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Interactive Comment

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

Anonymous Referee #1

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This is certainly a fairly novel take on both the reasons for the observed glacial-interglacial variability in atmosphere CO2 as well as how the '100 kyr' oscillation in ice volume and climate itself could arise. My gut reaction is that it is all pretty unlikely - certainly there are a number of fundamental elements in the hypothesis and its model incarnation that seem problematic at best. But my 2 cents worth of pop science philosophy would be to note that at the end of the day, many published papers will turn out to be at least partly wrong. What is more important is that some advance in critical thinking and/or understanding and/or education should arise. Because this hypothesis address a range of interacting elements of the Earth system in a provocative way, I think that with improved description and expanded discussion of some of the issues I touch on below it is worth publishing.

EGU

Firstly, I would like to point out that the ideas contained here are not entirely novel (although in fairness, the author makes no explicit claim to this effect) - I would really like to see a little discussion in the context of this work of the ideas of Franzen, Klinger and friends, for instance; Franzen [1994] “Are wetlands the key to the ice-age cycle enigma?” (Ambio 23), Franzen et al. [1996] “Principles for a climate regulation mechanism during the late Phanerozoic Era, based on carbon fixation in peat-forming wetlands” (Ambio 25), and Klinger et al. [1996] “The potential role of peat land dynamics in ice-age initiation” (Quaternary Research 45). Although these authors focussed on peat lands rather than soils and vegetation and had no mechanistic model, their work is highly relevant. Indeed, Ning might like to consider peat land carbon, estimated to currently represent some 300-500 PgC [Laine et al., 1996] (Ambia 25), as an additional or even more viable source of carbon compared to vegetation and soil carbon. In this vein, and although it admittedly has only just been published, a paper by Karen Weitemeyer and Bruce Buffett (Global and Planetary Change 53) relating glacial-interglacial changes in CH4 to the accumulation and burial of methane clathrates under ice sheets, could also be included in a wider discussion.

Something which I think came up in association with the peat land based hypothesis of Franzen/Klinger was a lack of evidence for preserved peat carbon dating back to the previous interglacial or before. One might expect that the expelling of organic carbon from under the ice sheet at deglaciation would not be perfect, and that glacial sedimentary deposits (moraines, tills, etc) would contain some evidence of substantial carbon burial under an ice sheet (but incomplete destruction of the buried carbon). Are there semi-silicified hundred thousand year old trees buried in gravel anywhere or similar? Where would one look?

However, I find it difficult to envisage an ice sheet cleanly overriding vegetation and soil carbon with relatively little loss (i.e., oxidation rather than burial) in the first place - would a ‘bulldozing’ action not be more likely, perhaps with the preceding glacial outwash plain degrading the biosphere and available carbon inventory in advance of the ice sheet margin? Would the potential for buried carbon be more likely restricted to areas of ice sheet nucleation? Or rather, areas experiencing many years of continued snow accumulation prior to being overridden by the ice margin? At the other end of the glacial, the 'deglaciation switch' presumably requires that basal flow becomes substantial simultaneously virtually everywhere under the ice sheet (and also simultaneously under both major Northern Hemisphere ice sheets), otherwise the carbon release would be considerably extended in time. If this is true, what is the synchronizing mechanism? As such a critical element for the entire mechanism to work, we need to learn more about this transition in ice sheet dynamics as well as about any geomorphological evidence or process-based model support for it.

Interpretation of the marine d13C record is all too briefly alluded to at the end of the introduction. While arguments made over various parameterizations and assumptions regarding ice sheet dynamics may not easily resolved one way or another, as first noted by Nick Shackleton in a seminal paper in 1977 observed changes in Pacific benthic foraminiferal d13C require substantially more carbon to be present on land during the (early) Holocene compared to the glacial. A canonical estimate of 500 PgC has established itself in the literature (e.g., see Crowley [1995] (Global Biogeochemical Cycles 9)). Current dynamic vegetation models suggest even more, which Maslin and Thomas [2003] (Quaternary Science Reviews 22) reconcile with the marine d13C data by means of a relatively small addition of highly negative clathrate carbon at deglaciation. However, regardless of whether one prefers 500 or nearer 800-1000 PgC as the figure for the enhancement of the terrestrial carbon reservoir during interglacials, the fact remains that the hypothesis as encapsulated in the model (see Figure 3d) requires the opposite change - more terrestrial carbon storage during glacials compared to interglacials. This is arguably the most critical weakness of the hypothesis. At the very least, considerably more discussion is required other than just noting that "re-examination of a large amount of observations and theoretical ideas" will be needed.

