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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 greatly appreciate the very constructive and detailed comments by G. Munhoven. While we look forward including many of his suggestions in a revised version of the manuscript, we would also like to address a few issues here, because we do not agree that there is robust evidence to reject the permafrost hypothesis at this point.

A more detailed discussion of our TOC record in comparison to atmospheric CO2 and dD, as suggested by Munhoven (C1026), would probably be an over-interpretation of this single record. Local effects on TOC will be evaluated in the future when additional records of this type and results from modeling studies become available. We emphasize that the robust and most important finding of our record is that organic carbon contents are generally higher when temperatures (dD) are lower (and this is independent of any uncertainties in the age control) Of course, our record should also not be considered to be a typical Siberian permafrost sequence. Not all sites will have the same loess accumulation rate, sites further north will not show the same mineralization intensity during interglacials, and sites further south have thawed completely during interglacials and lost most of their glacial organic carbon, and possibly also their intact stratigraphy. That's why “The authors immediately dismiss their own estimate of 300 Pg C [. . .] to embrace the 1000 Pg C estimate of Zimov et al. (2009)” (C1026). The latter is based on a much “more sophisticated up-scaling approach, using a permafrost-soil carbon model”. We fully acknowledge that this number should be carefully evaluated in future studies. To our knowledge no other permafrost carbon modeling studies have been published yet to allow an evaluation or comparison. Hence, Zimov's estimate could both over- or under-estimate the real amount of permafrost carbon release during the last termination. The permafrost glacial hypothesis should not be rejected based on (unsubstantiated) doubts about the existing 1000 Pg C estimate.

“Two more important questions not addressed in the paper” (C1027): 1. We estimated the timescale of permafrost degradation at first order to be the duration of the termination (5 ka, i.e. roughly 17-12 ka BP). Our main conclusions would not be affected even when assuming continuing permafrost degradation during the early Holocene, i.e. a degradation over $\sim$10 ka. 2. We fully acknowledge that part of the glacial permafrost carbon may not have been released to the atmosphere, but instead was buried e.g. in the coastal zone. Quantification of this potential effect is, however, challenging, and in view of uncertainties in all glacial hypotheses, this concern should not lead to rejection of the permafrost glacial hypothesis.

“A revised role for the ocean does, unfortunately, not stand any critical analysis. (C1027) [. . .] The fatal flaw derives from the omission of the terrestrial biosphere changes outside the permafrost regions. [. . .]. Accordingly, I do not see how this paper could be published in Climate of the Past unless it undergoes a major revision.” (C1028) We greatly appreciate the detailed, constructive feedbacks and are looking forward to refining our discussion of this part of the manuscript, yet we do admittedly not fully understand why G. Munhoven refutes the permafrost hypothesis. His words: "Adopting the 1000 PgC figure for the permafrost release during deglaciation, the biosphere regrowth (a conservatively estimated 600-850 PgC) would leave 150 to 400 PgC for the atmosphere/ocean to take up." (C1041) Acknowledging the uncertainties in all these estimates, would this not mean that the ocean might have acted as carbon sink rather than source during the termination? Can the proposed revised role of the ocean be rejected based on marine proxy data? As discussed already in our previous author comment (AC C945: 'In defense of the permafrost hypothesis', Roland Zech, 02 Nov 2010), we disagree with "The sign of the global ocean d13C change appears to be robust and this is the important fact here." (C1037) Taking at face value that "on global average the foraminiferal shells of the LGM had a 0.32‰ lower d13C than those from the Late Holocene (Duplessy et al. 1988)" one would, of course, come to the conclusion that "the total storage of organic carbon on land must have been 300-700 PgC smaller at the LGM than at pre-industrial time (Bird et al. 1996)" (C1036) and that "the newly proposed permafrost storage increase during glacials would necessarily have to be neutralized by a decrease of an organic carbon reservoir elsewhere (e.g. rest of the biosphere, continental margins)" (C1037). However, many factors may influence the d13C of foraminiferal shells. G. Munhoven, for example, acknowledge the carbonate ion effect ("If we applied the 0.32‰ correction suggested by Spero et al. (1997) to the whole ocean, the estimated glacial-interglacial average d13C would only reduce to zero." C1037). Moreover, we note again that the most recent compilation of d13C data does not provide a revised update of the whole ocean d13C change, because "we consider the coverage too incomplete to directly construct a time-series of δ13C inventories" (Oliver et al., 2010). Changes in ocean circulation and proposed changes in the marine biological pump are other examples of factors that need to be taken into consideration when interpreting d13C records. We agree with G. Munhoven that "neglecting these basic and well-established facts will inevitably lead to erroneous conclusions" (C1028), but disagree that "the authors fail to recognize [such effects]" (C1027), and suggest that, in the absence of other robust proxies, past changes in oceanic carbon storage on glacial-interglacial timescales remain insufficiently constrained. One should therefore remain open-minded to the possibility that, contrary to the currently accepted paradigm, the ocean might have acted as carbon source during glacials.

Last but not least, we would like to clarify that the permafrost hypothesis does not claim to explain the whole 100 ppm glacial-interglacial change of atmospheric CO2 with changing carbon storage in permafrost soils alone. Large parts of the proposed permafrost carbon changes can be compensated by changes in other carbon reservoirs (e.g. the biosphere) without weakening the arguments for the proposed important role of permafrost carbon dynamics for triggering changes in atmospheric CO2 and thus potentially controlling the pattern of the Pleistocene ice ages. The hypothesis explicitly does not question the fact that other feedbacks and mechanisms were also involved in changing atmospheric CO2. Yet, (1) the sensitivity of the permafrost carbon pool to external forcing (i.e. integrated annual insolation), and (2) the positive feedback of permafrost dynamics to global temperature perturbations make the permafrost hypothesis a compelling subject for further research. This will undoubtedly have to involve climate and carbon modeling studies in order to more precisely quantify changes in the various carbon pools, which would clearly be beyond the scope of the present manuscript.

Interactive comment on Clim. Past Discuss., 6, 2199, 2010.

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