

Interactive comment on “Geogenic organic carbon in terrestrial sediments and its contribution to total soil carbon” by Fabian Kalks et al.

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Received and published: 25 September 2020

We thank the reviewer #1 for the very helpful comments to our manuscript. We will take all comments into account for a revised version of the manuscript. See below our comments to the issues raised by the reviewer.

Methods 1. Provide Info on soil depths and weathering depths. It should be rather easy to see from the cores where soils start and end to give at least a relative indication of difference in soil depth between the three cores. This is important as you argue later on with variable C inputs which should have an impact on weathering. - We will add more detailed information about the soil characteristics and weathering depths based on soil profiles. For the purpose of comparability of OC stocks we will keep the subsoil

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definition for 150 cm depth.

2. Clarify – How has the sample been chosen for each depth increment that was later analyzed and treated? Is this a composite of each 1m increment, taken at the centre of the increment etc. - We will add information on soil sampling for chemical analysis: samples have been taken from a depth of ~ 90 cm of each 100 cm increment (so 1.9 m, 2.9 m, 3.9 m etc). This decision was based on two aspects: First we wanted to have the same depth for each material. Second, Loess samples were obtained in closed cylinders that could only be opened at the top or bottom. To avoid taking samples from possibly contaminated edge of the cylinders we removed the first 5 cm and also removed the outer material inside the cores (see l. 129).

3. Due to the very low carbon concentrations that we have been measured in these sedimentary rocks, giving confidence in the reliability of the measurements is extra important. For example, 1M HCL was used for decarbonatization of samples for ^{14}C analyses, but for the rest inorganic C was assessed using loss on ignition parallel to dry combustion for total CN. Can you say something on the uncertainty of the methods, detection limit and replication for loss on ignition vs total combustion vs acid hydrolysis? I assume the uncertainty varies considerably, varies with depth and concentration and might related due to incomplete and variable assessment of inorganic C which would affect a number of conclusions - We will add information about the uncertainty of the method. We measured carbon contents per loss on ignition because we wanted to know organic and inorganic contents and not just the organic carbon content (that we would have received if we used decarbonised ^{14}C samples). The uncertainty arised from the measured values and repetitions. Lowest measured values of total C in the cores samples ($n = 3$) was found in sand samples ($0.04 \text{ mg C g}^{-1} \text{ soil}$) while the same samples showed C values of 0.00 and $0.01 \text{ mg g}^{-1} \text{ soil}$ after the OC was removed at 450°C . Because this variation between 0.00 and 0.01 was random between the replicates, we assumed sand samples to be free of inorganic carbon. Furthermore $0.01 \text{ mg C g}^{-1} \text{ soil}$ can be seen as the detection limit of the used ignition method and 0.03 mg

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OC g-1 therefore as a reliable content. For 14C measurements, respective samples always were decarbonised.

4. One thing that concerned me with the methods was that Loess deposits seem to be free of inorganic C -which shouldn't be the case for unweathered deposits- except for some spikes at greater depth. - According to Wagner (2011) ("Spatial analysis of loess and loess-like sediments in the Weser-Aller catchment, Lower Saxony and Northern Hesse, NW German") the investigated Loess at the Ahlshausen site belongs to the Leine Ihme Basin and is referred to as "loamy loess" or "loess loam" that has been decalcified during weathering and soil genesis. We will add this information in the result section.

5. During the incubation, have any amendments except keeping water constant been made? 533 days is a long time without additions and microbial activity will be affected. You state in your discussion that you expect some C input through exudates to play a role, so this would be something to consider when interpreting your respiration. - During the incubation no further amendments have been made – they were completely sealed. Although we considered the fact that we did not add substrate during the incubation in the discussion (as we observed mineralisation only by adding water) this will be discussed more detailed.

