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Interactive comment on "Collapse of the Atlantic Meridional Overturning described by Langevin dynamics" *by* Jelle van den Berk et al.

Anonymous Referee #2

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This is an interesting paper, which aims to describe the collapse and hysteresis of the AMOC observed in intermediate complexity climate models subject to freshwater forcing by low-dimensional Langevin dynamics as a stochastic bifurcation of a double well potential. Substantial revisions are necessary to improve the clarity of the manuscript and to support the conclusions.

General comments

1.) It is not clear what the purpose of the paper is. The authors do not state what their model is able to explain or predict. Is the purpose to predict the exact parameter value of a collapse? Or at least to develop a method to do this? Are there prospects to apply the method to observational data? Or is an aim to understand dynamically what is happening in realistic climate models? This should be stated in the introduction

(P2L29ff). It is also unclear whether they want to only/mostly model the AMOC collapse (as stated at some points in the paper) or also the resurgence.

2.) Regarding the conclusions, how can the authors say that the model successfully captures the dynamics? They don't compare with other models of higher or lower complexity, nor do they have any metric that shows goodness of fit or anything similar. This would be necessary to make such a conclusion.

3.) The manuscript is not very well written and hard to follow. The terminology is often unclear. (E.g. what is a "track", and how does the use of "stability landscape" apply here? See specific comments.) Some corrections are given under "technical corrections", but the language and terminology has to be generally improved throughout the manuscript. Furthermore, I believe the manuscript can be shortened severely. What the authors want to get across can be said more efficiently. Many things are mentioned twice or more (see specific and technical comments for suggestions). Finally, the labels in multiple figures are unreadable.

4.) The data acquisition seems problematic. I am not sure whether it is viable for this journal to present a data analysis based on visually extracted data from a figure of another publication. Accordingly, the quality of the data is a major drawback of the study (e.g. arbitrary smoothing and AMOC metric). Their main problem in fitting the data might be due to the specific metric that is shown in the Rahmstorf et al. (2005) figures, so it is a shame that the authors are not able to resolve that.

5.) The description of their method contains many errors, and is incomplete. An explicit expression for the likelihood, as well as details of the Metropolis-Hastings implementation are missing. In the discussion, the authors name difficulties in the numerical implementation as a possible reason for the failure of their fit to describe the lower AMOC branch, but it is for the reader not possible to assess whether this is relevant, since no details or robustness tests are given. Furthermore, it is not stated how many data points the respective data sets contain, and it is not mentioned that the authors assume successive data points to be independent. It is also not mentioned how the maximum of the posterior parameter distributions is picked.

6.) Finally, several questions regarding the methodology. a) Why do the authors not try to estimate sigma with their Bayesian method? Why not include observational noise? This could handle the fact that the data is filtered arbitrarily. It could also completely change the locations of the inferred bifurcation points. b) To make the paper more understandable it would be good to note explicitly early in the manuscript that the movement of mu is actually known. c) Why not try multiplicative noise? (see also e.g. Das/Kantz Phys. Rev. E 101, 062145, 2020) This should relatively easily give a model that describes the asymmetric behavior. d) It should be noted explicitly that there is no time dependency of the data. I wonder why they choose not to fit to time series instead? This would allow to treat the non-equilibrium nature of the data. Also, it would be much more applicable to observational data and to make predictions. e) Why not only move along alpha at a certain fixed beta? Is moving both parameters supported by the data significantly better?

Specific comments

Abstract: "Machine learning": To my knowledge MCMC is not considered a machine learning technique. The abstract needs to be expanded to better reflect the motivation of the study, what their method enables them to do, and their conclusions.

P2L42-45: This is a not a very clear explanation of the salt advection feedback. The main point is that North Atlantic salinity anomalies (positive/negative) are amplified by their effect on the overturning flow (strengthening/weakening), the strength of which controls the North Atlantic salinity. This is thus a positive feedback and leads to bi-stability with the associated possibility of abrupt transitions.

P2L53: "…number of solutions for a given value of the freshwater forcing goes from 2 to 3… ". Should say "goes from 3 to 1" as the bifurcation point is crossed. (There are 2 solutions precisely at the bifurcation point, but I think this saddle-node fixed point is not relevant here.)

P3 Caption Fig.1: The terminology of this figure is not appropriate and furthermore not understandable at this point within the manuscript. No trajectory is shown, but a bifurcation diagram. They have to be more specific with what they mean by a deformation of the “trajectory”. Also, at this position within the manuscript, it is completely unclear what they mean with “trench of the distribution”. Either leave out or explain in the main text. Furthermore, I suggest to use the term “resurgence point” for mu-, and use that terminology throughout the paper. Note that e.g. in P5L91, mu+/- are being referred to as “collapse points”.

P4L64: Can the authors elaborate why they think a double well potential has mainly been studied qualitatively? I would argue that this simple and general mathematical model has been studied quantitatively to an exceptional degree.

