We thank the three reviewers below for their helpful comments and the time spent assessing the manuscript. This is much appreciated. Reviewer comments are in black font, our responses in blue font, and any new text added to the manuscript is quoted in italics.

Reviewer #1

The authors have attempted to revise the manuscript, and I acknowledge the effort put into this version. However, I consider that the changes made do not reach the expected standard for publication in this journal.

> Thank you. We are pleased that the changes have been noted, but we fully recognise the concerns you still have, as outlined below. Please see our responses below.

Despite the modifications, the paper remains exceptionally dense mathematically, even when accounting for the specialized scope of this journal. The overwhelming volume of equations continues to obscure the core narrative and the actual scientific contribution of the work. Because the manuscript still lacks the necessary balance between technical rigor and conceptual clarity after revision, I cannot recommend it for publication.

> The rationale for this manuscript arose from discussions of dynamical systems to mimic potential tipping point behaviours of Earth system components. Despite careful searching, we were unable to find, in either the literature or textbooks, the mathematics needed to relate the parameters of any dynamical system to a process of interest. In the current version of the paper, we have moved most of the algebraic calculations to Methods. We were initially surprised by how complex the algebra is. The remaining equations are the absolute minimum needed for a user to perform this mapping.
>
> We are, though, very happy to place greater emphasis on the core narrative and its scientific contribution, and to elevate clarity above the technical details.
>
> For instance, we now write: "*We begin with a moderately basic nonlinear ordinary differential equation, Eq. 1, which exhibits tipping point and hysteresis effects. This equation, of "cubic" form, has been proposed by many to describe features of the Earth system (see Section 2.1 for references), but there is presently no routine method to describe how to map climate tipping point features onto this model*". Section 2.1 includes the additional references – please see response to Reviewer #2, which quotes that extended section.
>
> Just as an aside, we were curious about the download statistics for the preprint. We were surprised to find that roughly 50% of online views led to a download of the full paper. Hence, we humbly suggest that this paper will prove useful in the long term to those involved in representing tipping points as dynamical systems.

Reviewer #2

The authors describe a generic dynamical system that can be used to model climate tipping elements and their hysteresis properties. More concretely, they formulate a third-order ordinary differential equation (ODE) as the minimal system to exhibit all desired properties, and subsequently derive relationships between the ODE parameters and relevant system characteristics. They analytically investigate the model's safe parameter regions, both in the context of bifurcation-tipping and overshoot scenarios. The manuscript is highly instructive and well written. It establishes a full manual for the application of this model in the context of climate tipping points and can serve as a basis for many existing studies that employ the heuristic concept of bifurcations for tipping points.

> Thank you. Indeed, we aim to create an ODE as a "minimal system to exhibit all desired properties". We are glad the paper is considered "well written". However, we also note the concern below.

I have one major problem with the current presentation though. Throughout the manuscript it sounds as if this ODE is a novel model for climate tipping points with hysteresis. This is simply not the case and caused me to be very skeptical of the manuscript at first. The mathematical derivations and in-depth description of the model properties from first principles warrant publication, but the manuscript should give a history of the prior usage of this model, rather than present it as a novel development.

> Yes, the "cubic" form has been considered for other tipping processes. Please see below for additional citations of research that does this, and we are sorry for not including these in the original paper.

The authors briefly cite one important prior contribution already (10.5194/esd-12-69-2021). It should be made clear how the current manuscript and its results relate to that work. Beyond this, I found the following publications which use the discussed model for climate tipping point hysteresis and there may be others:
- Berglund and Gentz, 2002, Metastability in simple climate models: Pathwise analysis of slowly driven Langevin equations
- Dakos and Kefi, 2022, Ecological resilience: what to measure and how
- Boers et al., 2022, Theoretical and paleoclimatic evidence for abrupt transitions in the Earth system
Historically, Cessi, 1994, should also be mentioned as an early occurrence of this model in climate science.

