Review on 'Erosion, deposition and replacement of soil organic carbon' by Nadeu et al.
Below you will find some specific remarks: others can be found annotated on the pdf of the MS. A general appraisal is given at the end of this document.

Authors: Thank you for your comments and suggestions to improve the quality of the manuscript. We answer to the questions below. However, we are unsure on what you refer to as "remarks annotated on the pdf of the MS", since we did not receive a version of the MS with comments.

Replace Fig. 1 with one containing a more detailed topographic map of the subcatchments that were selected

Authors: Figure 1 now shows contour lines within the subcatchments and the location of the soil pits.

P 8356: the catchments you selected are still quite large (over 10 ha); yet the number of eroding profiles that was sampled is only four. Their location is not adequately described. I think you need to show a map with their exact locations (see remark above) and you need to justify as to why you took them at these locations. The same is, to a lesser extent, true for the depositional sites. We also need land use information on these sites.

Authors: The location of the sampling sites has been added to Figure 1. Soil profile locations were selected on slopes (20-30%) that had undergone no land use change during the 1950-2010 period and that kept a maximum distance of 50 m to the depositional site. The last premise was kept to ensure that the selected profiles could have been a potential sediment source to the depositional site during the studied period, though, evidently, not the only source. It was previously shown that, in this region, a large percentage of the sediment stored behind check-dams originates from nearby slopes (Romero-Díaz et al. 2002). This argumentation for selection of sampling locations has been included in the text. In relation to land use, it is now explained in the text that three of the four soil profiles in each subcatchment were located under forest cover and the remaining one under shrub (*Rosmarinus officinalis*). The depositional sites are currently covered with a low density of grasses and some shrubs (*Crataegus monogyna*, *Juniperus oxycedrus, Rosmarinus officinalis, Rosa sp.).*

p. 8357 As far as I can see the delta 14C values are not reported: add them to table 2. Also provide the ages calculated from the 14C data. Make sure the reader has all the data necessary to replicate the calculations you describe in paragraph 2.6. The reader now has to believe you and has no insight into the quality of the data behind the estimation of I and k.

Authors: The ^{14}C data has been added to Table 2.

p. 8358: we do need more information on this methodology to calculated carbon decomposition rates: am I right in supposing that the 14C age is taken as the average carbon age at a given point. Is this correct? Please use a couple of extra sentences to explain this more clearly.

Authors: Yes. The reported ^{14}C age is indeed the average for a given depth. We have included more information in the text in section 2.6 to explain this.

Then, I have two additional questions:

- What about contamination with bomb 14C ? Is this not a problem, especially when taking samples in arable land where this bomb 14C can be mixed into the topsoil?

Authors: We understand your concern and capture from it that the text was lacking clarity in some points. First of all, none of our samples come from (current or past) arable land, nor is there evident mixing in topsoil by burrowers at the sampling sites. Second, our soil profiles show modern values of 14C which correspond to "bomb 14C" on the topsoil allowing us to observe and detect presence of modern carbon (recent fixation).

- How do you deal with the fact that these are eroding/depositing profiles so that there are lateral losses/gains of SOC ? Is this important in your calculations? I would think so, because erosion and deposition change the input term in your calculations.

Authors: In our calculations we do account for the fact that the profiles have lateral eroding/deposition dynamics that modify the C input term through time. The input term is obtained by solving equation 2 using a least squares approach to find the best fit to measured data of the C inventory vs. mean age at each depth. Thus, the value we report as the annual C input (from equation 2 in the MS) represents a mean C input throughout the whole period of formation of the soil profile (as it is a profile-integrated approach). The other parameter derived from this procedure is the "k", value, that represents C decomposition and C losses by erosion at the same time.

p. 8365: I do find the discussion on C replacement to be too speculative and not very well focused. If you state that 'enough plant derived-OC input was produced' you should refer to the hard, quantitative data you have to (i) calculate erosion rates of sediment and carbon and (ii) calculate OC input. Furthermore, I do have a fundamental problem with the concept of over-replacement. If you take it literally, it would mean that once you start eroding a slope you will automatically gain in carbon storage as the carbon will be more than replaced. That will definitely not be the case as the equilibrium SOC inventory will not be larger under eroding conditions as compared to stable conditions. So, you need to frame this discussion differently: the question on replacement can be solved by comparing how the profile actually looks now with how it would look if there would not be any erosion. This can be simulated by using your decay values and imposing different erosion conditions on the profile and comparing the results (see also Van Oost et al., 2007).

