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Climate of the Past Discussions

Interactive comment on "A permafrost glacial hypothesis to explain atmospheric CO_2 and the ice ages during the Pleistocene" *by* R. Zech et al.

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We thank reviewer #1 for his/her detailed feedback. We appreciate the possibility to comment on the raised concerns, and thus hope to be able to change reviewer #1's impression that our "[permafrost] hypothesis is premature and not publishable. [. . .] We would have to rethink a lot of the current understanding of the carbon cycle, and therefore all available evidences for and against it should be weighted in such an attempt to finally come to a firm conclusions."

We think indeed that a lot of rethinking is necessary. Before addressing the main issues in reviewer #1's details,

(i) we acknowledge that the carbon cycle is too complicated to review ALL evidences for or against the various glacial hypothesis, but that to the best of our knowledge, our permafrost hypothesis is not violating existing data, including 13C and 14C (see below), particularly when keeping in mind that most proxies can't be interpreted robustly in a straight-forward way.

(ii) We are NOT "totally neglecting other changes in terrestrial carbon content induced for example by vegetation changes, which might have led to a glacial carbon source of similar size but opposite sign than the carbon uptake in the permafrost"; we are drawing attention to an additional, huge, yet 'forgotten' carbon pool, whose dynamics might be crucial to explain the glacial-interglacial change in atmospheric CO2 and which should be included in future global carbon models.

(iii) We need to correct the wrong impression that our "whole [permafrost] hypothesis is based on one single time series of changes in TOC over 220 kyr. [. . .] It tries to make some statements over a time window (last 1 Myr), which is simply not cover by the data set (220 kyr)". We acknowledge that our own spatial up-scaling approach is very simplistic, but "In a different study cited herein (Zimov et al., 2009) the glacial-interglacial difference in C stored in permafrost soil was also estimated, but based on some process understanding (condensed in a model) of carbon input and decomposition in soil." We then simply extend the lessons learned from our permafrost profile and the cited soil carbon model to the Middle and Early Pleistocene: Integrated annual insolation provides an intriguingly simple forcing for permafrost thawing/expansion at its southern boundary (see further details below).

In summary, although some points in our manuscript will need more detailed explanation (and rough calculations), we feel that overall the arguments brought up by reviewer #1 don't justify rejection of the manuscript. We also acknowledge that the current style of the manuscript (including the title) may be a bit too provocative, and we are willing to soften the style, if the editor advises us to do so.

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Details:

4. While “there are various ideas and also modeling results out in the literature, which can explain quite a lot of the observed glacial-interglacial change in CO2”, this is not an argument against our proposed permafrost hypothesis. “Summing up for those processes, for which we have a good scientific understanding”, as suggested by reviewer #1, leaves us with only a few ppm being explained! (by the solubility effect, changing sea level and terrestrial carbon storage excluding permafrost, see for example Kohfeld and Ridgwell, 2010, their Fig. 2). We cite from reviewer #1: “All other processes, such as changes in ocean circulation, Southern Ocean or North Pacific ventilation, marine biological pump, sea ice, dust, brine rejection, etc etc have some merits and shortcomings and one can certainly find arguments for and against them [. . .].” Passages from Kohfeld and Ridgwell (2010) read: “The assessment of different (primarily) model-based estimates is not exhaustive, nor can the estimated glacial-interglacial CO2 contributions a priori be an entirely objective judgment. [. . .] The level of scientific understanding is subjective.” With regard to proxy evidence, these authors caution: “The important lesson to acknowledge is that each change we measure can have a host of meanings, and it is up to us to interpret theses proxies in the most meaningful way based on our combined understanding of the physical, biological, and chemical systems.”

We reiterate an important point by citing a recent modeling study: “[. . .] when all plausible factors are accounted for, most of the necessary CO2 change remains to be explained. This presents a series challenge to our understanding of the mechanisms behind changes in the global carbon cycle during the geologic past” (Tagliabue et al., 2009). Other models might perform better, but as Kohfeld and Ridgwell (2010) acknowledge: “There is no unique or “correct” model for the glacial carbon cycle, if for no other reason than to create the perfect model would require that the causes of low glacial CO2 were a priori precisely known, which is the unanswered question being addressed in the first place. [. . .] Mechanisms, of which very little is understood if only because of a historical deficit of analysis, are our 'unknown unknowns'."

In summary, we would be concerned, if new glacial hypotheses would not be published with the argument that we have already achieved a sufficient level of understanding of the Earth system.