P4L65ff: It is a bit confusing when the authors first say that 2 parameters are enough to describe bi-stability, but then use another 2 parameters to scale and shift to the AMOC variable. Maybe it would be better to first explicitly say that by a shift and scale of the variable x, one can eliminate the third order term as well as the fourth order coefficient. Both of these transformation do not influence the global bifurcation behavior. Then, they can state that a shift and scaling is considered when fitting to the climate model data.

P4L78-81: Can the authors elaborate why they obtain these rough estimates for the parameters, and how they are insensitive to other parameter values?

P5L90-91: When speaking about “solution” what exactly do the authors mean?

P5L92-97: This section is a bit unclear. Can the authors define a “track”, and what does it mean to be one-dimensional? The fact that alpha and beta are called normal and splitting factor is better mentioned earlier. A more clear distinction of “parameter” and “variable” would be appropriate.

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Discussion paper

P5L101: This argument is unclear to me. The fact that the AMOC is scalar variable should not constrain the path through the stability landscape in any way. Do the authors rather want to say that in the climate model experiments there is only a single control parameter mu, and that by assuming a linear dependency of both alpha and beta on mu, they can express some parameters by the extremal values of mu?

P8L127-129: Maybe the authors can elaborate more specifically on why these arguments are relevant in order to neglect a non-linear change of either mu or alpha/beta?

P8L141-146: Improve this explanation. When introducing stochasticity, the asymptotic dynamics for each parameter value give rise to a stationary density. In the case of the scalar potential, this distribution can be given analytically up to a normalization factor. Thus, the distribution can be used as a likelihood function (if I understand correctly) for parameter inference with MCMC.

P9L157: What is the "sampled" bi-modality region?

P9L160: "These changes correspond directly to the potential functions in Fig. 2." What is meant by this?

P10L174: Can the authors explain why lambda < nu in general?

Caption Figure 5: The terminology is unclear. What is meant by a "singular" maximum? What is meant by the dominant and the weak mode, and what is the inversion? There are also grammatical errors ("...in the middle and inversion from weak. . .").

P11L180: I think a more precise statement would be that they estimate the posterior probability distribution of the parameters, given the data psi(mu). Furthermore, the following equation does not define the likelihood but the posterior distribution.

P11L183: Why linearised?

P11L184-188: This statement of Bayes' rule is not correct, please revise. The right hand side is not called Bayes' factor (which arises in model comparison).

P12L209-219: The constants muS+ etc. are not properly introduced and should be shown in one of the figures. The footnote 2 needs to be explained better.

Caption Table 1: Why is a linear function used and not a higher order polynomial? This does not seem to be very suitable to the data.

P13L228-230: This is not very precise wording. What do the authors mean with “unstable” and “more stable” solutions?

P14L242-245: What do the authors want to say here? It comes as a surprise to me that suddenly only the data for mu > mu+ should be relevant? And why do they now claim that the model C-GOLDSTEIN does not “appear” to show a collapse?

P14L252-253: Unclear what the authors are trying to say.

P14L259: What is meant by “non-linear degradation”?

P15L267: In what way is the model sufficient to describe the data? Certainly the “reinvigouration” is not well captured.

P17L308-310: Unclear what is meant here. What are “non-admissible” solutions?

P17L313-315: Unclear. Smoothing might be due to other reasons?

P17L323: What is meant by “direct numerical stochastic integration”?

P17L330: How exactly does this paper present a step forward to assess the likelihood of a future collapse of the AMOC? The method presented here relies on previously modeled collapses of the AMOC with realistic climate models. How does the method generate additional information?

Technical corrections

P1L15: last glacial maximum and early holocene -> last glacial period

P2L26: . . . which are presumed to be functions . . .

P2L30: ...and tipping points in the climate, it has not been . . .

P2L38: . . . or an increased surface freshwater flux by changes in precipitation minus evaporation. . .

P2L46: scalar variable obtained by integrating ...

P2L52: . . . one of the two basins of attraction vanishes . . .

P7L126: rather “(delta alpha, delta beta)”?

P8L142: Remove: As shown by Cobb (1978), this distribution belongs to the exponential family.

P8L144: The polynomial potential introduced in the previous section, we had already. . ., gives the probability . . .

P8L148: Note that C = C(alpha,beta), which does not have...

P8L150: . . . because of the scaling ...

P8L152 and Fig. 4 caption: In what way is this a sample collapse trajectory, or an example trajectory?

P9L161: . . . a change in only one . . .

P9L163-165: Why not say this at P8L148? It is a bit redundant otherwise.

P10L168: arrived at -> described

P10L171: independent of each other

P11L180: The method used is not considered machine learning.

P11L190: Does not seem to be relevant, as it is not done here.

P11L194-196: This is partly redundant, and it is not clear why the authors mean that the model can be fit with uninformative priors.

P11L199-200: Redundant.

P11L207: Redundant.

P11L207-208: An overview of priors is: The following prior distributions are used:

P14L243: do -> to

P14L254: Why “sample” paths? The authors are showing distributions, which is exactly contrary to showing sample paths.

P14L255: Redundant.

P15L265: couples -> models. Why “additionally”?

P16L275: Do they mean arg(max(|psi|)) ?

P17L309 till its -> until it

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