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Earth System Dynamics Discussions

Interactive comment on "Cascading transitions in the climate system" *by* Mark M. Dekker et al.

Mark M. Dekker et al.

m.m.dekker@uu.nl

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We thank the referee for the careful reading and the useful comments and will adapt the manuscript accordingly. Below is a point by point reply with the referee's comments in bold font, our reply in italic font and the changes in manuscript in normal font.

1. Comment of the referee:
Section 2: The authors first describe possible scenarios of cascading tipping by combining the normal forms most relevant for applications and involving only one or a pair of stability exponents crossing the imaginary axis. As such, the framework is suited for coupled systems for which both the leading and the following systems are close to a saddle and/or a Hopf bifurcation, a situation relevant for the applications considered here. However, the climate system is a

high-dimensional system with a large number of positive Lyapunov exponents, whereas the bifurcations considered here involve only one or two-dimensional attractors rather that chaotic sets. As such, while the mathematical framework considered here appears to be an important direction to explore for climate applications, I would consider it only as a first important step towards understanding more complex abrupt climate changes, such as the one studied in section 4. This point could be discussed more by the authors.

Author's reply:
We agree that abrupt climate changes in reality are connected to more complex chaotic sets and impossible to attribute to a single bifurcation or two bifurcations. As the referee also points to, the aim of this paper is to give a framework of cascading transitions with mathematical examples, analyses and applications to conceptual models. The step towards the real climate system should be taken with care. Especially in the beginning of the paper this can indeed be made more clear. In the discussion section, this was already mentioned (e.g. page 16, line 5-7).

Changes in text:
We will address the connection between the idealized cases of cascading tipping here and transitions in the real system in the revised introduction en discussion.

In the beginning of section 2 we will add: 'In this section, we present a mathematical framework for simple cascading transitions, that acts as a first step towards analysing the more complex transitions happening in reality.'

2. Comment of the referee:
In bifurcations involving meta-stable states, such as the double saddle node bifurcation, or bifurcations involving strange attractors (e.g. (Tantet, Lucarini,

Lunkeit, & Dijkstra, 2018)), a critical transition occurs through a saddle point, or a strange saddle. In this case, although the saddle set is globally unstable, its stable manifold may be responsible for a slowing down at the vicinity of the saddle, resulting in what also looks like a two step transition. Could you discuss why the cascading bifurcations may or may not be a better candidate to explain the two-steps transitions such as observed during the Eocene-Oligocene transition?

Author's reply:
In DeConto and Pollard (2003), it is suggested that the atmospheric CO2 concentration influences the existence of an ice sheet on Antarctica, via its effect on the ice-albedo and height-mass balance feedbacks. As the box model by Gildor and Tziperman (2000) contains these feedbacks, and a boxed ocean in which Tigchelaar et al. (2011) found multiple steady states for the meridional overturning circulation, a cascading event (of two bistable systems) could be simulated here (as written on p. 15 line 29 to p. 16 line 2). Of course, the comparison with the Eocene-Oligocene transition as found in proxy records should be made with care, because of the simplicity of the model used in Tigchelaar et al. (2011). In the present manuscript we have added this mainly as an example, but clearly further work is necessary to substantiate the hypothesis of cascading tipping being relevant for the Eocene-Oligocene transition. In particular, the coupling of the two bistable systems via the carbon cycle (determining the atmospheric CO2) requires more attention. This goes beyond the scope of the present manuscript and will be elaborated on in a follow-up study.

Changes in text:
We will cite and shortly discuss the Tantet et al. (2018) paper. In the revised discussion, we will add: 'Although from a physical perspective, this is a potential example of a cascading transition, we make no claim about whether such a transition likely occurred at the Eocene-Oligocene transition.'

3. Comment of the referee:
Section 4: In Fig. 7, there is indeed a strong correlation between the temperature gradient and the wind stress. However, as the author remark, there is also a strong spread, which should result in a strong variance in the estimate of the coefficients in Eq. 21. Could you use an ensemble method such as bootstrapping or a Bayesian model to test the probability that such a cascading tipping indeed occurs when sampling the different values of the coefficients of Eq. 21? This would allow to discuss the robustness of the results to the dependence of the wind stress on the temperature gradient.

Author's reply:
This is a good suggestion. Note that the results shown in Fig. 8 and 9 are dependent on multiple parameters and choices made (not only the ones that are derived from Fig. 7). To be precise, these are the definition of the North Atlantic and the Equatorial Atlantic regions, the zonal wind stress region, the reference wind stress parameter (τ_0), and it also turns out that the temporal resolution and running mean may dramatically change the values of α_τ and γ_τ in Eqn. (21).

Changes in text:
We will add such results (using bootstrapping with different values of α_τ and γ_τ in Eqn. (21)) to the revised paper.

4. Comment of the referee:
You explain well how the parameters of the wind stress equation are estimated from the model runs. However, it is not clear to me how the parameters of the Stommel and of the Timmermann are chosen. Are the parameter values used

the same as in the references? Are they chosen so as to be as close as possible to historical data? So has to reproduce the mean state and variability found in observations? Or so as to favor the occurrence of the cascading tipping? In any case, I understand that estimating the parameter values of minimal models from observations or complex models is a difficult and not always relevant task. However, the sensitivity of the occurrence of the cascading tipping on the parameters of the coupled Stommel-Timmermann model should be discussed to better assess the likelihood of such tipping to occur.

Author's reply:
For the Stommel and Timmermann models, we have used the standard values as in the original references, except when stated otherwise (for example in the case of the freshwater forcing in Eqn. 23). The parameter mu (that partly determines the closeness of the ENSO system w.r.t. the Hopf bifurcation) has been chosen to be near critical for the Timmermann model.

Changes in text: We will explicitly mention how the value of mu (in caption of Fig. 9) was determined. On page 14 we will add: 'In the Stommel and Timmermann models, we use the standard parameter settings, as given in the references, unless stated otherwise.'

5. Comment of the referee:
Discussion: Salinity biases, such as found in the GCM used in this study, have shown to have a strong impact on the bi-stability of the AMOC (Mecking, Drijfhout, Jackson, & Andrews, 2017). Considering that the strengthening of ENSO also occurs in the control run, could you discuss whether this is/is not an important factor to take into account when asking whether or not such a cascading tipping of the AMOC+ENSO system could occur in the future.

Authors reply:
Whether a cascading tipping event is what actually occurred in the HOSING-1 runs, is not known. Probably there is a more complicated reason behind the increased SST variance in HOSING-1 with respect to the standard runs, and likely a mix of different effects. The effect of salinity biases on the bimodality and hence on the AMOC-ENSO coupling is interesting but outside the scope of this paper. In the coupled Stommel-Timmermann model, we know in which parameter regime of the freshwater flux there is bimodality in the AMOC because that follows directly from the Stommel model's design.

Changes in text:
In the revised discussion, we will shortly mention the effect of salinity biases on the bimodal behavior of the AMOC in GCMs, and its potential effect on the cascading behavior (and cite the relevant papers).

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