

## ***Interactive comment on “A permafrost glacial hypothesis to explain atmospheric CO<sub>2</sub> and the ice ages during the Pleistocene” by R. Zech et al.***

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### **1 General comments**

R. Zech and co-authors present here a new  $\delta D$  record for the past 220 kyr that they obtained from a loess-paleosol sequence from the Tumara Valley in Northeast Siberia. They combine this  $\delta D$  with a TOC record from the same paleosol sequence already published before (Zech et al., 2007, 2008). The new  $\delta D$  is then used to confirm the previous interpretation of the observed sequence of organic carbon rich and organic carbon poor units as the succession of glacial and interglacial deposits. The interpretation of the new  $\delta D$  record as a temperature record is then discussed, and by correlation, the link between high TOC contents and  $\delta D$  values reflective of cold periods

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established. The authors then shift focus on the TOC record and explain that the TOC record holds information about permafrost carbon dynamics. They suggest that carbon storage is increased during glacials and reduced during interglacials/interstadials. An up-scaling of their results to the global scale leads them then to advance their *permafrost glacial hypothesis* announced in the title. The rest of the paper is devoted to first trying to convince the reader that the oceans do not play as an important role in controlling atmospheric  $pCO_2$  on glacial-interglacial time scales as currently accepted. The discussion then continues with more speculative ideas on how permafrost dynamics and orbital forcing act together to generate glacial terminations every 80 to 120 kyr only.

The  $\delta D$  and TOC data are archived in the supplementary material provided, together with additional information on the age model.

The English language is used in a very fluent way. The text is, however, often vague, inaccurate and imprecise; adopted figures are too often only approximative, not all of them are correct and they are generally given without reference (see specific comments below).

The paper without any doubt falls within the scope of *Climate of the Past*. The  $\delta D$  record is original and the authors intelligently use it to improve the confidence in the dating of their TOC record, which is not straightforward to do with classical methods. I would still have expected to read more about the obvious discrepancies between the ages measured and the ages in the age model (the sample dated at 150 kyr ranges at about 200 kyr in the age model; the 50 kyr datum moves to 70 kyr—these are large changes in my opinion), but it is possibly the best that can be done. The study contributes to improve our understanding of the glacial-interglacial changes in the terrestrial carbon reservoirs. Permafrost soil reservoirs are not yet taken into account in global vegetation models as their importance has only recently been convincingly established (Tarnocai et al., 2009). This study provides additional evidence for the *potentially* large changes

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in carbon storage in permafrost soils between glacial and interglacial times, previously suggested by Zimov et al. (2009). The original results presented here would deserve to be discussed more in detail. The paper, e.g., remains silent about possible shortcomings in the interpretation of the TOC record in the framework set up in the manuscript. Whereas the jumps in the TOC record at the boundaries between MIS7 and MIS6, MIS6 and MIS5e, and MIS5 and MIS4 boundaries are extremely well-pronounced, the transition across the MIS2-MIS1 boundary is almost continuous. Termination I is thus completely atypical. There is more of a continuous decrease throughout all stages from MIS4 into MIS1, which can difficultly lead to the observed sharp deglacial CO<sub>2</sub> rise. The discussion could furthermore be more quantitative (e.g., an order of magnitude estimate for possible corrections related to evaporation would be useful). Yet, this first part of the study is interesting to read and very instructive.

Once we proceed to the quantitative upscaling (subsection 3.4) of the predicted increase of the carbon storage in permafrost soils during glacials, the discussion becomes more and more incomprehensible. The authors almost immediately dismiss their own estimate of 300 PgC for that increase—an original contribution of the study—as it “most likely underestimates the real effects” (p. 2208, l. 5) without even attempting to quantify a confidence range of that figure, to embrace the 1000 PgC estimate of Zimov et al. (2009). This latter estimate is not critically discussed either. The following discussion almost exclusively relies on this 1000 PgC estimate and readers therefore need to be informed about the significance and reliability of that figure. It derives from a model: what are the basic hypotheses of that model? what are the data that support it? how was it calibrated?

