

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

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Response to Reviewer #2

C392 “Thus a statistical relationship does not guarantee a causal relationship.” We agree and are careful not to use the word ‘causal’ anywhere in the manuscript. This should be less of an issue now that the revised version of section 2.2 no longer emphasizes the decomposition of the Π matrix.

C392 “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” Two issues here. Yes, the model does not describe climate dynam-

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ics because we have not imposed overidentifying restrictions. We address this issue in a second paper which focuses on the long-run relations among variables and the dynamics of disequilibrium. This is described in the Conclusion. Nor are these overidentifying restrictions needed for the purpose of the analysis. To explain, we add the following material:

As the purpose of this paper is to simulate the long-run glacial movements and to identify endogenous vs. exogenous drivers of climate cycles (whereas not to identify a long-run structural relationship for each endogenous variable) it is more advantageous to use Πx_{t-1} rather than $\alpha \beta' x_{t-1}$. Because a cointegration relation that exhibits fairly pronounced persistence can have significant predictive power in simulations over the long run (while over the short run it does not add much explanatory power) the rank of the Π matrix conditional on the exogenous drivers has been liberally selected. This means that that the simulations are based on a CVAR model with a dynamic structure containing a characteristic root which is fairly close to, but not on the unit circle.

C393 “The most important finding of this study is the analysis of the relative importance between the different components of the orbital forcing in the observed temperature record.” This is an excellent point. We expand this section of the manuscript by using the S2a statistic to compare the accuracy of in-sample forecasts generated by various versions of model 2. These results are shown in a new Table 6. This is an explicit test and so alleviates the difficulty associated with the fact that the “9 curves on top of each other.” We remedy this difficulty by creating a new set of figures, Figures 4a-c that show results for Models 2e-g and Model 3a. The results contained in the new table and Figures are discussed on more detail in a revised section 4.4

C393 “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.” There might be some misunderstandings regarding the calculation of the degrees of freedom: as we add more variables, the degrees of freedom decline, not increase. Thus the diagnostic statistics has a built-in penalty for using additional ex-

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planatory variables. An increase in the number of explanatory variables reduces the degrees of freedom, making it more difficult to reject the null hypothesis that the results are statistically meaningful. For example, each equation in Model 1 (the model with the fewest explanatory variables) has 380 degrees of freedom whereas Model 4, (the model with the largest number of explanatory variables) has 339 degrees of freedom. Because the degrees of freedom is large in both cases, the reduction in degrees of freedom has a tiny effect (third decimal point) on the critical value of t and chi square distributions that are used to evaluate diagnostic statistics. For example, the significance level for a t statistic of 2.0 based on 380 degrees of freedom is 0.04621155, compared to 0.04629769 for 339 degrees of freedom. We add this notion to the manuscript as follows:

Differences in the number of explanatory variables among models affect the degrees of freedom that are used to assess the statistical significance of the results. Because the large number of observations is large (391 observations), the penalty for adding additional explanatory variables is small. For example, each equation in Model 1 (the model with the fewest explanatory variables) has 380 degrees of freedom. Model 4, (the model with the greatest number of explanatory variables) has 339 degrees of freedom. This reduction in the degrees of freedom has a tiny effect on the critical value of t and chi square distributions that are used to evaluate diagnostic statistics. For example, the significance level for a t statistic with a value of 2.0 and has 380 degrees of freedom is 0.04621, the significance level is 0.0463 for 339 degrees of freedom. Consistent with this small effect, increases in the number of explanatory variables does not automatically increase explanatory power. Section 4 describes several comparisons in which increasing the number of explanatory variables does not increase the explanatory power of the model.

The issue of adding additional variables is illustrated by changes in the performance of Model 1-3. For example, Model 2 adds nine variables to Model 1 and generates a statistically measurable increase in the performance of Model 2 relative to Model

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1. Conversely, Model 3 adds seven variables to Model 1, but this increase does not generate a noticeable change in the performance of Model 3 relative to Model 1. This issue is addressed directly by modifying the discussion on lines 24-28 on page 600

While adding variables does 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), adding more variables does not automatically improve a statistical model's skill, as indicated by the performance of Model 3 and Model 1. Model 3 has seven more endogenous variables than Model 1, which implies and additional 14 parameters. Despite this increase, the in-sample simulations for Ice generated by Model 3 are not 'more accurate than those generated by Model 1 (Table 4 Figure 1c).

C393 "In the same way it is rejected that cumulated 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 relevant comparison is Model 2 and Model 2d. Model 2d is the same as Model 2 save the summer insolation variables as calculated by Huybers and Denton (2008). Originally, this is explained on page 601, lines 17-22

Consistent with this result, removing Insol275 and Insol550 from Model 2 (Model 2(d)) generates little change relative to Model 2, which indicates that cumulative summer insolation (at these two thresholds) has little explanatory power relative to the other variables for solar insolation. Together, these results imply that Insol275 and Insol550 make only a small contribution to the ability of Model 2 to simulate glacial cycles."

