,~~ i.e. ~~(~~0-10 cm) ~~generates CO_2 fluxes which exceed those observed at the summit position (Table 2), due to the major contribution (~~contribute~~s~~d for~~e.~~ ~~approximatively~~c. 90%~~)~~ of ~~the surfacing soil OC to~~ the total CO_2 flux~~es~~ at the footslope position (Fig. 7). This can be explained by environmental conditions specific to this 0-10 cm layer playing in favor of both microbial respiration and gas diffusion. ~~Indeed,~~ There are no ~~close to the soil surface,~~limitations related to both diffusion barriers and ~~limitation of the~~ access to the oxygen disappear close to the soil surface. Hence, the only ~~residual~~ impact of soil VWC on soil respiration is its positive effect ~~due to the increased~~as it provides a more easy access ~~of~~ for soil micro-organisms to their OC substrate, and to the enhancement of their metabolic activities by water (Akinremi et al., 1999; Castellano et al., 2011; Herbst et al., 2008; Howard and Howard, 1993; Šimůnek and Suarez, 1993). The combination of this high amount and high quality of soil OC (Fig. 1, as described by Wiaux et al., 2014a) with this net positive effect of soil VWC results in a strong increase of microbial respiration rates.

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Finally, Oour results suggest that buried soil OC in colluvial deposits is stored for a long timeeffectively protected from mineralization below 10 cm depth, which corroborates the assumption of a long-term stabilization of deeply buried OC in downslope colluviumsal soils as suggested in the literature (e.g. Doetterl et al., 2012; Berhe et al., 2008, 2012a). Hence, despite that deep soil OC (e.g. in colluvial deposits) has been shown to be highly processed (Rumpel and Kögel-Knabner, 2011), our results suggest that this OC turnover occurs in a closed soil sub-system that is potentially disconnected from the atmospheric C pool. This absence of OC turnover along the footslope profile is also supported by the results of one of our previous publications (Wiaux et al., 2014a), showing thatThis explains why the total and labile soil OC pools remain important and homogeneously distributed along the entire soil profile at the footslope, while it exponentially decreases with depth at the summit (Fig. 1, as described by Wiaux et al., 2014a).

Some studies suggest that net C sequestration occurs at the depositional sites (e.g. Smith et al. 2005), while others negate the apparent C sink caused by soil OC burial at depositional sites (e.g.Yoo et al. 2005). Here, our results reconcile two seemingly contradictory assertions: (i) buried soil OC at a footslope is efficiently stored in the subsoil and submitted to a long term stabilization (Doetterl et al., 2012;Berhe et al., 2008, 2012a), and (ii) the footslope profile emits more CO_2 than the summit (Reicosky et al., 2005; Webster et al., 2008b ; Wiaux et al., 2014b).

4.3. Quantification of soil-atmosphere CO_2 fluxes at the hillslope scale

Despite the fact that peaks of CO_2 fluxes are the highest at the summit position, cumulative CO_2 fluxes are the highest at the footslope position from year 2011 to 2013 (Figure 8). As a consequence, the total annual flux averaged along 3 years of simulation is c. 1.5 times higher ($p<0.01$) at the footslope relative to the summit (Table 2). These observations are consistent with the results of Webster et al. (2008a; 2008b) in forest fields, who observed 1.6 higher median respiration fluxes at footslope and toeslope positions compared to the crest and convex shoulder positions. These observations are also in agreement with the recent findings Wiaux et al. (2014 b) based on punctual surface Licor chamber measurements on the same site, showing that mean respiration fluxes (standardized at 15°C) were c.

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1.3 times higher at the footslope and 1.5 times higher at the toeslope ($p \leq 0.05$) relative to the summit position. The reason of this trend is that factors that control soil OC respiration are heterogeneously distributed across sloping landscapes (Reicosky et al., 2005; Webster et al., 2008b; Martin and Bolstad, 2009; Wiaux et al., 2014 b).

Our estimations of annual CO_2 fluxes may be associated with large uncertainties due to error propagation on measured variables. Despite the modest performance of the SoilCO_2-RothC model in reproducing the CO_2 fluxes observation (R^2 ranging between 42 and 64%, Table 1), the model uncertainties (given by ME values, Table 2) are lower than the difference of annual CO_2 fluxes between slope positions (Table 2). In addition, this model error (sum of the difference between observations and simulations for each day of the year) observed in this study (from 4 to 91 g CO_2-C m^{-2} yr^{-1}, Table 2) are lower than the uncertainty on the CO_2 fluxes directly measured at the catchment scale using the Eddy-covariance technique (from 100 to 200 g CO_2-C m^{-2} yr^{-1} at non-ideal sites like sloping plots, Baldocchi, 2003). Hence, this supports the validity of the quantitative comparison of CO_2 fluxes between slope positions carried out in this study.