Both estimates are converted in terms of their potential effects on atmospheric CO<sub>2</sub> (increases by ~150 ppmv and ~500 ppmv, resp.). Although arithmetically correct these effects do not have any significant meaning: the at first impressively looking increases in atmospheric pCO<sub>2</sub> are going to be reduced by a factor of ten as a result of ocean uptake; the actual maximum increase will depend on how fast that carbon would be

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released. This leads me to two more important questions not addressed in the paper:

1. What would be the time scales of disintegration of the permafrost stock during a deglaciation? of the build-up during glaciation? In the conclusion section, a 5 ka duration for the 1000 PgC release during deglaciation is mentioned within brackets, without any justification.
2. Does the whole organic carbon stored in the permafrost soil necessarily go to the atmosphere at the end of a glaciation? Another possibility that cannot be excluded is that one part of it gets transported in particulate form by rivers to the coastal zone, where it could possibly be buried without further interaction with the atmospheric carbon reservoir.

The following discussion on the role of the uptake of the CO<sub>2</sub> released from permafrost soils during deglaciation (subsection 3.5, “A revised role for the ocean”) does, unfortunately, not stand any critical analysis. The text aims at providing a quantitative comparison of the respective roles of oceanic and the permafrost storage change. The oceanic role is estimated after the net response of the permafrost carbon release is deduced from the total observed glacial-interglacial change. The calculations consistently omit the role of glacial-interglacial changes in the terrestrial vegetation and soil reservoirs outside the permafrost regions which will neutralise large parts if not all of the effect coming from the storage changes proposed here. The argumentation concentrates almost exclusively on the marine  $\delta^{13}\text{C}$  for the purpose of the discussion. Unfortunately, the authors fail to recognise

- that the surface-to-deep-sea gradient of the seawater  $\delta^{13}\text{C}$  is internally controlled in the ocean,
- that transfers of carbon between the ocean-atmosphere and terrestrial organic reservoirs can only change the global mean  $\delta^{13}\text{C}$  in the ocean,

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- and that there is a strong link between surface ocean and atmospheric  $\delta^{13}\text{C}$  on time scales of tens to hundreds of years.

Neglecting these basic and well-established facts will inevitably lead to erroneous conclusions.

After all the misinterpretations and omissions are corrected (see detailed comments below), it turns out that, contrary to the claim that the proposed “scenario notably contradicts the current notion of the role of the oceans in controlling atmospheric  $\text{CO}_2$  on glacial-interglacial time scales ...” (p. 2209, ll. 7–8), the argumentation completely fails in establishing this.

The fatal flaw derives from the omission of the terrestrial biospheric changes outside the permafrost regions. This does not mean that the permafrost storage changes can be neglected in the global picture. They must be taken into account but their role should be stated at fair value.

Unfortunately subsection 3.5 is paramount in establishing the permafrost carbon storage changes as the single-most important mechanism controlling atmospheric  $p\text{CO}_2$  on glacial-interglacial time-scales and to set it up as *the* hypothesis.

Because of these flaws and shortcomings the proposed “hypothesis to explain atmospheric  $\text{CO}_2$  and the ice ages during the Pleistocene” announced in the title is not tenable. Accordingly, I do not see how this paper could be published in *Climate of the Past* unless it undergoes a *major revision*.

The presentation of the new  $\delta\text{D}$  data requires no or only minor changes. The authors could actually decide to limit the revised paper to that part and include only some short, realistic, careful and fair outlook type of discussion on the potential role of permafrost storage changes on atmospheric  $p\text{CO}_2$ , that must be rooted in current knowledge. If they prefer to maintain a more substantial part related to carbon cycling on

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glacial-interglacial time scales, then the role of the permafrost storage change in driving atmospheric  $p\text{CO}_2$  needs to be correctly put into the global context, considering the complete framework of relevant reservoir changes. A decent review of existing literature on that subject must be provided. The marine  $\delta^{13}\text{C}$  must be discussed in a fair and complete way.

