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Climate of the Past
Discussions

Interactive comment on "Testing Hypotheses About Glacial Dynamics and the Stage 11 Paradox Using a Statistical Model of Paleo-Climate" *by* Robert K. Kaufmann and Felix Pretis

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Response to Reviewer 3
Robert K. Kaufmann & Felix Pretis

We thank the reviewer for their detailed review of the manuscript. Our responses are are described point-by-point below.

This contribution is based on a statistical model called CVAR, standing for "cointegration vector autoregression". The model is calibrated on the latest four glacial-interglacial cycles, and then shown to fully reproduce the sequence of the latest eight glacial-interglacial cycles, except for the deglaciation leading to the stage 11. The authors conclude that this deglaciation, associated with the mid-Brunhes event, is to be considered as an anomalous event rather than marking a regime transition. By inspecting model errors, they also conclude that the cause of this "stage 11 paradox" is to be attributed to an anomalous CO_2 *rise. They also observed that, CVAR being "largely linear", nonlinearities and thresholds do not play an important role in the major part of the latest 800,000 years, and reject the hypothesis that glacial-interglacial cycles could occur without orbital forcing. Statistical modelling indeed has much to offer for analysing and understanding glacial-interglacial cycles. It is a good approach for detecting an "anomaly" and then enquire about the causes of this anomaly. I, however, see two problems with the present contribution. First: reading is tedious, notations and tables not always clear, and the model description should be more self-contained. The second problem, perhaps more serious, is what I would consider a misuse of statistical reasoning and dynamical systems theory.*

We will edit the manuscript to make it more readable and self-contained.

It should here be reminded that satisfactory model performance over most of the latest 800,000 years is not enough for rejecting alternatives. There is, in the present contribution, no statistically framed attempt at comparing models. Some alternative models actually do a fairly convincing job in reproducing the full sequence of the latest 800,000 years, including stage 11, and some of these models actually are limit cycle synchronised on the orbital forcing. Similarly, that one model can produce the full sequence of the last eight glacial-interglacial cycles without parameter change does not reject the hypothesis that a regime change actually occurred.

As far as we know, our manuscript is the first to assess the accuracy of the simulation, both in- and out-of-sample using these statistical rigorous methods. As such, it goes beyond conclusions about models '*doing a fairly convincing job.*' The reviewer is correct; we do not explicitly compare results among models. Instead, conclusions about nonlinearities and thresholds are based on Occam's razor. Conditioning on only orbital geometry, our model is able to account for glacial cycles (both in- and out-of-sample) without nonlinearities and threshold effects. Occam's razor implies that if nonlinearities and/or threshold effects were critical, a linear model would be unable to simulate glacial cycles. As we state in the abstract (line 13-15), our results "suggests that nonlinearities and/or threshold effects do not play a critical role in glacial cycles."

Yes, "*that one model can produce the full sequence of the last eight glacial-interglacial cycles without parameter change does not reject the hypothesis that a regime change actually occurred*" but Occam's razor suggests that any regime change is not important. An important change would certainly limit the model's ability to accurately "*produce the full sequence of the last eight glacial-interglacial cycles.*"

Furthermore, a linear dynamical system forced by harmonics (sines and cosines) can only output harmonics (you can show this by reasoning on the Fourier transform). The orbital forcing is a sum of harmonics. Hence, non-linearity is needed to transform this sum of sines and cosines into the characteristic saw-tooth-shaped, 100,000 year-long nature of glacial-interglacial cylces, which is most visible over the latest 400,000 years. So even though the authors qualify the CVAR model as "largely linear" (p. 9), it must nevertheless contain the nonlinearity necessary to reproduce these characteristics. Yet, the authors are silent on the consequences on this non-linearity, and in particular(for reproducing which features) when it is critical.

Our linear model is able to "*transform this sum of sines and cosines into the characteristic saw-tooth-shaped, 100,000 year-long nature of glacial-interglacial cylces*" So,

non-linearity is not necessarily needed to transform this sum of sines and cosines into the characteristic saw-tooth-shaped. There is no 'hidden' nonlinear aspect of the model, however, we apologize; we should have been clearer regarding the linear nature of the CVAR model. The CVAR model is linear in parameters. That is, the first cointegrating relation in Table 2 of Kaufmann and Juselius (2013) and Supplementary Table S.1 is a linear relation between Temp and CO_2 and the third cointegrating relation is a linear relation among Ice, CO_2, and eccentricity. These linear long-run cointegrating relations, which lie at the heart of the CVAR, is what we refer to as a 'linear model'. We will clarify this in our revised manuscript (see also the response to Reviewer #2).

