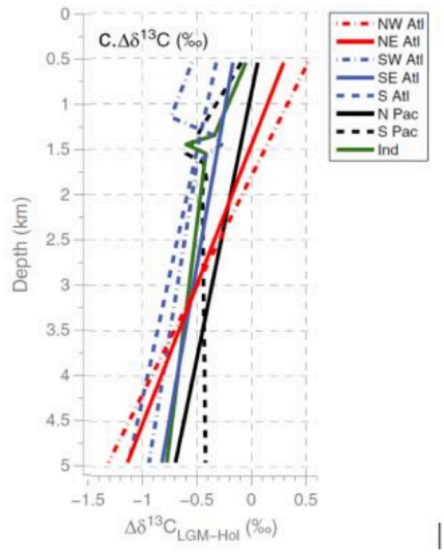
Review of « Ideas and perspectives: Emerging contours of a dynamic exogenous kerogen cycle » by Thomas Blattmann

I am happy to see that, finally, this new version of the manuscript attempts to discuss the question of the carbon isotopic budget as a fundamental constraint on the origin of the deglacial atmospheric carbon increase. But still, the author seems to cherry pick only some oceanic data as a way to stick to his original hypothesis and therefore does not provide a fair account of the litterature on this topic. As a result, I feel the author tries to blur the marine isotopic evidence in order to make his point that kerogens contributed significantly to the pCO_2 glacial-interglacial increase. Overall, I believe this is damaging to the paper, since it does not provide a fair account of the available litterature on this topic and a fair account of the actual numbers.

<u>In the response :</u>
« generalizing the positive Last Glacial Maximum to Holocene $\delta^{13}C$ shift to the global oceans is imprecise… »
<u>In the revised paper :</u>
« the global deglacial increase in carbon isotopes shows a notable exception: For much of the North Atlantic, the Holocene stable carbon isotope values of DIC are lighter than those of the Last Glacial Maximum … This is notable because the northernmost Atlantic is the locus of major downwelling driving global thermohaline circulation … »



The question is not to « generalize » the isotopic shift to the world ocean, but simply to compute the **net global budget**. From the above Fig. (from Peterson et al 2020) the author does « cherry-pick » the North Atlantic intermediate waters (the top part of the red curves) as an example of oceanic ^{13}C data that heavier during the last glacial. Without doing any complex computation, my conclusion from this Figure is that most of the Ocean (and in particular the heavy players like the Pacific) are lighter during LGM. Since the DIC in the ocean accounts for about 95% of the Earth surface carbon, since most curves on this figure are negative, since doing carbon (or any) budget implies accounting first for the largest reservoirs, I conclude that the **global carbon** signature was negative, therefore the contribution of light carbon (living organic matter, permafrosts or kerogens) is globally to stock more carbon during the Holocene than during the LGM. Again, this is well known for many decades in the carbon community and it stands indeed as a major contraint. It therefore « does not help » to solve the pCO_2

increase, but on the contrary raises the burden for the oceanic contribution. Peterson et al (2020) conclude their paper with a revised estimate of this global budget, on the Figure below (the stars with error bars).

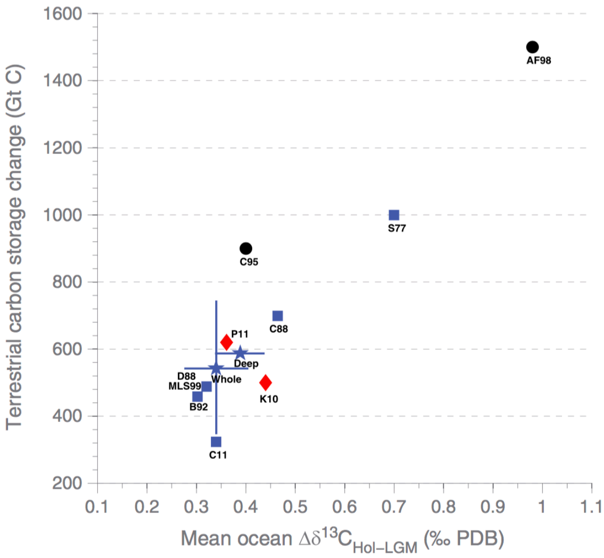


Figure 5. Comparison of our deep-ocean and whole-ocean estimates with previous studies. Marine estimates are denoted by blue squares, our estimates are blue stars; terrestrial carbon storage change estimates for these studies are calculated using equations (1) and (2), as indicated in Table 1. Model-based estimates are red diamonds, and pollen-based and vegetation-based reconstructions are black circles. The error bars on our estimates are 2σ; however, uncertainty estimates from other studies are omitted for clarity. S77 = [*Shackleton*, 1977]; C88 = [*Curry et al.*, 1988]; D88 = [*Duplessy et al.*, 1988]; B92 = [*Boyle*, 1992]; C95 = [*Crowley*, 1995]; AF98 = [*Adams and Faure*, 1998]; MLS99 = [*Matsumoto and Lynch-Stieglitz*, 1999]; K10 = [*Köhler et al.*, 2010]; P11 = [*Prentice et al.*, 2011]; C11 = [*Ciais et al.*, 2011].

It is interesting to note that ALL estimations since the very first one (Shackleton 1977) agree that the **MEAN** ocean ^{13}C signal was lighter during the LGM, and therefore that the (terrestrial) light carbon stocks (living organic matter, permafrosts or kerogens) were therefore smaller during LGM by several hundreds of GtC. This appears to me quite a strong and robust consensus on this question, and it seems to me not fair to avoid this piece of evidence by downplaying it. Of course, if the deglacial terrestrial vegetation regrowth is very large, this may allow for a significant release of permafrosts or kerogens : the isotopic constraint applies only to the overall budget.

