

## ***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 manuscript is interesting and valuable as a review of the perspective on future climate change gained from ice core analyses. Future climate here refers both to anthropogenic global warming and to the timing of initiation of the next ice age, both tremendously important topics. The main reason for having a discipline of paleoclimatology is to address issues like these, so the relevance to the journal is obvious, and the interest to a broader audience also obvious.

The primary difficulty for rigorous analysis here is that the paleoclimate record is an empirical reconstruction whereas future climate changes need to be modelled. The

only way to bridge this logical gap is to also use the models to simulate the paleoclimate record. The authors have done this using LGM simulations. Thus they have avoided the clear danger of producing a bad manuscript and instead have produced a rather good one.

The manuscript covers a lot of territory. The writing style is not particularly good, but clarity is sufficient that the reader can interpret the discussion well, though without enjoying the process.

I think some of the points made in the manuscript are really very good. In particular they make the long overdue argument that there is no perfect orbital analog for the present, and that obliquity certainly matters in the comparison of MIS11 to the present. Also one main point is that the LGM simulations underestimate the cooling relative to present. This is a tremendously important issue, as such simulations provide one of the few ways to evaluate the estimated magnitude of fast feedbacks in climate models. It is too bad that the manuscript here does such a minimal job of exploring this issue, devoting only a few sentences to noting the absence of aerosol and land surface forcings in the models but without citing the substantial literature on the topic. Yet elsewhere in the manuscript many paragraphs are devoted to review of elementary and widely known concepts.

This illustrates a general weakness of the manuscript – the authors try to combine such a wide range of topics into one manuscript that all of the important ones are given too little discussion. But I suppose there is value in having it all together in one place, it does make the reader think about it as a unified problem.

The referencing is not particularly good. There is a strong tendency to cite papers from the last few years without mentioning more thoughtful and original works from earlier.

The only major conceptual problem I had with the manuscript concerns the very odd choice of time frame for comparisons of temperature change rate. To compare periods of rapid warming in model and reconstructed worlds, it is necessary to choose a time

period over which the rate of temperature change is calculated. The authors appear to use one millenium. Such a long period guarantees that the rapid warmings in the Greenland record will be brought down to the level of the future anthropogenic warming. Why this choice? It makes no sense either from the perspective of human interest (one to two centuries would be better) or climate physics (the rapid warmings are much debated and widely known now outside the paleoclimate community). Skeptics will suggest that this choice of long time interval is designed to make the results sound most dramatic.

A few other thoughts: Incorporating the deuterium excess 'correction' to the Vostok record makes the obliquity signal in the temperature history weaker. Does this matter?

p. 409. The authors (some of them) for the first time acknowledge the borehole temperature results for the Holocene are a major issue for their simple interpretations of Eemian warming. They should look at the analysis I did with Shawn Marshall again (Nature 2000) to see what the result is of an analysis that simultaneously honors the borehole data and the ice sheet elevation effects. The Eemian warming we calculated does seem unreasonably large, but it is what the data imply when treated consistently.

p. 413 "period from 800 to 60 ka" should be "800 to 600 ka"?

p. 423. Why are the "two main features ... involved in the latitudinal temperature gradients" cited here more important than the obvious difference in latitudinal distribution of forcings? The albedo effect is supposed to be 60% of the ice age forcing, and is concentrated in polar regions, whereas the CO<sub>2</sub> forcing is global. Not sure what "asymmetry induced by prescribed northern hemisphere ice sheets..." means in the preceding paragraph.

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