What I deeply miss in the problematic discussion part of the paper (mainly subsection 3.5) is a constructive attitude: to build upon the existing knowledge, which should be accurately summarized and assessed in order to identify existing shortcomings, which need to be clearly discussed, in order to propose acceptable adaptations and extensions.

This is truly unfortunate.

The authors apparently do not realise that they may hold in their hands an important piece that could help to reconcile several independent and currently contradicting facts regarding glacial-interglacial environmental changes (references and more details are given in the specific comments below): (1) terrestrial vegetation mapping based on palynological or sedimentological data indicate that the carbon stock in terrestrial soil and vegetation increased by 750–1900 PgC during the deglaciation, with permafrost generally not considered; (2) vegetation model results provide estimates of 600–1100 PgC for this increase, also lacking explicit representations of the peculiar permafrost soil dynamics; (3) the marine carbon isotopic data suggest a lower *net* transfer of only 300–700 PgC of organic carbon from the combined ocean+atmosphere to the land reservoirs (vegetation, soil, permafrost, ...), and, if corrected for a possible fractionation related to changing carbonate ion concentrations in the ocean between glacial and interglacial times, these estimates may at worst reduce to zero.

Why not explore how the emerging experimental evidence for permafrost storage changes data can help to reconcile these contradicting evidences, instead of trying

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to demonstrate by all means that the oceans can only play a secondary role during the deglaciation, which requires to bend a number of fundamental and well-understood properties of carbon cycling between the atmosphere and the ocean?

The permafrost storage estimates presented by the authors and by Zimov et al. (2009) can be combined with the estimates for the land biosphere uptake during the deglaciation based upon the data and the model simulation experiments and the new estimates presented here to one consistent picture and thus contributing to lift a long-standing disagreement between marine data based and terrestrial data and vegetation modeling based pieces of evidence related to glacial-interglacial carbon cycling. This would really represent a major step forward in improving our understanding of glacial-interglacial carbon cycle changes.

I am looking forward to reading the revised paper.

## 2 Specific comments

Page 2199, Title: Presenting this paper as “A permafrost glacial hypothesis to explain atmospheric CO<sub>2</sub> and the ice ages during the Pleistocene” does not reflect the main contribution of this paper. Please change.

Page 2200, lines 2–15 (Abstract): This is not an abstract, but rather an *Introductory paragraph* as in *Nature* papers. For *Climate of the Past*, a quantitatively informative summary of the paper would be more appropriate. It is striking that the most original contribution of the study, the new  $\delta D$  record, is not even mentioned here.

This paragraph describes the sequence as “spanning two glacial cycles (~240 ka).” As far as I can see, the bottom of the sequence is dated at 220 ka BP. It would thus only be correct to say “spanning 220 ka (almost two glacial cycles).”

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Page 2200, line 18: The currently available CO<sub>2</sub> records span altogether 800,000 years, not “~1 Ma.”

Page 2200, line 19: Shackleton (2000) does not present any CO<sub>2</sub> measurement results and should be discarded. It would certainly be more appropriate to include Siegenthaler et al. (2005).

Page 2200, lines 21–23: it is not only “The large size of the carbon pool in the ocean. . .” that makes it the most probable candidate for the control of atmospheric  $p\text{CO}_2$  over time scales of several hundreds to thousands of years: the sediments and crust with its tens of millions of PgC in carbonates and organic carbon would then be a much better candidate! It is the combination of size and exchange (and buffering) capacity that are the key reasons for the ocean’s being in control of atmospheric CO<sub>2</sub> concentration on time scales of several tens to thousands of years. The two papers cited (Broecker, 1982; Sigman et al., 2010) furthermore add that the terrestrial biosphere contracted when entering the glaciation, and that the oceans therefore remain the only possible candidate. Sigman et al. (2010) only refer to Sigman and Boyle (2000) for that argument. It would therefore be more adequate to cite this latter paper instead of the former.

Page 2200, line 22: “~40 times” should read “~60 times”: there were 590 PgC in the atmosphere and 38,000 PgC in the ocean at pre-industrial times (Sarmiento and Gruber, 2002). Notice that if we would compare the glacial values, we might even expect to get close to 90.

