Clim. Past Discuss., 6, C1109–C1118, 2010
www.clim-past-discuss.net/6/C1109/2010/

Climate
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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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Received and published: 9 December 2010

I thank R. Zech and his co-authors for providing a first reply to my comments before the closing of the open discussion, giving me the opportunity to answer a few of their questions and correct a several persisting inexactitudes in public.

The arguments put forward revolve around the following questions and postulate

1. Can the oceans be sinks of carbon during deglaciation, when $pCO_2$ rises?
2. The marine $\delta^{13}$C is not a robust proxy.
3. The permafrost hypothesis: what does it claim?

In addition, the title of the reply also asks a question: permafrost carbon neutralized by organic carbon pools elsewhere? My answer to that one is ’Yes, certainly a large part of it, possibly even all of it, but not necessarily’. Details on these changes are readily available in the abundant literature on the subject (a wide selection of references can be found in the review).

I address the other three points in turn below.

## 1 The oceans: can they be sinks during deglaciations?

The short answer is ‘Yes!’ The additional questions are: is this relevant? does this have any implications? Here my short answers are ‘No!’ and ‘No!’

The question does actually not make much sense as it is ill stated.

1. The question is incomplete as it must be stated for which other reservoir the oceans would be a sink of carbon: probably, it is the atmosphere that is implicitly meant here, but as one may realize below, the oceans may also act as an important sink for the surface sediment. Or should we possibly consider the ocean as a sink if its contents are increasing? This could still allow them to act as a source for the atmosphere at the same time.
2. I am not aware of any ‘currently accepted paradigm that the oceans have acted as carbon sink during glacials and as a carbon source during deglaciation’. Obviously, the authors need to explain what they understand by the oceans as a sink.
3. Most important: it is unrealistic to reduce the oceans role in controlling atmospheric $pCO_2$ to a one-dimensional game of carbon source and sink. The oceans exert their control on atmospheric $pCO_2$ via the joint action of *two* variables: DIC,

which is the related to the carbon stock *and* alkalinity. Alkalinity changes are just as important as DIC changes and may either amplify or counterbalance the effects of them.

The answer to the question whether the oceans can act as sinks during the deglaciation is almost completely given in the review. For the sake of this example, let us assume that the terrestrial biosphere outside permafrost regions takes up 600 PgC during and after the deglaciation. Figures for the 850 PgC uptake by the terrestrial biosphere growth can be found in Table 1, together with an explanation how these figures were obtained. For more details, please refer to my earlier review of the paper (RC C1024, http://www.clim-past-discuss.net/6/C1024/2010). For 1000 PgC released into the atmosphere from permafrost soil, 600 PgC are taken up by the rest of the terrestrial biosphere, 400 PgC are thus left in the atmosphere, 360 PgC of which are absorbed by the oceans on time scales of a few thousand years, leaving 40 PgC in the atmosphere (i.e., 19 ppmv – with the 850 PgC terrestrial biosphere regrowth, only 15 PgC would be left in the atmosphere, i.e., 7 ppmv), simply as a result of ocean chemistry and carbonate compensation, without any change in any oceanic process. *Changes* in *oceanic processes* are then required to pump 160 PgC into the atmosphere in order to produce the net 200 PgC increase observed. There is no other reservoir left that could possibly fulfil this role.

Several things are worth noticing here:

1. Obviously the oceans may globally act as a sink during deglaciation, although they make atmospheric $pCO_2$ increase: adding up the sink and source terms shows that the oceans act as a net sink of carbon during the deglaciations (they are responsible for a net uptake of $360 - 160 = 200$ GtC); if carbonate compensation is considered the figures change considerably (see Table 1), but, unless carbonate compensation plays a major role in the final stage where changes in oceanic processes are required to provide the missing 160 PgC, the oceans glob-

**Table 1.** Carbon transfers among reservoirs (PgC)

| Stage | Land | Atmosphere | Ocean | Sediment |
|---|---|---|---|---|
| *Permafrost Release of 1000 PgC* | | | | |
| Release into atmosphere | $-1000$ | $+1000$ | 0 | 0 |
| Ocean buffering & carbonate compensation | | $-900$ | $+1800$ | $-900$ |
| *Alternative 1: Biospheric uptake of 600 PgC* | | | | |
| Uptake from atmosphere | $+600$ | $-600$ | 0 | 0 |
| Ocean buffering & carbonate compensation | | $+540$ | $-1080$ | $+540$ |
| Total net land processes | | $\underline{+40}$ | $+720$ | $-360$ |
| Ocean processes & extra carbonate compensat. | | $\underline{+160}$ | $-160{\mp}X$ | $\pm X$ |
| *Alternative 2: Biospheric uptake of 850 PgC* | | | | |
| Uptake from atmosphere | $+850$ | $-850$ | 0 | 0 |
| Ocean buffering & carbonate compensation | | $+765$ | $-1530$ | $+765$ |
| Total net land processes | | $\underline{+15}$ | $+270$ | $-135$ |
| Ocean processes & extra carbonate compensat. | | $\underline{+185}$ | $-185{\mp}X$ | $\pm X$ |

