Response to reviewer 2

I would like to thank the reviewer for the careful reading and the numerous suggestions to improve the manuscript. He/she has also made many suggestions to improve the figures which I have also taken into account. However new figures I could not supply since my request for a prolongation of the deadline was not answered. Instead I extended many of the explanations and
5   formulated everything in words. I hope that the manuscript is now much better readable. I will now answer all the concerns of the reviewer point-by-point except for the corrections in English language, which I have all taken into account.

The paper explains tipping phenomena so clearly that it could be a chance to also explain a few necessary basics of dynamical systems theory, in particular, what a "manifold" and "saddle points" are (terms are used a lot but not explained). This could make the paper more accessible for a broader readership.

10   I would like to thank the reviewer for this suggestion, now the main basics in nonlinear dynamics are explained in a separate paragraph as follows:

The terms are now explained as follows: The unstable state (red ball) located on the hill of the stability landscape marks the basin boundary. This boundary separates the two basins of attraction, i.e., the two set of initial conditions which all converge to one of the respective attractors. In higher dimensional systems these unstable states on the boundary are of saddle type,
15   possessing stable and unstable manifolds. The stable manifolds are hypersurfaces in state space whose dimension is equal to the number of stable directions or stable eigenvalues of the corresponding Jacobian matrix of the saddle, while the unstable manifolds correspond to hypersurfaces determined by the number of unstable directions or eigenvalues. In the special case of a two-dimensional system the saddle steady state has two eigenvalues one stable and one unstable and the corresponding stable and unstable manifolds are one-dimensional. The stable manifolds along which trajectories move towards the saddle make up
20   the basin boundaries.

The manuscript cites the relevant literature as far as I can judge. There is a new study by Ritchie et al. about a very similar topic which could be cited: https://journals.aps.org/pre/abstract/10.1103/PhysRevE.95.052209,

I have included now this interesting reference.

Title: Why "in ecosystems and climate"? The systems presented are so general that they are not restricted to these fields.
25   *Environmental change is of course a good context and illustrates the relevance of non-autonomous tipping, so the examples in the text are useful, but I am sceptical if the title should make the scope more narrow than it actually is.*

The reviewer is correct in saying that the material presented is much more general than the title says. The title is chosen according to the readership of this journal, which are in general from geosciences. We are working already on a more general review for the broad readership of physicists, which will be submitted elsewhere.

30   *Comments about all figures showing the state space (X1 versus X2), i.e., Fig. 5, 6, 8, 9, 10*

I would like to thank the reviewer for the suggestions made to improve the figures. All figures are completely new and took almost all the suggestions into account, in particular the size of the labels, the number of trajectories shown, the color coding, the size of the symbols of steady states, using a) and b) instead of left right. I also changed completely the captions to explain all things shown in the figure. However, I could not realize some of the suggestions. These are: It is impossible to include the
35   whole vector field since it would only represent the vector field at one particular time instant, but I wanted to show the entire time evolution. This works best with showing the whole time evolution of trajectories including the parameter drift. To avoid too much confusion, I reduced the number of trajectories to 4 'and omitted the indication of the different time intervals which the trajectories go through (to the first and the second saddle-node bifurcation and the stop of the drift, which was indicated by dashed, dotted and white lines). It was meant to illustrate how far the trajectory got in state space, when the bifurcations occur.
40   But I agree with the reviewer, that the most important parts, the dotted and white lines were barely visible so that the original purpose was not well illustrated. Now all trajectories are shown as full black lines. Since arrows were difficult to include and did not look well (since I was not plotting a vector field) I have now indicated the starting point of the trajectories. I changed the colors of the nullclines from candy-colors to darker ones, but I did not change the colors in different panels. The colors represent at which time instant the nullclines are plotted, since they are all time-dependent, one color corresponds to the starting
45   point $t = 0$, the other to the end point $t = Tend$. This was only explained in the text, but I included it now also in the caption to make that very clear. The color coding of the non-autonomous basins of attraction is now also mentioned in the caption.

These disturbances correspond to the displacement of the state from the valley of the fixed stability landscape, depicted as a vertical path of perturbations in Fig. 2. On the other hand, disturbances in the system parameters or external forcings change the stability landscape. Both types of disturbances are possible and have very different effects (Schoenmakers and Feudel,

50 *2021).” Is this always clearly separable? For example, perturbing water density (state variable) and applying freshwater flux (forcing) in an ocean model could be argued to be essentially the same thing.*

Of course not and I mentioned that already in the text in the next sentence line 104-106. But I cannot follow fully the argumentation of the reviewer, because in his example one can separate the two, since changing the freshwater flux is the cause, but the change of the density is the response. Following the reviewer, one can also say that a perturbation of density (e.g.
55 by noise) could be interpreted as fluctuations of the freshwater flux, which would also be a possible interpretation. However, I think that this discussion could also be very misleading since it is closely related to the example and could lead to confusion of the reader. Therefore, I omitted it here.

line 125: “flow patterns in the ocean” => ocean circulation would probably be the more typical expression.

