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Interactive comment on “Quasi-100 ky glacial-interglacial cycles triggered by subglacial burial carbon release” *by* N. Zeng

N. Zeng

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The reviewers have raised substantial questions and made many insightful comments. Many of these questions have also been raised by colleagues since the publication of Zeng (2003) in which the glacial burial hypothesis was proposed. Below I respond extensively to those questions as well as discuss additional considerations in two parts: the first to discuss the merit of the glacial burial hypothesis and why the terrestrial biosphere might be a ‘helper’ for the glacial CO2 problem, rather than a ‘burden’ as traditionally thought; the second part discusses the deglacial triggering mechanism by burial carbon ejection. These additions amount to about 12 double-spaced pages and have been incorporated into the revised manuscript where the references can be found.

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1 The glacial burial hypothesis and why terrestrial biosphere may be a helper in the glacial CO_2 problem

In the glacial burial hypothesis (Zeng, 2003) in which boreal vegetation and soil carbon is buried and preserved under such as the Laurentide icesheet during glaciation. Together with several other factors such as continental shelf carbon at lower sea level, the glacial burial hypothesis claims more storage (about 500 GtC in a model simulation; 1 GtC is 1 gigaton or 10^{15}g carbon; simply Gt hereafter) of carbon on land during a glacial maximum that is preserved and released during the ensuing deglaciation, thus contributing to deglacial CO_2 rise. For the ease of discussion, I will call this the 'helper' scenario, while the traditional view of a deglacial increase in terrestrial carbon pool will be termed the 'burden' scenario (below).

In a well established view, the glacial terrestrial carbon storage was smaller than at interglacial, based on marine C13, pollen-based vegetation reconstruction and terrestrial carbon models (see Table 1 of Zeng 2003 for a summary). Despite of the uncomfortably large range of uncertainty among these estimates (Crowley, 1995), the number 500 Gt has established itself in the literature, corresponding to a 0.35‰ lower mean glacial ocean δ^{13}C (Curry et al., 1988; Duplessy et al., 1988). Thus the regrowth (expansion) of the terrestrial biosphere at deglaciation would take 500 Gt out of the atmosphere-ocean system. Since atmosphere CO_2 increased by about 90 ppmv (approximately 180 Gt) from glacial maximum to interglacial, the ocean would have to accommodate this additional terrestrial carbon increase, thus land is a 'burden' for the ocean mechanisms if there were to explain the full amplitude of atmospheric CO_2 increase.

If land releases 500 Gt during deglaciation (helper), it would produce 15 ppmv (i.e., about 6% of the 500 Gt) increase in atmospheric CO_2 after the oceanic 'buffering effects' of deep ocean invasion and $CaCO_3$ compensation equilibrize with the atmosphere (e.g., Sigman and Boyle, 2000). However, since the $CaCO_3$ compensation timescale (about 8000y) is comparable to the deglacial timescale on which land-ocean carbon transfer takes place, a 500 Gt ocean-land carbon transfer leads to about 30 ppmv (12%) increase in atmospheric CO_2 (Zeng, 2003), thus ocean mechanisms only needs to account for 60 ppmv atmospheric CO_2 increase. In contrast, if land takes up 500 Gt (burden), the ocean mechanisms would have to explain an additional 30 ppmv, thus a toal of 120 ppmv increase in atmosphere (Ridgwell, 2001; Koehler et al., 2005). Thus, a land helper would enable other well established active oceanic mechanisms including changes in sea surface temperature and coral reef to explain comfortably the full amplitude of the observed CO_2 change.

1.1 Oceanic and atmospheric C13 and C14

Such a different terrestrial scenario would require the reexamination of a large amount of observations and theoretical ideas. One prominent example is the marine δ^{13}C records that suggest about 500 Gt terrestrial carbon storage increase from glacial maximum to interglacial, inferred from an average 0.35‰ lower glacial oceanic δ^{13}C assuming a terrestrial δ^{13}C signature of -25‰ (Shackleton, 1977; Curry et al., 1988; Duplessy et al., 1988; Crowley, 1995). However, such changes in benthic foraminiferal δ^{13}C can also be explained by many other factors (Lea et al., 1999). These factors together with the sparseness of the data especially over the large Pacific Ocean lead to large error bar in the -0.3 to -0.4‰ mean value (Matsumoto and Lynch-Stieglitz, 1999).