6. How have you treated problems of oversaturation with CO₂ if the containers were packed air-tight or have they been flushed with ambient air except for a shorter time before CO₂ measurements to accumulate gas for sampling? There would be gas exchange in nature in these sediments and standard deviation in Figure 3 reveals quite high variability for the 533 day sampling points, especially for loess. And if they were gas tight, why not analyzing CH₄, which is more important in oxygen deprived environments. - The containers have been kept air tight for the whole period of incubation. The air volume of the containers was 5 l and due to the comparatively low production of CO₂ we can assume that no oxygen limitations occurred. We further had no problems with oversaturation, since total CO₂ values stayed within the calibration range (maxi-

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mum value was 7650 ppm) and concentrations did not exceed typical concentrations of CO₂ in subsoils. We also analysed all samples for CH₄ concentration which were on a very low level. Oxygen concentrations were all the time >20 Vol%. We will add this information in a supplement graphic. Thus, there were no indications for oxygen limited conditions.

7. How many replicates have been used during incubation? - We used 4 replications for the incubation. This is mentioned in the material and methods section (l. 206 in chapter 2.4) and also in the caption from Fig. 3. Since some few samples were not air tight at the end and were removed, especially after 533 days, which reduced the number of repetitions.

8. Give an overview on these mineralization rate constants that you took from Qualls and Haines. As the authors know, a lot has happened since then in terms of re-defining pool models and C turnover and the Qualls and Haines study was on dissolved organic carbon and turnover there. I think you need to provide some confidence why the rates and equations provided there are applicable for such a different system as your soil/powdered rocks experiment. Given the uncertainty surrounding the assumptions behind the pools I wonder if it won't be better to leave out the pool model altogether and just work with observed data assuming a linear trend of respiration between two measurement points along the timeline. I believe 14C measurements on the respired C across the length of the incubation experiment would have helped. - We used a two pool model because we assumed that mineralisation behaviour can be explained by the degradation of a labile and a stable OC pool. However, measurements after 533 days are less reliable (due to removed non-air tied samples) and there was a large time lag between sampling at day 63 and at day 533 days indeed. We will take the reviewer comment into account and apply a linear respiration model. Additionally, we will leave out the sampling point after 533 days and focus on the comparison between the first and the second incubation experiment (graphics will be changed and results and discussion applied). 14C measurements might have helped and have been intensively

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discussed during preparation of the incubation experiment. But due to several technical limitations while working with sedimentary samples (e.g. extremely low CO₂ production from the sedimentary samples leading to the problem of not detectable changes in the ¹⁴C signal) we could not apply them. The background CO₂ in the bedrock material could hardly be removed in order to start with a CO₂ free incubation.

9. For the analysis of how much C has been mineralized, were soil and sediment samples measured after the incubation again to check if your CO₂ loss calculation and mineralization rates make sense? - No, we did not measure OC concentrations after the incubation experiment. The incubation indicated that roughly 1 % of OC was lost due to mineralisation. Having a mean relative standard deviation of 13 % (analytical error derived from laboratory replicates) for the sand samples we do not think that measure samples after incubation will lead to results that could allow for a verification. Furthermore the blank samples from the incubation showed no trend of increasing CO₂ (in fact the concentrations slightly decreased within the 533 days) so that we can exclude a possible contamination being responsible for the observed mineralisation.

10. What confidence can you give for the CO₂ respiration assessment between days 63 and 533? Figure 3 shows that the curves differ a lot between those two phases of the experiment. - See comment on 8. We will restrict the main assessment to the first 63 days and we will show the respiration behaviour for the whole 533 day period in the supplement for each single sample without grouping them.

11. Stats: I could not follow the authors argument why standard error and significance could not be displayed in the manuscript. Yes, the model output might be tricky, but other measured and experimentally assessed parameters can and should be displayed with some statistical confidence to avoid speculating on outliers in the depth trends of the data. - We will add the mean relative standard deviation from the laboratory replicates for each sample. Since we have only two field replicates and one Loess core we cannot use standard deviations from field replicates., We further will add results from the two cores in Fig. 1 to show the variation for the OC, D13C and IC content

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derived from two cores.

