

Interactive comment on “A permafrost glacial hypothesis to explain atmospheric CO₂ and the ice ages during the Pleistocene” by R. Zech et al.

Anonymous Referee #1

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This study presents a new paleo time series of changes in δD as a temperature proxy from a terrestrial remote site in Northeast Siberia over the last 220 kyr. This new record is shown together with already published total organic carbon (TOC) content of the same core. The authors extend the discussion of their records to an alternative hypothesis — the permafrost glacial hypothesis — *to explain atmospheric CO₂ and the ice ages during the Pleistocene*.

Major comments:

I think the underlying new terrestrial δD record is worth publication. However, the main part of the manuscript is about this new hypothesis. In its present form this hypothesis is premature and not publishable. This will be explained in details below. If the new

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hypothesis is taken at face value 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.

One main argument, why the authors think their hypothesis might be correct and timely is the still unexplained drop in atmospheric $\Delta^{14}C$ by 190 ‰ during the Mystery Interval of the last Termination. They argue, that the old carbon depleted in ^{14}C necessary to explain the reconstructed $\Delta^{14}C$ is still not found in the deep ocean, it might simply be that it is found on the terrestrial ground. But then they stop discussing this any further. I would at least expect some rough calculations on the effect of C bound in permafrost on atmospheric $\Delta^{14}C$ to see if and how this new hypothesis would improve our understanding of $\Delta^{14}C$.

This is one example, how the arguments brought up for their hypothesis are not fully exploring their potential. Others are the effect of their study on our knowledge on $\delta^{13}C$. The authors finally only discuss their estimated glacial carbon sink in permafrost against the ocean, 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.

What I believe this study is worth is that it can indeed bring our attention to the part of the carbon cycle which is underrepresented in the models. I believe carbon in permafrost might play a role, but if the authors want to push this idea they should support their estimates with as much data as possible. This would then not mean we have a new hypothesis explaining the whole variability in CO₂ or even glacial cycles, but it would add certain pieces to the puzzle of understanding the whole system. In saying supporting with as much data as possible, I suggest, that the authors should estimate realistically what their proposed amplitude of glacial C burial would imply for ^{13}C and ^{14}C . This would then not explain the rapid drop in $\Delta^{14}C$ in the mystery interval, but maybe a part of it. It would also NOT explain marine $\delta^{13}C$, but maybe highlight what part the ocean really has to explain (I think one of the earlier studies of the authors

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included also changes in $\delta^{13}\text{C}$ of TOC over the 220 kyr). In this “putting-the-pieces-together” exercise they should also not forget about the vegetation changes - or in change in terrestrial C storage elsewhere. My impression is that all changes in the terrestrial part summed up might lead to a neutral effect on CO_2 (but not on $\Delta^{14}\text{C}$), but maybe also the timing of changes in vegetation and soil carbon might help here.

Furthermore, the whole hypothesis is based on one single time series of changes in TOC over 220 kyr which is then extrapolated to the whole Siberian permafrost region. Can we be sure, this can be done? What about local effect in the core? I think for a rough back-of-the-envelope-calculation this is possible but I would feel a little uncertain about the uncertainty introduced by this up-scaling procedure. 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. There I had the feeling, that the effect of temperature on SOC can be followed up from the stated principles. An in my mind more robust extrapolation approach would use this kind of process understanding demonstrated in Zimov *et al.* (2009) to (a) reproduce the TOC variability in the Siberian site, for which these data were obtained by the authors by using their new δD temperature proxy as input data, (b) extending this exercise to the whole of Siberia, which has then to be fed by local temperature variability which might be obtained by Earth System models.

The final part of the discussion (section 3.7) in which they try to explain the Mid Pleistocene Transition is too brief to be convincing. It uses assumptions which can in my view not be defended, but see point 12 below for details. It furthermore tries to make some statements over a time window (last 1 Myr), which is simply not covered by the data set (220 kyr), which I think over-stresses what might be learnt from the TOC data.

In summary, I think a final judgement on the effect of C in peatland on atmospheric CO_2 is so far not possible. There might be something in it, but it needs at least a major revision (with another round of reviews) including some careful argumentation which

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should include all the following details to come to sound scientific reasoning. I do not think a revision can improve the final part concerning the *overarching permafrost hypothesis* and the editor has therefore to decide, if an improved draft would be considered as revision or new submission.