> Thank you. We have revisited the literature and now describe tipping point research that has employed the "cubic" model.
>
> We now write in the Section "Mathematical Model" the following:
>
> *"Many use this form of "cubic" model (Eq. (1)) to describe generic thermohaline circulations, often to better understand the effect of additional stochastic noise (e.g. Cessi, 1994) or to explain features of paleo records (Boers et al., 2022). Boers (2021) investigates Early Warning Systems (EWSs) specific to the Atlantic Meridional Overturning Circulation (AMOC) by considering high-frequency variability combined*

> *with such a dynamical system. Recently, Wunderling et al. (2026) presented such a model to describe the risk of deforestation and related atmospheric feedbacks that may cause the remaining forest to also transition to grassland, with their detailed use of an illustrative dynamical system highlighted by Huntingford (2026). Coupled cubic dynamical systems are used to investigate the risk of cascading tipping points (Wunderling et al., 2022; Wunderling et al., 2021).*
>
> *A more general analysis of the cubic form of a dynamical system with noise added is presented by Berglund and Gentz (2002). Similarly, Clarke et al. (2023), with such a dynamical system, consider whether EWSs designed to alert to approaching tipping may be made more accurate when accounting for any temporal autocorrelation in forcing. Ditlevsen and Johnson (2010) illustrate, using such a model, that standard EWSs based on critical slowing down should be considered trustworthy only if both variance and autocorrelation increase. The difficulty of understanding equilibrium states that may be far from contemporary observations is shown by such a cubic dynamical system (Dakos and Kefi, 2022). Associated with tipping points is the mathematics of related EWSs that additionally entrain spatial variation. Clarke et al. (2026) consider cubic models at multiple locations with spatial connectivity, finding that, in some circumstances, accounting for spatial variation can yield more refined predictive statistics for distance to* tipping. *Hence, the form of Eq. (1) in the literature of large-scale environmental change is well established, and our aim here is to make its application to further Earth system components easier when the values of key underlying defining attributes are known"*

Apart from this, there are the following minor points to address.
- The framework silently assumes X_HYS > X_TP, i.e. that the state variable increases upon tipping. The orientation convention should be stated explicitly and caveated with the fact that many natural systems like the AMOC tip downwards. I assume all the derivations carry over symmetrically but it is still good to mention this I think.

> We have numerically verified that setting X_TP > X_HYS yields the expected decrease in the state variable. Thank you for this, and we have adopted some of your wording. We now write in the paper:
>
> *"The algebra is also applicable if $X_{TP} > X_{HYS}$, and therefore valid for any system that tips downward, which could correspond to an event such as the shutdown of AMOC or the loss of rainforest cover in the Amazon"*

- The Ritchie et al. (2019) criterion behind Eq. (24) is asymptotic for small overshoots near the fold. Here the overshoot (0.5 K) is not small relative to the hysteresis width (1.9 K), so the agreement with Fig. 4 is empirical rather than guaranteed, this might be worth a sentence.

> We thank the reviewer for this comment. We agree that the criterion derived by Ritchie et al. (2019) is asymptotic for small overshoots near the fold. Although our overshoot lies outside the strict asymptotic limit, the criterion is expected to provide a useful approximation over a broader range of overshoots, consistent with the

> agreement shown in Fig. 4. We are also grateful for this comment, as we realise that during the last reworking of the paper, which moved the detailed analysis to the Appendix, the reference to Ritchie et al. (2019) was accidentally omitted from the main text.
>
> We now add:
>
> *"This inequality is given by (Eq. 24) and is the same threshold, but in our notation, as in Ritchie et al. (2019). This inequality is expected to be broadly valid for a range of temperature overshoots, not just those that are asymptotically small."*
>
> The Ritchie et al threshold is intended to assess whether temporary overshoot is feasible. Where it projects major system changes during overshoot, there could also be hysteresis, but the Ritchie et al (2019) inequality is not designed to determine its magnitude. That question could, however, form an interesting research question.

- The baseline definition of ε is given three different ways: "1880–1900 inclusive" (text), $t = 0$ to 21 (formula under Eq. 8), and "1880 to 1899 inclusive" (Fig. 3 caption), please reconcile.

