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Climate of the Past Discussions

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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 #2

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The authors use a statistical model to fit time series with a 1kyr resolution, of 10 proxy records (temperature, CO2, methane, SST, land ice volume, sea level, Fe, Na, SO4, etc) of the ice ages over the past 391 kyr, and then to simulate the past 791 kyr. The forcings are prescribed orbital parameter time series. Their conclusions are that nonlinearities are not important, and they discuss at some length the model failure to simulate the period around 400 kyr. If I understand correctly equation (2) and the following text, the model has a *huge* number of parameters: there are several 10x14 and 10x15 matrices of model parameters, involving hundreds of parameters being fit. If this is indeed the case, I don't know that it is a surprise or an achievement that the model achieves a satisfactory fit. Saltzman (not cited by the authors) developed his

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"Earth system models" with the philosophy that the goal of glacial models is to produce a good fit to the ice volume record with the smallest number of parameters, although it is not clear if one actually learned about the dynamics of ice ages by such a fit. In any case, he used a nonlinear model and the order of 10-15 parameters, which, if I understand correctly, is much less than is used here. I can see some (vague) similarity between the Saltzman philosophy and the one taken here, in the sense that the authors try to fit the record without suggesting a mechanistic understanding of ice ages. This could still have led to a useful insight if they could indeed show that nonlinearity is not important, although as I discuss below I don't believe they have shown that.

First a comment on the data: while ten proxy records are being used (the authors should plot them), they are likely not very independent, as they tend to mostly vary together with each other, so the number of observations being explained/ fitted is not as large as it might seem superficially, making the number of model parameters effectively even larger.

The authors emphasize as their main conclusion that their study rules out previous claims that nonlinearity must be important. This would have been novel and interesting, but I don't find this convincing for two reasons. (1) Their model is somewhat nonlinear. They need to completely linearize it, repeat the analysis, and demonstrate that the results and conclusions are robust. (2) Given the very large number of model parameters, I suspect their approach could fit any low-order *nonlinear* dynamics successfully. To make a satisfactory case, they need to test this hypothesis as follows: build a simple nonlinear model based on 10 weakly coupled nonlinear oscillators (e.g., Van der pol oscillators, see Crucifix 2013 "Why could ice ages be unpredictable?" Clim. Past, 9, 2253-2267); fit a similar stochastic model to this model output. The null hypothesis could be that such a model output would be possible to fit using the nearly-linear model used by the authors, although the dynamics are clearly strongly nonlinear. If the null hypothesis is not satisfied, the authors would have a stronger case.

Page 2, line 15-20: That CO_2 is not an external variable seems obvious. I don't know

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that this adds anything new to our understanding.

Page 3, lines 5-10: it seems less plausible that glacial terminations are driven by atmospheric or oceanic dynamics. The trigger needs to be a change to a climate system component that has a long 90 kyr time scale, which reaches some critical threshold, starts changing, and that then leads to changes in the faster components such as sea ice, AMOC, atmospheric circulation, etc. The only such slow component is land ice, which may take 90 kyr to reach some critical size that then affects other components. I realize there is much in the literature about the AMOC triggering terminations etc, but the above argument seems to suggest that these ideas are not likely to be realistic.

Page 8, lines 10-15: it seems to me that the model's ability to simulate glacial cycles during the out-of-sample period does not mean the model is correct. It just seems to suggest that the dynamics of the glacial cycle are the same throughout the past 800 kyr. It seems still possible that the model simply fits the record due to its very large number of parameters.

Because of the issues mentioned above, it seems that me that the manuscript as it stands now does not make a strong case for the suggested conclusions. I recommend a major revision, that it my opinion needs to require new analysis and results rather than just a rewrite and further explanations. I hope the authors find these comments are helpful.

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