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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 #1

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This manuscript describes a new statistical technique applied to the glacial-interglacial problem. Obviously, the authors are not familiar with the subject of paleoclimatology and their premises are often quite shaky or even plainly wrong. The authors are also not familiar with climate dynamics and the set of "climate variables" that they choose and discuss in this paper is probably difficult to justify. I am furthermore not convinced at all that the statistical technique they are using can provides any new information to the problem of glacial cycles. A good illustration of their conclusions is given in Table 5: Ice volume is linked to southern insolation (South 70°N) and sea level is linked to northern insolation (North 60°N). This is in my opinion sufficient to illustrate how an uninformed use of paleoclimatic data, combined with poor climate physics can lead to

obvious inconsistencies. I therefore strongly recommend rejection of this manuscript.

Some basic comments:

1 - The choice of data time series appear very arbitrary and it is not explained how it relates to the glacial-interglacial problem. There are several critical issues here: - Why using only 391 kyr when most data sets shown here are available down to about 900 kyrBP ? A traditional problem of glacial-interglacial studies, in particular of 100 kyr cycles, is the small length of the data (about ten cycles, only during the last million years). Why do the authors restrict themselves to the last four ones ? Four is a poor statistical sample of GI cycles. Ten is a bit better. - In the manuscript, the authors are often mentionning "temperature" instead of "Antarctic temperature". This is not a detail... What is the relevance of Antarctic temperature to the glacial-interglacial problem? This question is even more critical for other variables such as CH4 or other chemicals found in the ice cores. Why using SST only in the Southern ocean ? If the authors are not aware of it, they should look at Northern hemisphere SST or other temperature records to see that data are very different at different locations. Why this very strange choice of climate variables ? Is it a scientific choice or a random one, based on the availability of some sets over the internet ? - The core phenomenology of the glacial-interglacial problem is the ice volume, or the sea level. I do not understand why the authors are using two conflicting data sets for the same physical variable. The difference between these two simply illustrate our poor knowledge of this variable. They both represent sea level, or ice volume which for the problem at hand is the same variable (the subtleties between sea level and ice volume are much much smaller than the uncertainties in both data sets). The authors need to understand that paleoclimatic data are not without errors and should be taken for what they are: estimations of climatic variables, not direct measurements. In this particular case, if the data sets are inconsistent, this clearly means that at least one of them is not accurately estimating what it was meant to. Fitting both of them as two different parameters, and building theories on these results, is meaningless.

2 - The "standard" insolation forcing is NOT the "cumulative annual solar insolation at 65°N" (page 593, line 22). This forcing (insol0) is used throughout the paper as the standard one against which all conclusions are drawn. In contrast, the Milankovitch theory says that "Summer insolation at 65°N" should affect the size of northern hemisphere ice-sheets and often the paleoclimatological litterature is using the daily insolation at the summer solstice. In this manuscript, the standard forcing used in Model1 (insol0) is almost a pure obliquity signal as shown by results on Fig.1a (Model1). It is not really a surprise that Model 2 and 4 (which account for seasonnality in the forcing) are better than model 1 and 3 (which do not). I don't think this kind of result brings any information to the scientific community.

3 - The conclusion that the more complex model (model4) better fits the data is also obvious, since it has more degrees of freedom. Again, I don't see any value in this finding. Standard statistical tools exist to define a trade-off between number of degrees of freedom and goodness of fit (AIC or BIC criteria for instance). I am not sure model4 would stand such classical tests.

4 - The authors have a strange notion of what "feedback" actually means. In paragraph 4.2, they "test" how model1 could be improved by adding "endogenous feedbacks". That model1 was doomed to fail is obvious (see point 2). But I don't understand why adding more data time series (chosen with unknown criteria) could brings more "feedback mechanisms" to the model. Feedback, as well as non-linearity, has obviously nothing to do with the mere size of the problem.

5 - Paragraph 4.6 and non-linearities. The authors state that "the only non-linear component is the asymptotic manner in which endogenous variable adjust towards their long-term equilibrium". Since the model is linear (see equations, and line 19 page 590), I do not understand how the authors can talk about non-linearities.

6 - More fundamentally, the method presented here is applicable to non-sationnary models that "cointegrate" (page 590 line 22), which means that some linear combina-

tion of the variables is stationnary. This restricts considerably the interest for such a method when applied on physical problems in general, and climate in particular. I am not convinced at all that, for instance, ice volume and Antarctic temperature should "relax" to some constant (linear) relationship in the absence of (non-stationnary) forcing (in particular since ice volume is in the North, and Antarctic temperature is in the South...). This restricts considerably the dynamics of the system and excludes many if not most (non-linear) theories of Quaternary climates. Furthermore, non-stationnarity in statistics is not the same as non-stationnarity in physics (non-stationnarity in physics often means that other forcing mechanisms that are not accounted for, may change through time). This is in particular the case when looking at the astronomical forcing over the million year time scale.

7 - Annual mean insolation (page 602) is almost equal to obliquity and does not depend on latitude (beyond a multiplying factor changing the absolute value and amplitude). Using different latitudes will therefore not change the result. There is no ned to perform statistics.

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