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Climate of the Past Discussions

Interactive comment on "Potential analysis reveals changing number of climate states during the last 60 kyr" *by* V. N. Livina et al.

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The present paper diagnoses the number of stable (or meta-stable) states in the GRIP and NGRIP delta-18O records. The method relies on the estimation of the distribution function of δ^{18}O values, and then estimation of the pseudo-potential by resolution of the Fokker-Planck equation. The authors conclude that 2 stable states have co-existed throughout MIS 3, but a bifurcation to one state occurred around the last glacial maximum, then three or four states around the last deglaciation, to end up with one stable state at the Holocene.

Overall, creative methods of system identification applicable to palaeoclimate records are very welcome contributions to palaeoclimate research. The present work is in this vein. It appears as a follow-up of a previous study by Kwasniok and Lohmann

(henceforth KL): *Deriving dynamical models from paleoclimatic records*. [1] Equation (5) of the present paper corresponds to equation (23) of KL, where it is presented as "an alternative method" to identify the potential function [2] The paper of Kwasniok and Lohman is of course duly cited in the present manuscript.

The problem with this method is that it relies on two hypotheses that may be unmet in the present application: stationarity over the analysis window (the $\partial/\partial t$ term is dropped in equation (3)), and additive and stationary Gaussian noise (otherwise additional terms would appear in equation (3), invalidating the solution (4)).

Stationarity is particularly problematic during the deglaciation. The authors claim to identify four distinct states, which they admittedly cautiously interpret as representative of the full-glacial, Younger Dryas, Bölling-Alleröd and Holocene states. However, the residence time in these different states is large enough compared to the deglaciation time scale to call into question the stationarity hypothesis.

Second, Ditlevsen (1999) identified a strong alpha-stable noise component in the Greenland Ca record, that may interfere with the robustness of the state identification algorithm because it alters the form of the Fokker-Planck equation. A suggestion would be to test the method with assumptions (3) and (4), but using surrogate data including an alpha-stable noise component.

Finally, the method fundamentally relies on the 1-D nature of the state-space model, where the drift is parameterised as the gradient of a pseudo-potential. The difference between the sample autocorrelation obtained with this model, compared to data, is uncomfortable (figure 6 of KL). KL hypothesized that the memory at large lags, which they identified in the observations, may be due to the nonstationarity. Now that Livina et al. have identified a non-stationarity, would it be possible to test a model where this non-stationarity is effectively taken into accont and verify the shape of the autocorrelation

[1] That paper is not yet out of press but is already accessible on one of the author's website.
[2] (KL focus on the parameter estimation with the unscented Kalman filter).

function ?

More minor points :

1. As far as I recall Saltzman and Verbitzky did not adhere to the multiple-stable state paradigm, but rather interpreted the trajectory in the ice volume - CO_2 phase space as the signature of a limit cycle.
2. Equation (1) is inconsistent for purists of stochastic differential equations. η should rather be written as the increment of a Wiener-process.
3. How do we know, after for example rejection of $L = 4$ due to negative a_4, that a model with $L = 6$ would not provide a much better fit. Admittedly this makes intuitive sense, but a more rigorous justification would be welcome.
4. How was the level of noise determined in the surrogate data ?
5. Ditlevsen (1999) used Ca data rather than δ^{18}O, the former offering a much better resolution. What justifies the present choice of δ^{18}O ?

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