Authors: The suggestion is very interesting and it is true that over-replacement cannot be sustained over a long period of time, since accumulation can't continue indefinitely. Equilibrium conditions have to be reached at some point. As we have tried to clarify in the revised manuscript, the fact that OC replacement takes place doesn't necessarily mean that OC stocks are higher compared to a non-eroding situation. We think the replacement rate values we obtain can be misunderstood and, thus, we have tried to place them better into context. The rate of OC input we report is a profile-integrated average, and the erosion rate a subcatchment mean for the last 30 years. Therefore, the combination of both must be regarded as an indication of the current situation on dynamic replacement. For instance, if current erosion rates greatly exceeded the mean OC input to soil, we would conclude that our profile was reducing its stock, while seeing the contrary means that we are increasing our stock. The exact value it takes is, at this point, less important to us (and probably more arguable) than the observed trend to answer the question of whether we can assume dynamic replacement is taking place (if the balance between OC input and output by erosion rate is negative or positive). As said before, each of the considered profiles is only expected to continue accumulating OC until its maximum storage potential for OC under equilibrium conditions is reached. In addition, and to further place this into context, the discussion surrounding replacement rates of eroded OC only address one part of the equation of soil carbon balance. Full accounting of carbon storage should still consider losses of C through decomposition and/or leaching in addition to input from plant productivity and input and/or output of C with erosion and deposition. Nonetheless, though, it has been established before that erosion does create the possibility of a dynamic replacement (Harden et al., 1999; Quine and Van Oost, 2007). The discussion of these results was modified to better define the limitations of their interpretation.

We would also like to state that the approach suggested by the reviewer can potentially give interesting additional information. However, it has its own set of difficulties hampering implementation. Comparison of eroded vs. uneroded profiles cannot be simply done by modifying the erosion parameter. The current profiles have evolved as a function of eroding conditions. For a more extended modeling exercise one would need additional data which we do not currently have including field measured C input to soil, and additional profiles.

Overall I would say that this study contains valuable data (especially the 14C data are quite unique, I believe) but there needs to be more work done before this paper can be finally accepted for publication:

Authors: We have revised the manuscript in accordance with the reviewer's comments. In addition, we have presented additional data (that was not included in the previous version) as has been suggested by the reviewer.

- *We need more information on the location of the profiles and the reasons why these locations were selected as they will affect*

Authors: see comments above related to figure 1.

- T*he paper lacks analytical rigour on some important points.*

o We do need detailed information on how the values of the crucial SOC model parameters were derived for each profile (k and I). See also the remarks above and in the text.

Authors: Additional data has been added in Table 2, and we revised the manuscript for clarifications that show our approach in the calculation of the model parameters and make the data more transparent.

o *The paper is based on a (very) limited number of data on soil profiles on eroding sites: for a first study this may be OK, but the implications of this should be discussed. How representative are your profiles ? What kind of variability would you expect within your catchments and what may be the implications of this variability for SOC dynamics?*

Authors: We agree that limited number of data is a constraint in studies using radiocarbon measurements due to the high cost associated with the analyses. Nonetheless, the sampled profiles can be considered representative of a large part of the subcatchments; they are located under forest (pine) cover, which is currently the dominant land use (accounting for >80% of the area) and on slopes of 20-30% steepness (mean subcatchment steepness for both C51 and C24 is ~30%). When comparing the OC stock and particle-size distribution for the 0-10 cm depth with data that we have from former field campaigns that cover a broader area in each subcatchment (used in Boix-Fayos et al., 2009 and Nadeu et al. under preparation) we see that the means are not significantly different for most of the measured parameters.

Regarding OC content, for instance, mean OC concentration in the top 10 cm for soils (n=4) in C51 and C24 is 2.3±1.4% and 3.5±0.7% respectively, while for the former samples (n=6) it is 1.7±0.3% and 3.2±1.4% (for C51 and C24 again). Further, the location of the soil pits makes them potential important sediment sources to the main depositional site.

The variability of SOC within each catchment, which we cannot quantify with our current data, will be basically related to land use, land use history and topographical position. We are of the opinion that it will have little effect on the first part of our study (the use of ^{14}C as a tracer) since it seems reasonable that with a similar type of soils (mostly Calcaric Regosols and Cambisols) and a similar vegetation cover, all soils will show modern C in the topsoil. As to SOC dynamics, the variability would indeed affect a total SOC budget and overall C input and output rates. Consequently, throughout the manuscript we refer to the soil OC dynamics in the analyzed profiles.

o *The discussion on carbon replacement needs to be further fleshed out: I cannot see (but I may be wrong) another way of doing it as by running model simulations whereby you simulate inventories with and without erosion effects (over a relevant timescale) so that you can see how much replacement may indeed have taken place.*

Stating that there is over-replacement because the input is (as expected) much larger than the amount of SOC that is eroded does not solve the issue (in my opinion). You should run a series of scenario's, whereby you may investigate what would happen to the SOC inventory in the soils in the catchment if you would assume that they are eroded at the average catchment erosion rate (which you know) and/or whereby you vary erosion rates through time (as LU has changed).