The apparent non-linear dynamics can stem from two sources. First, the individual variables (specifically, the first difference of each variable) such as temperature adjust towards disequilibrium as a linear function of disequilibrium in the level of the variables in the previous time period. This creates a seeming non-linear change in the level. However, the model is linear in both first differences and levels.

The reviewer's claim about a hidden nonlinear component can be addressed with the clever experiment suggested by Reviewer #2. He/she also was skeptical of a linear model's ability to simulate the climate record. To test this skepticism, he/she suggested that we use our CVAR model to estimate a model among endogenous variables generated by a van-der-Pol oscillator that is driven by an exogenous sine-wave function. If the CVAR model embodies nonlinear relations, anonymous reviewer #2 postulates that the CVAR model would be able to simulate the nonlinear data generating process. But as indicated in our response to anonymous Reviewer #2 (Figures 1-3), the CVAR model is not able to simulate the nonlinear data generating process in a statistically significant fashion. Instead, the results are consistent with the alternative hypothesis proposed by anonymous reviewer #2, that nonlinearities do not play a critical role in glacial cycles, because the CVAR model is able to account for glacial cycles in a statistically meaningful fashion both in- and -out-of-sample.

Inspecting figure 1, it seems that the CVAR is missing two important tests: the termination five (which the authors focus on), and the termination one leading to the current interglacial. Both occurred despite a relatively weak orbital forcing, and both actually justify the widely held assumption that the deglaciations involve non-linear dynamics, perhaps catastrophic dynamics (the idea that a "mature" glacial stage is unstable).

The manuscript does not ignore "*the termination one leading to the current interglacial.*". The CVAR model's inability to accurately simulate the last interglacial is described in the last two paragraphs of the Conclusion, which links both difficulties to atmospheric CO_2.

As indicated in Figure 1a, the statistical methodology indicates that the CVAR model does not simulate Ice accurately during the current interglacial. But this does not "*justify the widely held assumption that the deglaciations involve non-linear dynamics, perhaps catastrophic dynamics (the idea that a "mature" glacial stage is unstable).*" As described on page 13, the literature contains another testable hypothesis "Holocene warming is amplified by anthropogenic emissions of carbon dioxide and methane (Ruddiman 2003; 2005; 2007)." And this effect can be tested by our model. As stated on page 13, "These CVAR simulations also will be used to assess the early Anthropogenic hypothesis by evaluating the degree to which anthropogenic emissions of carbon dioxide and methane can account for outliers and persisting errors in Ice and other climate variables during the Holocene."

Page 2, line 18: "reject the hypothesis that carbon dioxide or methane is exogenous to the climate system". It's not the purpose here to comment and criticize KJ2013, but I must, however, say that I find this statement puzzling, or at least misleading. No one seriously disputes that CO_2 is somehow generated and cycled within the earth, and in that sense it belongs to the climate system. Whether CO_2 dynamics, in a given model, is treated as endogenous or exogenous is not an ontological statement about

the nature of the system. It is a working hypothesis that helps to addressing a specific question.

As the reviewer states "*It is a working hypothesis*" and that working hypothesis is rejected by our model, which is able to simulate CO_2 endogenously in a statistically significant fashion. We emphasize that the CVAR simulates CO_2 endogenously because the working hypothesis that CO_2 is exogenous undermines the results of many previous models. For models that treat CO_2 as an exogenous variable, analysts cannot separate the explanatory power of the model from the explanatory power of carbon dioxide. Observed values of atmospheric CO_2 can account for much of the variation n in the climate system, which is simply 'apportioned out' to the other components of the climate system by models conditioned on CO_2.

Page 3, line 6: "glacial cycles are driven by the same dynamics before and after the MBE". There may be a type confusion here. In what sense are dynamics "driving" something? In common language, a driver rather refers to an external agent (as in the sentence: "glacial-interglacial cycles are driven by orbital forcing").

Yes, the wording is imprecise. We will revise the sentence to read glacial cycles are driven by the same short- and long-run relations between orbital geometry and the climate system and among components of the climate system before and after the MBE.

