

## ***Interactive comment on “Ice-driven CO<sub>2</sub> feedback on ice volume” by W. F. Ruddiman***

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### General comments

I agree with the author's first sentence that "the origin of the major ice-sheet variations during the last 2.7 million years remains a mystery." Milankovitch's proposal that Earth orbital variations influence Pleistocene ice-volume variations is certainly helpful. From the papers on the detection of Milankovitch variability in climate records (many of which cited in the present manuscript), several are milestones in the development of a theory of Pleistocene climate evolution. Before such a theory is wide and accurate enough to be accepted by the community, however, several empirical problems and inconsistencies should be removed. The present manuscript attacks some of those, such as the role of orbital-scale changes in atmospheric greenhouse-gas concentrations, the stage-11 problem, the 100-kyr problem and what might be termed the EPICA problem, namely that glacial-interglacial amplitudes in several climate variables increased after marine isotope stage 11.

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However, I personally believe that constructing a coherent and wide theory is very difficult because of the many uncertainties in the data (measurement noise, proxy noise, dating errors). This can lead to a situation with several, competing theories for the late Pleistocene climate change, theories that all might be very difficult to falsify. Another 'deficit' of the data comes from the comparable shortness of the late Pleistocene. The 100-kyr cycle exists only since ca. 650 kyr before present. (The paper by Mudelsee and Schulz (1997) shows that after the mid-Pleistocene climate transition in the time domain, an increase in ice volume at around 900 kyr, main power in ice-volume variations was first at 77 kyr period, and only since ca. 650 kyr at roughly 100 kyr period.) Because of the shortness, it is not possible to make accurate estimations of the exact period of the 100 kyr cycle. This adds uncertainties to the theories. Improvements of our understanding from the data side comes in form of longer records of greenhouse-gas concentrations (EPICA ice core). It would be great if we found proxy variables for atmospheric water vapour content and cloud coverage that allow reliable inference on Pleistocene timescales, but here I am pessimistic. Improvements from the modelling side of the problem, I anticipate to come either from simulating the Pleistocene climate at near-GCM level or by making significant conceptual suggestions. The author does the latter, in a scientifically profound manner, and with an eloquent and self-confident style.

However, CP readers should be aware that data and models have uncertainties, a fact that sometimes gets lost when reading the manuscript. There is indeed spectral power besides the Milankovitch periods of 19, 23, 41 and 96 kyr, as also noted by my fellow co-reviewer, Peter Huybers. Not all variability in the Pleistocene climate system follows the variations in the Earth's orbit. This is my major request to the author: to acknowledge this point and guard CP readers at the outset of this paper. The author indeed mentions the uncertainties at several places through the manuscript, but I think a short paragraph at the end of the introduction would be beneficial to readers. The second request to the author to be more specific on the maximum size of the time lag between carbon dioxide concentration changes and ice-volume changes that is

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compatible with his 'feedback' view. Based on my analyses of phase relationships (see below) I estimate that carbon dioxide variations lead over ice-volume variations by nearly 3 kyr ("overall phase"). It is not clear to me whether this value is still compatible with Ruddiman's 'feedback' view.

Below I make some specific/technical comments, which hopefully help revising this interesting manuscript.

Specific/technical comments

P45L14 (Page 45 Line 14)

Use the references from P54L3-4 for the 41 kyr cycle.

P45L20 Why not consider temperature as an "important orbital-scale variable", indicated by, for example, Vostok delta-D? Do you equate temperature and greenhouse gas concentrations? In principle, water vapour content of the atmosphere is one of the most important climate variables, despite the problem that yet no good proxy variables for it exists on Pleistocene timescales.

P45L27 "Several thousand years"—it would be helpful to distinguish between both views if you gave a number here.

P46L3 "... little or no lag"—give a number.

P46L18 "SPECMAP": the acronym often read, almost never explained. Please do. I personally have to concede that I do not know whether it means "Spectral Mapping" or instead "Spectral Analysis, Mapping, and Prediction".

P47L2-4 Shackleton (2000) argues that delta-18-O-air is a better ice volume proxy than delta-18-O in forams: do you agree? Let CP readers know.

P47L25 "1993" instead of "19993".

P49L22 Replace "time series analysis" by "spectral analysis". Time series analysis is

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more general.

Section 3 It would especially here be helpful if also temperature variations were considered.

P50L1-3 Sawtooth: good point! In my opinion, the proper way to estimate leads/lags, that is, phase relationships, in frequency dependence would be to develop a cross-spectral analysis where not sinusoids but rather sawtooths (of varying increase/decrease length) are superimposed. Although I see no principle difficulty for that, it seems not to have been done yet.

P50L16 "The most obvious message ... is one of very close similarity." This statement is pure per-eye speculation and not corroborated by quantitative analysis. The curves shown (CO-2, delta-18-O) have independent timescales. A phase estimation based on those data can have substantial errors.

P51L16 "The coincident CO-2 minima": per-eye speculation.

P50-51 (Section 3.2) I am not convinced by the material discussed that CO-2 variations exhibit "basic similarity in timing" with ice-volume variations. On P51L1 it is indeed conceded that there exists a lead of CO-2 variations of ca. 3 kyr in the analysis by Broecker and Henderson (1998).

Phase relationship estimation between CO-2 and ice volume variations can in principle be done in two ways: (1) Vostok CO-2 and Vostok delta-18-O-air, (2) Vostok CO-2 and marine delta-18-O (e.g., benthic forams). Both ways have pros and cons.

(1) Vostok CO-2 and Vostok delta-18-O-air The advantage here is that both variables are measured on air bubbles contained in the ice and have therefore no independent timescales. This reduces the error contribution from uncertain timescales. The disadvantages here are that (i) Vostok delta-18-O-air has other influences such as the Dole effect and (ii) the uncertainty in the atmospheric turnover time of O-2 (Mudelsee 2001).