Authors: Here we use the ^{14}C approach as an alternative to modeling scenarios for this study. Even though we consider modeling scenarios as a very interesting, follow up exercise for this study, we believe more variables should be considered before taking that step. For instance, to simulate C inventories under different conditions we would need to first obtain an empirical C decomposition rate for our profiles, since the k value that we currently obtain from the Berhe et al. (2008) approach requires estimation or determination of soil and C erosion rates to separate it into its two components (erosion and decomposition). However, we have no information as to how erosion rates varied through time. Further, the reviewer suggests to use the modeling approach to obtain a general replacement rate for the entire subcatchment. This would be interesting as well and necessary to construct a total carbon budget, but is out of the scope of our present study. We are working on a much more detailed modeling study of OC erosion and deposition in which we will take this into consideration and discuss this aspect in full detail though (Nadeu et al. in prep)

o *On various locations throughout the MS you refer to dominant erosion processes and changes in those processes: however, you do only report a map of these processes: are there any quantitative data available? If so, could they be discussed? In any case you do have an idea of average catchment erosion rates and they should be reported.*

Authors: We have reported average catchment erosion rates from two different sources; a mean for the last 30 years obtained through quantification of accumulated sediment behind the check-dam and a specific soil erosion values for the profile locations obtained from the WATEM/SEDEM model application (both used in Boix-Fayos et al. 2008).

In relation to the first part of the comment, unfortunately we do not have historical quantitative data on the individual erosion processes. The approach we use for our analyses comes from the combination of two sources of information: (i) Fluvial morphology interpretation based on the geomorphological maps of the channel and the connection slopes-channel following Hooke (2003) and (ii) the land use change analysis performed from a time series of air photos. Previous geomorphological studies done in the same area and in the larger catchment of Taibilla (from which Rogativa is a subcatchment) have established the connection between dominant present and past erosion processes and land use changes using this same approach (Boix-Fayos et al., 2007; Quiñonero-Rubio et al., submitted). At other study sites, similar geomorphological approaches have been widely used to interpret fluvial morphological changes in relation to changes in dominant erosion processes and land use change in mountain areas (see for instance Kondolf et al., 2002, Beguería et al., 2006, Fryirs et al., 2009 and Keesstra et al., 2009) . Our local interpretation of changes in dominant erosion processes in C51 and C24 using the methods above cited fits perfectly with the catchment erosion dynamics in relation to land use change described for Taibilla (Quiñonero et al., submitted) and Rogativa (Boix-Fayos et al., 2007 and 2008).

o *The discussion on SOC mineralization in the deposits contains a number of valuable elements but there needs to be additional discussion on the fact that a lot of SOC is lost with the sediment that is leaving the catchments, despite the presence of check dams.*

Authors: We have added explanation of this fact at the beginning of section 4.3 in the discussion. It seems clear that part of the eroded OC will be leaving the subcatchments as the check-dams are not 100% effective on sediment retention and OC can leave as well through suspended sediment that flows through the check-dams. However, we suspect that given the clay and silt enrichment ratios, slightly larger than unity, in the sediment profiles, OC is not selectively leaving, at least not in large proportions.

o *The discussion on the differences in delta 14C profiles in the sediments of C51 and C24 is very interesting and shows how 14C data may potentially become a powerful tool in diagnosing where sediment is coming from. However, your explanations do to fully match the observations. The temporal evolution of LU in C51 may indeed be reflected in changes in the delta 14C values that you observe. You also state that C24 has had a relatively stable LU, reflected in a stable delta 14C profile in the sediment. However, if you take these two observations together you would expect that, at present the sediments of both C51 and C24 should have a similar signature and that is clearly *not* the case. On the contrary, both catchments appear to diverge at the moment. So you are clearly missing a crucial point in explaining SOC dynamics. I find this a fascinating observation, but you do not provide an explanation nor a hypothesis as to why this is the case. If we want to explain this we need to look at differences between the catchments. I think that an important reason why older carbon is exhumed from C51 is the difference in stream behavior: the stream in C51 is incising (as a response to LU change) thereby mobilizing old SOC stored on the alluvial valley floor and in the banks (as a result of centuries of deposition of sediments eroded on the hillslopes). The stream in C24 is aggrading, thereby acting as a filter letting the SOC coming from the slopes through (to a large extent) and retaining part of the sediment. This may also explain the high ER for sand in the Bprofile of C51 ;-).*

Authors: We fully agree with the reasoning and believe that it is exactly what we had tried to express in the first section of the discussion. Apparently, the text was not clear enough so we have revised the section to better communicate this important message. We have also divided the discussion section on OC sources in two subsections: old C (C51) and young C (C24). By doing so, we expect to help the reader understand the different behavior in the stream and slope-stream connections in both subcatchments.

In short, I believe this can be made into a paper that may be published in Biogeosciences, but you need to be more rigorous in your analysis. Also, you may consider to redirect its focus somewhat: I think the most interesting observation coming out of your data is the diverging 14C profiles in both catchment, despite their similar LU at the moment and I would propose to center the paper around the use of delta14C as a tool to understand (the evolution of) sediment sources and the implications of your (first) findings. As I tried to indicate, I think this can be only understood by looking at the river dynamics. Analysis of one or more profiles taken in the river banks for 14C might help tremendously to make the paper more rigorous and therefore increase its impact.

Authors: We appreciate the reviewer's recommendations and thank you for your constructive comments. Unfortunately we are not currently able to perform more ^{14}C analysis, but we did adjust our discussion to reflect the limitations of our data.

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