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

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Manfred Mudelsee's review makes numerous helpful points that will be useful in the revision of my paper. His main point bears on the inadequacy of my discussion of the small CO2 lead versus the O18 (˜ice volume) response. This criticism has made me realize that I erred in shortening that part of the paper (p. 48-51) to try to keep it focused. In so doing, I omitted important arguments and caveats.

My revision will have more or less the following structure. I will again point out that both cross-spectral (Fourier) analysis and simple 'eyeball' inspection show that the CO2 lead versus O18 (an ice-volume proxy) could not possibly be as large as 12,000-15,000 years, as concluded by Imbrie et al. (1993) and Shackleton (2000). The cross-correlation analysis of Mudelsee (2001), which I will cite, supports this conclusion. This evidence eliminates the possibility that CO2 acts as a very early forcing of extremely 'sluggish' ice sheets.

I will also again note that a smaller CO2 lead over marine O18 exists in cross-spectra at the 23,000-year cycle and on terminations. And I will add the 2700-year CO2 lead

from Mudelesee's analysis. Most papers have implicitly or explicitly viewed this CO2 lead as evidence that CO2 forces ice sheets. I proposed instead that CO2 and O18 are so nearly in phase as to suggest the opposite interpretation — that ice-sheet control of CO2 is the dominant process. As part of this interpretation, I suggested that the ˜5,000-year CO2 lead over O18 at the 23,000-year cycle could have produced a small overall CO2 lead on terminations, despite the primary relationship of ice control of CO2.

As this point in the manuscript, I omitted a full discussion of alternative interpretations. The most obvious other possibility is that the primary process at work in the CO2/ice relationship is CO2 forcing of ice volume with a small ice lag. This interpretation requires a very rapid ice response to CO2 forcing. One way to evaluate this possibility is to use the arctangent relationship from Imbrie et al (1984) to calculate the time constant of ice response required to produce a lag of 2700 years to CO2 forcing at a period of ˜100,000 years. This calculation yields an ice time constant of ˜2700 years, nearly the same size as the observed lag.

The question is whether this small an ice-sheet time constant can be justified. While it does not sound unreasonable for the rapid disintegration of marine portions of ice sheets via calving to the ocean, it is more problematic for the bulk of interior ice. The strongest argument against such a short response is the marine O18 lags of 5,000-8,000 years behind summer insolation forcing at cycles of 41,000 and 23,000 years. These lags were the basis for the widely used SPECMAP marine time scale, and they require mean ice-sheet response times of 5,000 to 10,000 years (Imbrie et al.,1993; Ruddiman, 2003). An ice time constant of 2700 years lies well below this range of values. I will add a discussion of this CO2 forcing interpretation to my revision.

An additional comment (and a request): I searched the literature for any papers that proposed direct ice-sheet control of CO2 changes, but found only the one by Clark et al (1999), who noted this possibility only in passing. If anyone knows of other papers that have come to such a conclusion, I would like to cite them in the revision.

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