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

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I appreciate the careful and thoughtful review from Peter Huybers on the paper I submitted to COP. My revision of the paper will reflect his input. Here I respond to his main points.

Huybers asks why I "asked readers to choose between a fast CO2 response driven by glaciers or a slow glacial response driven by CO2". He suggests that I should "use terms like interaction rather than terms implying a strict cause and effect". Response: This comment goes right to the heart of my paper. In my opinion, saying that CO2 and ice volume are '"interactive" or "part of a highly coupled system" is true, but also a truism. Generalized terms like that don't really tell us anything useful about the actual linkages or mechanisms. The main purpose of my paper was to pose two clear alternatives that can be tested against paleoclimatic data and evaluated on their merits. Either CO2 drives ice sheets with a lag, as many papers have said or implied. Or ice sheets drive CO2 with little or no lag ($<$1000 years). The evidence I summarize suggests that ice sheets mainly drive CO2 (except at the 23,000-year cycle).

Along this same line of thought, Peter asks whether I shouldn't consider other options, such as a fast ice-sheet response to CO2. Such a response could complicate the simple either-or choice that I posed. Response: Marine-connected portions of ice sheets can obviously respond quickly to forcing, which presumably would include CO2. But a range of evidence continues to show that the vast bulk of the ice sheets lags thousands of years behind summer insolation forcing. This conclusion was implicit in the paper by Hays et al. (1976) that substantiated the Milankovitch hypothesis and in the work of Imbrie et al. (1984) that defined the SPECMAP time scale (still widely used). Steadily improving constraints on ice volume from U-series dating of coral reefs and sea level have not changed the assessment that the bulk of the ice-sheet responses lag well behind insolation forcing.

Peter also asks why the 41,000-year signal is enhanced by CO2 feedback while the 23,000-year signal is not. Response: The reason for this difference obviously depends on the mechanism(s) that link ice volume and CO2, and at this point those links are not known with any certainty. Because of that, I decided that this issue goes well beyond the scope of this paper. It may however, be relevant to note that some of the ice-CO2 links I proposed in the paper show very weak 23K power compared to 41K — for example, dust fluxes in most regions (the fertilization mechanism of Martin, 1990), and changes in depth of NADW penetration in the North Atlantic (the polar alkalinity mechanism of Broecker and Peng, 1989). These observations are consistent with the idea that strong CO2 feedbacks operate at 41K but not at 23K (whatever the reason).

Huybers says that I cite so many roles for CO2 that it "gives the impression that CO2 is a panacea". Response: I would characterize what I have done differently. The first half of the paper summarizes a range of evidence that reveals the relationship (phasing) between CO2 and ice volume. The latter half of the paper then explores the direct implications of these findings for a wide range of orbital-scale issues. Thinking through the implications of one's findings is standard scientific methodology. The fact that I found many possible implications that arise from CO2 being a source of feedback at

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41,000 years and at ˜100,000 years, but part of the forcing at 23,000 years, could be viewed as one of the major strengths of the paper, rather than a 'panacea'.

Huybers questions my use of the term "100,000 year eccentricity band" because it seems to pre-suppose the cause of the late-Pleistocene glacial cycles. Response: My paper cites specific links of ice volume to eccentricity (via modulation of precession) that were implicit in Broecker (1984) and more explicit in Imbrie et al (1993) and Raymo (1997). I agree with those authors that eccentricity plays a role (along with other processes I discuss in the paper) in pacing the oscillations we once called "100K cycles" (but will now probably call "pseudo-periodic oscillations centered around a period of approximately 100,000 years").

Despite Peter's criticism about the weak logarithmic relationship between CO2 and global mean temperature, it seems to me premature to dismiss the possible effects of such a well-known non-linearity on long-term ice-volume responses.

My analysis of feedbacks at the last glacial maximum implicitly included the large and well-known effects of water vapor and the poorly known effect of clouds. These feedbacks were embedded in the global-mean climatic responses to greenhouse gases and ice albedo. I will make this point clear in the revision.

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