Based on the results in the new Table 6, this section is rewritten as follows:

Furthermore, removing Insol275 and Insol550 from Model 2 allows Model 2d to generate a more accurate in-sample simulation of Ice than Model 2. This too illustrates that 'simply adding variables' does not improve a model's ability to simulate glacial cycles. Together, these results imply that Insol275 and Insol550 make little or no contribution

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to the ability of Model 2 to simulate glacial cycles.

The use of cumulated 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.” Our results do not invalidate physical laws, they simply indicate that cumulated summer insolation does not have a lot of explanatory power relative to the other variables in the Model 2. This result can have two interpretations; (1) cumulative summer solar insolation is unimportant or (2) the effect of cumulated summer insolation is subsumed by some other measure of solar insolation that is included in Model 2. Because the reviewer’s comment raises an important point that allows us to clarify the interpretation of the statistical results, we add the following to the revision described above:

This result can be interpreted two ways; (1) the effect of cumulated summer insolation is subsumed by some other measure of solar insolation that is included in Model 2 and/or (2) the effect of cumulated summer insolation is embodied in some other variable—perhaps cumulative summer solar insolation drives changes in surface temperature, and these changes in surface temperature drive changes in ice volume. This result can be interpreted two ways; (1) cumulative summer solar insolation is unimportant or (2) the effect of cumulated summer insolation is subsumed by some other measure of solar insolation that is included in Model 2.

Specific Comments

C394 This is not arbitrary, but based on the reasoning that that is the latitude where the (southern rim of) the ice sheet are waxing and waning. The reviewer is correct and we use this argument to aid the interpretation of statistical results. Specifically, we rewrite the material on page 599, lines 16-21 as follows:

The inability Model 1 to reproduce the climate cycles may be sensitive to the choice of variable to represent solar insolation (here $Insol_0$). For example, Milankovitch and others argue that mid-summer insolation at 65oN is critical because that is the latitude

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where ice sheets wax and wane. To test the importance of the ‘Milankovitch forcing’ we specify Model 1b, which includes insolation on the first day of summer, 21 June at 65oN. Using the Milankovitch forcing generates little improvement in the ability of Model 1 to simulate temperature, CO₂, or Ice over the last 391Kyr (Fig. 1a–c). This result indicates that the direct effect of solar insolation at the southern margin of the ice sheets is not the sole driver of glacial cycles. Nor is it the most important component of solar insolation, as will be described in section (4.3) in relation to the ability of Model 2 to simulate glacial cycles.

The data analyzed (Table 1) should be presented graphically in one figure. We have tried to do so, but it gets very confusing. If the editor feels that this figure is critical, we will add it.

It will then be apparent by, say, plotting Temp, CO₂ 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 Yes, the reviewer’s comment highlights an important point that we have not made sufficiently clear in lines 1-15 on page 599. To clarify the point, we revise lines 22-24 as follows:

The inability of Model 1 to reconstruct glacial cycles accurately in spite of very strong correlations among temperature, CO₂, and Ice highlights the power of the CVAR model and the importance of exogenous drivers of the climate system. When the data are nonstationary, correlations are statistically incorrect as measures of association. The CVAR replaces correlation as a measure of association with the more correct measure of cointegration. That Model 1a cannot simulate glacial cycles even when given the historical observations for CO₂ suggests that the “correlations” among temperature, CO₂, and Ice are driven by some other factor, either a component of solar insolation not included in Model 1 (see Model 2) or some other component of the climate system not included in Model 1 (see Model 3). The potential relevancy of the latter is under-

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mined by the results generated by Model 3 (see section 4.2) while the importance of components of solar insolation other than the 'Milankovitch forcing' is highlighted by the ability of Model 2 to simulate glacial cycles (see section 4.3).

First of all, how do the authors know that the analyzed records are not stationary? The conclusion that the variables are not stationary is based on the trace test, which is a multivariate Dickey-Fuller type of test combined with the result that the characteristic roots of the CVAR model are at or very near to the unit circle. To highlight this approach, we add the following paragraph to section 2.2

The number of common driving trends is equal to $p-r$. The rank is determined based on the so-called trace test, which can be thought of as a multivariate Dickey-Fuller test. Note, however, that a univariate Dickey-Fuller test of each variable is an inefficient procedure that frequently leads to misleading and internally inconsistent results. It should, therefore, not be used in a multivariate context. .

