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

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I thank Peter Kohler (2006) for his helpful comment, which will help clarify the revision. Peter focuses mainly on several mechanisms by which ice sheets might have controlled CO2 at the last glacial maximum: northern sea ice, ocean carbonate chemistry, terrestrial carbon storage, and sea level. He notes the large uncertainties in our current understanding of these processes, and he concludes that taken together they suggest little or no ice-sheet control of CO2, contrary to my claim. I have two basic responses to this conclusion.

First, the mechanisms I cited in my paper were for the most part not the ones Kohler lists at the end of his. I specified dust fertilization, polar alkalinity, and polar sea-ice and stratification. I mentioned these processes for two reasons: (1) proxy records show that they have the right phase and periodic behavior to link ice volume and CO2; and (2) other scientists have made the case that these factors can drive changes in CO2 (although not specifically in the context of their having been controlled by ice sheets). I also noted that the amplitudes (and even the direction) of the effects of these

processes on CO2 are highly uncertain, and there is an obvious need for much more work in this area.

Second, and more fundamentally, the main focus of my paper was not on mechanisms, but on what proxy records of the relative phasing on CO2 and ice volume can tell us about how the climate system has operated. I concluded that the in-phase behavior of ice volume and CO2 at 41,000 and ˜100,000 years is telling us that ice sheets have controlled the CO2 signal at those periods, but not at 23,000 years. These results seem to me to be basic facts that future research should explore.

Kohler also mentions the sequence of events early in the last deglaciation. 'Early' responses have been evident in the Southern ocean since the CLIMAP era. I noted in my paper that a small part (˜10-20%) of the CO2 response (the component at 23,000 years) has this early phase and thus acts as a forcing of ice volume. Also, in a previous paper (Ruddiman, 2003), I suggested that this early CO2 response arises from winter insolation forcing of sea ice in the Southern Ocean. In any case, this early CO2 response is independent of northern ice sheets and might contribute to the initial CO2 rise and some amount of ice forcing early on terminations.

However, I think it would be unwise to focus only on the early Antarctic response. Northern hemisphere air temperatures over Greenland also responded very early on termination I (Alley et al., 2002). And, with rising summer insolation levels in northern latitudes, heating of Asia also began early and contributed to an early increase in atmospheric methane concentrations. All of these responses significantly precede ice volume. The chicken-and-egg problem —"which came first"? — on terminations is very complicated.

Once deglaciations get underway, however, I think that the more fundamental CO2 role — providing positive feedback to ice melting — takes over. Terminations primarily result from feedbacks (Imbrie et al., 1993), and I make the case in my paper that CO2 is likely to be the strongest of all the feedbacks. Viewed as a whole, the small lead of

CO2 versus ice volume on termination I is just the result one would expect if a small fraction of the CO2/ice phasing reflects early CO2 forcing of a lagged ice response at 23,000 years, while most of it reflects the fast (in-phase) CO2 feedback at 41,000 and ˜100,000 years.

References

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