More importantly, recent advances suggest the existence of alternative explanations (at least possibilities) that reduce this value, do not require a lower glacial terrestrial carbon storage or even reverse the direction. I highlight three possibilities here:

1. The higher surface carbonate ion due to lower glacial atmospheric CO_2 may directly influence surface (Spero et al, 1997) and possibly benthic foraminiferal δ^{13}C value (Lea et al., 1999), so that the observed lower glacial δ^{13}C values can be explained without

input from light terrestrial carbon.

2. A more stratified glacial ocean (Toggweiler, 1999) would reduce the 'range of influence' of the negative deep ocean δ^{13}C values. Because the surface ocean tends to differ little between the Holocene and LGM (even higher δ^{13}C in the N. Atlantic) (e.g., Fig.8 of Matsumoto and Lynch-Stieglitz, 1999; and Fig.1 of Toggweiler et al., 2006) so that the inferred terrestrial carbon change would be smaller than the traditional inference corresponding to a mean -0.3 to -0.4‰ change (Curry et al., 1988).

3. A terrestrial carbon release at deglaciation may offer more straightforward explanation to a number of perplexing issues such as the deglacial minimum and the transient behavior observed in the atmospheric and surface ocean δ^{13}C records (Smith et al., 1999; Spero and Lea, 2002) Indeed, recent data from EPICA (Leuenberger et al., 2005) support the earlier results of Marino et al. (1992) and Smith et al. (1999) in the deglacial minimum in atmospheric δ^{13}C, a natural outcome of a terrestrial light carbon release during deglaciation (see Fig.9 of Zeng 2003). These authors attempted (albeit vaguely) to explain this deglacial transient behavior based on changes in the ocean, but a terrestrial input during deglaciation provides a more straightforward explanation. In contrast, such transient behavior had been largely ignored in earlier glacial CO_2 studies, and sometimes the low δ^{13}C values at deglaciation may have been inadvertently averaged into the glacial mean value.

Another interesting consequence of a deglacial terrestrial carbon input is that it would drive down atmospheric carbon 14 because the carbon from previous interglacial (about 100 ky old) would be C14 'dead', i.e., containing practically no C14 due to the short life time (half-life 5730 years) of C14. However, a glance at the observed atmospheric C14 over the last 50 ky (e.g., Hughen et al., 2004) shows that C14 variations are dominated by production rate, although it clearly contains information from carbon cycle and climate changes (Hughen et al., 2000; Laj et al., 2002). Also importantly, in a more realistic picture, part of burial carbon pool will be younger than 100 ky because of the decay and regrowth of vegetation in response to icesheet advance and shrinking on sub-100ky cycles (next subsection). Thus the amount of C14-dead carbon would be smaller than the total burial carbon input of 500 Gt. This picture would also predict that atmospheric C14 decrease due to C14-dead carbon input to take place at the later part of a termination, because these C14-dead carbon from the previous interglacial would have been best preserved in the central and colder northern portions of an icesheet which would melt later in the deglaciation.

Thus the identification of burial carbon signature in atmospheric C14 may be a challenging task, and a fruitful way is perhaps to follow the model-data approach of Laj et al. (2002) and Hughen et al. (2004). Because the production rate tends to vary more slowly, the faster climate-related changes such as millennial-scale variations can be relatively easily identified (Hughen et al., 2000), and I suspect changes on deglacial timescale of a few thousand years may also be identifiable in the atmospheric C14 data. If one assumes about 1/3 of the 500 Gt burial carbon to be C14-dead, then assumes this 150 Gt being released towards the end of deglaciation over a period of 2000-5000 years, this should lead to a detectable drop in the C14 data.

Another important constraint is $CaCO_3$ dissolution events at deglaciation which also poses a challenge to the case for a land helper, e.g., as reviewed by Keir (1995) and Crowley (1995) and simulated by Ridgwell (2001). This is not discussed here due to my limited knowledge on this subject, and will be addressed in future work.