As a corollary to this; what impact would the rapid release of ca. 250 PgC of relatively

CPD
2, S336–S340, 2006

radio carbon ‘dead’ carbon that have buried under the ice sheet for almost 100 kyr have on the evolution of atmospheric and/or surface ocean D14C? Would one expect to see a fingerprint of this release in D14C records spanning the last deglacial transition?

I am surprised/impressed how well the ca. 100 kyr ice sheet oscillation seems to work. For instance, I can see no sign of an explicit (or even semi implicit) term representing the interaction between ice sheet and bedrock, which is a natural generator of instability on orbital frequencies. The equations for ice sheet growth and decay appear superficially similar to the classic model of Imbrie and Imbrie [1980] (Science 207). I would be interested to see some discussion regarding how the current model compares to previous conceptual ice volume oscillation models. Also, since no orbital forcing is applied here, I would be interested to see what happens if it is. Do the timings of the terminations become correctly phase locked for instance?

Finally; an unfair accusation maybe, particularly considering that my English grammar is so desperately pitiful, but the manuscript could really do with a little copy-editing and re-phrasing here and there.

As a card-carrying pedant, I also have a number of minor comments and suggestions;

o Page 373 / lines 6-9; “It is very difficult, if not impossible, to simulate the observed glacial cooling in comprehensive models without changing CO2. Thus carbon-climate interaction may provide key internal feedbacks that have rarely been included in comprehensive models interactively.” I agree entirely. You could expand on this subject even. But importantly, a few references here would be useful; e.g., Li et al. [1998] (Clim. Dyn. 14), Ridgwell et al. [1999] (PO 14).

o Page 375 / line 14; I think that I know what you mean, but please spell out what you see is a model with “balanced complexity”? (I am still suffering severe ‘intermediate complexity’ hangover Ě)

o Page 376 / Equations 1 and 2; these also appear in the Appendix and so do not also need to appear here as well (or vice versa).

o Page 377 / line 3; a mean SST reduction of 6°C and 55 ppm of CO_2 change seems rather a lot. For instance, a variety of box models and GCMs, as much as they agree on anything, seem to come up with a range of 14-21 ppm for the ocean temperature contribution between glacial and interglacial. If the unresolved physical climate processes affecting atmospheric CO_2 (e.g., ocean circulation change) account for at least half of the 55 ppm, they should at least be explicitly stated and discussed a little. The author would find a recent publication by Robbie Toggweiler and colleagues [Toggweiler et al., 2006] (PO 21) as well as a previous one [Toggweiler, 1999] (PO 14) quantifying contributions to atmospheric CO_2 via the deglacial ventilation of the deep ocean useful in this regard.

o Page 377 / line 14; I am told in reviews of manuscripts of mine that using PgC instead of GtC is the apparently the current IPCC-correct thing to do. Please do not stand in the way of this great advance in progress. Even though everyone was quite happily used to GtC Ě

o Page 382 / lines 17-20; Has the Devil's Hole calcite record problem highlighted by Winograd et al. [1992] not been adequately resolved now? See; Edwards et al. [1997] (Science 276).

o Figure 3; Each predicted interglacial appears to be extremely short in duration (maybe as little as 1 kyr, but it is extremely hard to estimate things from the too small figures). What then is the explanation for the relatively protracted duration of the current interglacial?

o Figure 3; Because the model is run in 'model time' years, it would be helpful to highlight the glacials and interglacials (perhaps with a shaded grey bands or something) or at least mark each deglacial transition on the figure.

Interactive comment on Clim. Past Discuss., 2, 371, 2006.