5. We thank reviewer #1 to draw our attention to Skinner et al. (2010). We read it as further evidence for the current gaps of understanding the carbon cycle: "It has been impossible so far to find the supposedly large pool of 'old' radiocarbon trapped in the glacial deep ocean, which would be required to corroborate the 'ocean hypothesis' (De Pol-Holz et al., 2010;Broecker and Barker, 2007)." (from our manuscript, introduction). Compare to: "Indeed, none of the available marine 14C reconstructions reveal the occurrence of a relatively aged and widely exported deep-water mass before the initiation of the so-called "mystery interval" at ∼17.9 ky B.P. (HS1) [. . .] We can only say that if the ∼2000-year reduction in benthic-atmospheric age offsets recorded across HS1 in MD07-3076 (their core) was experienced by ∼30% of the ocean [all water deeper than the next deepest 14C constraint from the glacial Pacific], this could explain just over half of the 190‰ drop in atmospheric D14C across the mystery interval." (from Skinner et al.'s introduction and conclusion, respectively).

9. We consider a 10°C temperature reduction from the interglacial to glacial a reasonable estimate for our rough first-order calculations of 'excess' carbon storage in glacial permafrost areas. We thank reviewer #1 for the reference Braconnot et al. (2007), who present similar values based on modeling studies.

Reviewer #1 suggests that "this section [spatial up-scaling] should include estimate on changes in other carbon pool, e.g. permafrost in North America, soil in other regions and vegetation changes to come to a firm estimate, what the magnitude of change from the terrestrial pools might be." While we acknowledge that such estimates are necessary for global carbon models, this is beyond our expertise and beyond the scope of our paper. Apparently, however, such estimates are already available: "Knowledge

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from pollen data and vegetation models (e.g. Joos et al., 2004) come to some conclusions in the absence of C in permafrost so far." Thus the permafrost carbon can be considered an additional carbon pool that should be included in future global carbon models. We think the permafrost carbon pool might dominate (or at least significantly change) the role of terrestrial carbon pools; reviewer #1 himself acknowledges: "A deglaciation scenario with carbon release in Siberia would be accompanied by C uptake in North America. Based on the area extends I would roughly estimate, that this would reduce the CO2 amplitude of the peatland (we assume he here means our permafrost?) hypothesis by a factor of 2." This would still be a net release of 500 Pg C during deglaciation, and we would still have to rethink our existing glacial hypotheses!

10. We acknowledge that this section (3.6 A revised role for the ocean?) may need rewording, more detailed calculations and explanations. However, we don't consider the arguments of reviewer #1 robust enough to justify rejecting our permafrost hypothesis:

13C ocean:

"The bulk global effect of changing mean oceanic d13C is seen in the deep Pacific [. . .]", suggesting that the more negative carbon signals there during glacials provide evidence for the (negative) terrestrial carbon pools to act as sources. We argue that this negative deep ocean signal (i) does not necessarily reflect the mean ocean signal, (ii) nor does the 'traditional' interpretation of benthic foraminifera isotopes take into account uncertainties of this proxy. We emphasize in our manuscript that "a persistent, yet unexplained finding is that the upper ~2000 m of the oceans were more δ13C positive during glacials (Curry and Oppo, 2005;Matsumoto et al., 2002)". Oliver et al. (2010) recently synthesize all available data and probably wisely conclude: "We consider the coverage too incomplete to directly construct a time-series of δ13C inventories." They also acknowledge that "a ïňĄnal caveat is that much remains to be learnt about how changes in properties other than seawater δ13C have inïňĆuences the δ13C record." Some of the obvious problems with interpreting d13C are changes in ocean circulation, biomass production and remineralization, and "that there is a tendency for benthic foraminifera to incorporate isotopically light 12C when the rain of organic carbon to the seafloor is especially high and therefore is not always a faithful indicator of the d13C of DIC of ambient seawater” (Kohfeld and Ridgwell, 2010).

13C ice cores:

“The two studies mentioned there (Koehler et al., 2010; Lourantou et al. 2010) are able to explain the observed d13CO2 ice core data without an additional permafrost carbon contribution, thus I can not see who they can support this discussion here”. In order to highlight that this is no argument against our new permafrost hypothesis, we would like to cite again Kohfeld and Ridgwell (2010): “There is no unique or “correct” model for the glacial carbon cycle, if for no other reason than to create the perfect model would require that the causes of low glacial CO2 were a priori precisely known, which is the unanswered question being addressed in the first place. [. . .] Mechanisms, of which very little is understood if only because of a historical deficit of analysis, are our ‘unknown unknowns’.”

Our reasoning is that d13CO2 during the Holocene and the LGM is very similar in ice cores, and we can therefore infer that the salinity and temperature effect (∼0.5‰ more negative values during glacials) was offset largely by sequestration of (negative) carbon in biomass . . . either in the ocean, or, as we suggest, in expanding permafrost soils. Reviewer #1 asks for at least a rough calculation: Sequestration of 1000 Pg -30‰ permafrost carbon would leave the ocean + atmosphere pool (∼40 000 Pg) depleted by ∼0.7‰This is roughly what is needed to offset the salinity and temperature effect during the LGM.