Page 2201, lines 10–14: This statement is rather unfortunate and I would urge the authors to rethink about it. First of all, it is not justified by any references to the literature. Second, it does not reflect reality. No serious study considers terrestrial carbon pools

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to be negligible for the understanding of glacial-interglacial carbon cycle changes. Let us have a quick look at what commonly cited review and other comprehensive papers on the subject say on that subject:

- Broecker and Peng (1993) calculate that the glacial-interglacial change in the terrestrial forest and soil reservoirs accepted at that time would have led to a 47 ppmv higher atmospheric CO<sub>2</sub> content in the glacial atmosphere than in the interglacial one, if a simple equilibrium is assumed; the difference reduces to about 25 ppmv;
- Sigman and Boyle (2000) summarize the knowledge of the late 1990's about glacial-interglacial changes in the carbon storage on land and estimate that distributing 500 PgC (to mimic the reduction of the terrestrial biosphere at the LGM) between the ocean and the atmosphere would lead to a 45 ppmv increase at the LGM, which reduces to about 15 ppmv after carbonate compensation completes (on the time scale of 5–10 kyr);
- Archer et al. (2000) find that a 500 PgC decrease in the continental biospheric C content leads to a 40 ppmv change in atmospheric CO<sub>2</sub>, of which 17 ppmv remain after carbonate compensation;

Köhler et al. (2005) evaluate the effect of the regrowth of the terrestrial biosphere on atmospheric CO<sub>2</sub> at about 20 ppmv (after carbonate compensation has completed). These figures are by no means negligible.

The fact that the terrestrial carbon pools have so far been considered to "... act as sources rather than sinks during glacials..." was based upon the available *data* and *modelling results*. It would be correct to state this and provide relevant figures and references (see below for a broad selection).

Page 2201, line 16–19: The comparison with the atmospheric reservoir is potentially  
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misleading. 1670 PgC would indeed represent nearly 790 ppmv of CO<sub>2</sub> in the atmosphere, i.e., almost three times the pre-industrial content.<sup>1</sup> However, if we would release 1670 PgC of CO<sub>2</sub> into the atmosphere, there would not be any persistent increase of the atmospheric CO<sub>2</sub> concentration by 790 ppmv. Because of the buffering capacity of the ocean about 84% of that amount will be absorbed by the oceans (on time scales of several hundreds to a few thousands of years), leaving only about 125 ppmv in the atmosphere. As a result of such a large CO<sub>2</sub> uptake by the ocean, the carbonate compensation mechanism gets strongly perturbed. After carbonate compensation will have readjusted (over time scales of several thousand to ten thousand years) another one third to one half of those 125 ppmv will have been absorbed by the oceans, leaving only the equivalent of 60–85 ppmv in the atmosphere, i.e., only 7–11% of the initially possible 790 ppmv. These are rough estimates only, based upon a global average Revelle buffer factor of 12 (Sarmiento and Gruber, 2006). The important thing to notice here are the time scales over which these processes act: these are exactly the time scales of the deglacial CO<sub>2</sub> rise and must therefore be taken into account right away if the net effect of an external perturbation, such as the release resulting from the oxidation of permafrost soil carbon is to be evaluated realistically.

Page 2203, lines 20ff: How realistic is this? What factors could possibly influence metabolic fractionation of D against H? How strong can the effect of soil water evaporation typically be in this type of environment?

Page 2205, lines 3–7: Please provide at least an order of magnitude for this correction, even if you would not apply it to your data. The reasons put forward for not applying it are perfectly understandable.