Results and Discussion

12. You might want to give the discussion some more sub-headers. For example, l.429-439 vs the section before and after seem to be distinct from each other. Will help to structure it - We will add subheaders for section 4.1 of the discussion

Parts of the discussion are speculative. Here the examples I would see some revision on: 14. Roots and root exudates have been named for deep biogenic C inputs. Name the depths you are referring to for this and provide some estimates on rooting depth if available. As your data does not show strong depth trends for >4m soil depth, which would be expected if C cycling is still tied to DOC inputs - We will add information on possible rooting depths for the sites and the implications for the observed trends in the discussion. In particular in sandy substrate with tree vegetation rooting depth can be very deep.

15. What evidence do you have to expect soil burial and soil formation during the Pleistocene avoiding a circular argument with GOC as an indicator? I am not sure what to make of this argument. - We added this information to explain the discrepancies between bulk parameters and incubation results from different depths of the Loess sample. The reason to expect soil burial and soil formation is that we observed different layers within the loess sediment in terms of colour and OC contents. These layers formed during different climatic periods (warmer, cooler) as described in detail by Jordan & Schwartau (1993) who investigated the loess site and could assign the different layers to specific sedimentation periods during the Pleistocene.

16. You need to discuss the fact that you incubated at 20°C, whereas temperature at the depths in which these sediments reside will be at the mean annual temperature of the study region. So roughly 9°C. That's a giant step in terms of potential energy available for microorganisms. - The incubation should not simulate degradation under real environmental conditions but the potential biodegradability and to make it compa-

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rable to other incubation results. We will add explanation about the biodegradability of GOC in dependence of temperature regarding the different Q10 values in the discussion section. In the material and methods section we will add an explanation why we chose 20°C.

17. I think some discussion on the quite varying depositional regime between the three geologies is necessary as part of the discussion on why sedimentary C is bioavailable or not. Some of the more degradable components might be lost before sedimentation and overpower variability in biodegradability compared to stabilization of C in soils and sediments. - We will add information about the quality of OC in the sediments (unpublished work about the quality of GOC in the sediments) and the different microbial communities that could be derived from the sediments and their potential respiration rates (e.g. the paper from Fredrickson & Balkwill 2006) in the discussion section 4.2.

18. L. 550-556 Seems disconnected to me from the rest of the discussion. Consider deletion - This part is indeed somehow disconnected from the rest of discussion in this section, but we think it is important to draw attention to the situation of having igneous parent materials and a global classification of our findings. Therefore we will keep and revise it and we will add a new header for this small section.

Further comments 19. The title is a bit misleading. I think it would be better to say “contribution to terrestrial(or soil) carbon cycling” – as the study also involves incubations and isotope work and not just stocks or similar as indicated at this point. - This would move the focus of our study from the contribution and stability part towards a global carbon cycling view which we think is too broad for our experimental design. The focus of the study should remain on the contribution of GOC in subsoils.

20. pH measured with what? H₂O, KCl, CaCl₂? - Specify in methods - Measured with 0.01 mol L⁻¹ CaCl. Will be specified in the method section.

21. The text is well written, but there are shortcomings in wording and grammar all over the manuscript. Its nothing that stops the reader from following, but I suggest a

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native English speaking colleague checks this manuscript before submission. Some examples (I only picked a few, but there are more): l. 135. Grammer: “heated” instead of “heating”. l. 214. Pouring “bulk” density is the correct term I believe. l.441 median of 0.27g kg⁻¹ of what? l.542. Check gramma - We will again check for these grammar mistakes. However, this manuscript was already checked by a native English speaker and professional language editor.

Interactive comment on SOIL Discuss., <https://doi.org/10.5194/soil-2020-34>, 2020.

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