<u>In the paper :</u>
« In contrast to DIC of the oceans, atmospheric carbon isotope composition of CO2 directly measured from ice core recovered CO_2 reflects a well-mixed, global signal. »

Indeed, but it only accounts for 1 or 2% of the Earth surface carbon (about 600 GtC compared to 40000 GtC) : the isotopic signal is interesting for the dynamics of the deglaciation, in particular the timing of the different contibutions (vegetation, permafrost, ocean, ...) since it stands « at the center » of these exchanges. But it is certainly not very relevant for the overall glacial-interglacial budget.

<u>In the paper :</u>
« Reconstructed stable carbon isotope composition of DIC stems primarily from foraminifera which may also include bias from vital effects (e.g., Erez, 1978; Spero et al., 1997; Lea et al., 1999; see also Schmittner et al., 2017). Unlike the global, nearly unison rhythm of the glacial-interglacial marine oxygen isotope record, the global deglacial increase in carbon isotopes shows a notable exception »

There are also many notable exceptions in the oxygen isotopes... as well as many unconstrained vital effects. Still, the carbon isotopes measured in modern foraminifera follows closely the carbon isotopes measured in modern seawater, and they have been calibrated and used for almost 50 years as THE main tracer of carbon in the ocean in paleoceanography. I therefore do not agree with the author's sentence, whose purpose seems only to avoid discussing seriously the isotopic budget problem.

In the response :

« in summary, the modeling work by Ciais et al. (2012) and Crichton et al. (2016) suggest that the observed δ13C patterns in atmosphere and ocean are compatible with kerogen oxidation. »

Of course they are… WHEN accounting for the problem and accepting that (basically) MORE than 100% of the glacial-interglacial carbon came from the ocean, since the NET organic matter contribution is globally negative. The figures below (from Crichton et al. 2016), also cited by the author in his response, are very explicit on this point when discussing the role of permafrost.

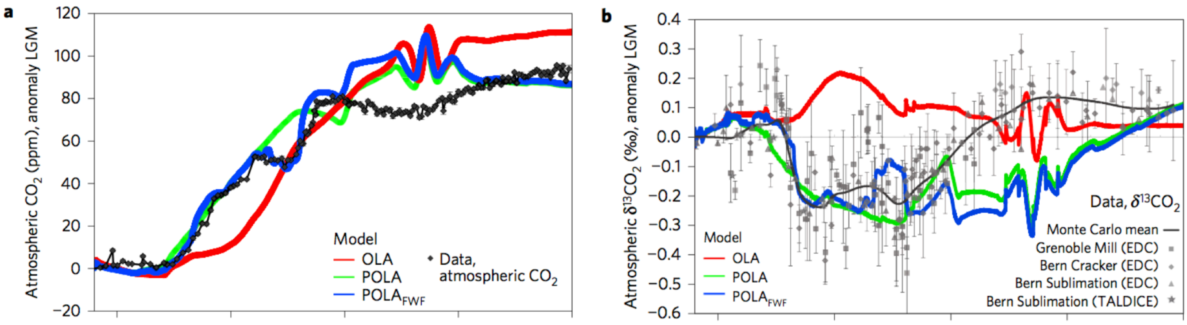


Figure 2 | Evolution of atmospheric CO_2, $\delta^{13}CO_2$ and Northern Hemisphere temperature in model and data. a, Atmospheric CO_2 concentration anomaly from LGM in ppm for three model outputs and data[1], **b**, Atmospheric $\delta^{13}CO_2$ for three model outputs and data[4].

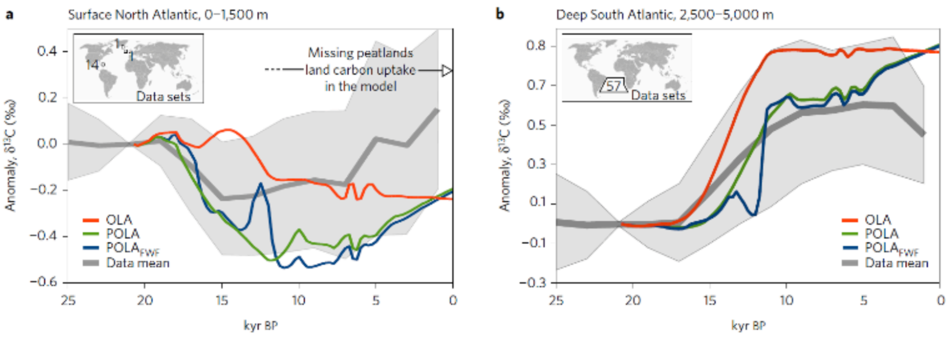


Figure 3 | Evolution of ocean $\delta^{13}C$ in model and data. a,b, Data[19] are for benthic $\delta^{13}C$ with one standard deviation data range (grey shading). Model output is for matching depth and latitude bands as data. The number of data sets and their locations are indicated on the maps. Missing peatlands expansion period shown. **a**, Surface North Atlantic waters (data only for the western current). **b**, Deep South Atlantic waters.

The red curve corresponds to the « ocean-only » (including vegetation changes) contribution, which explains (more that) entirely the pCO_2 rise as well as the (South Atlantic) oceanic ^{13}C signal, due to a net increase in terrestrial organic carbon stocks linked to vegetation regrowth. This of course may leave some room for a permafrost (or a kerogen) contribution that may help explain the atmospheric ^{13}C signal as shown in the Crichton paper, to the extent that it is smaller that the vegetation regrowth (since the net organic carbon contribution must remain negative).

To conclude, I want to stress that I have no objection against the author's hypothesis that kerogens may have some role in the deglaciation. But his paper would be much more interesting and valuable if it would present an unbiased view of the current knowledge on the glacial-interglacial carbon problem.