Page 2208, lines 1–2: "... this excess carbon storage would be ~300 Pg, equivalent to ~150 ppm atmospheric CO<sub>2</sub> and thus easily exceeding the observed glacial-

<sup>1</sup>There are 2.12 PgC per ppmv of CO<sub>2</sub> in the atmosphere (following Sundquist, 1985).

interglacial difference.” It must be stated here right away that a  $\sim 300$  PgC uptake of  $\text{CO}_2$  from the atmosphere will *not* lead to a 150 ppmv decrease as the oceans will restore more than 90% of that removal on the time scales of interest. Following the same rationale as above, equilibration of the atmosphere and the ocean restores about 250 PgC to the atmosphere; after carbonate compensation completes, the atmosphere will only have lost 25 to 30 PgC, which corresponds to a  $p\text{CO}_2$  reduction by 12–14 ppmv only. The final net effect is thus only about 10% of the claimed magnitude. Delaying that discussion would be acceptable if only minor corrections or uncertainties would have to be discussed, but not if the figure needs to be revised by a factor of ten!

Page 2209, line 1: “. . . roughly another 50 ppm would be a simple amplification effect due to a warming ocean . . .”: where does this 50 ppmv figure come from? This is totally unjustified! Most of the published estimates for the warming effect fall in the range of 17–18 ppmv (e.g., Broecker and Peng (1993): 18 ppmv; Köhler et al. (2005): 17 ppmv), more than half of which is neutralised by global ocean salinity changes due to sea-level rise resulting from the freshwater input from the melting ice-sheets (Broecker and Peng (1993): 11 ppmv; Köhler et al. (2005): 6 ppmv) leaving about 7–11 ppmv only! Sigman and Boyle (2000) use a more rough method and obtain a 30 ppmv increase for the temperature effect alone.

It should furthermore be noticed that the 10% fraction from Archer et al. (2004) used here already considers the temperature feedback. It is therefore inconsistent to call upon warming to accommodate the remaining 50 ppmv.

Page 2209, line 6: the paper by de Boer et al. (2010) does not even mention permafrost and includes only a short remark on the potential impact that changes in terrestrial carbon reservoirs could have. It is not clear what point the authors want to make here and why that paper is their first choice reference.

Page 2209, lines 12–13, 15, 27: The style of this part of the text is rather peculiar.  
C1034

Why is “independent” set between quotes? Why should we believe that the isotopic constraint of global carbon pool changes is “apparent”? Why should we more robustly establish the “total” glacial  $\delta^{13}\text{C}$  changes” instead of the “total glacial  $\delta^{13}\text{C}$  changes”? Readers might find such stylistic elements offensive as they could possibly see them as mockery. Please be careful!

The review that follows is brief indeed, but very narrowly focused. I also do not find it critical but rather one-sided and selective.

Page 2209, lines 14–27: The reasoning behind the developments in this paragraph is not entirely comprehensible.

The marine  $\delta^{13}\text{C}$  cannot tell us anything about permafrost dynamics alone, but only about all of the terrestrial organic carbon stocks together. Any possible gross underestimation of the role of permafrost-related carbon dynamic can only have been due to the lack of data, which became available only recently. It is certainly worth reminding that Adams and Faure (1998) already wrote that “[. . .] the hypothesis of a major store of organic carbon underneath the world’s ice sheets (or frozen into permafrost in the periglacial zones) at the LGM remains tentative. The idea would merit much further study.”

Let us then analyse the discussion presented. First of all, the paper by Duplessy et al. (1984) is about interglacials only; a far more relevant reference is Duplessy et al. (1988). It is correct that the deep ocean had a lower  $\delta^{13}\text{C}$  during glacials. The isotopic signature recorded in benthic foraminifera is 0.46‰ lower for glacial age than for Holocene specimens (Curry et al., 1988). It is also correct that foraminiferal tests representative of the upper 2–2.6 km of the ocean had a greater  $\delta^{13}\text{C}$  during glacials than at the Holocene (Duplessy et al., 1988; Matsumoto et al., 2002; Curry and Oppo, 2005). The authors omit, however, to precise that, on global average the foraminiferal shells of the LGM had a 0.32‰ lower  $\delta^{13}\text{C}$  than those from the Late Holocene (Duplessy et al.,

1988).