These figures are derived by using the following assumptions. (1) 90% of an initial release (forcing) $F_0$ are taken up by the ocean as a result of buffering and carbonate compensation ($\Delta C_1 = 0.90F_0$). (2) The carbonate compensation adjustment leads to the net dissolution of an amount $\Delta C_2$ of carbonate carbon, also bringing in an amount $\Delta A$ of alkalinity: $\Delta A = 2\Delta C_2$. (3) Carbonate compensation acts such as to restore the deep-ocean $CO_3^{2-}$ concentration to the pre-perturbation level. We therefore require that, after carbonate compensation completes, the total alkalinity increase is equal to the total carbon increase in the ocean: $\Delta A = \Delta C_1 + \Delta C_2$. Accordingly, $\Delta C_2 = 0.9F_0$. The systematics for an uptake $F_0$ from the atmosphere are obtained similarly. The final contribution of pure oceanic processes are derived by deduction of the net impact resulting from the land processes from the required total atmospheric 200 PgC change (underlined figures).

ally remain a net sink (if we assume that the ocean are considered as a carbon sink when their content increases).

2. Changing oceanic processes must be responsible for 80% (>90% with the 850 PgC estimate for the biospheric regrowth) of the net change between glacials and interglacials, re-emphasizing the dominant role of the oceans in driving atmospheric $pCO_2$. Put the other way around: ignoring an active role of oceanic processes means that only about 10–20% of the observed change can be explained, even with such high permafrost soil carbon release as 1000 PgC.

The example illustrates well that stating that “the ocean exerts the major control on atmospheric $pCO_2$ on the time scale of the deglacial rise” is not equivalent to “the ocean acts as a source during times of increasing $pCO_2$”.

Clearly, the dominant role of the ocean in controlling atmospheric $pCO_2$ on glacial-interglacial time scales remains unchallenged to date. Its content during deglaciation may be decreasing or increasing, depending on the joint alkalinity changes.

## 2 The marine $\delta^{13}$C proxy

I am by now starting to get really concerned by the authors’ persisting in their depreciation of the marine $\delta^{13}$C. Unquantified doubts and over-interpretation of out-of-context citations still make up too much of the argumentation. If only the global picture emerging out of the arguments put forward was consistent, there would be a basis for discussion. But the picture is even contradicting!

It appears that the authors have a different reading than me of the conclusion of Oliver et al. (2010), repeatedly cited in the replies: Oliver et al. (2010, p. 669) “consider the coverage too incomplete to directly construct a time-series of $\delta^{13}$C inventories”. I do not see here any questioning and even less a rejection of the value of the currently

accepted figure for the global mean glacial-to-interglacial increase in $\delta^{13}$C of 0.32‰ put forward by Duplessy et al. (1988). My reading of the cited statement is that Oliver et al. (2010) do simply not consider it possible to produce *time-series*! Regarding the changes between glacial and interglacial times, R. Zech and co-authors seem to overlook that Oliver et al. (2010) further write that

- “[their] data synthesis reveals a high degree of spatial coherence in $\delta^{13}$C variability in the deep Atlantic and Pacific Oceans, with *high values during temperature maxima* and *low values during temperature minima*.” [my emphasis]
- “High global deep ocean $\delta^{13}$C, indicating isotopically heavy carbon, is obtained during Marine Isotope Stages (MIS) 1, 3, 5a, c and e, and low $\delta^{13}$C during MIS 2, 4 and 6, which are temperature minima, with larger amplitude variability in the Atlantic Ocean than the Pacific Ocean.”

To dispel any possible doubt, I thought it would be best to ask the main author, Kevin Oliver, directly, which I have done. He kindly allows me to quote from his reply:

- K. Oliver thinks that it *is* possible to derive estimates of the glacial-interglacial difference in global, but that the error bars on such an estimate would be very large (>50% of the best guess);
- he thinks that the sign of the change *can* be inferred;
- he thinks that the *figure* for the LGM-to-Holocene increase lies well within what he considers to be plausible;
- he says that their study (Oliver et al., 2010) does *not* invalidate the *figure* of 0.32‰ from Duplessy et al. (1988), but he emphasizes that he feels uncomfortable with the *approach* of Duplessy et al. (1988), who based their estimate solely on Pacific data extrapolated to the global ocean, which makes it difficult to derive

meaningful estimates for the error bars, and that the provided error bars furthermore tend to be forgotten when cited;

- he writes that all of the available evidence, including their study, points to lower mean $\delta^{13}$C at the LGM than the Holocene, that this signal is coherent throughout the deep ocean ($>$2500 m) where data exist, over a full glacial cycle and that the picture is less clear in intermediate waters, but that the magnitude of the change of whichever sign there is small.