Focusing on CO_2 fluxes aggregated at the scale of the entire hillslope, such a loamy hillslope emits 683 ± 36 g CO_2-C m^{-2} $year^{-1}$ while a flat landscape would only emits 583 ± 61 g CO_2-C m^{-2} $year^{-1}$ (Table 2). Hence, for our study site, accounting only for soil C dynamics representative of flat landscapes would under-estimate annual soil-atmosphere CO_2 exchanges by c. 20 %. This supports similar conclusions drawn under a forest eco-system by Webster et al. (2008 a), who highlighted a risk of under- or over-estimation of soil respiration at large scale reaching up to 30% when topography is not accounted for. Our results provide a thorough quantification and a better understanding of the soil-atmosphere C exchanges specific to an agro-ecosystem on the loess belt in Belgium, which may be of high importance to adopt strategies to mitigate climate change.

The CO_2 emissions values reported in literature studies as soil heterotrophic respiration (considering that heterotrophic respiration fluxes constitute c. 30% of the total ecosystem respiration and c. 78% of total soil respiration, according to Suleau et al., 2011)are ranging from c. 170 to c. 456 g CO_2-C m^{-2}

year^{-1} in similar conditions i.e. temperate loamy croplands (adapted from Boeckx et al., 2011; Kutsch et al., 2010; Paustian et al., 1990), from c. 140 to c. 144 g CO_2-C m^{-2} year^{-1} in forests ecosystems(adapted from Dai et al., 2012; Webster et al., 2008a), and reach c. 1811 g CO_2-C m^{-2} year^{-1} in temperate grasslands on organic soils (adapted from Renou-Wilson et al., 2014).However, most of these studies were carried out on flat landscapes. To our knowledge, no equivalent quantification of the hillslope aggregated CO_2 fluxes already exists for agro-ecosystems. The values of CO_2 emissions presented in this study are in the same order of magnitude but are slightly higher than literature studies on flat croplands (Boeckx et al., 2011; Kutsch et al., 2010; Paustian et al., 1990). This may be explained by the hilly relief of this study site and the lateral transfer of soil particles enriching the downslope area in soil OC (Wiaux et al., 2014a), inducing higher respiration rate relative to a flat uneroded position (Wiaux et al., 2014b).

The higher heterotrophic respiration at our study site compared to other temperate loamy croplands (Boeckx et al., 2011; Kutsch et al., 2010; Paustian et al., 1990) could also be explained by some experimental biases: (i) a priming effect due to the land use change (soil kept bare and undisturbed during 3 years); (ii) any heading due to the dark geotextile installed at the surface of the measurements stations; and (iii) the modest model performances in terms of predictivity (Table 1). Hence, the absolute estimation of the hillslope aggregated CO_2 fluxes in this study should be interpreted carefully, and the focus should be on the relative difference between emissions from flat and sloping landscapes (i.e. 20%, Table 2).

In order to understand the impact of these findings in terms of C balance, it is important to compare these heterotrophic respiration fluxes to other soil C inputs and outputs. Among other things, soil heterotrophic respiration fluxes discussed here only constitute c. 30% of the total ecosystem respiration, also composed of aboveground and belowground autotrophic respiration fluxes (Suleau et al., 2011) which were not considered here. However, this exceeds the scope of this study and should be explored at the scale of hillslopes in future researches.

~~Notwithstanding these elements, our results support that, when modeling soil C dynamics and when quantifying soil-atmosphere CO_2 exchanges, this is of paramount importance to consider slopes and elevation effects rather than a flat landscape, and to account for dynamic processes (e.g. lateral transfer of soil OC and heterogeneous distribution of soil VWC) occurring along hillslopes.~~

5. Conclusion

In this study, we evaluated the factors controlling soil carbon dioxide fluxes for two soil profiles along a hillslope characterized by contrasting physical and chemical characteristics. At the summit position of the studied hillslope, the time course of surface soil CO_2 fluxes ~~clearly follows the time course of~~was strongly related to soil temperature ~~(Fig.5-6,~~and ~~-~~maximum CO2 fluxes were observed during the summer~~)~~. ~~At this position of the hillslope~~Here, the observed soil CO_2 emissions are directly related to ~~the CO_2 production through~~ soil micro-organisms respiration and associated biotic CO_2 production. In contrast, the ~~At the footslope (contrary to the summit),~~ higher levels of water filled pore space observed at the footslope profiles, ~~disables~~ strongly limit~~s~~ed the transfer of biotic CO_2 ~~along~~ throughout the soil profile.~~,~~ ~~and~~ Here, the soil surface ~~soil~~ CO_2 flux substantially ~~increases~~ increased for limited amounts of time when the gas diffusivity ~~exceeds~~ exceeded a ~~given~~ threshold value~~e~~. ~~Hence, on a wet footslope~~As a result, the time course of observed soil CO_2 emissions ~~is~~ wereas to a large extent ~~more determined~~~~controlled~~explained by ~~the~~ physical transfer mechanisms: long periods of accumulation alternate with shorter periods of important ~~surface~~ CO_2 release. ~~Considering these elements, the entire hillslope emits c. 20 % more g CO_2-C m^{-2} $year^{-1}$ compared to a similar flat plot.~~ ~~This results support the need to consider slopes when modeling soil-atmosphere C exchanges.~~