If we may assume that the documented changes actually reflect the evolution of the  $\delta^{13}\text{C}$  of dissolved inorganic carbon, the global  $^{13}\text{C}$  mass balance of the ocean requires a carbon source with a low  $\delta^{13}\text{C}$  during glaciation (e.g., carbon coming from organic carbon oxidation from the terrestrial or shelf reservoirs) or a C sink with a high  $\delta^{13}\text{C}$  (none known). Bird et al. (1996) solve the complete carbon-isotope mass balance equations and find that the total storage of organic carbon on land must have been 300–700 PgC smaller at the LGM than at pre-industrial time.

The paper fails to mention that there is evidence that is completely independent of the marine isotopic record and showing that the terrestrial biospheric carbon stock was significantly reduced during glacial times. Conservative estimates based upon terrestrial data (excluding permafrost storage) indicate that the terrestrial carbon reservoir (including soils, except for permafrost) was 750–1050 PgC lower at the Last Glacial Maximum (LGM) than during pre-industrial times (Crowley, 1995). (Adams and Faure, 1998) find a range from 900 to 1900 PgC, with a preferred value of 1700 GtC. Most of the estimates derived from vegetation models (still excluding permafrost reservoirs) generally range between about 600 and 850 PgC (see, e.g., François et al., 1998; Kaplan et al., 2002; Joos et al., 2004), but may be as large as 830–1110 PgC (Otto et al., 2002). This carbon removal from the atmosphere can thus easily counterbalance the effect of the permafrost storage release, leaving the ocean again in charge of most of the net response (ca. 80–95% as my own estimates show). The situation could even get worse if we were able to correctly estimate the  $\text{CO}_2$  release resulting from the oxidation of shelf organic matter during glacial sea-level low-stand, which is completely unconstrained at present.

Notwithstanding the omission of some terrestrial reservoirs in the discussion, the removal of  $^{13}\text{C}$  depleted carbon from the atmosphere has no impact on the surface-to-deep-sea gradient. A stronger surface-to-deep-sea gradient such as the observed one

C1036

can only be maintained by a sustained more intense vertically dominated internal cycling associated with a process that fractionates  $^{13}\text{C}/^{12}\text{C}$ . A diffuse uptake of carbon depleted in  $^{13}\text{C}$  by permafrost soils during glacials cannot increase  $\delta^{13}\text{C}$  in the surface 2 km of the ocean only. Such a perturbation will spread throughout the whole ocean.

The interpretation of the foraminiferal  $\delta^{13}\text{C}$  record in terms of seawater  $\delta^{13}\text{C}$  variations is, however, not as straightforward as assumed above. Culture experiments have shown that the  $^{13}\text{C}$  uptake by *planktonic* foraminifera is subject to fractionation depending on the ambient carbonate ion concentration (Spero et al., 1997). These authors argue that, if this fractionation effect is taken into account, the terrestrial carbon storage change between glacial and interglacial times could possibly be reduced to zero. However, this possibility arises from strong hypotheses: the required correction for carbonate ion induced fractionation must extend to deep sea, which is not established and possibly not correct (as mentioned already by Spero et al. (1997) themselves). Large carbonate ion concentration changes in the deep-sea are in contradiction with the sedimentary record of  $\% \text{CaCO}_3$  (Catubig et al., 1998). Finally, even if we applied the 0.3‰ correction suggested by Spero et al. (1997) to the whole ocean, the estimated glacial-interglacial average  $\delta^{13}\text{C}$  change would only reduce to zero.

The sign of the global ocean  $\delta^{13}\text{C}$  change appears to be robust and this is the important fact here. The amplitude may be reduced, if it turns out that a similar  $\delta^{13}\text{C}$ -[ $\text{CO}_3^{2-}$ ] fractionation effect exists for benthic than for planktonic foraminifera.

In case the global average  $\delta^{13}\text{C}$  would have remained constant between glacials and interglacial, the terrestrial storage change would simply not have contributed to the marine  $^{13}\text{C}$  budget. This would mean that the newly proposed permafrost storage increase during glacials would necessarily have to be neutralised by a decrease of an organic carbon reservoir elsewhere (e.g., rest of the biosphere, continental margins).