Page 3, line 34: "six climate and four mechanisms": again, this seems to be a type confusion. How can a "variable" be a "mechanism"? Mechanism refers to a chain of causes, not to a variable (ditto page 4, lines 10-11).

Yes, the wording is imprecise. Some of the variables in the model are important indicators of the climate system per se, such as sea surface temperature and the extent of land ice. But other variables are important because they proxy aspects of the climate system. For example, the model includes Na because it proxies the extent of sea ice. We will modify the text accordingly.

Page 3, line 21: it is perfectly acceptable, for this kind of study, to adopt a com-mon timescale, and EDC3 can indeed do the job, even though it has been subject to some revision, especially around stage 11 (see the AICC2012 time scale, Bazin et al.2013, 10.5194/cp-9-1715-2013). However, synchronising CO_2 with sea-level records remains a challenging exercise, which could have implications for the interpretation of the results, especially when it comes to discussing the timing of model errors.

This hypothesis is investigated by Kaufmann and Juselius (2016), who find that "This result is unaffected by astronomically tuned data; repeating the analysis with a time series for land ice that is not tuned (Huybers, personal communication) generates similar results regarding the nature of the tenth cointegration relation (results available from the authors)." We will include this result when we revise the manuscript.

Page 5, line 5: the alpha matrix of coefficients seems to play a very important role in the dynamics of the model. It is presented as a matrix of relaxation coefficients, but later in the document it seems that these are not simple linear relaxations (page 9, line 21) since the authors mention that it is a source of non-linearity. Again, the authors should be more explicit about the construction of this matrix, and about the implications of the possible non-linearities.

As part of the CVAR model, the matrix of error correction coefficients (equation 3 on

page 5) is estimated statistically. As indicated by equations (2) and (3), individual variables (specifically, the first difference of each variable) such as temperature adjust towards disequilibrium as a linear function of disequilibrium in the level of the variables in the previous time period. This creates a seeming non-linear change in the level. However, the model is linear in parameters in both first differences and levels (see also our response on non-linearity above).

Page 5, line 31: "values from 792 kyr BP through 392 kyr BP constitute the out of sample period". This seems to be the only place where the out-of-sample period is clearly defined. If I understood correctly, the whole statistical analyses supporting the present contribution is based on one out-of-sample period, and one in-sample one. What about swapping the in-sample and out-of-sample? Are conclusions unchanged?

This is an interesting idea, but it is not possible to swap the in- and out-of-sample periods because of limits on the availability of data. As indicated in Table 1, 466 kyr BP is the first observation for proxy data for sea level from Siddal et al., (2003). As such, it would not be possible to estimate the CVAR for the 792 kyr BP through 392 kyr BP out-of-sample period. This highlights the strength of the out-of-sample period. The model is not initialized on observations for sea level (and CO_2, CH4, Fe, Na, SO4, Ca, and Level). Rather, the model is initialized with values that correspond to their sample mean and spun-up from 800-792 Kyr BP. Given this procedure, the model's accuracy starting in 792 Kyr BP appears remarkable to us. Only Na and SO4 fail tests of statistical accuracy at the start of the out-of-sample period.

Page 6, line 29: The Y_{ji} (an index i) are not defined.

We will correct this in the revised manuscript.

Page 9, line 9: "together, these results suggest that the test results reveal information about the statistical ordering of simulation errors" : I'm afraid that I could not make any sense of that sentence.

To address the reviewer's comment, we will clarify this section.

Page 11, lines 28 to 36: the mechanism discussion in fact mainly contains (with a few exceptions) references about the sequence of Heinrich events and Yourger Dryas, and not so much about the initiation of the deglaciation. Many of these articles are not related at all to stage 11.

We will re-read these papers and eliminate those that are not related to stage11.

Page 12, line 34: "together, these results suggest that terminations in general, and termination five in particular, are driven by changes in atmospheric carbon dioxide". First, the authors must clarify what they mean by "driven". The overall stance of the article is that orbital forcing is driving all "endogeneous" variables of the climate system, including carbon dioxide. But we can understand that the authors mean that the CO_2 rise, whatever its cause, is a crucial element of the causal chain that leads to the deglaciation, and that "something" caused its rise, which is not ice melt or sea-level rise (this is the meaning which I could give to the sentence at the end of page 12 : they contradict the notion that changes in carbon dioxide are a positive feedback loop in Earth system as opposed to a cause of glacial terminations").