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The vertical partitioning of the soil CO_2 fluxes for the footslope profiles ~~shows~~ showed that, during the observation period, ~~c.~~ 90 to ~~c.~~ 95 % of the surface CO_2 fluxes originate~~s~~d from the ~~10~~ first 10 centimeters of the soil profile~~ at the footslope~~. ~~However, the total annual flux averaged along 3 years of simulation show that the top soil layer (0-10 cm) of the footslopegenerates CO_2 fluxes which exceed those observed at the summit position. Hence, our results reconcile two seemingly contradictory hypotheses, i.e. (i) these support that soil OC at such a footslope is stored along the main part of the soil profile and submitted to a long-term stabilization.~~This study highlights the need to include soil physical properties and their dynamics directly into soil OC models. This is not in contradiction with previous findings showing that, and (ii) at the same time these support that the footslope profile emits more CO_2 than the summit, as we showed here that CO_2 fluxes at the footslope are generated by the top soil layer (0-10cm).

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Author contribution

F.W. designed the experiments,carried them out, and performed the model simulations and carried out the research. M.V., K.V.O. and F.W. discussed analyzed the results and selected the messages to highlight. F.W. wrote the main part of the paper and prepared the manuscript with contributions from all co-authors.

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Tables

Table 1. Range of standard deviation and averaged values of triplicated measured hourly CO_2 concentrations at each depth, both at the summit and at the footslope position. This range is indicated by minimum (Min) and maximum (Max) values encountered along time (hourly time series) during the 6 months measurement period.

	Summit position				Footslope position			
Soil depth [cm]	Min mean [%]	Max mean [%]	Min S.D. [%]	Max S.D. [%]	Min mean [%]	Max mean [%]	Min S.D. [%]	Max S.D. [%]
10	0.07	1.39	0.00	0.71	0.26	4.75	0.00	3.13
25	0.06	1.83	0.00	0.68	0.30	3.93	0.00	5.32
45	NI	NI	NI	NI	0.12	3.96	0.00	1.96
95	0.15	2.83	0.00	1.42	0.48	7.52	0.00	2.48

~~**Table 1.Regression coefficient (R^2, %) as an indicator of the goodness of fit (GOF) between observations and simulations.**~~

	VWC†		Temperature†		CO_2 flux‡	
	2012	2013	2012	2013	2012	2013
Summit						
	64	73	100	100	63	64
Footslope						
	82	89	99	100	61	42

~~† For VWC and temperature, the R^2 value is given as an indication representative of the 10 cm depth soil layer (but similar values are encountered all along the soil profile).~~

~~‡ For CO_2 fluxes, the observations compared to model simulations are punctual surface measurements with the Licor chamber system in 2011 (n=8 days) and 2012 (n=15 days), while it is continuous daily time series of gradient-based fluxes in 2013 (n= 129 daysat the summit, and n=137 days at the footslope). Data are not shown for 2011 (too low number of observations for CO_2 fluxes, no observation for the other variables).~~

Mis en forme : Police :9 pt, Indice

Mis en forme : Légende

Tableau mis en forme

Mis en forme : Justifié, Espace Après : 10 pt, Interligne : Multiple 1,15 li

Mis en forme : Justifié, Espace Après : 10 pt, Interligne : Multiple 1,15 li

Mis en forme : Justifié, Espace Après : 10 pt, Interligne : Multiple 1,15 li

Mis en forme : Justifié, Espace Après : 10 pt, Interligne : Multiple 1,15 li

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Mis en forme : Justifié, Espace Après : 10 pt, Interligne : Multiple 1,15 li

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Table 2. Yearly simulated CO_2 flux [g C m^{-2} $year^{-1}$] at different slope positions.

	Summit	**Footslope**	**Diff.**	**Hillslope**
2011 (± ME)	545 (−22)	944 (+26)	399 (+ 4)	685
2012 (± ME)	654 (+ 136)	842 (+63)	188 (− 73)	719
2013 (± ME)	553 (+ 47)	826 (+ 138)	274 (+ 91)	647
Average ± SD	583 ± 61 a***	870 ± 64 b***	287 ± 106	**684 ± 36**c*

Diff. is the difference between the footslope and the sumit yearly CO_2 flux.

ME is the model error (sum of the difference between observations and simulations for each day of the year). A positive value means that model underestimates the flux, and inversely.

SD is standard deviation.

Mean values with different letters are significantly different from each other (Student test, *:0.05<p<0.1; **:p<0.05; ***:p<0.01).

To calculate mean yearly CO_2 fluxes representative of a hilly landscape (hillslope), we achieved a weighted sum of the summit and the footslope fluxes, according to the fact that the footslope colluvium covers c. 35% of the surface of the studied hillslope.

