Clim. Past Discuss., 6, C1275–C1278, 2011
www.clim-past-discuss.net/6/C1275/2011/

Climate of the Past Discussions

Interactive comment on "Glacial cycles and solar insolation: the role of orbital, seasonal, and spatial variations" *by* R. K. Kaufmann and K. Juselius

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Received and published: 10 January 2011

The authors apply a statistical model, called 'Cointegrated vector autoregressive model' to EDC and LR04 palaeoclimatic records 'converted to a common time scale'. The article is partly tutorial in nature, with some elaboration on the difference between stationary and non-stationary processes. The statistical methodology was introduced elsewhere (Juselius 2006), although, unfortunately, this reference is not listed in the bibliography. The objective of the authors is to disentangle the influences of internal dynamics from the effects of astronomical forcing on the dynamics of glacial cycles. The conclusion proposed here is that 'solar insolation associated with changes in Earth's orbit have the greatest explanatory power, and that obliquity, precession, and eccentricity are needed to generate an accurate simulation of glacial cycles'. There are further

considerations about ‘seasonal variations insolation playing a lesser role’.

As a palaeoclimae scientist, I feel that that involvement of statisticians in the difficult task of inferring information about the dynamics of the climate system from palaeoclimate records is not only welcome but necessary. Although my position in the past few years has been to favour Bayesian modelling for combining physically-rooted information on climate dynamics with observations, the present article, based on a more traditional frequentist approach of null-hypothesis testing of a very general model triggered my interest.

Statisticians seeking an application of their work face the challenging obligation of learning about the specificity of this application in order to have enough grasp on what the scientific problem actually is. This is the reason why many such papers involve several co-authors with complementary areas of expertise, for example, a statistician and a palaeoclimate scientist.

On reading the abstract I felt under the uncomfortable feeling that the authors did not demonstrate enough background on insolation theory nor on empirical palaeoclimatology to yield convincing insights into the extremely difficult problem that they choose to address. Disturbing errors in the historical background introduced in the first section and lacking elements of insolation theory strengthened this feeling [1].

A first major issue arises as the authors consider a fairly large list of explanatory variables, including the traditional astronomical elements and many measures of insolation at different latitudes, at different time of the year, daily mean or season integrated. Indeed, most of these different quantities are almost co-linear. Specifically, most measures of insolation, whatever the latitude, season integrated or not, can be approximated to excellent accuracy as linear combinations of $e \sin \varpi$, $e \cos \varpi$ and ε with e,

[1] authors may which to have a look at *Berger* (1988), *Crucifix* (in press) and *Crucifix et al.* (2009) for some more historical background

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eccentricity, ϖ, heliocentric true solar longitude of the perihelion and ε, obliquity [2].

The above is fairly standard knowledge among insolation experts, and on this basis it even is not clear how the 'great explanatory power of obliquity, precession and eccentricity' may be accommodated with 'seasonal variations in insolation playing a lesser role'. Likewise, considering that precession has five times more effect on insolation than obliquity, as implied at the top of p. 2559, does not make much sense as long as which insolation is considered is undefined.

The lack of discussion about chronology accuracy is also worrying. Astronomical tuning assumptions, unavoidable even in a time-scale such as EDC3, may challenge inferences on the effect of astronomical forcing on climate dynamics and this issue ought to be discussed with care in a paper like this one.

I am left with with a number of remarks on misprints or more innocent inaccuracies (e.g.: surface temperature is not a proxy, but delta-Deuterium may be seen as one; there is not such a thing as the 'periodicity of ice sheets'). I have to confess great difficulties in following the flow of the statistical theory presented here in spite of my interest in statistics. This might indicate a too poor articulation between the scientific framework of the authors and what is standard in palaeoclimate science to warrant a significant contribution.

I would like to support and encourage the investigations of the authors, but before one may hope to come to any level of agreement susceptible of yielding publication the above issues have to be addressed.

[2] Particular cases are solstice-centered insolations (linearly independent of $e\cos\varpi$), equinox-centered insolation (linearly independent of $e\sin\varpi$) and annual-mean insolations (linearly independent of $e\sin\varpi$ and $e\cos\varpi$). Exceptions are the globally-averaged, annual mean insolation (depends on e, but vary very little anyway) and fairly exotic measures such as the annual maximum of insolation at a given parallel between the two tropics, which rectify precession).

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Interactive comment on Clim. Past Discuss., 6, 2557, 2010.