1.2 Pollen-based reconstruction and terrestrial modeling

Compared to the constraint from marine C13, pollen based paleoecological evidence suggests even less glacial land carbon storage (by 700-1500 Gt excluding some extreme values; Adams et al., 1990; Crowley, 1995; See Table 1 of Zeng 2003 for a summary of marine δ^{13}C, pollen and terrestrial carbon modeling results) that has been difficult to reconcile with the 500 Gt inferred from marine δ^{13}C (Crowley 1995; Adams

2002, unpublished note available from J. Adams or this author). Terrestrial carbon models (forced by reconstructed climate) generally simulate smaller changes but tend to be in the same direction of the pollen and marine inferences. Then how is it possible that the result of Zeng (2003) be consistent with these observations which point at large changes in the other direction? I argue that there are enough uncertainties in these estimates to allow the possibility of a land helper. The main reasons are:

1. Paleoecological reconstructions and other glacial terrestrial models did not include carbon stored under ice (i.e., they assume bare rock/soil after icesheet retreat). Zeng (2003) estimated this to be about 500 Gt. The likelihood of this mechanisms is discussed in section 2.3 below.

2. After icesheet retreat, re-establishment of the boreal carbon pool is a slow process. While vegetation growth takes only decades to few hundred years, soil development can take thousands of years or longer, as evidenced by the large area of bare rock or shallow soil in the present-day Canadian shield and Scandinavia, 10000 years after last deglaciation. Another potentially important factor is soil nutrient buildup which can also take thousands of years as suggested by studies of vegetation and soil development on a volcanic lava sequence in Hawaii spanning 4 million years (Vitousek, 2004). Because most of the carbon in cold climate is stored in soil, not vegetation, even if vegetation grows quickly on glacial tills, it will take a long time before most of the boreal carbon pool can be re-established. No delay was assumed in previous terrestrial modeling studies, with the exception of Kaplan et al. (2002) who however only considered the delay of vegetation growth not soil.

The release of glacial burial carbon would be partly compensated for by regrowth uptake on the deglaciated land. Their relative timing would play an important role in the net contribution to deglacial atmospheric CO_2. Zeng (2003) assumed ‘in situ’ burial and release for glacial burial carbon, and a somewhat arbitrarily chosen timescale of a few thousand years for soil development, and the model showed that despite of the release of 500 Gt glacial burial carbon, the regions previously covered by icesheets had only a net release of about 150 Gt after the compensation from regrowth uptake (see his Fig.7). The magnitude of this net release depends on the relative timing of burial carbon decomposition and regrowth. This ‘in situ’ assumption will be relaxed in the current work by including icesheet dynamics.

3. The impact on carbon storage of a different glacial climate may not be fully reflected in pollen data. For instance, since pollen record is an indicator of above ground vegetation, a ‘modern analog’ approach needs to be used to infer soil carbon (Adams et al., 1990). The much colder glacial climate at higher latitude may lead to slower decomposition, thus higher soil carbon storage which may have been underestimated by the pollen reconstructions.

Thus, the not so dry but much colder glacial climate can allow more carbon to accumulate in soil regardless of the uncertainty in above-ground biomass or vegetation type changes. Indeed, there is emerging evidence from studies in Siberia that the periglacial environment during the last glacial maximum (LGM) was much more productive and carbon-rich than in an arid environment as typically depicted (Zimov et al., 2006).

Another major uncertainty concerns the dependence of plant productivity on atmospheric CO_2 level, i.e., the ‘CO_2 fertilization’ effect. Lowered CO_2 level at glacial times means lower productivity. Traditionally, models used parameterizations for CO_2 fertilization that were strong enough to explain the modern ‘missing CO_2 problem’ (e.g., Sarmiento and Gruber, 2002). However, recent evidence attributes much of the missing CO_2 to other factors such as forest regrowth (Pacala et al., 2001; Caspersen et al., 2000), though the strength of CO_2 fertilization is still highly uncertain, to a larger extent related to the multiple limitations imposed at ecosystem level and longer timescales (Field, 2000; Luo et al., 2006).

