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Climate of the Past Discussions

Interactive comment on "Glacial cycles: exogenous orbital changes vs. endogenous climate dynamics" *by* R. K. Kaufmann and K. Juselius

Anonymous Referee #2

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In this manuscript a linear statistical analysis, cointegrated vector autoregressive model (CVAR) is applied to paleoclimatic data in order to empirically investigate the connection between different climate variables and external orbital changes in insolation. CVAR is a technique which has mainly been applied in analyzing financial data, where little is known about causal relationships among variables. In climatology on the contrary a lot is known based on physical understanding of the dynamics. The conclusions drawn solely on statistical grounds should be always tested against our physical understanding of the problem. Thus a statistical relationship does not guarantee a causal relationship. In that sense I agree with reviewer # 1 that the manuscript reveals some lack of knowledge of knowns and unknowns in climate dynamics and especially what the analyzed climate proxies represent.

The analysis is interesting and could potentially add to our understanding, thus I recommend the manuscript published after some revision, especially with respect to interpretations in terms of climate dynamics. The overall conclusion that glacial cycles are due to the orbital forcing and not solely internal dynamics, confirms Milankovitch theory, which is hardly scientific news. And even that conclusion cannot be drawn based on the analysis, unless it can be argued that there are no (unobserved, unanalyzed, hidden or forgotten) internal (endogenous) variables which could do the job. Not that I think there are, but this is based on physical reasoning.

The most important finding in this study is the analysis of the relative importance between the different components of the orbital forcing in the observed temperature record. This part is actually difficult to read from the manuscript. For instance it is stated that precession and eccentricity together with obliquity (on top of insol0) is better than only obliquity (on top of insol0). This is seen by comparing models 2a and 2e in figure 3b. That is almost impossible with 9 curves on top of each other. Furthermore, in this case, as also objected by reviewer # 1, there should be some penalty for the use of more records to fit the data, or at least an explanation for why not.

In the same way it is rejected that cummulated summer insolation explains much. However, that is done by comparing models 2c and 2d, where the latter contains four seasons records, and both contains insol0. This is obscure to me. The use of cummulated summer insolation is based on physical reasoning (melting period for the glaciers and Keplers 2. law to explain why the precessional is sub-dominant) which to me is quite strong, also compared to empirical significance tests. (see Huybers and Denton, 2008).

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Specific comments:

The introduction gives a brief overview of the theories of glacial cycles with some inaccuracies: The introduction of the 65N mid-summer insolation is due to Milankovitch, and often exactly this component of insolation is referred to as the Milankovitch forcing. This is not arbitrary, but based on the reasoning that that is the latitude where the (southern rim of) the ice sheets are waxing and waning. It seems to me that discussing the 100-kyr problem without discussing the stage 11 problem and the Mid-Pleisocene transition is missing essential information. The authors argue in the response to reviewer # 1 that they only analyze the last 391 kyr in order to avoid the "non-stationarity" of the mid-Pleistocene transition, but that is around 800-1000 kyr BP, thus the full EPICA record should be safe to analyze. For the Milankovitch theory and the 100 kyr problem there are much newer references, which the authors should consult (Palliard, Nature 391, 1998, Huybers, Quat. Sci. Rev., 26, 2007, Ditlevsen, Paleoc., 24, 2009, Huybers, CP, 5, 2009).

The data analyzed (Table 1) should be presented graphically in one figure. It will then be apparent by, say, plotting Temp, CO_2 and (-Ice) that these three records are so strongly correlated that very little information can be gained from using all three and not just one, especially taking the uncertainty in the proxies into account. The authors should comment on this. (The comment on page 599, line 22-24, makes absolutely no sense to me).

The CVAR analysis is new to most climatologists (including this reviewer), thus it is worth explaining its virtues in a little more detail. First of all, how do the authors know that the analyzed records are not stationary? After all, AR models have also been used for modeling climate data (Roe and Steig, J. Clim, 17, 2004). The equation (1) describes the evolution of $\Delta \mathrm{Temp}_t$ etc. Is this $\mathrm{Temp}_{t+1} - \mathrm{Temp}_t$? If so, is the CVAR

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process (1) just an AR(2) process? What is the difference? It is also hard to follow the point 3. (p 591, line 15): The two quantities $(\text{Temp} - b_1\text{CO}_2 - b_2\text{Insol} - b_{01})$ and $(\text{Temp} - b_3\text{Ice} - b_4\text{Insol} - b_{02})$ should be explained better.

Minor comments:

p 588, line 8: is this a recently developed technique (Johansen 1988 or something newer?)

p 588, lines 16-19: In this manuscript there are only intercomparisons between linear models. It is not clear to me that the (best among those) is so accurate (in some absolute sense), that it out-rules non-linear dynamics to be essential.

p 289, line 8: CO_2 and methane are not local quantities, they are for all practical purposes well-mixed in the atmosphere.

p 592, line 23: The quantity R^2 is non-standard in climatology and must be explained. Here it is stated that it is in general misleading to use R^2 for nonstationary variables, however this is being done anyway (Table 3). Please explain.

p 593, line 16: "changes in orbital cycles" should perhaps be "changes in Earth's orbital parameters".

p 595, line 23: The π matrix? Is that the matrix with γ's? (p 607 it is the Π matrix). Please define, and be consistent.

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p 600, line 8 and other places: Level = Sea level? Please spell out.

p 602: Models 2f-g are, as I see it, not shown anywhere? And again, the figures 3a-c are to busy to read.

p 604, lines 3-8: This seems to be quite essential, should the quality of a model be judged not only by its ability to fit the data, but also by having "sensible" coefficients. To me the latter seems to be equally important, but it is never quantified.

p 604, lines 13-19: To repeat reviewer # 1's point: The analysis of the 100 kyr problem should include the last 8-10 glacial cycles, not just 4.

p 605: It seem to me that the analysis of Northern vs. Southern hemisphere is to simple. Parts of the insolation are in phase (ecc. + obl.) parts in anti-phase (prec.) between the two hemispheres, and the hemispheres are not uncorrelated. Thus an apparent correlation could as well be a lagged response to the insolation in the other hemisphere. Some physical reasoning would be helpful.

Interactive comment on Clim. Past Discuss., 6, 585, 2010.