Answers: green
Comments by reviewer: violet

Comment on esd-2021-95

Peter Ditlevsen (Referee)

Referee comment on "Changes in stability and jumps in Dansgaard–Oeschger events: a data analysis aided by the Kramers–Moyal equation" by Leonardo Rydin Gorjão et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-95-RC1, 2022

First of all, we would like to thank the referee for his careful and constructive review and the overall positive feedback. We will in fact adopt most of his suggestions and are convinced that this will improve the manuscript substantially. The changes that we will make to our manuscript will become clear from our point-by-point answers to the referee's comments below.

In this paper two paleoclimatic ice core records are analyzed. These, the water isotope, d18O, and the dust concentration records are analyzed for the period 59-27 kyr BP, which is the glacial period dominated by regular occurrences of Dansgaard-Oeschger events. There are two major points in the paper: Firstly, the data are modelled as a stochastic process using the Kramers-Moyal equation to investigate the importance of (discontinuous) jumps in the noise. Secondly, the two records are modelled as a two-dimensional joined process.

It seems to me that the two points are only loosely related, and the authors could consider presenting them in two separate papers.

We thank the reviewer for the suggestion. We felt that the results presented – which as pointed out, could be separated into two manuscripts – still warranted a single manuscript, for the following reasons:

1. A 1D approach is a natural starting point for our investigation, from which our analysis unveils various inconsistencies motivating an investigation in a 2D setting.
2. The methodology is the same for both analyses. We easily could imagine a referee asking for the 2D investigation if we presented the 1D analysis exclusively.

In a strict sense, however, we do not provide full models. We had already considered including an explicit stochastic model for the discontinuous phenomena, but felt it was premature to include this. If the referee and editor still feel that our results would be better presented in two papers, we would of course be glad to consider this further, e.g. in terms of a Part I and Part II?

I enjoyed reading the paper and find it publishable. However, there are a few issues below calling for revisions before publication. I have two major concerns regarding the two parts, and some minor points: As to the first point, I have not seen the Kramers-Moyal (KM) equation applied to these data before, so this is a novel approach. The equation, for which the Taylor expansion of the conditional probability density function is taken to higher order than two, covers the case where the noise term in the governing Langevin equation is not gaussian, but contains jumps. It is stated that in the case of Levy processes the Fokker-Planck equation does not apply.

Actually, for the most relevant class of Levy processes, the alpha-stable Levy processes an extension of the Fokker-Planck equation based on the characteristic function of the alpha-stable process exists (see: Samorodnitsky and Taqqu (1994) ‘Stable non-gaussian processes, Chapman and Hall, NY. or Ditlevsen, PRE, 60, 172-179). The challenge in applying the KM equation is the estimate of the higher order coefficients (eq. 6) for the data series: Since the higher order terms are (increasingly) dominated by the extremes in the increments the finite time series very quickly “dilutes”. A main result (eq. 9 and figure 3) includes the sixth moment of the increments. I thus miss an analysis of uncertainties and reliability in these estimates. I find that this is essential for publishing this (nice!) result.

We thank the reviewer for this comment, it is a clear oversight on our part to state that one *cannot* describe Lévy-driven processes (or Lévy-noise driven Langevin equations) with a Fokker–Planck equation. We will amend the related statements in the manuscripts and point to the correct references wherein this is discussed. Similarly, we will include an explicit formulation to flash out what exactly is meant by discontinuity in our context, as also requested by the other referee in his review.

In order to estimate the uncertainties in the Q-ratio estimates in Eq. 9 and Fig. 3, in the revised paper we will introduce a metric for estimating the uncertainty of estimating higher-order moments in the conditional moments needed to estimate Q. Since we do not propose any explicit stochastic model for the discontinuous contributions in $\delta^{18}O$, we will analyse in place the uncertainty of Langevin processes with identical drift and diffusion as those estimated for the dust and $\delta^{18}O$ proxies, and will show that the Q-ratio of the discontinuous $\delta^{18}O$ behaves substantially different from a conventional continuous Langevin process.

