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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

Anonymous Referee #3

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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 CO2 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

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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.

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.

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.

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).

Line by line comments

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 CO2 is somehow generated and cycled within the earth, and in that sense it belongs to the climate system. Whether CO2 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.

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")

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).

Page 3, line 21: it is perfectly acceptable, for this kind of study, to adopt a common 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 CO2 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.

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.

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?

Page 6, line 29: The Y_ji (an index i) are not defined.

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.

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.

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 CO2 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 : "

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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").

Now, that CO2 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 accumulation 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.

Furthermore, an early rise in CO2 does not mean that it causes the deglaciation. Rises in CO2 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 CO2 increase has to be considered as "the" nudge which triggered runaway deglacial dynamics (e.g. whether it is a "proximal" cause, see Wolff et al. 2009, dx.doi.org/10.1038/ngeo442) is perhaps an interesting question, but it does not make it the explanation of the deglaciation.

Figure 4 : a legend within the figure would be helpful

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?

Title: Paleo-Climate is not standard spelling.

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