I changed it in the text, since the cited papers indeed only include ocean circulation.

60 *line 127-128: “In those systems, bi- or even multistability has been discovered”. Could be misleading. There are some (often simple) models that show multistability, but complex ESMs usually do not, and there are large uncertainties. line 127-128, line 133-134: “even multistable” Strictly speaking, “multistability” includes bistability (so “or even” is not really adequate). line 130-131: “The other stable state would be a reverse circulation pattern.” This may be the case in very simple models like Stommel’s model, but as far as I know not in higher-complexity models. There, the alternative state is an “off” state, often with*
65 *still some (weak) overturning, but no flow reversal. I suggest the author checks and adjusts this.*

I agree with the reviewer, that in this part the formulations were a bit sloppy. All facts the reviewer mentions are of course clear to me. Indeed, bistability is part of multistability. I wanted to make a distinction between bistability and “true” multistability with more than 2 attractors coexisting on purpose, since the latter is related to much more complex behavior and is much less understood. I have changed the text to highlight this difference in another way and added a very recent paper on that by
70 Lohmann et al. 2023. I have rewritten this part completely and it reads now: In those systems, bi- or even multistability, i.e. the coexistence of more than 2 stable states for the same environmental conditions, has been discovered. For the AMOC, mostly two different stable flow patterns exist: one of them can be considered as a conveyor belt transporting heat to the Northern latitudes, releasing this heat to the atmosphere, forming North Atlantic Deep Water (NADW) which is transported back to the Southern latitudes at considerable depth. This would be the state, where the heat transfer to the north in “on”. The other stable
75 state is related to an “off” state. This bistability can give rise to a possible breakdown of the AMOC, which has been discussed employing several conceptual models (Stommel, 1961; Rahmstorf, 1996; Rooth, 1982; Wood et al., 2019). In those conceptual models often the second state is related to a reverse circulation. In large ocean circulation models this bistability has also been confirmed (Weijer et al., 2012), with an “off” state which does not relate to a reverse circulation but to a very weak circulation northwards. In large ocean circulation models it has been shown, that the system can even exhibit the coexistence of several
80 different flow patterns related to different spatial patterns of heat transfer to the atmosphere (Rahmstorf, 1995). This occurrence of multistability has been confirmed recently with other high-resolution models (Mehling et al., 2022; Lohmann et al., 2023).

140-141: “As a result, the system tips or collapses from a coral-dominated into an algae-dominated reef (Holbrook et al., 2016).” Again, my impression is that there is much more uncertainty than this sentence suggests. I’m not so familiar with this research field, but I believe that the question whether coral reefs display several stable states is inconclusive.

85 I agree with the reviewer that one should formulate more carefully, though Holbrook et al. use the terminology of tipping points in their study. That the collapse occurs and what are the possible reasons for it, is widely accepted in the ecological literature based on observations. However, that this can be interpreted as a tipping in the dynamical systems sense, is questionable as the existence of thresholds in ecology in general is controversially debated in ecology. I have added a corresponding citation Hillebrand et al. 2021. Therefore, I cut the word “tips” in the sentence. This part reads now: Examples of alternative states
90 in ecosystems have been discussed in the literature (cf. (Folke et al., 2004) and references therein), though the existence of thresholds in ecology is controversely debated (Hillebrand et al., 2020). A prominent example in which such transitions from one stable state to another are nowadays already observed are tropical coral reefs, which are found to be overgrown with green algae due to climate change and other anthropogenic and non-anthropogenic influences. As a result, the system collapses and exhibits a shift from a coral-dominated into an algae-dominated reef (Holbrook et al., 2016).