The picture thus presents a number of interesting and important nuances.

When it now comes to the arguments put forward in the reply AC C945 to the comment by Anonymous Referee #1 (RC C896), we, unfortunately, once more have to point out that the authors argumentation is in contradiction with the data: releasing 1000 PgC from permafrost soils at $-30$‰ during the deglaciation and dissolving them into a 40,000 PgC reservoir of carbon (atmosphere+ocean), will make *decrease* the global average $\delta^{13}$C of that reservoir by 0.7‰ on average, which is, contrary to the claim of the authors, not exactly what is observed, but exactly opposite and to what is observed!

There are *difficulties* in interpreting the sedimentary $\delta^{13}$C record (as outlined, with lots of nuances and references in my detailed comments in the review), but these do not totally invalidate the proxy! As written in my review, in the – highly unrealistic – worst case where the correction for carbonate ion related fractionation is applied uniformly throughout the ocean, the glacial-interglacial difference might reduce to zero. After all of the uncertainties have been taken into account, there is virtually no space for an increase in global ocean $\delta^{13}$C during glaciation or a decrease during deglaciation, and certainly not an 0.7‰ decrease during the deglaciation!

So, I think it is entirely justified to re-emphasize my conclusion: while the amplitude of the glacial-interglacial global mean $\delta^{13}$C remains not very accurately know, the sign of the change is robust.

In my comments, I have provided a consistent framework for the global picture, based upon the available information and corroborated with data available from the literature. If the authors do not agree with that framework or with the state-of-the-art knowledge on global ocean $\delta^{13}$C changes, I invite them to provide quantitative evidence, instead of sticking to the expression of some vague doubts.

Readers are urged to remain open-minded to the ideas of the authors, in the submitted paper and in the reply. Does the marine $\delta^{13}$C community not deserve the same open-mindedness to their understanding of global ocean $\delta^{13}$C changes, rooted in hundreds of records throughout the oceans, paired with a broadly adequate understanding of the processes of cycling within the ocean and the exchange processes between the ocean, the atmosphere and the surface sediment?

## 3 The permafrost hypothesis: what does it claim?

The confusion gets complete when the authors …

> "[…] would like to clarify that the permafrost hypothesis does not claim to explain the whole 100 ppm glacial-interglacial change of atmospheric $CO_2$ with changing carbon storage in permafrost soils alone."

Going back to the submitted paper, I read (from p. 2208, l. 26 to p. 2209, l. 3) that

> "Estimating the glacial-interglacial permafrost carbon pool changes to 1000 Pg (Zimov et al., 2009), this would be equivalent to 50 ppm atmospheric $CO_2$ – a significant portion of the carbon balance during the ice

> age cycles. As roughly another 50 ppm would be a simple amplification effect due to a warming ocean (which holds less dissolved $CO_2$), permafrost carbon dynamics alone might be able to explain the glacial-interglacial atmospheric $CO_2$ differences of $\sim$100 ppm."

not to forget the title that announces "A permafrost glacial hypothesis to explain atmospheric $CO_2$ . . . " I do definitely not see any possible way to reconcile those two claims.

On the basis of all the quantitative arguments presented in my review, to some extent reformulated and extended above, it is clear that the permafrost hypothesis can explain only a small but possibly still significant part of the observed changes. I am, however, asked to review the submitted paper. As formulated in that manuscript, the permafrost hypothesis for explaining glacial-interglacial $pCO_2$ changes is not tenable! I am therefore still obliged to consider the hypothesis as not viable!

A carefully reformulated version could possibly be viable but certainly not the one presented in the submitted manuscript.

## Conclusions

I can only reiterate my main conclusions and recommendations from the earlier review.

- The permafrost storage changes must be taken into account if we are to understand the dynamics of carbon cycling on glacial-interglacial time scales. It must be considered as part of the global network of carbon exchanges between the atmosphere, the ocean and the terrestrial biosphere, and consistently integrated into that framework.

- There is no justification for revising the role of the ocean in explaining glacial-interglacial $pCO_2$ changes in the atmosphere: ocean based mechanisms appear to be required to provide 80%, possibly even more than 90% of the total net glacial-interglacial $pCO_2$ change, even if the permafrost storage change estimate of 1000 PgC of Zimov et al. (2009) is taken at face value.
- Even if there remain uncertainties regarding the amplitude of glacial-interglacial changes of the global mean $\delta^{13}$C in the oceans, the sign of the change appears to be robust: the oceans had a lower $\delta^{13}$C at the LGM than during the Holocene. This provides a constraint on the glacial-interglacial transfer of carbon depleted in $^{13}$C between the ocean+atmosphere and land reservoirs, that may not be simply dismissed.
- Please build upon the existing knowledge and integrate your mechanisms into the existing framework, which needs to be reviewed with all the care it deserves.

## References

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