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Interactive comment on "Long-term dynamics of buried organic carbon in colluvial soils" *by* Z. Wang et al.

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Summary/General Comments The manuscript submitted by Wang et al. describes the role of soil organic carbon (SOC) burial in colluvial soils and its potential inïňĆuence on landscape-scale C budgets. The authors collected soil cores from three colluvial sites and used a combination of techniques to establish chronologies for the soil cores and also estimate the amount of buried SOC that may be susceptible to respiration over time. By comparing against properties of SOC from cores collected from reference sites (no-erosion) the authors concluded that colluvial SOC burial efficiency eventually reached about 17% by about 1000-1500 years post-burial. In general, the methods are appropriate and I believe that the conclusions are supported by the data (see, however, my comments below). Following the publication by [Van Oost et al., 2012],

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this work represents some of the only insight into longer-term dynamics of buried SOC (by anthropogenic scandards). This has important implications for understanding landscape response to management practices over time periods relevant to development and expansion of agriculture. For this reason, I believe this work is suitable for eventual publication and will be of wide interest to the broader Biogeosciences audience.

We welcome this assessment.

Specific Comments There are some assumptions in the methodology that I feel need additional discussion and detail; specifically:

Establishing sedimentation chronology by P profiles. The authors rely on P enrichment as a marker for the past 100 years owing to industrial development of P fertilizer. This approach seems to rely on the assumption that 1) no other form of P was ever used to support agriculture, and 2) P application always occurred in excess of agronomic demands – resulting in P buildup. While the assumption of excess P application is reasonable, it is very likely that pre-industrial P fertilizer (manure) has also been used and this makes the identification of the 1910 horizon much more speculative. This introduces additional uncertainty into sedimentation rates estimated via this method.

We fully agree and now include a detailed description of this additional source of uncertainty. This has been added in the text (Line 196-198).

The authors state that 137Cs is employed as a marker for 1954. However, atmospheric testing of thermonuclear bombs continued (and increased) until the nuclear test ban treaty went into effect in late 1963. Atmospheric fallout from bomb testing is more generally regarded as a marker for 1963-1964. Sediment accumulation rates should be recalculated to reflect this. (Refer to [Ritchie and McHenry, 1990]).

1954 and 1964 are the age boundaries of the soil or sediments that get adsorbed by 137Cs fallout. With respect to this study, we think that early boundary (i.e., 1954) would be more appropriate: if the time lag for the sediment to be transported to the deposition

locus is not considered, the depth where 137Cs can be detected should be dated back to the initialing moment of the 137Cs moment, i.e., 1954. In the year of 1954, the 137Cs horizon deposited in 1954 was mixed within the plough layer thereby reducing the 1954 horizon to the bottom of the plough layer. Therefore, the depth where 137Cs can be detected can be regarded as the depth of 1954 137 Cs horizon plus the plough layer. Contrary to the 1954 137Cs layer, the 1964 137 Cs would be more difficult to identify due to the soil mixing caused by tillage operation.

The application of d13C values as a proxy for SOC quality is questionable. Firstly, at least some decomposition studies have shown the opposite to be true; i.e., d13C values decreased with decomposition (not increased as stated by the authors in this study). A classic example is shown by [Benner et al., 1987]. Second, the vegetation and cropping history of the study area includes a mixture of C3 (wheat, sugar beets) and C4 plants (maize) which have dramatically different bulk d13C values. Third different plant tissues can exhibit slightly different d13C values (again, see [Benner et al., 1987] as an example) and colluvial SOC in deeper soil layers is likely comprised by a greater proportion of aboveground plant tissues than SOC from reference sites (which are more likely to reflect root-derived precursors). Given all of these factors, I do not think that bulk 13C values can be realistically employed as a proxy for SOC quality.

It is true that selective preservation of recalcitrant lignin will result in decrease of d13C values. However, as predicted by Benner et al (1987), a decline of 4 per mil of d13C requires a lignin concentration of 81.4%, which is unrealistic in the mineral soils. For example, Amelung et al. (1999) reported that the lignin component is less than 10%. Also, we admit that there should be shift of C3 and C4 plant, but this exerts effects on both the stable and depositional site. We are aware of these issues and added a cautionary statement in the text (Line 353-358) to illustrate that d13C data are supportive of our argument rather than evidence.

Were incubation experiments only performed on profiles with only a 50 yr. deposition history? If so, how might this inïňĆuence interpretation of respiration rate results on

BGE for soils with 1500 year deposition histories?

The profiles have a much longer deposition history than 50 yrs and we have revised this section as this was apparently not clear. The samples for incubation experiment were taken from site A where tillage erosion is more dominant in comparison to water erosion due to the fact that it is located at the field border. 137Cs and P indicate that sediment deposits of the last century correspond to the depth of about 80 cm (Figs. 1 and 2). The soil cores we collected at site A were 2 meters in length (Fig. 6), and this covers a deposition history of more than 300 years considering that the sedimentation rate should be significantly lower before the application of mechanized agriculture. The reason why we only show the result of the last 100 years in Figs. 4 and 8 for site A is because we do not have correct sediment age of older sediments at this site (i.e., OSL or 14C data).

