Clim. Past Discuss., 6, C1642–C1645, 2011
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Interactive Comment

Interactive comment on "Glacial cycles and solar insolation: the role of orbital, seasonal, and spatial variations" *by* R. K. Kaufmann and K. Juselius

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Received and published: 23 June 2011

My reaction and concerns regarding this manuscript very much echo those raised Michel Crucifix in his earlier review. The involvement of professional statisticians in climate research should be encouraged and can be of great value, but I am deeply concerned regarding how insolation forcing was prescribed and how chronology issues are dealt with in the present analysis. I should also note that these concerns are essentially the same as those that I expressed privately to the first author some years ago when he shared these results with me, and I find it unfortunate that these issues have not been adequately addressed in the manuscript nor in the reply to M. Crucifix.

With respect to the various forcings that are applied and interpreted, I concur with M. Crucifix. The seasonal and orbital forcing functions are either colinear or could be made to be colinear, such that the distinction drawn between different varieties of forcings seems largely, if not totally, arbitrary. Note that insomuch as the orbital forcing functions are not colinear, this would essentially only reflect the choice of phase for precession, the sign of obliquity, and the relative amplitudes of these two forcing components, with one important exception involving eccentricity that I get to below. The distinction between the skill of insolation and cumulative summer energy is puzzling in the same regard because, atleast for calculation using the threshold of 275 W/mˆ2, the resulting variability is essentially a linear combination of obliquity and precession. (Using a threshold of 550 W/mˆ2 is too high to be a practical contender for driving glacial cycles because only when eccentricity is large will one realize non-zero forcing.)

Returning to eccentricity, this orbital element is generally argued to not directly drive glacial cycles for two reasons: 1. its influence on insolation is small, and 2. there is a blatant mismatch near 400 ky and 800 ky ago with the proxies considered in this study. This study appears to side-step these two issues in two ways. 1. In any measure of insolation considered in this manuscript, eccentricity will be a much smaller constituent than the other orbital effects, but when eccentricity is alone used as a forcing agent, it can contribute arbitrarily large amounts of forcing, as done for one version of the second hypothesis which perhaps explains the inferred skill of that result. 2. Only the last 391 ky of climate variability are considered, which is troubling given that the records used here actually extend further back in time, suggesting some arbitrary truncation. It is known that variability during stage 11, just prior to when the records are truncated, does not follow the seemingly apparent relationship in the more recent glacial cycles (e.g. see the textbook by Muller and MacDonald, 2000). The omission of older data and the ability of eccentricity to provide arbitrarily strong forcing calls into question the inferences drawn from the model.

With respect to chronology, the analysis and discussion provided in the manuscript and response to M. Crucifix is inadequate. Chronological errors are not random, because the time series used for these studies have been adjusted, or tuned, to the very orbital

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variations that are explored as forcing agents. That is, the time values ascribed to individual observations are not independent of the exogenous forcing variables that are introduced. The fact that the forcing found to be most important in this study also plays a leading role in how time was estimated for the climate variables could very well be more than coincidence. The results will not be convincing before the authors use climate records whose timing has not been estimated using aspects of the forcing that they wish to explore or once the rather complicated statistical relationship that is built-in by this situation is explicitly accounted for.

Other notable issues:

It is argued that the climate signals analyzed here are non-stationary or, "nearly non-stationary", because of presence of persistance. This seems inappropriate. A sine wave shows persistance, but is not non-stationary. I am aware of no actual demonstration that late-Pleistocene climate is non-stationary, and it should be noted in this manuscript that non-stationarity is an assumption.

Degrees of freedom. After citing Yule (1929), it seems surprising that 391 degrees of freedom are assumed to be present because these are "the number of observations". First, this is the number of points that the data were interpolated onto and does not reflect the actual number of observations. Second, the data are highly auto-correlated, as is emphasized in this manuscript, so that the degrees of freedom will be less than the number of observations.

More minor concerns:

p2562 "increments of a deterministic trend are constant". I don't understand this. If $y=t\hat{}2$, the trend is deterministic but for equal increments of t, the change in y will not be constant.

p2564 "whereas for a stationary, non-trending time series the best guess would be the average value". This is only true, in the this context, when rho=0. For rho <1 and >0, it

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will be some value between the average and the prior value.

Hypothesis 1: both Imbrie et al (1993) and Kukla et al (1981) call upon essentially obliquity and precession, but here annual average insolation at 65N is described, which would have no precession component and depend almost entirely upon obliquity, as appears to be the case in Figure 1. A different discussion/motivation for using this forcing should be offered.

Huybers and Denton (2008) actually argued the duration of the summer was the more important control on ice core measures of temperatures because of the nonlinearity associated with blackbody radiation. Also, Huybers and Denton (2008) did not drive a model with cumulative annual insolation, but instead used the full seasonal cycle, which was a central point of that paper. Finally, Huybers (2006) discusses summer energy, not Huybers and Denton (2008), which is here referred to as cumulative annual insolation.

p. 2578 I do not follow the logic that previous suggestions that the size of an icesheet determines whether a deglaciation will ensue from orbital forcing are unlikely because using insolation at other latitudes does not yield better results. The more fundamental issue is whether nonlinearities are present in the climate system that the co-integration model does not capture, which seems almost certainly the case.

The references for Jesulius (2006), Huybers and Wunsch (2005), and Yule 1929 are missing from the bibliography.

Interactive comment on Clim. Past Discuss., 6, 2557, 2010.

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