The impact of this uncertainty on the glacial terrestrial carbon modeling can be large. For instance, the 800 Gt increase from glacial to interglacial simulated by the model

LPJ (Kaplan et al. 2002) was reduced to about 200 Gt when CO_2 fertilization was kept at constant. Similar sensitivity was seen in the CARAIB model (Otto et al., 2002) with CO_2 fertilization almost solely responsible for the deglacial biospheric expansion. In comparison, the dynamic vegetation and carbon model VEGAS used by Zeng (2003) has a weaker productivity dependence on CO_2 as revealed in an application to 21st century carbon-climate coupled simulation (Friedlingstein et al., 2006), and its effect was largely compensated for by the temperature effect on soil carbon in his glacial simulation.

In summary, the compensating effects of the three main climate factors (temperature, CO_2, precipitation) are such that outside the area covered by icesheets at glacial maximum, overall terrestrial carbon pool change is moderate. The total land-atmosphere carbon transfer is a combination of contribution from non-ice/non-shelf areas, continental shelves, ice covered area (see his Fig.7 for the numbers from that model simulation), with the latter in turn depending on a partial cancellation between the decomposition of burial carbon and regrowth.

1.3 Freezer or bulldozer

A key assumption of the glacial burial hypothesis is that the vegetation and soil carbon accumulated during an interglacial was buried during glaciation, followed by preservation and later release at deglaciation. An alternative fate of this carbon pool is that it was destroyed as icesheets advanced. One may visualize the glacial burial hypothesis as the 'freezer' hypothesis, and the alternative the 'bulldozer' hypothesis. Zeng (2003) made the explicit freezer assumption, while other terrestrial carbon models made the bulldozer assumption either explicitly when continuous simulation over glacial-interglacial cycles demanded the accounting of this carbon pool (Koehler et al., 2006), or implicitly in most other models by assuming no carbon under icesheet in the timeslice simulations.

The view of an icesheet acting like a bulldozer stems from the observation of mountain glaciers and present-day icesheets in Greenland and Antarctica. These ice masses sweep down whatever is on their way, carry them together with fallen debris and sediments scraped off the bottom or mountain side, and then dump them in the ablation zone or leave them behind as glacial moraines and tills when they retreat. Because their movement is sufficiently slow, most fallen vegetation would have enough time to decompose and release carbon into the atmosphere. Some carbon could still be preserved in this case if they are protected by sediment soil and ice, or simply decompose very slowly in the cold and dry periglacial environment. Indeed, it is not uncommon to find old tree trunks or even animals preserved from the Little Ice Age in periglacial environment of mountain glaciers in the Alps and the Rockies, but overall the quantity of carbon preserved this way is likely quite small.

However, when continental-scale glaciation is considered, I argue for a drastically different picture. At glacial inception when climate becomes progressively colder, summer becomes colder and shorter. At the point when summer heating is not enough to melt away snow accumulated during the cold seasons, vegetation and soil would be covered by a perennial ‘blanket’ of snow that would accumulate as climate cools further. Obviously, there is ample time before this threshold is reached for vegetation to change, e.g., from boreal forest to tundra. Such vegetation and soil dynamics needs to be taken into account and it was modeled in Zeng (2003). It is important to note that such ecosystem succession does not necessarily imply that only a small quantity of carbon is available for burial, because tundra can contain large amount of soil carbon. There would also be a period immediately after permanent snow cover but before ice enclosure when dead carbon could be decomposed and released, but the bacterial activity should be sufficiently slow to allow the preservation of a large part of the buried carbon.

There has long been the debate on glaciation’s ‘highland origin’ (Flint, 1943) vs. ‘in situ’ formation (Yves, 1962), relevant respectively to the ‘bulldozer’ and ‘freezer’ hypotheses here. Although, glaciers formed in the mountains such as the Cordilleran or high arctic mountains could flow downhill and cover some surrounding lowland, the area extent would be quite limited. It is hard to imagine that such glaciers could flow thousands of kilometers across the Canadian plain. Most of the continental-scale glacial inception must have been largely in situ. Our understanding on such processes is robust enough now so that dynamic icesheet models nearly always produce glacial inception as the result of in situ snow accumulation (e.g., Vettoretti and Peltier, 2004).

