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Climate of the Past Discussions

Interactive comment on "Past temperature reconstructions from deep ice cores: relevance for future climate change" *by* V. Masson-Delmotte et al.

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This wide-ranging paper by Masson-Delmotte et al. offers a thoughtful case for the role of obliquity in global (and particularly polar) climate changes. In general, the methods and assumptions used and their associated uncertainties are clearly stated. I offer a few suggestions for possible future improvements.

In some respects, the paper seems like two (three?) papers combined into one. For example, p. 403 has the usual introductory summary of the sections that follow, but then p. 409 repeats the same exercise. I also had the impression that the first part of the paper went into unnecessary detail about things that are already widely known

about the climate system and ice-core parameters, so it could be shortened.

I fully agree with the authors that the role of obliquity needs to be considered in natural orbital-scale variations (see for example, Ruddiman, 2006a). But it seems to me that in making the case for the role of obliquity, some important considerations that support the opposite conclusion were downplayed or omitted.

For example, the authors emphasize evidence favoring strong moisture fluxes at the 41,000-year cycle as a mechanism for ice-sheet growth, yet the literature contains many examples of GCM sensitivity tests in which moisture fluxes and snow accumulation decrease considerably in the regions where ice sheets grow because cooling reduces the amount of water vapor in the air. In some experiments, this happens even though the moisture flux from low to middle latitudes increases. The extra moisture simply fails to make it all the way to the ice. Also, glaciologists familiar with ice-sheet mass balance tend to favor ablation as a more powerful factor than accumulation (Alley, 2004; Denton et al., 2005).

In making the case for the role of obliquity, the authors do not convey any real sense of the reason why others have emphasized precession as an important factor, especially during interglaciations. Broecker et al. (1968) first found U-series evidence from coral-reefs that high sea levels during interglacial isotopic stage 5 were primarily a response to changes in precession. Also, the prominent isotopic substages in marine stages 5 and 7 named by Shackleton (1969) have long been understood as being precession-related features. And while the community has not yet come to any consensus about the cause of orbital-scale CO2 changes, the trends show a close correlation with marine isotopic records, and at least part of this occurs because a common precession signal is present in both records during interglaciations.

The authors also say: "both the causes for natural CH4 fluctuations and the role of precession in driving them remains uncertain". I do not see this as an accurate summary of previous work. Chappellaz et al. (1990) and several subsequent ice-core studies

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have recognized the strong 23,000-year precession period present in methane signals during interglaciations and have linked it to orbitally driven variations in tropical monsoons (Kutzbach, 1981). Recent speleothem studies (Yuan et al., 2004) have shown very large 23,000-year variations in Asian monsoon circulations occurring at the phase predicted by the Kutzbach hypothesis (see Ruddiman, 2006b for a recent viewpoint on this issue).

The question remains how to align stages 1 and 11. The authors clearly favor the EPICA (2004) alignment and note that it was in effect based on obliquity. In contrast, the alignment I chose (Ruddiman, 2005) is based on the precession signal. Precession maxima and minima in stages 1 and 11 match those in Milankovitch's caloric half-year insolation index to within 1000-2000 years, because precession dominates even when the eccentricty amplitude is low.

Another point for thought: I recently concluded (Ruddiman, 2006a) that CO2 acted mainly as an in-phase positive feedback to the ice sheets during glacial-interglacial oscillations. Here, the authors compare these past changes to a future world in which CO2 will act (initially at least) as a forcing of climatic change. Are the effects of CO2 forcing and feedback directly comparable?

Minor issues:

p. 420: I recall that EPICA (2004) said that 12 kyr have elapsed in the current interglaciation, leaving another 16 kyr (not 12 kyr) of remaining warmth.

p. 421: I agree there has been no major Holocene cooling in the Antarctic region, but this can also be interpreted as the result of humans having kept late-Holocene climate warm.

p. 421: To my knowledge, the geologic evidence does not show any case during the last 2.75 Myr in which northern land masses have been free of ice sheets for 60,000 years. By accepting the argument that we are now in a gap of that size for natural rea-

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sons, the authors are in effect accepting the argument that nature has suddenly begun to exhibit unprecedented behavior. Is that really the 'least-astonishment' explanation?

Finally, my name only has one 'n' at the end (Scottish ancestry).

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