This could actually fit here.

C1037

Page 2209, line 28–page 2210, line 6: The authors correctly assert, but once more without a reference, that due to ocean temperature and salinity change, isotopic fractionation change would lead to 0.5‰ lower  $\delta^{13}\text{C}$  during glacials. They conclude that, since atmospheric  $\delta^{13}\text{C}$  showed little change, terrestrial carbon contributions must have offset this effect. This conclusion is not justified. Since the surface ocean had a higher  $\delta^{13}\text{C}$  during glacial than at pre-industrial times, the reduced fractionation simply contributed to stabilise the  $\delta^{13}\text{C}$  of the atmospheric  $\text{CO}_2$ . On time scales of several hundreds to a few thousand years, transfers of isotopically light carbon between the terrestrial biosphere and the ocean/atmosphere impinge on the *global average*  $\delta^{13}\text{C}$ ; the *partitioning* of  $^{13}\text{C}$  between the atmosphere, the surface and the deep ocean is controlled by oceanic processes and the air sea-exchange alone.

Page 2210, lines 7–14: A priori, the source could be terrestrial. There are, however, several pieces of evidence speaking against this possibility (Spero and Lea, 2002). The terrestrial biosphere was already expanding at the time of the spikes, thus contributing to make atmospheric  $p\text{CO}_2$  increase (and counterbalance the effect called upon by the authors, as discussed above). If the negative  $\delta^{13}\text{C}$  spikes would arise from the permafrost release, they should at least be recorded in the surface North Atlantic, where the uptake of  $\text{CO}_2$  from the atmosphere is strong. There are, however, no such minima in the North Atlantic planktonic records (Ninnemann and Charles, 1997).

Page 2210, lines 15–20: this “most important” argument is rather weak: absence of evidence is not evidence of absence.

Page 2210, line 27: whereas the  $p\text{CO}_2$  increases at  $\sim 220$  ka and 130 ka indeed correlate with sharp falls in %TOC, this is not the case for 15 ka, as can be seen on Fig. 3. Furthermore,  $\text{CO}_2$  remains comparatively stable during MIS6, whereas %TOC present strong variation.

C1038

Page 2211, line 17: “ $\sim 40$  ka” should read “41 ka”

Page 2211, line 19: “ $\sim 40$  ka” should again read “41 ka”, although the commonly used denomination is the “41 kyr world”.

Page 2211, line 17: Taking Fig. 1 as evidence for an expansion of permafrost regions as far south as  $45^{\text{circ}}\text{N}$  overstretches its significance. The delimitation of permafrost regions on Fig. 1 is based upon a hypothetical, uniform  $10^{\text{circ}}\text{C}$  temperature decrease. Reality is certainly far more complex than this.

Page 2212, Conclusions. I recommend a complete rewrite of the first paragraph of the conclusions in a far more nuanced way. There are problems in each sentence. I do fully support the authors plea for carbon-climate models to incorporate permafrost dynamics to improve their predictive skills, especially for the future, but also for glacial-interglacial times. However, to do this correctly the uncertainties in the estimates of that carbon sub-reservoir must be adequately evaluated. This could to some extent well be done in this paper.

Page 2212, lines 13–15: Almost all of the discussion in this paper exclusively relies on the 1000 PgC estimate for the permafrost storage change of Zimov et al. (2009). The estimate of 300 PgC for the permafrost carbon change obtained as an original contribution in the paper is much more conservative. The statement that “Our study highlights that the high-latitude carbon pools have been hugely underestimated in terms of their size and particularly their temporal dynamics [...]” is not entirely justified. The paper does not deal with the *size* of the permafrost reservoir and can thus not highlight its importance. Regarding the dynamics, could we not just as well interpret the estimate provided here as a revision downwards of the previous figure of a variation by 1000 PgC and more of Zimov et al. (2009)?<sup>2</sup> Unless some uncertainty range is tied to

<sup>2</sup>... and, pushing this reasoning to the brink, we would even be forced to conclude that the role of the high-

C1039



both figures, this argument is very weak and the conclusion not justified.