Even in the case of a large icesheet/cap moving laterally towards warmer region so that the land is significantly disturbed before covered by ice (rather than 'blanketed' by snow), there is still good probability for large organic carbon preservation. This is because the cold periglacial environment could contain significant amount of carbon in, e.g., the form of tundra soil or peat, that would decompose very slowly even if disturbed.

Nevertheless, when icesheet becomes large enough, subglacial movement will become significant especially when basal melting occurs. This may lead to ejection of glacial burial carbon that will be the focus of this paper. Icesheets also advanced and retreated on sub-100ky cycles, and this would destroy some carbon, especially at the edge of an icesheet. However, vegetation would grow back where icesheets had retreated, and became covered again when ice came back. If such oscillations were faster than vegetation and soil reestablishment, little carbon would be there for reburial. Faster oscillations (decades to centuries or millennia) generally corresponds to icesheet waxing and waning on much smaller area so this effect should not have a major impact on the overall carbon change on 100ky timescale. Over the 20 and 41ky cycles that dominate the sub-100ky spectrum, vegetation and soil would have enough time to reestablish during these cycles, albeit these carbon would be younger than the carbon buried in the central regions of the icesheet that do not melt during these cycles.

Is there any direct physical evidence of extremely ancient carbon preserved by ice, older than mammoths frozen in Siberia permafrost or ice man in the Alps? The answer is yes, with a few examples below.

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1) Exposed shrub near an retreating Andean mountain glacier was dated over 50000 years old (Thompson, 2004).

2) In Greenland, the last section of an ice core is brown-silt colored, with CO_2 (130000 ppmv) and CH4 (6000 ppmv) concentration orders of magnitude higher than in the atmosphere (Souchez, 1997), indicative of decomposed ancient organic matter.

3) At the bottom of a Greenland ice core, a piece of organic matter was suspected to be a needleleaf or a grass blade. Since it was at the bottom of an icesheet dome, its age is probably over 2 Million years.

4) Organic sediments dated from the Eemian interglacial (approximately 120ky ago) by biostratigraphy in the glacial deposits of Scandinavia suggest that they may have been preserved until the final deglaciation (Forsstrom and Punkari, 1997; Punkari and Forsstrom, 1995). Similar interpretation could be said to organic deposits from Hudson Bay lowland that was once the central part of the Laurentide icesheet (Dredge et al., 1990).

Obviously, these findings can not be taken as proof of a continental-scale carbon burial by ice, but they suggest the possibility of preserving carbon for long period of time by ice under a variety of conditions. Among them, (4) may be most relevant to the glacial burial hypothesis here. Since the strongest evidence would come from undisturbed ice-buried carbon from previous interglacials, I speculate that high arctic islands such as Ellesmere, Baffin, and Axel Heiberg in Canada would be good places to look for carbon from last interglacial. Since the Eemian was about 0.5-1°C warmer than the Holocene, these islands would have more exposed land for vegetation growth at that interglacial, which would have been buried during the subsequent glacial inception. Since it might have never been warmer during the last glacial-interglacial cycle, the Eemian carbon on these islands is perhaps just being exposed to the atmosphere as the ice caps retreat due to current warming since the 20th century (ironically and likely due to anthropogenic CO_2 emission).

Indeed, based on ecological and microbiological studies in these regions and Greenland, Welker et al. (2000) and Skidmore et al. (2000) hypothesized that the regrowth of tundra vegetation in the newly exposed land and microbial activities under ice are ‘feeding’ on ancient carbon and nutrient, although there has been no direct identification or dating. However, if significant disturbance occurred in these ice caps such as over the sub-100ky cycles, or during warmer than Eemian but brief (so do not show up in the low resolution paleo temperature record) periods, clean evidence of ice-buried Eemian carbon will be more difficult to obtain.

2 Some speculations

The glacial-interglacial cycles simulated here are self-sustaining without external forcing. These quasi-100ky cycles occur within certain plausible range of parameter values that need to be better identified perhaps in simpler models. Sensitivity experiments conducted so far indicate that they need relatively fast burial carbon release and carbon-climate feedback of sufficient strength. The key termination switch due to glacial burial carbon ejection requires only basal flow to become substantial. This needs icesheets to grow large and long enough, without the requirement of increase in solar forcing.