The equation (1) describes the evolution of ΔT_{empt} etc. Is this $T_{\text{empt}+1} - T_{\text{empt}}$? If so, is the CVAR process (1) just an AR(2) process? What is the difference? In its unrestricted form, the CVAR used in this paper is a VAR(2) process. When the reduced rank, $\Pi = \alpha\beta$, is imposed it is no longer exactly a VAR(2) process, but still approximately so.

Minor Comments

p 588, line 8: is this a recently developed technique (Johansen 1988 or something newer?) We have revised this sentence to read "In this paper, we use a statistical technique

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. We do not claim that our results out-rule non-linear dynamics to be essential. We clarify this by revising

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the sentence on lines 16-19 as follows: "Finally, results suggest that non-linear dynamics, threshold effects, and/or free oscillations may not play an overriding role in glacial cycles." We think the term "suggest" and the phrase "may not play an overriding role" limit the extent of our claim consistent with the results of the analysis. We are happy to change the sentence if the editor thinks we overstate the results.

p 589, line 8: CO₂ and methane are not local quantities, they are for all practical purposes well-mixed in the atmosphere. We agree and revise phrase "and therefore represent local conditions" as follows: Carbon dioxide and methane are well-mixed and so measurements from the Antarctic ice sheet probably represents global levels. The temperature measure represents local conditions, but can be converted to global values (Masson-Delmotte et al 2010; 2006).

p 592, line 23: The quantity R² is non-standard in climatology and must be explained. Here it is stated that it is in general misleading to use R² for nonstationary variables, however this is being done anyway (Table 3). Please explain.

R² (also known as the adjusted coefficient of determination) is misleading if used as an indicator of a model's explanatory power when the regressor is Y_t and Y_t is nonstationary. This is because in this case the model's explanatory power is compared to just using the average of Y_t . Any irrelevant trending variable is likely to do better leading to what is often called spurious regression results. In our model the regressor is $Y_t - Y_{t-1}$, implying that the calculated R² measures the model's explanatory power relative to a random walk. For nonstationary variables this is a relevant baseline. Although misleading, we retain the use of R² to facilitate comparisons with previous analyses. For example, we add the bolded material to a sentence from the Introduction "For example, the linear relationship between CO₂ and Antarctic temperature during the previous 800 kyr has an R² (also known as the adjusted coefficient of determination) of 0.82, which suggests that 82 percent of the variation in Antarctic temperature is associated with variation in atmospheric CO₂ (Lüthi et al 2008)."

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p 593, line 16: "changes in orbital cycles" should perhaps be "changes in Earth's orbital parameters". This is an excellent suggestion. Hypothesis 1 now reads Glacial cycles are generated by changes in Earth's orbital cycles.

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. Any confusion is completely our fault—we did not define the Π matrix. We remedy this solution with regard to a previous comment made by the reviewer (I agree with reviewer #1) by rewriting the entire section that describes the statistical model (section 2.2).

p 600, line 8 and other places: Level = Sea level? Please spell out. We use italicized Level to refer to the variable sea level. When we refer to sea level, we spell it out.

p 602: Models 2f-g are, as I see it, not shown anywhere? And again, the figures 3a-c are too busy to read. This is a good point. These variables are shown in a new set of figures, figures 4a-c and are included in a new Table 6, which shows comparisons, of the S2a statistic. And the interpretation of these results is described in a revised section 4.5.

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. For the purpose of identifying drivers of glacial cycles, the dramatic differences in the ability of Models 1-4 to simulate glacial cycles speak for themselves. We agree that making physical interpretations of the coefficients is critical. This is the focus of our next paper in which we impose over-identifying restrictions on the CVAR model. Hopefully the manuscript now addresses this point based on previous comments made by the reviewer. Specifically, the material added in response to comment C392 (I agree with reviewer #1).

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. For this initial effort, we prefer to

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focus on the 'Vostok period.' To clarify this, we add the following sentences.

This 'Vostok period' is chosen because it contains four 'glacial cycles' and reduces data aggregation across cores. As described in the Conclusion, subsequent efforts will expand the sample period to include the previous 800 kyr and test whether the relationships among climate variables during the 'Vostok period' is stable.

This avoids any difficulties associated with the use of data from different sources. We do plan to address this issue in future efforts. As we state in 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.

In short, this is simply the first effort to report how we are using the CVAR model to investigate glacial cycles. We recognize that that the CVAR model can be used to address many scientifically important questions. The Conclusion lists some of the many ways in which we will build on this effort. Nonetheless, the issues discussed in this manuscript are just a first step.

p 605: It seem to me that the analysis of Northern vs. Southern hemisphere is too 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.

Again, we agree that making physical interpretations of the coefficients is critical. This is the focus of our next paper in which we impose over-identifying restrictions on the

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CVAR model. Hopefully the manuscript now addresses this point based on previous comments made by the reviewer. Specifically, the material added in response the reviewer comment C392 (I agree with reviewer #1).

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

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