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Interactive Comment

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

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We thank anonymous commentator #1 for taking the time to read and comment on our manuscript "Glacial cycles: Exogenous orbital changes vs. endogenous climate dynamics." We strongly disagree with the comments. They clearly indicate that the commentator has misinterpreted the statistical methodology, its results, and its overall goals. Although they do not point to any substantive flaws, in some cases the commentator's points can be used to modify the manuscript to clarify issues. In the following paragraphs we reproduce each comment, describe its flaws and point to text in the manuscript that anticipate the comment. Note that some of the commentator's points have several foci and so the numbers below do not correspond to the commentator's numbering scheme.

Point #1

I am furthermore not convinced at all that the statistical technique they are using can provides any new information to the problem of glacial cycles. A good illustration of their conclusions is given in Table 5: Ice volume is linked to southern insolation (South 70N) and sea level is linked to northern insolation (North 60 N).

Table 5 does not indicate that "Ice volume is linked to southern insolation. . .sea level is linked to northern insolation" nor do we make such statements in the text. Rather, as described in section 3 on page 594, Table 5 reports results that are designed to test the hypothesis that insolation at 65oN generates the most accurate in sample simulation of temperature or any other of the ten endogenous variables. This section addresses an on-going debate as to whether glacial cycles are driven by solar insolation in the Northern Hemisphere or the Southern Hemisphere (references to this debate stretch from page 587-588). Table 5 reports results regarding which set of latitudinal variables for solar insolation generates the most accurate in-sample simulation for a given variable. For the time series Ice volume, the set of solar insolation variables at 70o South generates the most accurate in-sample simulation. This accuracy relative to solar insolation at other latitudes may be generated by a direct correlation between solar insolation a 70oS and Ice volume, or a correlation between solar insolation at 70oSouth and some other variable that allows that other variable to generate a more accurate in-sample simulation for Ice volume. And as described on page 605, our result is consistent with Huybers and Denton (2008), who find that using cumulative summer insolation in the Southern Hemisphere (as opposed to the Northern Hemisphere) to drive a single column atmosphere model generates the most accurate simulation of ice volume at Dome F. In summary, our modeling effort to identify the hemisphere/latitude where solar insolation has its greatest impact on glacial cycles is based on and adds to a considerable literature on this topic. We return to this issue in our answer to Point #14.

Point #2 The choice of data time series appear very arbitrary and it is not explained how it relates to the glacial-interglacial problem. There are several critical issues here: - Why using only 391 kyr when most data sets shown here are available down to about 900 kyrBP

We chose the sample period to avoid any instabilities in the relationship among variables associated that may be with the mid-Pleistocene revolution. Currently, the manuscript does not address this issue until point 4 (page 607-608) of the Conclusion:

"Test the hypothesis that the nature of glacial cycles changes over time. For most of the endogenous variables, data are available over the last 750 kyr. We will estimate the CVAR over this full period and test whether the long-term relationships and/or rates of adjustment change in a statistically meaningful way, with special focus on the so-called mid Pleistocene Revolution. We will also use the model to “backcast” the endogenous variables and compare the simulated values for ice volume over the last several million years, for which data are available."

As indicated by this point, we anticipate the commentator’s point. But we will move the caveat about the mid-Pleistocene Revolution to the data section in a modified version of the manuscript.

Point #3

In the manuscript, the authors are often mentioning "temperature" instead of "Antarctic temperature". This is not a detail...

The definition of temperature is given on page 589 “ Data for temperature, carbon dioxide, and methane are obtained from cores drilled into the Antarctic ice sheet, and therefore represent local conditions.” Once we have clearly defined this variable, we believe it would be redundant to repeat it throughout the manuscript. But we are happy to repeat it if readers believe there is some confusion.

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What is the relevance of Antarctic temperature to the glacial-interglacial problem? This question is even more critical for other variables such as CH4 or other chemicals found in the ice cores. Why using SST only in the Southern ocean ? If the authors are not aware of it, they should look at Northern hemisphere SST or other temperature records to see that data are very different at different locations.