95 “There is a confusion of terms here that unfortunately is very common in the “tipping points” / dynamical systems literature. A noise-induced transition refers to a (possibly radical) change in the pdf of the state when the noise intensity (a parameter of the stochastic system) is changed. This is also the definition used in Horsthemke and Lefever 1984, as well as Kuehn 2011, which the author cites here. An example of this phenomenon would be stochastic resonance (in case an oscillating

forcing is also applied in addition). A noise-induced transition is therefore not the same as N-tipping (Ashwin et al., 2012),
or "noise-induced tipping" as the author calls it here, which just describes a single event in a single realisation of the system.
It could be a good opportunity to clarify this common misunderstanding in the paper, or at least the paper should avoid the
misleading term of "noise-induced transitions"." I would like to thank the reviewer for pointing out this confusion in the two
notations, what a noise-induced transition is. In the literature indeed different descriptions are used for the phenomenon called
"noise-induced transition" To outline this a bit further I have changed the text accordingly and also cite again Ashwin whose
definition of N-tipping often used is again different. The text reads now: Though this description sounds like a result of a
single trajectory, it needs a stochastic description, since one has to study ensembles of trajectories with different realizations
of the noise and probability distributions (pdfs) over the state space. There is a vast literature on noise-induced transitions in
many different science disciplines and the notion of noise-induced transitions differs across the literature. While many studies
classify noise-induced transitions as qualitative changes in the aforementioned pdf with the noise strength as the bifurcation
parameter (Horsthemke and Lefever, 1984; Kuehn, 2011), other works focus directly on the transition from one stable state
to another mentioned under the influence of noise as outlined above. Ashwin et al. Ashwin et al. (2012) define N-tipping as a
system which leaves the neighborhood of a quasistationary state due to the influence of fluctuations.

line 161: "...living in the sediment of the North Sea at the end of the 90s" is a bit confusing (order of words). The 90ies
refers to the regime shift mentioned earlier I suppose.

This is a mistake in English, which I have corrected now. Indeed, the regime shift has been observed in the 90s.

Fig. 3: The cyan colour is hard to see; it is unclear how the potential is accelerated (to what side) – to the left according to
the trajectory of the ball, but the arrows point to the right; it is not visible where the ball would cross an unstable equilibrium
in case e (see point about absence of basin-crossing above). Fig. 3 and line 204-214: I am a bit confused why a large excursion
in phase space without crossing any equilibrium point counts (no basin crossing) as a tipping here. Isn't this definition a bit
arbitrary and too qualitative? If the ball never leaves its basin and does not even cross an unstable equilibrium (or basin
boundary), what excursion is large enough to constitute a tipping?

On purpose I have not provided an illustration for the basin crossing, since this is discussed later in the manuscript in more
detail. Fig.3 is meant to illustrate the other case of rate-induced tipping which is not related to a basin boundary crossing. It is
related to the mechanism discussed in Wieczorek et al. 2011 and Vanselow et al. 2019, where the rate-induced tipping occurs
without the existence of an alternative state. It occurs in slow-fast systems and requires a bended critical manifold. The exact
mathematical conditions are outlined in Wieczorek et al. 2011. I have extended the explanation of this part of Fig. 3 to make
this more clear. It reads now: Since all aforementioned tipping mechanisms are related to the coexistence of alternative states,
rate-induced tipping can also occur when there is only one stable state present and the system is characterized by different
timescales (slow-fast system). The dynamics of such systems can be described by so called critical manifolds in case of a
perfect timescale separation or slow manifolds, when the timescale separation is finite. In case of a complex structure of the
critical manifolds, for instance when these manifolds have stable and unstable parts which meet in a fold, then a rate-induced
crossing of this fold can make the trajectory visit very different parts of the state space far away from the original stable state
and perhaps even dangerous for the system. This mechanism of rate-induced critical transitions is illustrated in Fig. **??**d,e
where the whole stability landscape is moved at a certain rate. Suppose that the stability landscape in Fig. **??**d is pulled with
a certain rate towards the observer. Consequently, the ball will no longer be located in the minimum of the valley but will be
displaced to the left. The restoring forces will start acting, and the ball begins to "roll" to catch the moving minimum. If the
pulling rate is slow, then the stable state (ball) follows, or we say it *tracks* the minimum of the stability landscape. By contrast,
in Fig. **??**e, the rate of "pulling away" the stability landscape is much faster or comparable with the timescale of the restoring
forces. In this case, the ball lands in a completely different region in state space, leaving the minimum's proximity and leading
to qualitatively different dynamics. This large excursion in state space corresponds to rate-induced tipping since the system
visits very different parts of the state space with qualitatively different behavior. If the change in the environmental conditions
stops, this visit to a different state will be transient and, finally, the system returns to the stable quasi-stationary state, which has
moved. This transient dynamics could lead to qualitatively different states, like population collapse in predator-prey systems
Vanselow et al. (2019) or population outbreaks Vanselow et al. (2022).

line 224-232: I find the case of spatially extended systems particularly interesting. These systems are often neglected in other
"tipping point" related literature, so I here see the opportunity that this paper could add something. Some more text about this

case with an example might thus be a good idea. This could be merged with Sect. 3.5 which treats diffusive systems (but also too briefly).