This question may be caused by our lack of clarity regarding "the statistical ordering of simulation errors" and "driven by changes in atmospheric carbon dioxide." We use statistical techniques to identify the timing of model errors. By doing so, we try to

identify the variable(s) that the model fails to simulate at a particular time and trace if and how this error propagates through the system. This strategy is explained on lines 9-14 on page 12:

> "These competing hypothesis for the terminations in general and stage 11 in particular can be tested by the statistical ordering of the model errors. If changes in sea surface temperature initiate Termination V, the model's inability to simulate termination V will 'start' with its inability to simulate SST. This inability will be indicated by simulation errors for SST that precede and have information about the simulation errors for other variables. Specifically, simulation errors for other variables, such as CO_2, will not have prior information about the errors for SST and these errors will have prior information about the errors for the other variables, such as CO_2."

Beyond tracing these errors, we also condition the model on SST, Na, Ca, or CO_2, which proxy competing hypotheses for the Stage 11 paradox. As indicated in Figure 4, observed values for atmospheric CO_2 have considerably more explanatory power for Ice during stage 11 that the other variables. This result suggests that "that terminations in general, and termination V in particular, are driven by changes in atmospheric carbon dioxide." This result goes back to our response about models treating CO_2 as an exogenous variable. Figure 4 suggests that if our model is conditioned on CO_2 (rather than simulating it endogenously), the model would no longer perform poorly during stage 11.

Now, that CO_2 is indeed involved in the dynamics of the deglaciation is largely accepted by the experts of ice-age dynamics. The question is which roles it plays in the acceleration of deglacial dynamics, compared to the mechanisms of glacial instability (isostasy, buttressing effects, accelerated ice flows) or yet other phenomena (dust ac-

cumulation for example). This is a long ongoing debate, which is being investigated by considerations about the physics of ice sheets in ocean circulation, and by careful inspection of the climate records. Statistical analysis as the one presented here is part of the investigation, but it requires more attention to uncertainties associated to the dating and interpretation of palaeoclimate records.

As described above, results are not changed when the model is estimated using the time series for Ice that is not orbitally tuned. Also, our model would not simulate interglacial periods accurately (other than stage 11) if isostasy, buttressing effects, accelerated ice flows played a critical role because these mechanisms are not explicitly represented by the CVAR model. It may also be the case that the time-steps used in our analysis (1k years) mask some of the perhaps 'faster' glacial events such as accelerated ice flows.

Furthermore, an early rise in CO_2 does not mean that it causes the deglaciation. Rises in CO_2 have been observed throughout the latest glacial interglacial cycle (associated with the so-called Antarctic warming events), and did not yield deglaciations. Whether a 10 ppm CO_2 increase has to be considered as "the" nudge which triggered run-away deglacial dynamics (e.g. whether it is a "proximal" cause, see Wolff et al. 2009) is perhaps an interesting question, but it does not make it the explanation of the deglaciation.

As described above, we are not examining the rise or fall in any variable, such as an early rise in CO_2. We are examining the statistical ordering among model errors. To summarize, the CVAR model fails to simulate Stage 11 in a statistically meaningful fashion. For what variable(s) does this general failure first appear. Tracing the error back to a variable would suggest that the model fails to account for an important mechanism, and this failure spreads to the other variables, and ultimately causes the stage 11 failure in the CVAR. In this case, the source of model failure during stage 11 is

the failure to simulate CO_2, as indicated by this error proceeding the errors for other variables.

Figure 4 : a legend within the figure would be helpful

We are happy to add a legend in the revised version.

Table 2 is very hard to read. What the "distribution among Marine isotope status" means is not obvious at all. What are the units, which reading should one make of these numbers and what are the implications? What is the meaning of persisting errors?

We will clarify this in the revised manuscript. This table shows the results of tests that outliers and persisting errors are distributed randomly between the in- and out-of-sample periods and among the 19 marine isotope stages. The procedure that generated these results are described in Section 2.4 Identifying Periods of Simulation Failure on page 6. Outliers and persisting errors are defined on pages 5-6 in Section 2.3 Statistical Measures of Model Performance "Outliers refer to a change in the simulated value of variable x relative to the observed value for a single time step, while persisting errors are statistically significant differences that persist for two or more consecutive time-steps."

Title: Paleo-Climate is not standard spelling.

We will revise this.