Earth System
Dynamics
Discussions

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

Anonymous Referee #2

Received and published: 2 June 2018

The article "Cascading transitions in the climate system" by M. Dekker et al. conceptually explores "cascades" of tipping points. The phenomenon is interesting and relevant in many contexts, and the authors focus on the climate system here and present a modelling example. Tipping points and their precursors are a popular topic in many articles but to my knowledge they have not been analysed in this specific context before. The technical quality and presentation of the article (structure, language and figures) are on a relatively high level. To this extent, the manuscript merits publication in Earth System Dynamics.

There are however also some fundamental questions that I think are important to be (re-)assessed and clarified, and the manuscript should be revised accordingly:

1. It is not very clear to me what the authors see as the main aim of the paper. In the abstract it is stated that they aim at providing a new theory / a mathematical frame-

work. I am not convinced that these specific claims are supported by the contents of the paper. For example, isn't a theory something that provides an explanation for a certain number of facts? What is the new explanation here, and of what? I like how Sect. 2 systematically explores conceptual models of two combinations of generic bifurcations. In this section, I have the impression that one tipping point immediately triggers the next (instead of the tipping point in the leading system only bringing the following system closer to its own tipping point, which is then again triggered by the changing control parameter)? If this is the case, how can early warning signals even be used to predict the second transition? I will elaborate on this point below. In general, the early-warnings analysis in Sect. 3 is less clear to me than Sect. 2. I have the impression that the authors present two analyses of a single tipping, and not one analysis of an induced tipping, which somewhat questions the novelty of the approach. At first, I thought that the authors aim to predict the second tipping before the first, or infer what kind of bifurcation to expect. However, after the first examples of cascading tipping it seemed like the approach was to use early warning signals to first predict the first transition and after that predict the second transition, but to do that the concept of cascading tipping is not necessary, since they basically predict two tipping points independent from each other. In this context, I was also wondering why the external shock that the second system receives must result from a bifurcation in the leading system. Could it not also result from other kinds of tipping points, or a sudden step or peak in forcing like a volcanic eruption or a pulse release of greenhouse gases? Why is the leading system needed at all when the main aim is to detect if the first shock will trigger a transition in the following system? The model example in the end (ENSO-AMOC model) is interesting, but its purpose is not clear enough to me. Maybe the authors can clarify what it is that they want to demonstrate exactly and state this clearly in the introduction and draw conclusions using the results they show. The conclusion section should be extended by a discussion about what questions are answered and what the implications of the results are. What can we do or understand with the approach in this paper that we were not able to do or understand before? What should be done next?

2. The reasons for the choice of methods should be explained better. This is often linked to the problem mentioned above, i.e. the lack of clarity about the aim of the study. Once this aim becomes clearer, it should also be easier to explain why certain methods are applied.

2.1. Specifically, the choice of statistical indicators needs better justification. I currently do not see what the early warnings approach can add to previous studies. For example, why is DFA used as a warning signal instead of just the autocorrelation? Since autocorrelation is simpler to calculate and more intuitive, I would like to see an argument for the added value of DFA. The statement that "standard quantities not always provide an early warning signal" (page 6, line 26/27) should be backed up with an argument and references, and then it should be explained why DFA can cope with this. I would actually expect DFA to fail whenever autocorrelation fails, which happens when the system is more complicated than the typical Langevin equation / AR1-process with one fixed time scale. One argument the authors give is that "DFA copes well with non-stationarity". First: What is the explanation for this statement? What is the tradeoff when using DFA (more data needed?). Second: Couldn't one just remove non-stationarity with a high-pass filter (which is what the authors seem to do already) and then use traditional early warning signals? The authors use relatively simple models here, where the parameter can be varied as slowly as necessary to remove non-stationarity (or they could even make long stationary time series for different fixed parameter values). Another argument the authors provide is that DFA captures long-range correlations. But why should one expect such long-range correlations in the simple models the authors use? Can they even exist? So, in a nutshell, why is DFA needed in this paper? Then the authors generalise DFA to capture the involvement of several state variables (using DCCA). This could make sense if they were trying to detect something about the coupled system, for instance, which variable is leading, what will happen after tipping 1 and 2. However, the main results seem to consist in predicting tipping 1, and then detecting that the following system has moved closer to a tipping point (by the way, how do we know that there is a second tipping point? The

C3

fluctuations could just have changed for another reason.). As far as I can see, DCCA is not needed to do so, an AR1-analysis of each single variable may have sufficed. The explanation on page 7, lines 26-30 is unclear to me. In what way and to what purpose and why can Pearson's correlation "not be used"? And what is meant with a "one-to-one-relationship" (line 30)? Sect. 3.3.1: The authors state several times that DXA and DCCA are sensitive to the segment size and moving window size, but have different values been tried? It would be nice to show how sensitive they are, and what this means for the results. It could help already to just show more runs with different parameter settings. In section 3.1 The essential part of degenerate fingerprinting is the projection on the leading EOF in a multivariate system. However, the manuscript skips this part of the method, and therefore, right now, just explains the lag-1 autocorrelation and not degenerate fingerprinting. Could one learn something about a system with cascading tipping points by using degenerate fingerprinting on the multivariate signal? Sect. 3.3: Why are only the double-fold and fold-Hopf systems tested for the early-warning approach, and not the two systems with a Hopf bifurcation in the leading system? This choice should be explained or the other two examples should be included as well. Sect. 3.3.2, page 10, last paragraph: The oscillation seems to affect the measurement of autocorrelation. I think that one should here measure the auto-correlation of the residuals around a mean oscillation, either by subtracting this mean cycle somehow, or by defining a period and working with Poincare sections (snapshots after each period). Otherwise the result would probably be meaningless.

