Earth System Dynamics Discussions

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Comment on esd-2021-95

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Referee comment on "Stable stadial and interstadial states of the last glacial's climate identified in a combined stable water isotope and dust record from Greenland" by Keno Riechers et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-95-RC2, 2022

The paper “Changes in stability and jumps in Dansgaard–Oeschger events: a data analysis aided by the Kramers–Moyal equation” analyses d18O and dust data from a Greenland ice core in order to gain further understanding of the famous Dansgaard-Oeschger (DO) events. They preprocess the time series with the aim of establishing a stationary stochastic process. They estimate Kramers-Moyal (KM) coefficients, which could possibly reveal jumps in the process, outside the framework of the Fokker-Planck equation, corresponding to what can be naively seen as regime transitions. They explore the added value of joint fitting of the d18O and dust data over treating them separately.

I’m not very convinced that the applied methodology is suitable. As far as i see, the authors do not test their null-hypothesis (H0) of a stationary process. I would not think that a stationary process described by the KM equations is consistent with a hypothetical nonstationary process that could not be rejected. Say, we have a nonstationary Ornstetin-Uhlenbeck process of

dx/dt = -a*x + B(t) + c*xi(t), (OU)

where xi is white noise, and B(t) = b*sin(sin(2*t)+t) is some regular nonstationarity. It mimics some regime behaviour with sudden and regular transitions. We can easily see that the pdf of x is bimodal. If we didn't know the underlying process generating eq., and perhaps we somehow overlooked the regularity of the transitions, we might think the underlying model is:

dx/dt = F*x + c*xi(t), (H0)

where F = -V(x), V(x) being a double-well potential function. If we are in the small noise limit, we know that the pdf takes the shape of V(x), so, we could estimate V that way. Furthermore, we can estimate the noise strength 'c' in some standard way too. Now the question is if it matters at all that we have an H0 other than the true process OU. It would not matter if in any appreciable way the processes perform the same, i.e., when, loosely speaking, they are consistent or approximately equivalent. For example, we can derive the probability distribution of residence times from H0, and perform a statistical test if our residence time data is consistent with that, or we can reject H0.

We want to perform a so-called crucial experiment (experimentum crucis).

Considering H0 of the authors, another likely feature based on which H0 can be rejected is the saw tooth asymmetry, in particular, that the cold to warm, c2w, transitions are much more rapid than the warm to cold, w2c, ones. Looking at the dust time series of Fig. 1 with much naivity wrt. physics, but with some experience about dynamical systems, i would think that c2w is an attractor crisis, whereas w2c is a noise induced tipping, and there is some slowly drifting control parameter, i.e., a nonstationarity when we exclude that parameter from our state variables.

The authors also make a reference to attractor crisis, in terms of a saddle-node bifurcation, but in some other context. It is the context of slices of a 2D potential function. This does not sound correct.

The paper is very well written in a way, but it doesn't make for a very pleasant reading journeying through flawed results, starting with the single variable approach, and then — at least as i suspect — even the 2 variable approach.

I attach the pdf of the manuscript with comments saved as annotations. Hopefully the authors find it useful in some way.

Note: I always review non-anonymously, and never make recommendation for or against publication. The recommendation that i make is only to circumvent the rigidity of the submission system, and therefore please consider it void.

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Please also note the supplement to this comment:
https://esd.copernicus.org/preprints/esd-2021-95/esd-2021-95-RC2-supplement.pdf