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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 #3

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1 General comments

A statistical method historically developed for economic time series is implemented on paleo data and insolation time series. The method is called Cointegrated Vector Autoregression (CVAR) and is purely stochastic. It applies to non-stationary time series (i.e., variance and/or mean changing with time). Its purpose is to circumvent the pitfall of nonsense multivariate linear regressions which may arise when dealing with autocorrelated time series (autocorrelation violates linear regression hypothesis). CVAR models only handle stationary variables. The latter are obtained by linear transformations applied to the original time series. The autocorrelation order of the

time series must be assessed to determine the relevant transformations (the order determines if the time series have to be differenced once, twice or more, in order to be stationary). The new stationary variables are measuring growth rates rather than levels. For instance, if $X^p(t)$ is autocorrelated of order 1, $X^p(t) - X^p(t-1)$ is stationary. Given a known deterministic time series $F(t)$, the CVAR model reconstructs the observed differenced time series $\Delta X^p(t)$ for $p = 1, \ldots, N$. An Ordinary Least Square setting measures the distance between reconstructed and observed values and the regression coefficients are estimated. The CVAR model aims at testing the existence of an equilibrium state between variables and for that purpose includes a linear combination of $X^p(t)$ and $F(t)$. To be "cointegrated", the latter linear combination must be stationary. The linear combination is part of the CVAR model as a potential driver for the reconstructed growth rates $\Delta X^p(t)$. The identification of the related regression coefficient (α matrix, equation 1 p. 592) enables to determine if it is indeed stationary and if it acts as a restoring force toward an equilibrium state.

In the manuscript, the authors apply the CVAR method to arbitrate between different insolation signals and endogenous climatic variables as drivers for glacial/interglacial cycles, as recorded in atmospheric CO_2, Antarctic temperature and ice volume proxies. An autocorrelation order of one is assumed for all the time series. The set of data covers the last 391 kyr. Authors want to solve several issues among which the 100 kyr problem, the relevance of the North insolation signal as the standard driver for glacial cycles.

I recommend the rejection of the manuscript regarding the current state of the study. First, I am not convince that the CVAR method alone is able to solve the complex issues addressed by the authors, at least not in the manner the authors conducted their experiments. In my opinion, a single equilibrium state between climatic variables that would be valid on such a long time period (400kyr) is questionable. Moreover, the

CVAR method as a pure stochastic method (no physics) has few chances to lead to a physical equilibrium state. The CVAR method involves very strong assumptions, which should have been discussed step by step. In particular, autoregression order 1 for all the data series is questionable on long time periods, regarding the non-linearities expected in the climate system dynamic and the wide range of characteristic time scales for the feedback mechanisms. No statistical test is reported (Augmented Dickey-Fuller statistic) to test this assumption. In my opinion, the CVAR method might be relevant only if applied to very specific climatic data and on short time intervals.

The addressed issues (introduction) and experiment plan (section 2.3) are rather clear. Authors proposed themselves to test three types of hypothesis: (i) Glacial cycles are due to orbital parameter changes; (ii) Glacial cycles are due to endogenous proxies; (iii) The standard summer solstice insolation at 65N is relevant. But their research process is not convincing. I have doubts that the experiment design might help to test the adressed issues (see "Specific comments" 2.1). The results and discussion seem confuse to me. Some statistical tests are questionable (R2 coefficient) and very few elements (No tables for the α matrix component) are given to justify the main point of the manuscript: how authors conclude that the equilibrium state equations are valid or not (see "Specific comments" 2.3).

2 Specific comments

2.1 Experiment design

Authors extended a primary experiment (model 1) to more complex models by adding many degrees of freedom at the same time (insolation forcing for model 2, paleo data for model 3 and both time series for model 4). Reviewer 1 already underlined the not

very well-founded choice for insolation candidates to add from model 1 to model 2. I can only agree with him. The improved fit from model 1 to model 2b (discussed section 4.4) was easy to anticipate: annually integrated insolation or cumulative annual solar insolation ("Insol0" p.594) have no precession components (model 1), whereas daily insolation signals have (such as equinox and solstice insolation candidates SunSpr, SunSum, SunAut, SunWin). The same comment applies for model 2c, with candidates Insol275 or Insol550: the poor fit can be expected because no phase information is provided by the insolation candidates (although they have a precession component). Regarding the paleo data records added from model 1 to model 3, they are so numerous that one can doubt that any precise analysis might be done with the results. The improvement of the fit is very likely due to the increase of the number of degrees of freedom (as already mentioned by reviewer 1). Further on, their ability to represent endogenous feedbacks is questionable in this framework.

2.2 Statistical assumptions

I already mentioned my opinion about the autocorrelation order 1 assumption for all the time series. Further on, this parameter can be quite sensitive to the time serie sampling and obviously to the resampling process (section 2.1 authors mentioned resampling to 1kyr for all the series, but did not discuss the problem).

2.3 Statistical tests

In section 3, surprisingly, very few explanations are provided concerning the specific result brought by a CVAR analysis which is to say the "long term equilibrium state". The beginning of section 3 is unclear. Authors refer to the trace statistic "which strongly rejects the null hypothesis that they are no (zero) cointegrating relationship". What is the trace statistic? Further on, they wrote "Based on this result, the Π matrix is assigned full rank (the number of conintegrating relationships is set equal to the number of endogenous variables)". The Π is not defined in manuscript. References are at least necessary. Moreover, regarding long discussions and tables connected to coefficient of determination R^2 and S2a statistic test, more precisions are expected to justify their conclusions about the cointegrated relationships. Obviously, an adjusted coefficient of determination would me more appropriate to discriminate the explanatory power of the models, since they have growing number of degrees of freedom.

2.4 Figures

Too many experiments are compared on the same figure. Many times, a statement regarding a set of experiments refers to a set of figures, where the corresponding curves are not shown (e.g., model 2e, 2f and 2g are announced p.602, paragraph line 5 on Figure 3a-c but only model 2e is shown). This does not help to follow the discussion.