Another point which could be given a little more attention is the fact (as also correctly stated) that the strong time-asymmetry in the data (the sawtooth shape) cannot be captured by the model. How does this influence the relevance in including higher order terms (higher than second order) in eq. 5?

We thank the reviewer for the remark. In some sense our choice of showing first the two separate one-dimensional analyses, alongside with precisely the aforementioned observation of the sawtooth shape, is to point out time-asymmetry cannot be captured in a *one-dimensional* setting under a Langevin-equation. However, the statement we make in the manuscript

l.233 Note that the model equations employed here are by construction symmetric with respect to time, therefore, as it is, the model cannot reproduce the temporal asymmetry that is visually suggested in the dust record.

is not precise, in the sense that only after having estimated the drift and the constant diffusion and the absent 4th-order KM coefficient, the KM results for the dust are inconsistent with the apparent time asymmetry – this inconsistency is thus not by construction. This is also exactly the point the referee points to: a time asymmetric stochastic process is very likely to exhibit non-zero 4th-order KM coefficients. On the contrary, designing a time asymmetric stochastic process with drift and diffusion exclusively is far more difficult in a purely autonomous setting – yet not impossible. In short: the time asymmetry of the data is an indicator that one should include higher-order terms in the KM expansion.

In order to give an example: we can achieve time asymmetry very simply by considering a process like a Langevin process, just augmented with a discontinuous trajectory. A simple example would be

$dx(t) = - a(x)dt + bdW(t) + cdJ(t)$

where J(t) is a Poisson process with a jump rate $\lambda>0$. In this simplest of formulations, if there is at least 1 jump from the Poisson jump process, the process becomes time-asymmetric. (From an applied point of view, there are some considerations to be respected regarding the relation of the amplitudes of a(x), b, and c.)

In light of the construction of our paper – which deliberately avoids writing down specific stochastic processes as the one given above – we shun from including this, but included the higher-order terms from the Kramers–Moyal equation, which point at the existence of such discontinuities in the ice-core time series.

We note that the strong time-asymmetry can – and is – captured in a two-dimensional setting, just as we show in our two-dimensional analysis.

As to the second point, the major results are presented in figure 4. Obviously, when considering a one-dimensional record, the drift can always be seen as a result of a potential. This is not the case in two - and higher dimensions, where gradient drift is a non-generic case. I'm sure that the authors are aware of this, the drift is a two-dimensional flow field, as also shown in the small inserts in the subplots of figure 4. I find the construct of pseudo-one-dimensional potentials (V(x_1|x_2)) both confusing and useless. I suggest that the authors consider abandoning this all together (as well as the notion of a potential landscape). The interpretation in figure 4(c) of a double fold bifurcation is obscure, and -I believe- wrong.

We thank the reviewer for the comment. We ourselves have struggled with the "pseudo one-dimensional" potentials. It is clear to us that if the reviewer finds them unhelpful, then our doubts about their usability are confirmed. We will remove them in the revised version and directly showcase instead the two-dimensional drifts as quiver plots, which we hope are clearer.

The referee's comment on Figure 4(c) (together with a similar comment by the other referee) clearly shows that we have not conveyed our thoughts properly and therefore we will improve the manuscript as follows:

We will revise the discussion of Figure 4 to make our conclusions more precise. After reviewers' feedback we still keep the interpretation of Fig. 4(c) that it shows a double-fold bifurcation if one treats dust as the dynamical variable and $\delta^{18}O$ as control variable. Since we will not use the notion of conditioned potentials, we will explain this double-fold bifurcation in terms of the nullclines of the dust drift, conditioned on the $\delta^{18}O$. The definition of a bifurcation always depends on what is the variable and what is the parameter. For example, the standard form of a fold bifurcation is given by $x^2 = a$, where x bifurcates when the control parameter a crosses 0. Obviously, no bifurcation occurs if we reverse the roles of a and x.

There remains of course the possibility that the double-fold structure that we observe is spurious and simply an artefact arising due to the scarcity of data (especially in the region of the state space where we observe the saddle-node bifurcations). We pursued two approaches to rule this out.