Details (in chronological order):

1. Title: This hypothesis — if improved and accepted — would then not *explain atmospheric CO_2* and also not the ice ages. This title is much too ambiguous for the content.

2. Abstract: The abstract is much too unspecific.

Over glacial cycles CO_2 is not closely coupled to global temperature, but to Antarctic temperature (there is no proxy of global temperature so far).

Which are the proxy evidences, which do not support oceanic hypothesis? From reading the whole text I can only imagine that atmospheric $\Delta^{14}\text{C}$ is meant here, but then mention it.

The sentence ‘*Here we present results from the first permafrost loess sequences ...*’ suggest, that these data are new and presented here for the first time, but this is only true for the isotopic temperature proxy, not for TOC, which was published before. So please state, what is new and origin here.

3. Introduction:

Ice cores go back 800 kyr, not 1 Myr.

Complete citation for 800 kyr CO_2 are Petit *et al.* (1999) (Vostok 0-400 kyr BP), Siegenthaler *et al.* (2005) (EPICA Dome C 400-650 kyr BP), and Lüthi *et al.* (2008) (EPICA Dome C 650-800 kyr BP).

Unit of ice core CO_2 is *parts per million and volume (ppmv)*.

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Interglacial CO₂ is 250-260 ppmv (interglacials before 400 kyr BP) and 280-300 ppmv (0-400 kyr BP).

Changes in the Southern Ocean ..., because CHANGES in up-welling and ventilation ... accumulates respired CARBON from sinking organic particles ...

4. There are various ideas and also modelling results out in the literature, which can explain quite a lot of the observed glacial-interglacial change in CO₂, thus in opposite of what is mentioned here. One box modelling approach is also cited by the authors (*Köhler et al.*, 2010; *Lourantou et al.*, 2010), but they chose not to mention it. Others were just recently published (see *Climate of the Past* (including *Discussions*) of this year). I think what needs to be done to estimate the part of the glacial-interglacial amplitudes in CO₂ which can be explained is to sum up for those processes, for which we have a good scientific understanding THAT they did happen and WHAT the models are saying for the amplitude. I think there is a common understanding on the solubility effect of a colder ocean and on sea level change and the models do not diverge a lot on the effects of these processes on CO₂. Similar things applies to the terrestrial carbon storage in other reservoirs than the permafrost. 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 and also for the magnitudes in CO₂ they might explain. But see the excellent review of *Kohlfeld and Ridgwell* (2010) for details. In my understanding the only way to support another theory is, if this new theory would help to explain not only CO₂, but also other reconstructions, such as $\Delta^{14}\text{C}$ or $\delta^{13}\text{C}$.
5. The potential unexplained drop in $\Delta^{14}\text{C}$ misses one main recent publication (*Skinner et al.*, 2010), in which it was concluded based on marine data evidence that about 50% of the drop in atmospheric $\Delta^{14}\text{C}$ can be explained with old carbon in 30% of the deep ocean. This would give a constrain on what might be C900

explained from terrestrial sources and should be taken up and explored throughout the MS with some own estimates from the permafrost.

6. Material and Methods: I think the whole supplemental material should be included in the main text. Fig S1 and S2 should extend the methods and explain in detail the age model, they might be easily incorporated in the existing Fig 1.
7. Section 3.1:
It is acceptable to not correct for the ice volume / sea level effect on δD , but it would be helpful to give here a rough number by how much during glacial peak times one has to correct the signal.
8. I think the main result sections 3.4–3.7 need heavy revision, please see some suggestion below.
9. Section 3.4:
The up-scaling depends on the own results of a change in TOC of 1% and the change in the permafrost area, which is estimate to be $10 \times 10^6 \text{ km}^2$. This change in Siberian permafrost area needs to supported with more evidences. Is this only based on the shift in the -5°C isotherm towards -15°C under the assumption, that it is getting 10°C colder during LGM? If so, please support the temperature amplitude with other evidences, what is PMIP2 saying for example (*Braconnot et al.*, 2007)?

Furthermore, other studies (*Tarnocai et al.*, 2009) report also on large areas (about 30%) of peatland and permafrost in North America. Most if not all of these North American area would have been ice covered at LGM. Thus, 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 hypothesis by a factor of 2. This

story is even getting more complicated if one considers what happens to terrestrial carbon during glaciation (is it buried underneath the ice sheets, and if so, when will this be released?) (Zeng, 2007).