Figures

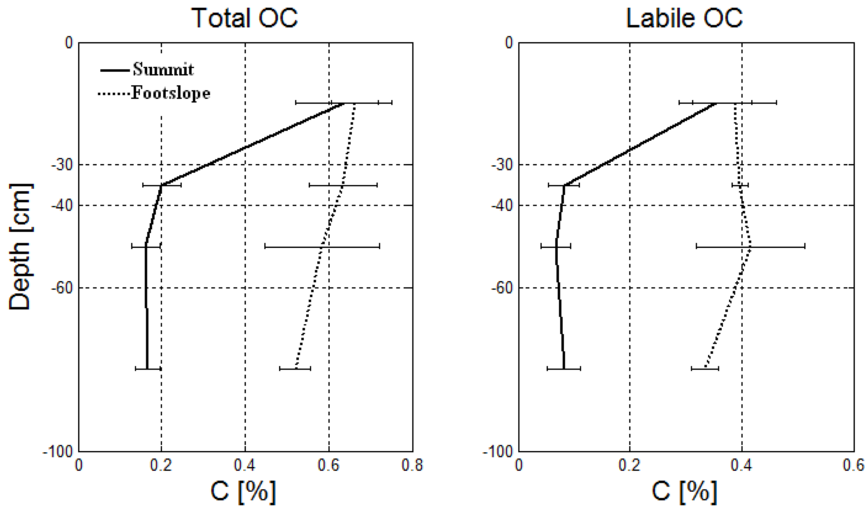


Fig. 1.Soil profiles (0-100 cm) of both soil total OC and labile OC pool concentrations [C%], at the summit and footslope positions. Error bars indicate 1 standard deviation (n≥3).

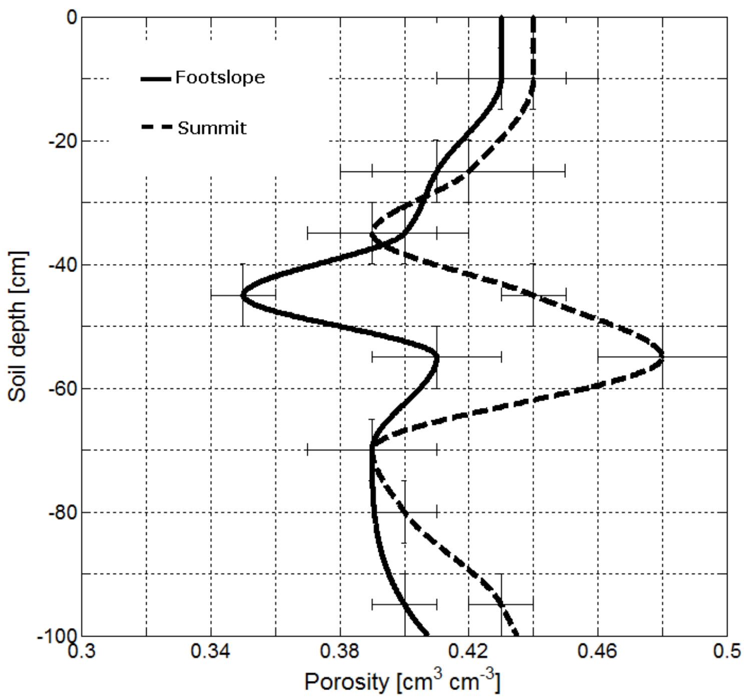


Fig. 2. Soil porosity profiles at the footslope (plain line) and at the summit (dashed line) positions. Error bars indicate 1 standard deviation (n≥3). Continuous lines are linearly interpolated values.

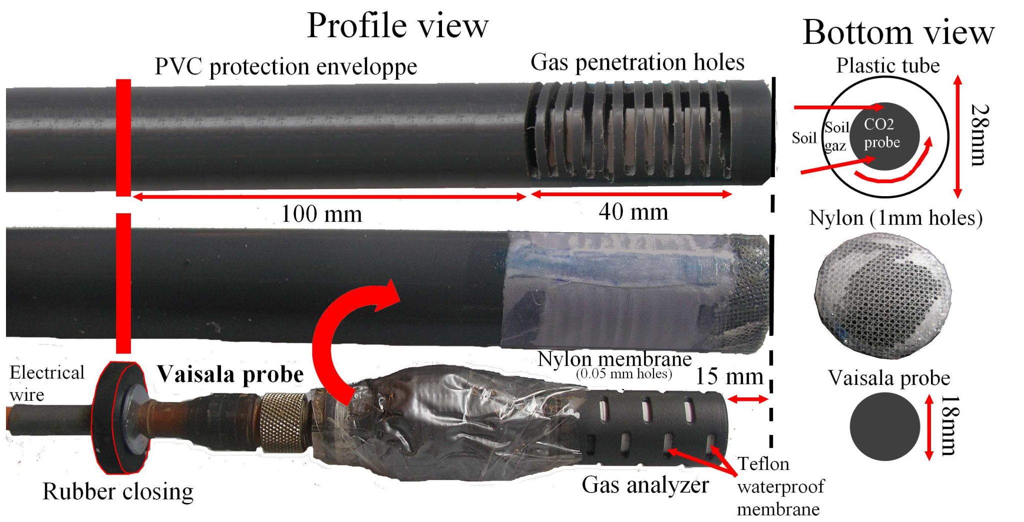


Fig. 3. Description of the probes used for CO2 concentration measurements inside the soil.