2.2 In both figure 1 and figure 2, the choice for the coupling of the two subsystems seems to be arbitrary. These choices could be explained better to make it more understandable for the reader. For example, one can shift the two systems versus each other (by varying parameter gamma1), such that the two tippings are well separated, or that they are really intertwined (one tipping inducing the other immediately). How would the stability landscape then look like, and what would we see in early-warning signals? I was also wondering why the values of gamma1 have been chosen in a way that gamma1 is 0 for the double-fold, <0 for the Fold-Hopf, and double-Hopf, and

C4

>0 for the Hopf-fold. Conceptually it would make a difference if the second tipping is triggered by the changing parameter or a direct consequence of the first tipping. It seems that the latter is always the case here for all parameter choices? This should be made clear from the beginning (as I mentioned above). Probably related to this point: In figure 2 it seems to be the case that the leading system tips before the following system, whereas the bifurcation plots seem to indicate that this happens at the same time. Where does this time delay come from? Similarly, a time delay can be seen in the Fold-Hopf system (Fig. 2b), while Fig. 1b would make me expect a discontinuous jump from a stationary solution to a cycle with some non-zero amplitude. Also, according to Table 2, the control parameter Phi increases linearly with time, but I do not see any change in state (or the amplitude of its oscillations) in Fig. 2, and on page 5, last paragraph, it is mentioned that at some point the amplitude would jump to a large value when both equilibria are accessed, but this is not seen in the Figure. In this context, Fig. 1 and 2 appear contradictory to me. This point is actually a crucial one because the period between the two tippings is used to detect early warning signals for the second tipping. How can it even be that there is enough time to detect them, when the system is already in the process of tipping? Here it looks like the second tipping is actually not caused directly by the first, but by the changing control parameter (in contrast to the impression I got in the previous section). If it is a real cascade (tipping 2 directly induced by tipping 1), wouldn't the system's state suddenly be very far from equilibrium after tipping 1. Can early warnings even be expected in this situation (mind they sample the equilibrium when the state fluctuates around it)? Moreover, I imagine that the relative time scale between the systems matters (controlled by the different coefficients in the equations). For example, in case of the Hopf-fold system, it would matter how fast and how large the oscillation in the leading system is compared to the following system's response time. So why has this particular coupling been chosen for the paper, and how representative is that for the climate system?

2.3 - The climate model (coupled ENSO and AMOC) seems very interesting. However, it is not completely clear to me what point exactly the authors want to make by showing

it. Sect. 4.3 is very short and I don't really understand its purpose. In the conclusion section and in the abstract it seems to be argued thatit illustrates that cascading tipping can occur in climate models, but as this is already known according to the introduction, and given that the model has been designed like this on purpose, what new information does this model provide? - Also, it should be more clearly explained how the two existing models have been coupled. I found it difficult at first to identify the common variables in the models that were linked. More precise wording might help (e.g. "through influence of the wind stress" - influence on what?, "in the original model" - which model?). It seems that the authors introduce an equation for the wind stress tau which links tau from the ENSO model to the temperatures from Stommel's model? Then one could say so from the start, followed by the details. - The model seems to be a representation of the Fold-Hopf case above? This should be explicitly stated from the beginning. - Why have the authors not done an early-warning analysis with this AMOC-ENSO model? This would be a natural step to do after the generic models above. The authors use data from a complex model to tune their conceptual model. What can we learn from that data directly about predicting each tipping, or the coupling (or whatever the authors aim to do)? Could one apply a statistical analysis and infer something about that model from the data?

Minor comments

- What I find most interesting is the analysis of the coupled deterministic systems, e.g. in Fig. 1. A very interesting aspect is the occurrence of intermediate (in terms of the state variable) stable states which are inaccessible when varying the control parameter. It seems that only noise can bring the system on these branches. This aspect is however not discussed in the paper. It is of course up to the authors if they want to go into this, but I would recommend them to at least comment on these hidden states, which I personally find more novel and exciting than the early-warning part of the paper. Could there be such hidden stable states in the climate system and how can they be found?

- Section 2.1 + Figure 1: It took me quite some time to understand what is going on. It could be helpful to create an X-Y bifurcation plot in addition to the phi-X plots and phi-Y plots that are shown already (to see how each system behaves in isolation). More emphasis can be put on explaining this figure, because this in itself is already an interesting result. The authors might even think of making an animation as extra material, to show how the subplots relate to each other. Also, it could be nice to show how figure 2 relates to figure 1.

- Several different names are sometimes used for the same thing, at least for the fold bifurcation (fold / back-to-back / back-to-back saddle-node). I had never come across the term back-to-back before. Is one term a subset of another? The authors should clarify this and unify the language.

- Some of the references are a bit outdated (e.g. Kutzbach 1996 on page 2; a lot has happened since then), or could be a bit more specific. Page 2: Scheffer 2009 is a review of some of the earlier papers like Held 2004, some of which are cited later; Peng 1994 is not about predicting tipping points. Also, note that there are papers from the 80ies dealing with statistical precursors already, e.g. by Wiesenfeld, 1984. page 1 (lines 17ff): Lenton et al. 2008 do not show evidence that there are tipping points in the climate system (though the paper is often cited in that way), so this paragraph should be formulated more cautiously. Also, the vegetation states found by Hirota et al. are purely ecological phenomena, and do not imply any tipping points in the climate.

- page 6, line 18: "close to critical transition" (2x), should be "close to a critical transition".

- page 10, line 17/18: "as it is no critical transition": why not? And what is a critical transition?

- In Fig. 8, I found it confusing that the labels are not next to the vertical axes but inside the figure. I do understand that this is consistent with the previous figures, so I don't have strong feelings about this.

C7