First: we have tried to adapt the data analysis as suggested by the reviewer (see our next reply). We applied PCA to obtain a new 'rotated' basis (p_1, p_2) and projected the data onto that new basis. Subsequently we applied the same method to the rotated data as we did to the original data. In this case we find one variable, p_1, which appears monostable and mostly independent of the other variable p_2. In contrast, the dynamics of the other variable p_2

strongly depends on p_1. Hence, the two processes do not decouple as hypothesised by the reviewer. Now, if we take p_2 as the dynamic variable and p_1 as a control parameter, we do no longer see an archetypal double-fold bifurcation as correctly remarked by the reviewer. However, the prime dynamic fingerprints are still there! The nullcline has the shape of a lying S (see Figure below) and if you rotate it back you rediscover the double-fold bifurcation.

Second: we generated synthetic data in a truly decoupled setting, with a double well potential in one direction and a single well in the other. Then we rotated the synthetic data, introducing a coupling between them, and again applied the estimation of the KM coefficients. The results (see Figure below) truly differ from our Fig. 4(c).

Together, these findings make us confident that the interpretation of Fig. 4c as a double-fold bifurcation in the dust (conditioned on $\delta^{18}O$) is meaningful.

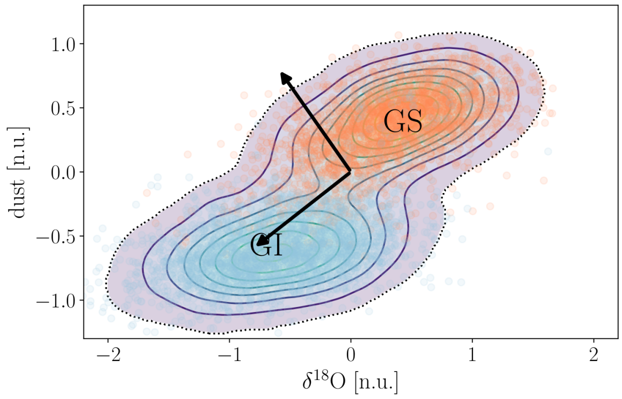
Taking this discussion further, the interpretation of the results depends very much on the interpretation of the variables. After all, $\delta^{18}O$ and dust are only two observables and $\delta^{18}O$ is only a proxy for the temperature (and dust an even more uncertain proxy for atmospheric circulation). Hence, it might be more appropriate to view them as indicator variables of more fundamental, unobserved atmospheric variables.

In conclusion, it is of course impossible to reach a definite answer on the entire physical mechanism leading to the observed bimodality from our approach. Still, we can derive some very interesting conclusions. In particular, the hypothesis that the bistability is rooted in the temperature, which then drives the dust variable as a dependent variable, is highly unlikely given our results. Furthermore, it should be further investigated if a bistability in circulation patterns exists, which can then drive a bimodality of the Greenland temperatures. In the revised version, we will sharpen the discussion section to make the interpretation clearer.

