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

Anonymous Referee #1

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Review Kalks et al. 2020

"Geogenic organic carbon in terrestrial sediments and its contribution to total soil carbon" by Kalks et al. is a well written, well structured and timely contribution to an interesting topic, namely the varying contribution of geogenic C to the terrestrial C cycle. For this the authors study C dynamics in a depth explicit way on a range of sediment cores taken from different sources of parent material for soil development in central Germany. It's a good manuscript that falls into the scope of the journal.

My main comment is the lack of confidence that the authors provide at this point in the several of the analytical measurement done and in the statistics behind it. The apparent low number and/or absence of replication for some of the analyses worries me a bit too. Especially as there are some datapoints that were discussed including mechanistic

interpretation (for example, inorganic C variability) that look more like outliers. I give more detail on this below and I am sure the authors can address this as part of the revision process.

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.

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 center of the increment etc.

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 14C 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

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.

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.

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.

7. How many replicates have been used during incubation?

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.

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 calculations and mineralization rates make sense?

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.

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.

Results and Discussion: 12. You might want to give the discussion some more subheaders. For example, l.429-439 vs the section before and after seem to be distinct from each other. Will help to structure it

13. 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

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.

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.

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.

18. L. 550-556 Seems disconnected to me from the rest of the discussion. Consider deletion

Further comments:

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.

pH measured with what? H_2O, KCl, $CaCl_2$? - Specify in methods

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 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 grammar.

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