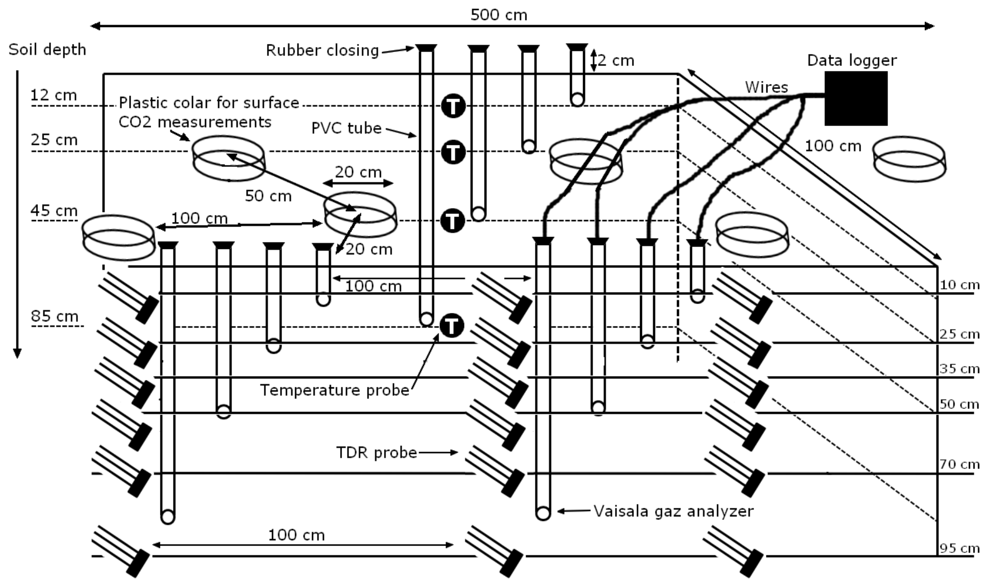
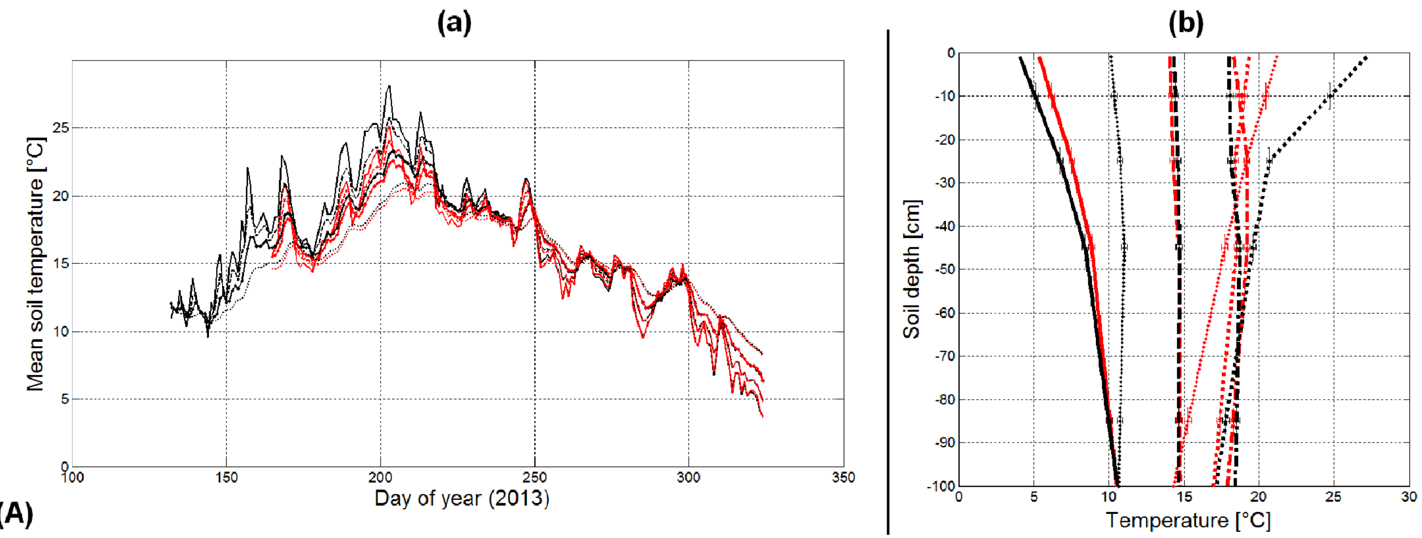


Fig. 4. Schematic description of the experimental plot (sampling design) at each slope position showing how temperature, VWC, CO_2 concentrations and CO_2 fluxes probes collocate with each others. Probes have been inserted at different locations both vertically and horizontally. Consequently, all of them are not in the same plane (i.e. depth lines with axes labels on the right hand-side illustrate the foreground profile and depth lines with axes labels on the left hand-side illustrate the background profile).