I have extended this paragraph with an example according to the suggestion of the reviewer. A merging with Sect. 3.5 does not fit, since the description of tipping in spatially extended systems deals with tipping in partial differential equations and in particular with pattern formation, while Sect. 3.5 deals with a simplified version of a kind of network approach with just two nodes. The extended paragraph reads now: Several examples have been studied by Meron and coworkers in dryland vegetation models Zelnik et al. (2013); Bel et al. (2012); Zelnik et al. (2018). These models of different complexity study the interplay between vegetation and soil water. Besides the homogeneous states "bare soil" and "full vegetation cover" there exist depending on the environmental conditions, in general the precipitation level, different patterns like holes in the vegetation, stripes and spots of vegetation. These patterns can coexist and fronts separating the different pattern can occur. The speed of the fronts determines the speed with which one patterns is exchanged by the other leading to a gradual tipping between different patterns in the whole area.

Sect 3.1: A figure with the equilibria of the population model would be a nice addition to the equations, maybe as part of Fig. 4.

Fig. 4 could have been extended by a second panel showing the potential landscape at the beginning and the end of the parameter drift. However, the time for preparing the figure was not sufficient before the deadline.

Caption of Fig. 4: why does it refer to "Eqs. 5"? Isn't this system described by Eq. 4?

I would like to thank the reviewer for pointing out this mistake. It has been corrected.

lines 314-333: Fig. 5 is mentioned only very late, could refer to it as soon as the concept of nullclines is introduced; I think this would help to get the idea.

The whole paragraph has been rearranged and reads now: Rescaling the time in terms of $\tau = \varepsilon_1 t$, it turns out that only the ratios between the intrinsic timescales of the different subsystems $\varepsilon_2/\varepsilon_1$ and the ratios between the timescale of transport or coupling and the intrinsic timescale (like, e.g., c/ε_1) are important. Therefore, we will continue the analysis with the rescaled equations:

$$\dot{X}_1 \quad = \quad -(X_1 - s_1)(X_1 - s_2)(X_1 - s_3) + cX_2, \tag{1}$$
$$\dot{X}_2 \quad = \quad -\varepsilon(X_2 - s_1)(X_2 - s_2)(X_2 - s_3), \tag{2}$$

where ε and c are the corresponding ratios.

System 2 appears as a driver or master for system 1. We analyze the dynamics in the most intuitive way, we use the concept of nullclines, which are given by the algebraic equations $\dot{X}_1 = f_1(X_1, X_2) = 0$ and $\dot{X}_2 = f_2(X_2) = 0$. While for the driver system 2, the nullclines are given by straight lines at the values of the three steady states of system 2, the nullcline of system 1 is represented by the cubic function $X_2 = (X_1 - s_1)(X_1 - s_2)(X_1 - s_3)/c$. The intersection points of $f_1 = 0$ and $f_2 = 0$ are the steady states of the master-slave system. Their stability can be computed from the eigenvalues of the corresponding Jacobian. An illustration of two possible situation is given in Fig. **??**a,b. Depending on the internal parameters ε and the coupling strength c, the system possesses two, three, or four stable, steady states in the considered parameter range of coupling strength c. The shown two cases serve as the beginning [Fig. **??**a] and the end [Fig. **??**b] point of the parameter drift along a linear ramp. We are fixing all parameters (frozen-in case) and compute the attractors and their corresponding basins of attraction by choosing a grid of initial conditions in a specified region of state space and integrating them all in parallel until they reach the attractor. This allows us to compute also the relative size of the basins of attraction $\mathcal{B}_\mathcal{A}$ as the quotient of the number of initial conditions converging to attractor $\mathcal{A}$ divided by the total number of initial conditions taken into account (Feudel et al., 1996). Fig. **??** shows that the state space is "partitioned" into different basins of attraction indicated by different colors with basin boundaries separating them. In the frozen-in case, the basin boundaries are invariant sets that cannot be crossed by trajectories and, hence, represent rigid boundaries in state space for the trajectories.

line 402 + Fig. 7b: It may be a bit misleading to refer to "tipping probabilities". This term makes me think of a stochastic system with always the same parameters and initial conditions but different noise realisations. e.g., see https://journals.aps.org/pre/abstract/10. But here, the outcome depends only on the distribution of the initial conditions. I would rather call it "frequency" or "fraction" of tipping trajectories.