It should be noted that we used these incubation data for a qualitative comparison between depositional and reference sites in order to explore relevant mechanisms rather than deriving quantitative information from these data and we have rephrased the text to clarify this. As such, we believe that as long as the mechanism we proposed is correct, the derived conclusion (based on sediment with ages up to 300 years) that SOC at the depositional site is more recalcitrant or stable in comparison to the reference site at the same depth is also valid for sediments that are older than the ones we studied. We clarified this in more detail in the revised text (Line 576, 598). However, it is clear that more data are necessary to study this in more detail.

Minor Comments and Technical Corrections

Abstract needs information about timescales relevant to BGE in this study.

Information about timescales for the CBE to reach constant has been added (Line 27).

Line 111: was there no agriculture at this site prior to 1950?

Historical topographical maps show that this site has been arable land for at least 250

years. The sentence has been changed in the text to avoid misunderstanding (Line 108).

Line 116: provide the depths to which cores were collected. How did you correct for compaction that typically occurs during core collection?

This information has been added in the text (Line 114, 117, 124).

Line 117: provide more detail about core collection. What is a rammer? (hydraulic-driven probe?, hand driven?, other: : :?)

A more detailed description of the coring device has been added (Line 116).

Line 119: mode details about reference sites are needed. Slope, upland contributing area, location coordinates (a study area figure would be helpful).

These details are available in Table 1.

Line 129: more instrumentation, calibration, and standard-correction details are needed for d13C measurements.

Such information has been added (Line 134).

Line 131: Was the HCl fumigation also performed on these samples?

No, they are not fumigated using HCl. The content of inorganic C in this study site is relatively minor in comparison to the organic C content (Lettens et al., 2004). We performed HCl fumigation for the samples of site A is mainly because d13C results are very sensitive to the existence of inorganic C.

Line 133: replace "methods" with "instruments"

'methods' has been replaced with 'instruments' (Line 142).

Line 136: what was the mass of sample used?

The mass is between 40 - 110 g, which has been added in the text (Line 146). The

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reason why we used varying mass of soils is because that the C content of the samples are different, and using different masses can ensure that the CO2 concentrations in the different incubation jars are of the same level.

Line 138: how was this amount of water determined? (based on mass?)

Yes, it is based on mass. Now this information is added in the text (Line 147).

Line 146: if the samples were in air-tight jars, how could there be any water loss during incubation?

One possible reason can be that the water was lost from the exchange of gases in the incubation jars with the ambient air during the gas sampling: the soil samples used in the incubation experiments are between 40 – 110 g, which only occupy a small part of the volume of the 1 L jars. At every time of gas collection, the gases in the jars were replaced with ambient air with low CO2 concentration. Due to the high water content (27%) used in the experiment, there should be loss of water from the soil to the gases in the jars. This water was flushed away with ambient air latter during gas collection. Mineralization of C in the soil also caused loss of sample mass, but this effect can be negligible considering the fact that the soil respiration rate per soil mass is at the magnitude of mg C / kg soil *day and the gas collection interval of the experiment is about two weeks. Now, modification has been performed to indicate that the loss of water occurs during gas collection rather than incubation (Line 155).

Lines 153-157: see my comments above about P and 137Cs.

This been addressed. See the correspondence above.

Line 167: replace "mode" with "made"

This sentence has been rewritten in the revised version (Line 181).

Lines 169-171: can't you just report the instrument minimum detectable activity? Also, report this error.

The range of this error has been added (Line 185).

Lines 181-182: report this error.

The range of this error has been added (Line 195).

Line 197: to be accurate, 14C data only provide information about the age of C-fixation. Sedimentation age may be younger, but not older than the age suggested by 14C.

To address this time lag, we have stated that 14C data provide information about the age of C fixation, and that it is still an approximation even when charcoal production immediately precedes deposition (Line 213) in the revised version.

Line 224: it would be more accurate to state that you are quantifying the sensitivity of the uncertainty.

This paragraph has been rewritten in the revised version (Line 242-246).

Line 251: provide references.

Rommens et al. 2007 has been added (Line 209).

Lines 275: did you measure bulk density?

Bulk density is not really needed here: The mass of the samples used in the incubation experiment was measured. Therefore, the soil respiration rate per unit of soil mass was calculated by dividing the generated CO_2 with the mass of the soil samples.

Lines 300-302: provide references.

Reyniers et al. 2006 has been added (Line 338).

Lines 305-306: provide data or a reference to support this.

Doetterl et al. 2012a has been added (Line 312).

Line 337: do you mean to say monotonically?

'monotonously' has been replaced with 'monotonically' (Line 346).

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