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Interactive comment on "Ice-driven CO_2 feedback on ice volume" *by* W. F. Ruddiman

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General comment

The paper considers the feedbacks between icevolume variations and atmospheric CO2. It is postulated that amplification of 40Ky glacial variability by CO2 accounts for the dominance of 40ky glacial cycles during the early Pleistocene and that CO2 feedbacks are involved in setting the rate of reglaciation and deglaciation during the 100Ky glacial cycles of the late Pleistocene. Understanding the role CO2 plays in glacial cycles remains an important climatic question. This manuscript helps clarify the influence CO2 may have had on glacial variability throughout the Pleistocene.

Specific comments (ordered according to importance):

A main topic of the manuscript regards the early Pleistocene 40Ky problem — that there is almost no precession period variability but strong obliquity period variability. It is proposed that CO2 selectively amplifies glacial sensitivity to variations in obliquity

and not precession. A concern with the manuscript is the lack of rational for why CO2 variations would enhance obliquity over precession period variations. What is it about the obliquity forcing during the early Pleistocene that leads to its enhancement by CO2 over precession? If CO2 follows the glacial state, then there still remains the major question of why climate is more sensitive to obliquity than precession during the early Pleistocene. While a number of physical mechanisms are cited which could influence atmospheric CO2 concentrations, it seems that no real mechanism has been provided to explain the presence of greater obliquity than precession period variability.

Later in the manuscript (page 63) CO2 is invoked to amplify the precession period forcing. Elsewhere it is also argued that CO2 accounts for the long interglacial following termination five. This gives the impression that CO2 is a panacea, resolving a host of unexplained features of ice-volume variability. Perhaps this is the case, but why CO2 would variously enhance different external forcings and at other times vary freely is not intuitive and, at the least, requires some discussion. In effect, it is argued that this is what CO2 would of had to do in order to explain the record, but a more detailed explanation should be sought.

At numerous points in the paper the reader is asked to choose between a climate in which there is a fast CO2 response driven by glaciers or a slow glacial response driven by CO2. Further distinctions are partly semantic, but what about a fast glacial response to CO2? More generally, if icesheets and CO2 nonlinearly interact, describing their relationship as cause and effect seems insufficient, as each drives the other. Presumably this sort of feedback motivates the title of the paper. It is suggested to use terms such as "interaction" rather than terms implying a strict cause and effect.

Another issue regarding terminology is the use of the phrase the "100 000 year eccentricity band". This would seem to pre-suppose that the late Pleistocene glacial cycles are caused by variations in eccentricity. But as is argued elsewhere in the manuscript, precession, obliquity, CO2 (and likely other internal climate variations) all play possible parts in the 100 000 year glacial cycles.

At the beginning of section 3, the discussion of time-series analysis seems out of place. First, general discussion of temporal variations in CO2 and ice-volume itself qualifies as time-series analysis. The discussion of ascertaining leads and lags using sinusoidal basis fucntions really only pertains to Fourier analysis. As there is later extensive discussion of leads and lags at particular periods (implicitly assuming a Fourier representation of the data) either this section should be revised or some other objective method for quantifying the relative timing of events should be utilized. Note that Fourier analysis of leads and lags would inform regarding the relative overall timing of CO2 and ice-volume signals even though, as is suggested, the representation of the sawtooth structure of the records requires spectral energy distributed over a large bandwidth. Indeed, my Fourier estimates of the relationship between CO2 and proxies of ice-volume indicate that within age-model uncertainty the two quantities are in-phase with one another (i.e. large ice-volume when CO2 is small).

Starting at line 5 on page 51 the spectral energy is partitioned according to the energy residing at the 100Ky, precession, and obliquity bands. The sum of this energy should not, however, account for 100\% of the variability as it is clear that much of glacial variability does not reside in the orbital bands.

The logarithmic relationship between temperature and CO2 is discussed at many points (e.g. page 53, line 10). However, as shown in Figure 6 the difference between glacial and interglacial CO2 is small relative to the logarithmic scaling and, thus, the glacial-interglacial temperature changes associated with changes in CO2 can be well-approximated as linear. Furthermore, as the logarithmic relationship between temperature and CO2 is itself approximate, the linearization of the relationship seems appropriate.

Near line 25 on page 52 it is argued that greenhouse gases were the dominant feedback at the last glacial maximum. The tally presented in the manuscript indicates that 50% of the cooling can be attributed to greenhouse gases, this when the albedo affects of changes in sea-ice are included as part of the greenhouse gas tally. It seems fair

to say that greenhouse gases were an important contributor to changes in global temperature, but not that they dominate. Possible influences on temperature from clouds, water vapor, atmospheric and ocean circulation, etc. could also be noted.

On page 54 line 6 the statement that the longer period associated with obliquity variations will lead to larger amplitudes of ice-volume variations assumes that ice-volume is the integral of the variations in insolation forcing. This argument would fail if icesheets rather come into equilibrium with the amount of radiative forcing, as is also plausible.

The discussion on page 60 line 26 of Huybers and Wunsch (2005) indicates that only obliquity was invoked to explain the 100 ky glacial cycles, but in common with the present manuscript, we discussed how multiple obliquity cycles must elapse before a termination could be triggered. In particular, we speculated that basal ice-sheet conditions were important for determining when a deglaciation can be triggered. Please also note that the discussion on page 62 line 18 relating to sufficient ice building up over two or three obliquity cycles prior to a termination occuring is similar to our discussion of the topic.

On page 62 line 10 it is argued that positive anomalies in obliquity and precession must align in order for a termination to occur, which is an appealing description, but which unfortunately fails as a description for many late Pleistocene terminations, termination five being a notable example.

Technical comment:

Regarding page 53 line 5: the wording makes it confusing whether the Bassinot or SPECMAP stack is being discussed.

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