There is no need to include different measurements of the same variable (e.g. geographically disparate measurements of near surface temperature) because the statistical methodology focuses on cointegration, which is a long-run relationship between variables. Yes, there are different time series for temperature and SST, but unless the commentator is willing to argue that there is no long-run relationship between near surface temperature or SST at different locations on the planet, then using a different time series will not affect conclusions about cointegration. The use of a geographically isolated measure of a climate variable (e.g. Antarctic near surface temperature) is common practice in efforts to estimate ΔT2x from paleoclimate data. For example, Kohler et al., (2010) compare the change in Antarctic temperature to the change in forcing since the last glacial maximum and modify this result with an estimate for the ratio of changes in Antarctic temperature relative to global temperature (Masson-Delmotte et al., 2006; 2010). We would address this point explicitly in a modified manuscript.

Point #5 The core phenomenology of the glacial-interglacial problem is the ice volume, or the sea level. I do not understand why the authors are using two conïňĆicting data sets for the same physical variable.

The hypothesis that ice volume and sea level are the "same physical variable" is rejected by the statistical results. If the time series for Ice volume and sea level represent the physical variable, there would be only nine cointegrating relationships (i.e. the same cointegrating relationship would describe Ice volume and sea level). Furthermore, the same cointegrating relationship would load into the equations for Ice volume and sea level and this too is rejected by the statistical results. Additional research aimed at identifying the cointegrating relationship (as described in the Conclusion on page 607) indicate that variables in the long run relationship for Ice volume and sea level are different as are the rates at which Ice and sea level adjust to changes in solar insolation and other variables endogenous to the climate system. Such results demonstrate the value of our approach. As stated on page 608, subjecting the hypotheses (including those described by the commentator) to the rigor of statistical analysis is one goal of our manuscript.

Point #6

The authors need to understand that paleoclimatic data are not without errors and should be taken for what they are: estimations of climatic variables, not direct measurements. In this particular case, if the data sets are inconsistent, this clearly means that at least one of them is not accurately estimating what it was meant to. Fitting both of them as two different parameters, and building theories on these results, is meaningless.

We are well aware of the errors in the data. We would ask that the commentator to wait with comments such as 'meaningless' until we report the results of the identified CVAR model and what it says about the long-run relationship among variables and their relative rates of adjustment. For example, commentator's argument implies that thermal expansion has no effect on sea level beyond ice volume. But preliminary results indicate that the CVAR model is able to quantify the effects of the thermal expansion of sea water. Such a result is consistent with our physical understanding of the climate system and provides further evidence against commentator's point #5.

Point #7

The "standard" insolation forcing is NOT the "cumulative annual solar insolation at 65_ N" (page 593, line 22). This forcing (insol0) is used throughout the paper as the standard one against which all conclusions are drawn. In contrast, the Milankovitch theory says that "Summer insolation at 65_ N" should affect the size of northern hemisphere ice-sheets and often the paleoclimatological litterature is using the daily insolation at

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the summer solstice.

We agree and this comment indicates that the commentator did not read the paper carefully. As described on page 599 we test Model 1(b), which specifies summer insolation at 65oN. And as described on page 599 “specifying insolation on the ïňĄrst day of summer, 21 June (Model 1b) instead of total annual insolation at that latitude has little effect on the ability of Model 1b to reproduce glacial cycles (Fig. 1a–c). For example, Model 1b is able to account for 31 percent of the variation in temperature.” Again, the ability to test (and reject) a widely held belief about the explanatory power of summer insolation at 65oN demonstrates the power of the statistical methodology.

Point #8

In this manuscript, the standard forcing used in Model1 (insol0) is almost a pure obliquity signal as shown by results on Fig.1a (Model1). It is not really a surprise that Model 2 and 4 (which account for seasonality in the forcing) are better than model 1 and 3 (which do not). I don't think this kind of result brings any information to the scientiïňĄc community.

We respectfully disagree. As described under the heading hypothesis #2 on page 594, a segment of the community argues that glacial cycles are caused by endogenous climate dynamics (as opposed to exogenous changes in solar insolation). If this is hypothesis correct, then adding seasonality in forcing would not improve the model's ability to simulate cycles. The result that Model 2 generates a more accurate in-sample simulation that Model 1 is not consistent with the hypothesis that glacial cycles are driven by endogenous climate dynamics. In this case, a result that may be obvious to the commentator is not obvious to those who argue for the importance of endogenous climate dynamics, and so constitutes an important contribution.

Point #9

The conclusion that the more complex model (model4) better ïňĄts the data is also obvious, since it has more degrees of freedom. Again, I don't see any value in this ïňĄnding. Standard statistical tools exist to deïňĄne a trade-off between number of degrees of freedom and goodness of ïňĄt (AIC or BIC criteria for instance). I am not sure model4 would stand such classical tests.