A release of 300 PgC during a Termination is NOT equivalent to a change in CO₂ by 150 ppmv, because of the carbon uptake by the ocean. This is later on (section 3.5) correctly mentioned (airborne fraction of 10%), but it should be mentioned at the first place here.

The authors also mention, that other studies estimate that permafrost might release 1000 PgC during Terminations and the reader is lost, which number to believe in. As mentioned earlier a more robust up-scaling approach combining both studies might be needed and would come to a new number here.

This section 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. Knowledge 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.

10. Section 3.5

The discussion in this section on change in marine $\delta^{13}\text{C}$ is very weak and confusing. The bulk global effect of changing mean oceanic $\delta^{13}\text{C}$ is seen in the deep Pacific, which contains the major part of the ocean waters. Understanding reveal in $\delta^{13}\text{C}$ in the upper 2000 m might certainly need an in-depth understanding of marine dynamics and their effects in $\delta^{13}\text{C}$, not only some rough arguments as brought up here. The recent $\delta^{13}\text{C}$ data compilation published last week (Oliver *et al.*, 2010) should be investigated in detail to scanned the present state of the art on that topic.

The arguments brought forward on the similarity in $\delta^{13}\text{CO}_2$ at LGM and Holocene in ice cores is totally confusing. The 2 studies mentioned there (Köhler *et al.*,

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2010; Lourantou *et al.*, 2010) are able to explain the observed $\delta^{13}\text{CO}_2$ ice core data without an additional permafrost carbon contribution, thus I can not see who they can support this discussion here.

If the authors think, they can explain data evidence from $\delta^{13}\text{C}$ by their data, they should at least make the calculation to convince the reader, but not only state that they can explain the signals.

11. Section 3.6

There is no major drop in atmospheric CO₂ at 190 kyr BP!

The authors argue, that summer insolation might be important for methane release from wetlands, but integrated annual insolation might be the key parameter for changes in the permafrost extend. This assumption is fair enough, but the resulting time series of changes in integrated annual insolation is then an important part in the chain of arguments and needs to be shown in the main text, not hidden in Fig S3 in the supplements. Some more arguments WHY this is the key trigger would furthermore help here (e.g some heat budget calculations, how much energy is needed to thaw permafrost and how much energy is provided by insolation). I could alternatively imagine, that the integrated amount of energy during positive degree days would be a key trigger, but maybe this is correlated to the integrated annual insolation.

12. Section 3.7

The section covers an effort to extend the observed time series back in time to explain shifts in glacial cycles during the Mid Pleistocene Transition. While I in principle think such hypothesis can and should be brought up in discussions, I also think in the way presented here the hypothesis is much too weak and not convincing for the following reasons (again all evidence should be brought in the main text, not in the supplement):

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- (a) The time series of TOC does not extend beyond 220 kyr it is therefore only speculation how this would change over time.
- (b) The idea is that if permafrost is extended towards 45° N then the annual integrated insolation (assumed to be responsible for permafrost thawing) is NOT following obliquity anymore, but is stronger controlled by the 100 kyr cycle of eccentricity. Okay, that is true locally for 45° N, but the further one goes north the more it is controlled by obliquity. 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.
- (c) The hypothesis is based on the idea, that changes in CO₂ drive changes in climate. The chain of argument is therefore: change in C in permafrost dominates change in atmospheric CO₂ which then triggers changes in climate. However, this would heavily rest on the assumption that CO₂ drives temperature. During glacial-interglacial timescales this so far does not seem to be the case. All lead/lag analysis between CO₂ and temperature in ice cores point in the other direction, that first temperature changes and the carbon cycle and CO₂ react as a feedback on that initial change (*Fischer et al.*, 1999; *Monnin et al.*, 2001; *Caillon et al.*, 2003; *Loulergue et al.*, 2007).

13. Conclusions

The initial drop in CO₂ in the early Holocene has also other explanation which also explain ice core atmospheric $\delta^{13}\text{CO}_2$ dynamics, which are therefore more convincing (*Elsig et al.*, 2009). But the study of *MacDonald et al.* (2006) cited here (establishment of peatlands over last 20 kyr) would be an excellent data set, which might be used during a revised extrapolation of the TOC content.

14. References: A lot of references name *et al.* for long author lists. This is not the reference style of the selected journal, all authors should be mentioned.

In Zimov and Schuur the name of the third author (Chapin III) is missing.

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15. Figures:

Fig 1 should say, that TOC was taken from another publication.

In Fig 2 the units are missing for nearly all y-axes labels.

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