(2) Vostok CO-2 and marine delta-18-O The disadvantage is obvious: independent

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timescales. The advantage may be that, despite Shackleton (2000), marine delta-18-O may be a better ice-volume proxy than delta-18-O-air.

In a former paper (Mudelsee 2001), I used both types of data pairs and the statistical method of lagged regression (building a relation between Vostok-CO<sub>2</sub>(t) and, e.g., Vostok-delta-18-O-air(t+lag)) to quantify the lag. Taking into account measurement and dating errors (by means of bootstrap simulations), I found that both data pairs yield a statistically significant lead of CO<sub>2</sub> variations. I averaged and concluded that "over the full 420 ka ..., CO<sub>2</sub> variations ... lead over global ice-volume variations by 2.7+-1.3 kyr." As a further result, across terminations, this lag value might have changed, which would mean for the present manuscript more difficulties to explain it within the Milankovitch framework.

It might be that at the 100-kyr period, the phase value is different from the above mentioned "overall" phase of 2.7 kyr, but this could be inspected by phase estimation using above mentioned data pairs. I have made one such cross-spectral analysis (Vostok-CO<sub>2</sub> and Vostok-delta-18-O-air) for review purposes but found considerable uncertainties of the phase estimate and dependence on selected parameters (spectral smoothing). This might come from the deviation from the sinusoidal form (sawtooth) or the disturbing influence by the Dole effect. (This point sheds light also on the difficulty to obtain reliable phase relationship estimates from cross-spectral analysis of pairs of data that have (i) a sawtooth-shaped trend (e.g., CO<sub>2</sub>, ice-volume, temperature) and (ii) a sinusoid-shaped trend (e.g., solar insolation or orbital parameters). Ruddiman's approach (P51L4-11) to divide the "overall" phase between the different Milankovitch periods is indeed clever, but it is not clear whether the division in terms of spectral power is appropriate (e.g., why not take square root of power?).

Hopefully the new, long EPICA core will shed more light on the phase relationships of carbon dioxide variations. Methodical advancements ("cross-spectral sawtooth Fourier analysis") might also be required. Summarizing, I myself see a lag of a few kyr of CO<sub>2</sub> variations over ice-volume variations, but I do not know whether this is small enough

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for taking Ruddiman's view (P46L1-8) and opposing the view (P45L24-28) of Imbrie et al. and others.

P52L15 4.5 deg cooling—what cooled, sea surface or land? Also give a reference.

P54L4 Replace "Muddlesee" by "Mudelsee". It is surprising that the manuscript cites Mudelsee (2001) only in regard to a CO<sub>2</sub> to ice-volume relationship (linear or else) and not in regard to the phase estimation (see above). Regarding the type of relation (Fig.6), I agree with Peter Huybers that for the data shown a linear model is not worse than a log model. (But Ruddiman's rejoinder not to ignore physics is OK. One might leave the log model for didactical reasons.)

P54L23 Define "GCM".

P56L1 and P56L17 and P63L19 "Lisiecki" instead of "Lisecki".

P59L24 "Ridgwell" instead of "Ridgewell".

P63L18 "Imbrie" instead of "Imbre".

P65L19 "Stauffer, B." instead of "Stauffer, J.".

P65L20 "concentration" instead of "concentrations".

P65L23 "The year is 1985 and not 1987. This has to be changed also in the citation(s) in main text.

P66L5 "Chappell, J." instead of "Chappell, J. M.".

P66L5 The page numbers are "137-140" instead of "137-138".

P66L11 Reference "EPICA Community Members 2004" is missing.

P66L11 "Yu E.-F." instead of "Yu, E.-I.". and "Bacon, M. P." instead of "Bacon, M.".

P66L15 "Korotkevich" instead of "Korotkevitch".

P66L26 "[Greek delta][superscript 18]O" instead of "O [superscript 18]".

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P66L48 “[Greek delta][superscript 18]O” instead of “d [superscript 18]O”.

P67L1 “Berger, A.” instead of “Berger, A. L.”.

NOTE: The further references have only sporadically been checked. A careful examination of them might be appropriate.

P67L25 “Anwendung” instead of “Andwendung”. (Own assessment: In non-German publications, over 50% of citations of this reference contain at least one error.)

P68L1 “Mudelsee” instead of “Muddlesee”.

P71, Figure 2a “Imbrie et al. (1992)” instead of “SPECMAP (1992)”.

P72, Figure 3 “Imbrie et al. (1993)” instead of “SPECMAP (1993)”.

P 78, Figure 9b The units given for “ice mass balance” is “m/yr”. It is not clear from this figure how to calculate mass (kilograms) from metres.

#### References

Broecker WS, Henderson GM (1998) The sequence of events surrounding Termination II and their implications for the cause of glacial-interglacial CO<sub>2</sub> changes. *Paleoceanography* 13:352-364.

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Mudelsee M (2001) The phase relations among atmospheric CO<sub>2</sub> content, temperature and global ice volume over the past 420 ka. *Quaternary Science Reviews* 20:583-589.

Mudelsee M, Schulz M (1997) The Mid-Pleistocene Climate Transition: Onset of 100 ka cycle lags ice volume build-up by 280 ka. *Earth and Planetary Science Letters* 151:117-123.

Shackleton NJ (2000) The 100,000-year ice-age cycle identified and found to lag temperature, carbon dioxide, and orbital eccentricity. *Science* 289:1897-1902.

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Interactive comment on *Climate of the Past Discussions*, 2, 43, 2006.

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