If the commentator's belief were accurate, then adding seven more endogenous variables to Model 3 relative to Model 1 also would allow Model 3 to fit the data better than Model 1. But this notion is rejected by both the statistical results and the in-sample simulations. Anticipating the commentator's misunderstanding, the bottom of page 600 reads:

"While adding variables should not diminish a statistical model's ability to simulate in-sample (additional variables that do not have a statistically measurable effect will be “zeroed out” by the estimation procedure), simply adding more variables does not improve a statistical model's skill as indicated by the performance of Model 3 relative to Model 1."

Furthermore, the commentator's notion that statistic tools would not favor Model 4 is rejected by the results reported in the manuscript. For example, adding variables to Model 2 to create Model 4 increases the number of cointegrating relationships (see bottom of page 595) and improves the accuracy of the in-sample simulations in a statistically meaningful fashion. Furthermore, the commentator has confused the statistical definition of degrees of freedom, which is the number of observations minus the number of parameters fit. For our data set, we have 357 degree of freedom for each equation of the CVAR. Clearly, the model is not overfit.

Point #10

4 - The authors have a strange notion of what "feedback" actually means. In paragraph 4.2, they "test" how model1 could be improved by adding "endogenous feedbacks". That model1 was doomed to fail is obvious (see point 2). But I don't understand why adding more data time series (chosen with unknown criteria) could brings more "feed-

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back mechanisms" to the model.

The notion of endogenous feedback implies that the climate system is out of equilibrium, and that this disequilibrium reverberates through the climate system and creates glacial cycles. The endogenous variables included represent various aspects of the climate system (e.g. sea ice, sea level, iron fertilization, etc.) that could transmit this disequilibrium and create the glacial cycles. As described above, just because the commentator does not adhere to this driver of climate cycles, there is a considerable literature that does (as described in our manuscript) and our expansion from Model 1 to Model 3 constitutes a legitimate test of this hypothesis.

Point #11

5 - Paragraph 4.6 and non-linearities. The authors state that "the only non-linear component is the asymptotic manner in which endogenous variable adjust towards their long-term equilibrium". Since the model is linear (see equations, and line 19 page 590), I do not understand how the authors can talk about non-linearities.

To summarize the arguments in section 4.6 on pages 603-604, if a linear model does a pretty reasonable job of simulating glacial cycles, then non-linearities, which are not present in the model, probably do not play a critical role in generating glacial cycles. Despite this general result, on page 603 we caution “This ability does not imply that glacial cycles are completely linear.”

Point #12

6 - More fundamentally, the method presented here is applicable to non-sationnary models that "cointegrate" (page 590 line 22), which means that some linear combination of the variables is stationnary. This restricts considerably the interest for such a method when applied on physical problems in general, and climate in particular. I am not convinced at all that, for instance, ice volume and Antarctic temperature should "relax" to some constant (linear) relationship in the absence of (non-stationnary) forc-

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ing (in particular since ice volume is in the North, and Antarctic temperature is in the South...).

The statistical methodology explicitly tests whether there are stationary combinations among paleoclimate data. The results are clear. For all models, there are statistically verifiable combinations of climate variables that “relax to some constant (linear) relationship. Again, the statistical model has evaluated (and rejected) a preconceived notion held by the commentator (and perhaps others) and further demonstrates its scientific merit.

Point #13

Furthermore, non-stationnarity in statistics is not the same as non-stationnarity in physics (non-stationnarity in physics often means that other forcing mechanisms that are not accounted for, may change through time). This is in particular the case when looking at the astronomical forcing over the million year time scale.

We agree and as described in point #2, that is why we restrict our sample period to the period more recent than the mid-Pleistocene revolution. To some degree, the commentator seems to want it both ways. In point #2, he/she argues that we should extend the sample period back about one million years, but here argues that there is no reason to expect stationary relationships over this longer sample.

Point #14

Annual mean insolation (page 602) is almost equal to obliquity and does not depend on latitude (beyond a multiplying factor changing the absolute value and amplitude). Using different latitudes will therefore not change the result. There is no need to perform statistics.

This may be correct for annual mean insolation, but that is not correct for the seasonal measures of insolation (spring, summer, fall, winter) and is not correct for the summer-time insolation that exceed a given daily threshold (as calculated by Huybers

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and Denton, 2008). And it is these variations that generate the statistically meaningful differences in the accuracy of in-sample simulations for the endogenous variables that are reported in Table 5. We are happy to add this point to a revised manuscript.

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