Since I am using the same measure as introduced in Kaszas et al. 2019 I have kept the name and inserted also the formula to avoid confusion, though I agree with the reviewer that "fraction" would be more appropriate. I have inserted this now in

the text: We follow here the approach introdcued in (Kaszás et al., 2019) and compute the tipping probabilities $\mathcal{P}\mathcal{A}1\mathcal{A}2$. We calculate first the relative size of the non-autonomous basin of attraction $\tilde{\mathcal{B}}(\tilde{A}2)$ for the quasisteady state $\tilde{A}2$ at the end of the parameter drift simulation $Tend$ and take its intersection with the frozen-in basin of attraction $\mathcal{B}$ of the frozen-in attractor $\mathcal{A}1$ at the beginning of the simulation. To compute a proper part of the initial conditions we have to normalize this quantity by the frozen-in basin of attraction $\mathcal{B}(\mathcal{A}1)$ at the beginning of the simulation. Loosely speaking, we calculate that fraction of initial conditions which would have converged to the attractor $\mathcal{A}1$ in the frozen-in case, but which during the parameter drift tip to the basin of attraction of the moving quasisteady state $\tilde{A}2$. In mathematical terms this can be expressed as follows:

$$\mathcal{P}\tilde{A}1\tilde{A}2 = \frac{\tilde{\mathcal{B}}(\tilde{A}2)\bigcap\mathcal{B}(\mathcal{A}1)}{\mathcal{B}(\mathcal{A})1)} \tag{3}$$

The continuous change of the tipping probabilities shown in Fig. **??** indicates that we observe partial tipping.

line 423-426: A bit short. Some more description and explanation in words about what differences we see between the plots, and why, would be great. Also, it's not clear to me why we need to epsilons. Could we not just remove epsilon 1 in this system? Eqs. 11-13. Again, why two epsilons? Would one not be enough to capture a time scale separation? As you write yourself in lines 450-454, only the ratio matters.

I have changed the whole notation in the new version of the manuscript and rescaled time by $\epsilon 1$ as discussed earlier in Sect. 3.4. This explanation in Sect. 4 is now removed, since the whole manuscript deals now only with the ratios of the parameters.

Fig. 10: It could help to have one figure showing the static system similar to Fig. 5 for the coupled system.

You could call Eq. 8-10 system 1, and Eq. 11-13 system 2; it would help when refering to these. Equations 6-7 and 8-9 are identical; could remove one pair.

Fig. 12: Due to the overlap, it is hard to see which two basins change change together. One might use a combination of colour and line type to solve this.

I tried it out, but it did not look any better, so I left the figures as they were. The line types are a combination of the two when overlapping, but I agree, it is hard to see.

Sect 3.5 appears only as an afterthought, though diffusive coupling plays an important role in many models of many different systems. I suggest to expand this section, adding a similar analysis as for models 1 and 2 above, or at least explain in some more detail how and why the results differ.

Since there is no qualitative difference between the results in Sec. 3.3 and 3.4 compared to Sec. 3.5. I expanded the section by an explanation why there are quantitative differences. Instead of presenting only the example with a timescale separation I have now added also the case of no timescale separation to compare with. Here is the explanation added: For ecological systems, this would be the appropriate coupling when considering two populations in two different habitats coupled through species migration. The same coupling would be used for coupled chemical systems, where diffusion is assumed to be the most important spatial transport process. Following the same protocol of numerical simulations with the same parameter values, we observe that qualitatively we observe the same behavior as for the other coupling schemes with one important difference. The masking effect occurs for much larger T_r, i.e., a much slower rate of environmental change, if the two systems are identical and no timescale separation occurs. This is due to the fact, that the effective coupling is much smaller than in cases of a master-slave coupling or a mutual coupling. The coupling strength is multiplied by the difference of the two variables in system 1 and 2 instead of the variable itself and this diminishes the effect of the coupling. Therefore, the overall effect for identical systems is much smaller than in the other cases (Fig. **??**a,b. By contrast, if we introduce the timescale separation shown in Fig. **??**c,d, we note, that the masking effect is again stronger now taken over by the timescale separation. This emphasizes again the role of the timescale separation between coupled systems.

|it line 535-537: "either the trajectory "meets" the saddle point directly or it approaches first the neighborhood of its moving stable manifold and travels along it until the saddle point is reached for the crossing." - This is a very qualitative distinction, isn't it?|

I agree with the reviewer that this is just a qualitative description. To make this more quantitative and precise, other methods are needed than simulations. This research is already under way, but will be published elsewhere.

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