(a)
Mean soil temperature [°C]
Day of year (2013)
(b)
Soil depth [cm]
Temperature [°C]

(A)

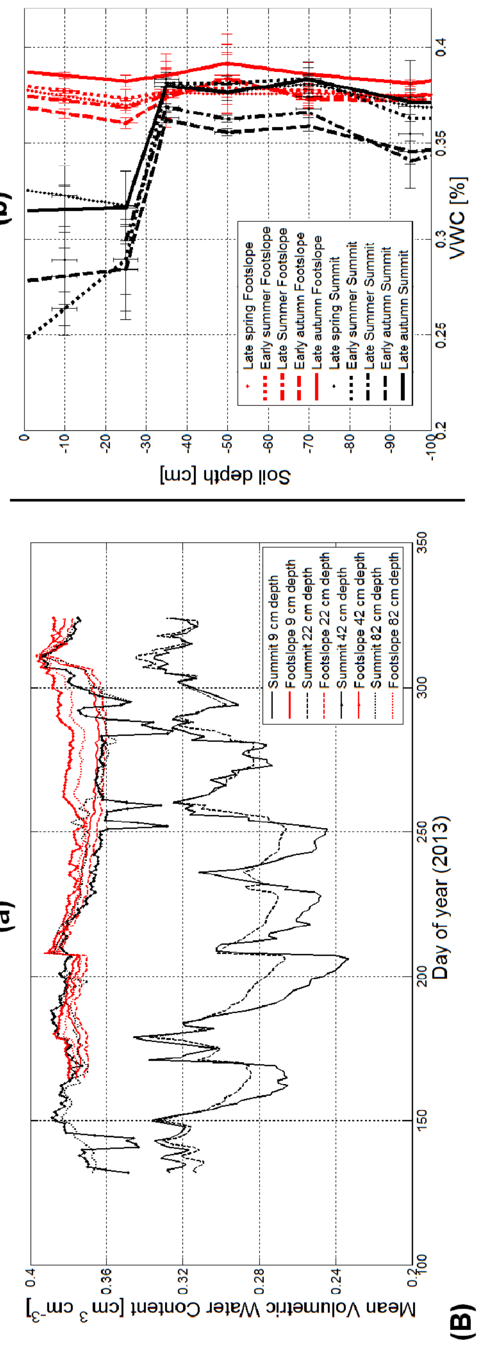
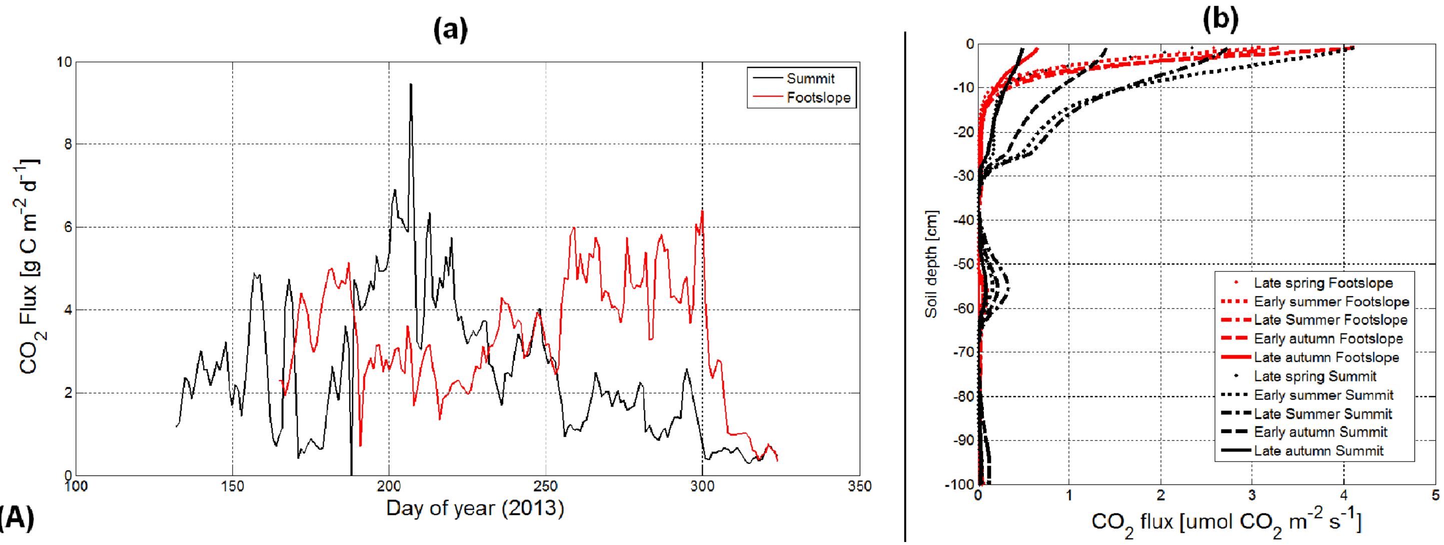
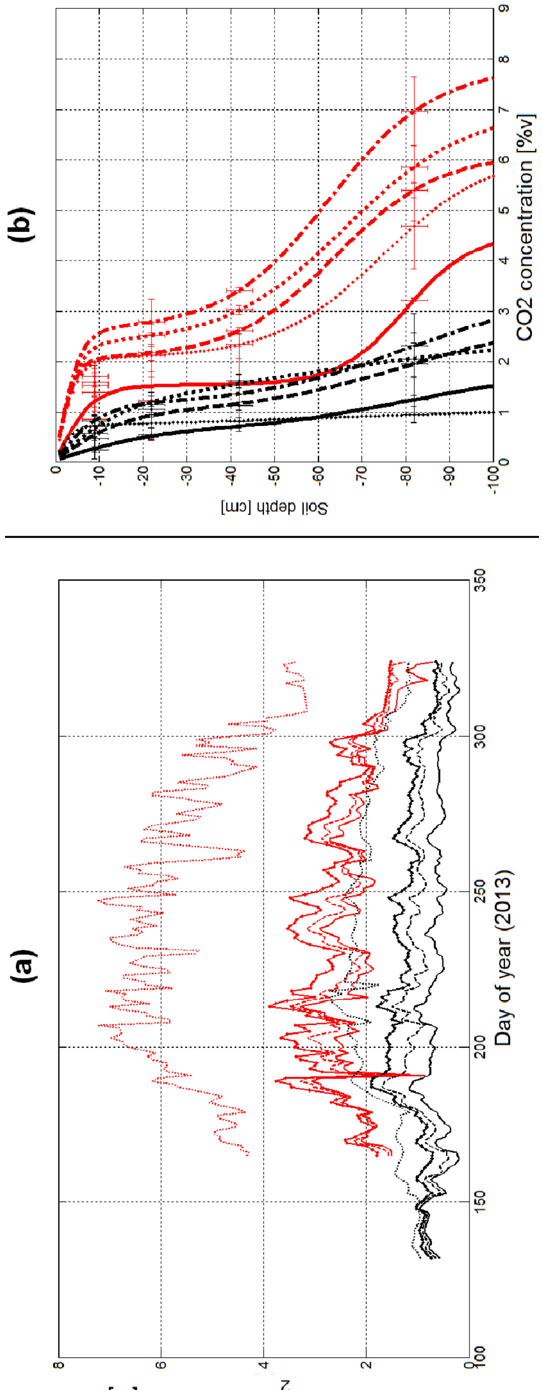