This internal triggering mechanism may play a role in the ‘stage-11’ problem (large deglaciation at a time of low solar variability). Perhaps more importantly, it provides a potential solution for the ‘causality problem’, i.e., observed deglaciation leads solar ‘forcing’ at Termination II, as suggested by the well dated vein calcite in the Devil’s Hole in Nevada (Winograd et al., 1992; however, say the unsettled debate, e.g., in Imbrie et al., 1993; Crowley, 1994; Edwards et al., 1997), and high precision dating of Barbados coral reef terraces (Gallup et al., 2002). These records suggest deglaciation started up to 10000 years before the rise in insolation at 60N, a standard marker of orbital forcing's impact on Northern Hemisphere icesheet dynamics (e.g., Imbrie et al., 1993).

Using an ingenious dating approach using Argon isotope in the air bubbles as temperature indicator, Caillon et al. (2003) was able to circumvent the gas age-ice age uncertainty associated with deuterium temperature indicator. Their results show a 800y CO_2 lag behind temperature at Termination III. At its face value, this would rule out the possibility of deglaciation being *driven* by CO_2 increase (though it does not exclude possibility of positive CO_2 feedback). Here I propose a somewhat more complex scenario where some terminations were initiated by orbital forcing, and some others were initiated by CO_2 increase. Termination III was likely an example of the former case (driven by orbital forcing). If looking for candidates for the latter (initiated by CO_2), I would (boldly) predict Termination II by putting faith on the accuracy of dating and underlying assumptions of the Barbados coral reef and Devil's Hole calcite evidence. By underlying assumptions, I mean those that link the variations in these records to Termination II, as opposed to possible regional explanations (Crowley, 1994; Herbert et al., 2001) for Devil's Hole, and any other potential interpretations for the sea-level data from Barbados.

Actually, there are more fundamental reasons to think this may be the case. One reason is that if glacial burial carbon ejection was the trigger without coinciding orbital forcing, a long and cold period preceding the termination would be needed to grow the icesheets to the point of large basal flow or instability. Any quick examination of the Vostok data will show that the glacial period before Termination II satisfies this requirement better than most other periods (the glaciation ending at LGM is arguably comparable in length but there was a much longer cold period before Termination II). In contrast, the glacial period preceding Termination III appeared to least satisfy this requirement over the last 400ky. Obviously, such a proposal would raise more questions than it answers, and significant further research is needed before it can be considered viable. Nevertheless, from the observational side, accurate dating of the phase relationship between CO_2 and temperature for Termination II using methods such as that of Caillon et al. (2003) will be most illuminating.

These two cases will have also very different characteristics beyond the simple lead-lag relationship between CO_2 and temperature that should help with their identification in ice core data. If CO_2 drives temperature, the initial rise in both would appear nearly synchronous at the resolution of paleo record because the atmosphere and land-surface respond to greenhouse forcing on the order of days to months (though ocean can take up to several hundred years). In fact, this is analogous to the projected future climate change in response to fossil fuel CO_2. In contrast, if temperature drives CO_2, the lag could be significantly longer, controlled largely by slow carbon processes in the deep ocean and slow soil carbon pools, though there are also faster responses in vegetation and surface ocean. However, complications will come from the phase difference or overlap of contributions from different terrestrial and oceanic mechanisms during different sub-stages of a termination.

I emphasize again that although orbital forcing is not included here so we can isolate a critical positive feedback process not considered before, our findings do not exclude the role of Milankovitch orbital forcing. On the contrary, the above discussion points to the tantalizing possibility that the carbon-climate-icesheet feedback and switch mechanisms identified here interact with orbital forcing in a complex way. In particular, some terminations may be triggered by internal feedbacks, and others by orbital forcing. We should not be discouraged by such complications and shy away from a seemingly complex solution because they nevertheless behave in an understandable way that can be sorted out by an interdisciplinary approach with an open mind for non-conventional possibilities.

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