Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-273-AC2, 2019

Biogeosciences
Discussions
Open Access

Interactive comment on "Ideas and perspectives: Emerging contours of a dynamic exogenous kerogen cycle" *by* Thomas M. Blattmann

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Received and published: 1 October 2019

Dear Reviewer,

Thank you for your feedback and input. Overall, the boundary conditions imposed by proxy information collected over decades of work basically amount to one undeniable fact: we are unable to fully reconcile and align glacial-interglacial CO2 variability with changes in the biosphere and gas exchange with the ocean. We therefore need a new hypothesis.

On the most basic level, the constraints imposed by radiocarbon clearly indicate we are missing one fundamental parameter: a source of carbon depleted or devoid of radiocarbon (Broecker and Clark 2010; Zhao et al., 2018). When asking ourselves what

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this source may be, the transition during times of highest CO2 increase between glacial and interglacial clearly reveals a negative stable carbon isotopic shift of atmospheric CO2 (Smith et al., 1999; Schmitt et al., 2012), which is a strong indicator of respired organic carbon acting as source to the atmosphere (Bauska et al., 2016).

With this, exogenous kerogen becomes an obvious candidate as the missing player in the global carbon cycle. It fulfills the basic requirements: it is abundant, it is isotopically light, and it is radiocarbon-dead. For an additional host of reasons, it appears plausible, as elaborated in the manuscript, with kerogen reburial efficiency higher during glacial episodes in Earth's history as a straightforward supporting observation (see references in Blattmann et al., 2018).

As pointed out by yourself (the reviewer), viewing the glacial and interglacial episodes as individual time intervals, one may draw the conclusion that there is biospheric uptake of isotopically light carbon on land (Shackleton 1977; Shackleton et al., 1983), which I agree, is most likely true. However, this view regards the two climate states as steady state conditions and does not consider transitional dynamics. Also, stable carbon isotopic changes are trickier to interpret as carbon isotopes fractionate as they rotate between different carbon pools changing in size; in contrast, radiocarbon isotope composition of atmospheric CO2, which is fractionation corrected, is more straightforward in its interpretation. Ultimately, biospheric activity and atmosphere-ocean exchange cannot account for the radiocarbon budget and its evolution from the Last Glacial Maximum to the present. However, yes, the kerogen cycle is one "cogwheel operating under manifold feedbacks in the greater Earth system" (original manuscript) and clearly this point needs to be expanded on to present a mediated view.

How important is this cogwheel? And you (the reviewer) rightfully points out that a back-of-the-envelope calculation is lacking. The "Glacial Burial Hypothesis" (Zeng, 2003), which contains parallels to the hypothesis presented here, takes care of this in a detailed manner. Zeng (2003) proposes the oxidation of ice sheet-covered soil organic carbon during glacial retreat. Overall, the modeling results of Zeng (2003, 2007), if

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soil organic carbon is reinterpreted as kerogen, lend further support to the idea that kerogen oxidation drives a substantial part of atmospheric CO2 increase in the wake of glacial episodes. In quantitative terms, Zeng (2003) attributes the oxidation of 547 PgC soil organic carbon to a 60 PgC increase in atmospheric CO2 (increase of 30 ppm). I view these estimates as conservative, because unlike Ning Zeng's assumption of soil organic carbon oxidation, the supply of kerogen shed and subject to temporary storage on land over millennia of glaciation is much vaster! Overall, the dynamism of the exogenous kerogen cycle over the Ice Ages may seems further strengthened by increased bedrock exhumation (Herman et al., 2013; Herman et al., 2015), thereby leading to greater detrital fluxes of kerogen reburied into ocean sediments and also enhanced supply of ground kerogen exposed to the elements in the wake of glacial episodes.

I thank the reviewer for his/her time and effort and look forward to future discussions and strengthening the manuscript based on this input and adding the references cited herein.

Sincerely,

Thomas Blattmann

01.10.2019 Yokosuka

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Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-273/bg-2019-273-AC2-supplement.pdf

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