Page 2212, lines 15–16: “Glacial-interglacial changes in terrestrial carbon storage far exceeded the observed changes in atmospheric CO<sub>2</sub>.” This can only be true if

- the high estimate for the permafrost storage increase during the glaciation of 1000 PgC is used
- and if the carbon release by the rest of the terrestrial biosphere is less than 800 PgC.

This latter point is far from established, as detailed above. Needless to say that, if the low estimate of 300 PgC obtained here by the authors was used, the statement is not correct.

Page 2212, lines 15–16: “Some ocean proxies might have to be re-evaluated in view of these findings.” First of all, it would be necessary to explain what is meant by “re-evaluated”! I am rather convinced of the contrary: the permafrost storage change helps to confirm the interpretation of the marine proxies and reconcile them with the other pieces of evidence for land carbon storage changes outside of permafrost regions.

Page 2212, lines 16–18: This sentence needs to be revised in the light of the corrections required to the study. The ocean remain in control of how much CO<sub>2</sub> the atmosphere may keep after a perturbation. That is an unavoidable result of carbonate chemistry in the ocean. In this case, the net terrestrial release during deglaciation (permafrost release minus biospheric uptake) is simply too weak to take over control.

latitude carbon pools have been hugely *overestimated* in terms of their glacial-interglacial impact, possibly by a factor of three. I do, however, not think that such a conclusion would be fair. Those numbers are simply still hugely uncertain.

C1040

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. Of these, only about 15 to 40 PgC would remain in the atmosphere after several thousands of years. We fall short of about 160 to 185 PgC (or roughly 80 to >90% of the ~200 PgC increase), which can now only be provided by . . . the ocean, as there is no other reservoir left that can provide such a large amount of CO<sub>2</sub> in a lapse time that is consistent with the data.

Page 2218: how well established (realistic) is the assumed 10°C reduction ?

### 3 Technical corrections

Throughout the manuscript: “Luthi et al.” should read “Lüthi et al.”

Page 2201, line 7: “Boer et al. (2010)” should read “de Boer et al (2010)”. Warning: this is a citation to a non peer-reviewed paper, which better had to be avoided. Are there no alternatives for backing this point?

Page 2201, line 16: please precise “1670 PgC”

Page 2204, lines 15: correct “employing the fact”

Page 2211, lines 27–28: strange hyphenation of “obliquity”

Page 2219, figure annotation: “makrofossils” should read “macrofossils”

Page 2219, figure annotation: should “MIS5” not better read “MIS5a-c”?

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Page 2220, figure annotation: "Oliquity" should read "Obliquity"

Page 2220, figure annotation: "Vostoc" should read "Vostok"

Throughout the bibliography: please provide complete author lists

Page 2214, line 24: "Cape" should read "CAPE"

Page 2214, line 29: " $\delta^{13}\text{C}$  of  $\Sigma\text{CO}_2$ " should read " $\delta^{13}\text{C}$  of  $\Sigma\text{CO}_2$ "

Page 2215, lines 24–25: title should read "Atmospheric  $\delta^{13}\text{C}$  and its relation to  $p\text{CO}_2$  and deep ocean  $\delta^{13}\text{C}$  during the late Pleistocene"

Page 2215, line 28: " $d^{18}\text{O}$ " records should read " $\delta^{18}\text{O}$ "

Page 2216, line 8: the correct author name is "North Greenland Ice Core Project members"

Page 2216, line 16: "DeltaD" should read " $\delta\text{D}$ "

Page 2216, line 17: "implications" should be capitalised

Page 2217, lines 25–26: the DOI of this paper has already been attributed (10.1016/j.quaint.2010.04.016); please include it as will remain valid after the paper comes out of press; discard "corrected proof"

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Page 2217, line 30: missing last author "Stuart Chapin III, F."

Supplementary material: The .doc file was, unfortunately, not correctly readable with my word processor: the page formatting was messed up and some characters unreadable. Please use the PDF format, as required by the instructions to authors.

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