Releasing the same amount of permafrost carbon during deglaciation would result in a ∼0.7‰ isotopic drop of atmospheric (and mean ocean) d13CO2, which is exactly what is observed, but attributed so far to deep ocean upwelling and release of depleted carbon from there.

The 190‰ drop in atmospheric D14C during the mystery interval:

Similar rough calculations can easily be made for the effect of releasing “old” carbon from permafrost during deglaciation: Releasing 1000 Pg permafrost carbon (radiocarbon dead: -1000‰ into the glacial atmosphere (∼400 Pg C) and accounting for the fact that ∼90% of that carbon will be taken up by the ocean within several ka, our rough calculation suggests that D14C in the atmosphere should drop by ∼200‰ (simply mixing of 100 Pg dead carbon into 400 Pg atmospheric C, and ignoring transient effects and radiocarbon decay, etc.). This shows that permafrost carbon release changes D14C in the right direction and in the right order of magnitude.

Thus, overall, the atmospheric and ocean carbon isotopes provide NO argument to reject the permafrost hypothesis. Both the ocean and the permafrost hypothesis suggest an “old”, and isotopically depleted carbon pool during glacials, which is released upon deglaciation. We would love to see the permafrost hypothesis to be further evaluated in sophisticated global carbon models, which can then also refine our very rough calculations above.

11. Reviewer #1 asks for more explanation concerning the insolation forcing for the permafrost: “Some more arguments WHY [integrated annual insolation] is the key trigger [for permafrost] would furthermore help here (e.g. some heat budget calculations)”. We acknowledge that we might have to clarify in the manuscript that (deeper) soil temperatures (and thus permafrost) are mainly controlled by mean annual air temperatures and thus forced by annually integrated insolation. However, we feel that at this stage of the permafrost hypothesis, it should be sufficient to explain that “[integrated annual insolation] is controlled by the orbital parameter obliquity (Huybers, 2006)” (cited from our manuscript) and to plot obliquity to illustrate the timing of the insolation forcing. Note that in the supplement we provide the actual integrated annual insolation for those latitudes (around 45°N) most critical for the overarching permafrost hypothesis explaining the Mid Pleistocene Transition.

Again, we would greatly appreciate the permafrost hypothesis to be picked up and being evaluated in more detail by the modeling community, which may then have to

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include head budget calculations.

12. Reviewer #1 thinks that "the [permafrost] hypothesis [extended to the Mid and Early Pleistocene] is much too weak and not convincing for the following reasons:"

a) "The time series of TOC does not extend beyond 220 kyr it is therefore only speculation how this would change over time." While it is true that our permafrost record does not extend beyond 220 kyr, the permafrost hypothesis is NOT only speculation; it builds on (i) the insights obtained thanks to our record and the permafrost-soil carbon model of Zimov (2009) (i.e. colder temperatures favor permafrost → soil carbon mineralization is reduced → more permafrost carbon is sequestered), and (ii) on the reasoning that changes in permafrost are externally forced by integrated annual insolation.

b) "One would need to calculate the area weighted annual integrated insolation over the whole permafrost region and analyse its frequency spectra to really say something here." The southern permafrost boundary is undoubtedly the most vulnerable region for thawing, and insolation forcing right there is thus most relevant.

c) "The hypothesis is based on the idea, that changes in CO2 drive changes in climate. [. . .] All lead/lag analysis between CO2 and temperature in ice cores point in the other direction." Although the lead/lag issue is interesting on a centennial timescale, we wonder to what degree Southern Ocean circulation versus CO2 is important in controlling Antarctic temperature proxies as well. On millennial and orbital timescales there is surely no doubt that increasing atmospheric CO2 levels (irrespective of its source) cause warming and a lead/lad discussion is irrelevant due to positive feedbacks.

13. "The initial drop in CO2 in the early Holocene has also other explanation which also explain ice core atmospheric d13CO2 dynamics, which are therefore more convincing (Elsig et al. 2009)." This conclusion of reviewer #1 is based on "mass-balance inverse model calculations performed with a simplified carbon cycle model" (cited from Elsig et al. 2009), and we feel that Kohfeld and Ridgwell (2010) could be cited here again to put such model results into perspective: "There is no unique or "correct" model [. . .]".

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Again, we would love to see the permafrost carbon dynamics being integrated in global carbon models. We always have to keep in mind that models per se can never rule out that we have overlooked an utterly important “unknown unknown”!

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