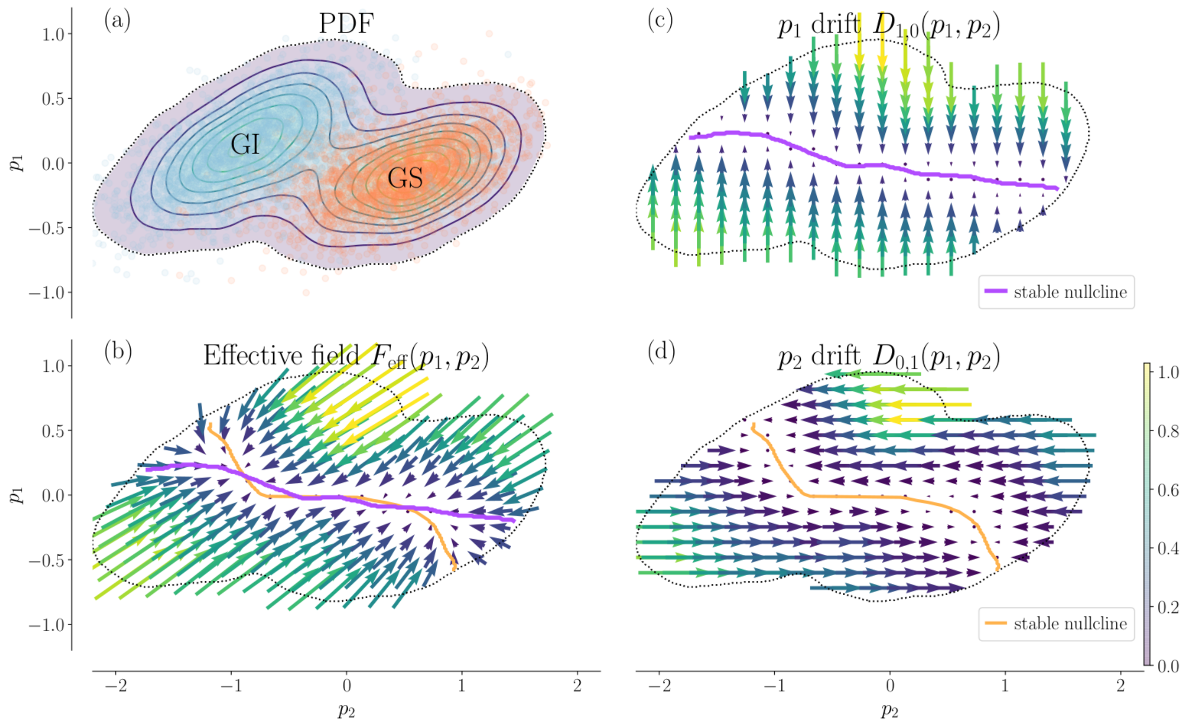
What I find interesting is the scatter plot in 4(a), which nicely explain the results in figure 2, namely that the stationary distribution for the dust is bimodal while it is unimodal for the d18O: This corresponds to the marginal distributions in 4(a) (projections onto the axis.

The authors could consider analyzing a rotation (linear combination of the two variables) of the data along an axis connecting the two maxima (GS and GI) and a perpendicular direction. In this way one would obtain a “clean” two state dynamics and a “clean” one state perpendicular dynamics. First thing would be to check for independence. Just a suggestion.

We thank the reviewer for the suggestion. We have considered a rotation to a new set of axes given by v_1 = [-0.80, -0.59] and v_2 = [0.80, -0.59] (obtained via PCA) , in relation to our frame of reference, which are orthogonal. Note that also as suggested, we are using the negative of the logarithm of the dust.



From this we draw two new “projection” time series, p_1 and p_2, that have a Pearson correlation $\rho(p_1,p_2) = 0.01$. We can similarly draw the drifts in a two--dimensional setting from these time series, as seen below:



Minor points:

Introduction: These data have been analyzed over many years and a lot is known. For a better overview and setting the present work in context, a representative presentation of work done over the years would be useful. There is a strong bias towards very recent publications.

In retrospect, we agree with the reviewer that there is indeed a bias towards the works in the more recent past. We will ensure we correctly cite earlier works in the revised manuscript.

Again: I'm not fan of the "potential landscape" metaphor.

We will remove the use of the term throughout the revised manuscript.

Figure 1: I suggest to plot the dust record upside-down. This will visualize the strong dependence between the two records, and also make the saw tooth shape in the dust record much more apparent. Make the figure full text width, Ylabel: ln(dust) (no units), d18O (permil). Or normalized w.r.t. std. dev.

We thank the reviewer for the suggestion. In fact, we meant to do this already in the submitted manuscript and even say in the text that we did it:

l.87 *Since the dust concentrations approximately follow an exponential distribution, we consider in the following re-scaled values by taking the natural logarithm and multiplying by −1 in order to emphasise the similarity to the $\delta^{18}O$ time series (cf. Fig. 1).*

So, we will multiply the log(dust) by minus one in a revised version of the manuscript.