Fig. 5. Space-time dynamic of soil temperature (A) and moisture (B) at the summit (red) and the footslope (black) position in 2013: (a) time series at different depths; (b) Profile at different dates.


(a)
Summit
Footslope
CO2 Flux [g C m-2 d-1]
Day of year (2013)
(b)
Late spring Footslope
Early summer Footslope
Late Summer Footslope
Early autumn Footslope
Late autumn Footslope
Late spring Summit
Early summer Summit
Late Summer Summit
Early autumn Summit
Late autumn Summit
Soil depth [cm]
CO2 flux [umol CO2 m-2 s-1]

(A)


(a)
Mean soil CO_2 concentration [%]
Day of year (2013)
(b)
Soil depth [cm]
CO2 concentration [%v]

(B)

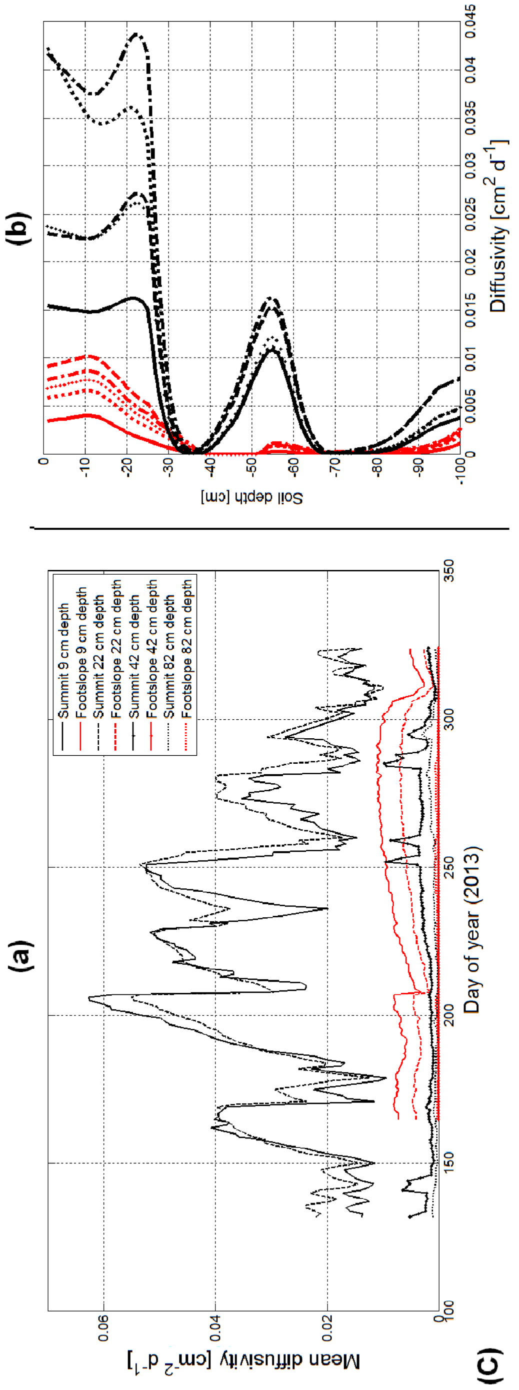


Fig. 6. Space-time dynamic of soil CO_2 fluxes (A) concentrations (B) and diffusivity (C), at the summit (red) and the footslope (black) position in 2013: (a) time series at different depths; (b) Profile at different dates.

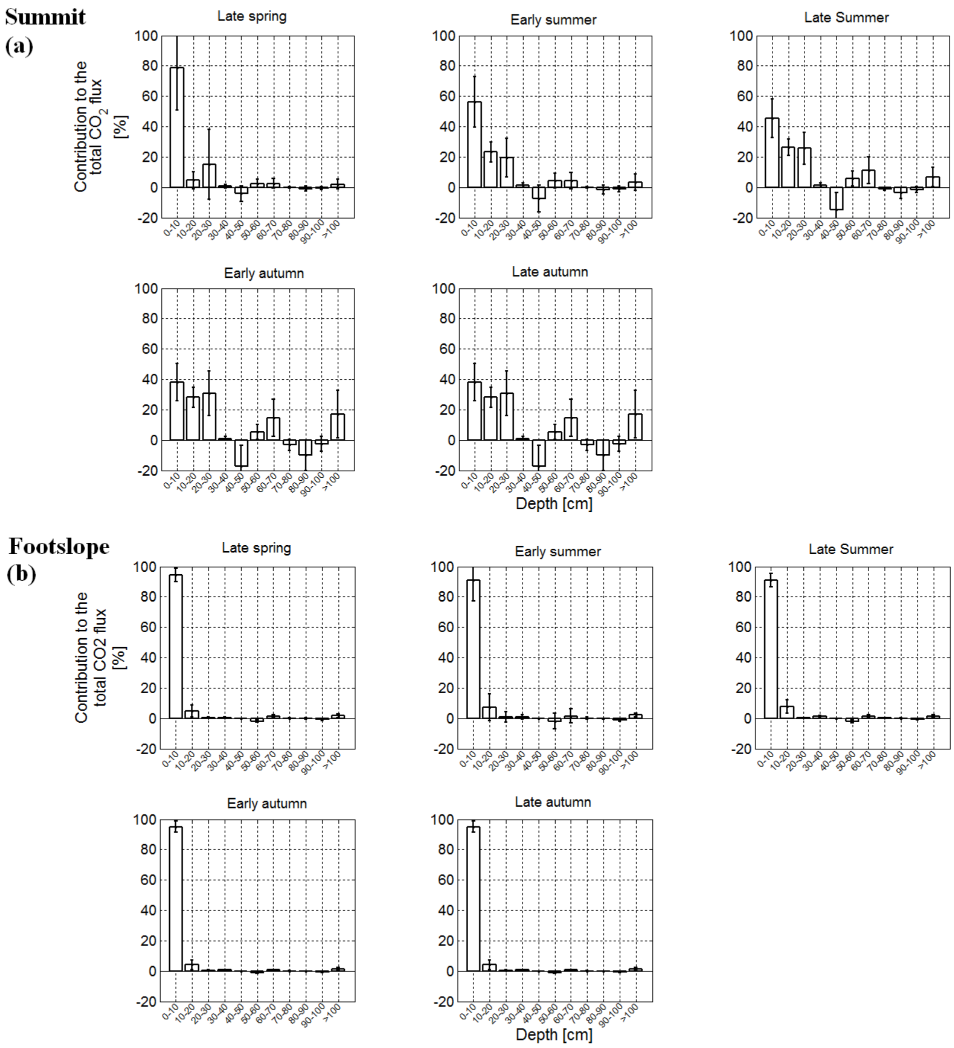


Fig. 7. Depth distribution of the relative contribution to soilsurface CO_2 fluxes in year 2013 averaged by semi-seasons (error bars represent the standard deviation of the time aggregation for each soil layer): (a) at the summit, and (b) at the footslope position.

~~**Fig. 8. CO_2 fluxes from 2011 to 2013 at two slope positions (footslope in red, summit in black): (i) simulation based on the SOILCO2-RothC model (plain lines), (ii) calculated fluxes with the gradient-based method (dashed lines), and (iii) spatial average of in situ measured fluxes with the IRGA Licor chamber (points with errorbars).**~~