L90: A discussion of concentration vs flux of dust could be added

We agree with the referee that this point merits being mentioned. In a revised manuscript we will shortly comment on this and provide corresponding references. We will not go into detail, since this manuscript relies on the proxy interpretations provided by other studies.

L91: Data is -> Data are

Thank you, corrected.

L96: This was pointed out by others previously: Rial and Saha (2011), Abrupt Climate Change: Mechanisms, Patterns, and Impacts, Geophysical Monograph Series 193, Mitsui and Crucifix, arXiv:1510.06290, Lohmann and Ditlevsen, 2018, Clim Past 14. (and probably others).

Thank you, we will include these citations, along with several others, to best reflect the present and past research around DO events.

L110: I do not understand why

This was an oversight, we included the auto-correlation in the appendix, not the auto-covariance.

L125: Stationarity require certain properties of a(x) (such that the process does not drift to infinity. It is better to denote it "homogenous" or "autonomous". (same comment on L138).

Thank you, our choice of wording was not the best. We will correct the statement in L125 and L138 to:

“[...] these terms are called *autonomous*.”

L126: Langevin equation is continuous -> Langevin equation generate realizations which are continuous

Thank you, this will be corrected.

L155: There is no Eq 4. (4a and 4b are hardly equations)

We agree, the relations do not merit two explicit equations. We will fold them into inline equations in the text

L185: The purely continuous process (gaussian process) diffuse proportionally to t^(1/2) not t. That is: (sigma(t)=sqrt(4Dt)). The most natural jump processes in this context are the alpha-stable processes, they do exhibit similar scaling relations with time (sigma(t)~t^(1/alpha)).

Agreed. We meant to point out that the second moment / mean-square displacement $<x^2> \sim t$ (and anomalous diffusion with different power). Confusingly we did not say explicitly we are referring to the mean-square displacement / second statistical moment. We will make this more explicit in the revised manuscript.

L205: Also referring back to Figure 1: Wouldn’t it be natural to rescale the data before doing the analysis.

In fact we did. The data has been rescaled into normalised units, as detailed in Appendix A. All figures are given in n.u. (normalised units), we scale the data to a characteristic scale of the state space in both dimensions. However, in a revised version of the manuscript we will scale the system with respect to its standard deviation.

L239: I do not understand the statement (which I believe is not correct): This indicates that d18O exhibits faster dynamics than dust. Please explain.

Typically, the term fast-slow dynamics refers to two coupled stochastic or ordinary differential equations one of whose right hand sides is multiplied by a time separating factor \tau. Here, we see that both the drift and the diffusion along the d18o dimension exceed the ones along the dust dimension – even in the normalized system. We could therefore extract the classical time separating factor in the SDE and we would find ourselves in the typical slow-fast setting.

The current version of the manuscript does not convey this very well, and we will consider bringing the equations in the typical fast-slow form in order to make this more understandable.

L245: The exotic explanation for monostability through a complex noise structure seems a little out of context here: The reason why the d18O record has a single maximum in the PDF (with a shoulder) is the sawtooth shape of the DO events masking the obvious two state nature of the record.

This is a fair point. Our target here is to point out that, in the context of regime switching, this phenomenon can arise from either complex noise structures, such as jumps, or via a coupling to exogenous, hidden variables (see our answer to the other referee). Both manifest changes that can potentially be encoded in the 4th-order KM coefficient. In a revised version of the manuscript, we will provide a clearer formulation and emphasise more strongly the connection to the higher-order KM coefficients, such that the reference to the 'complex noise structure' would be less out of context.

Section 4.2.1 is obscure to me.

We refer to our reply above relating to the double-fold bifurcation.

L273 and L288: 4 (d) <-> 4(c) .

Thank you, this will be corrected.

L456: What happened to the index _t in mu and sigma^2?

Thank you, this will be corrected.

I hope my comments are useful.

Very much so! We thank the reviewer for the very helpful commentary and for pointing out vital information we had failed to include in the manuscript.