> The Fig. 3 caption was wrong, and this now states 1880-1900. Thank you.

- Fig. 1 caption: "β, μ_0, a_1 and a_1", last should be a_0.

> Thank you. Corrected

- Abstract: "a method extrapolate their projections", missing "to".

> Thank you. Corrected.

- Eq. (22): stray comma before the closing bracket.

> Thank you. Corrected.

- Eq. (8): the ε line is an unnumbered continuation ending in a dangling comma.

> Thank you. It is correct that the equation is unnumbered, as it is not cited in the text. However, the comma is an error, now replaced with a full stop.

- Eq. (25): use $\approx$ rather than $=$ for rounded values.

> Changed as suggested.

- Fig. 3 annotation uses ΔT_p (lowercase) vs. ΔT_P in text.

> Thank you. Corrected.

Reviewer #3

This manuscript presents a one equation box model, with a detailed mathematical analysis of this model, that is intended to represent the role of inertia in climate tipping point problems. The manuscript is well written, if a bit detail oriented, and has some contact with climate literature, though much of this contact is ahistorical.

> Thank you for this summary, and we are pleased that the paper is seen as "well written". We acknowledge it is somewhat "detail oriented", and we now use the literature more carefully to place the analysis in a broader context (response to Reviewer #2 above and to yourself below).

The tone of the abstract is not appropriate for a technical journal. I don't think someone who makes it a habit to read NPG needs to have the concept of a dynamical system to be "defined" in the public talk sense in the abstract.

> Thank you for this comment. We agree that the original wording was not appropriate for the intended audience. We have therefore revised the sentence to avoid introducing a familiar concept and instead write:
>
> "*Advances in nonlinear mathematics include developing dynamical systems whose equations frequently contain a bifurcation parameter*."

There are multiple statements in the abstract I would disagree with as a climate scientist, and this will be revisited below. For example, it is the view of most climate scientists that ESMs are in fact not finely resolved enough. There is a role for many kinds of climate models (GCMs, EMICs, box models) with progress based on a combination of modelling strategies.

> We want to acknowledge your point explicitly, given its importance. We now write in the Abstract:
>
> "*Recent progress in ESM development often introduces higher spatial resolution, enhancing process representation and increasing accuracy in predicting local changes. However, mapping the broad behaviour of the components of ESM projections onto simpler dynamical system models may also offer many advantages, including the overall characterisation of climate models and a method to rapidly extrapolate their projections to a wider range of forcing scenarios*."

If the intent of the Introduction is to provide context, I would suggest focusing on at least a couple of areas and quoting papers over longer time scales. There is work on cessation of the AMOC, for example, that has a decades long history (it even led to a Nobel prize).

> We recognise that the initial paper lacked a detailed summary of the existing literature and, in particular, an explanation of where large-scale processes have been represented by a "cubic form" dynamical system. To address this omission, we have added two new paragraphs towards the beginning of the manuscript. This request is similar to that of Reviewer#2, but for completeness, we repeat the new text here as well.

We now write in the Section “Mathematical Model” the following: “*Many use this form of “cubic” model (Eq. (1)) to describe generic thermohaline circulations, often to better understand the effect of additional stochastic noise (e.g. Cessi, 1994) or to explain features of paleo records (Boers et al., 2022). Boers (2021) investigates Early Warning Systems (EWSs) specific to the Atlantic Meridional Overturning Circulation (AMOC) by considering high-frequency variability combined with such a dynamical system. Recently, Wunderling et al. (2026) presented such a model to describe the risk of deforestation and related atmospheric feedbacks that may cause the remaining forest to also transition to grassland, with their detailed use of an illustrative dynamical system highlighted by Huntingford (2026). Coupled cubic dynamical systems are used to investigate the risk of cascading tipping points (Wunderling et al., 2022; Wunderling et al., 2021).*

A more general analysis of the cubic form of a dynamical system with noise added is presented by Berglund and Gentz (2002). Similarly, Clarke et al. (2023), with such a dynamical system, consider whether EWSs designed to alert to approaching tipping may be made more accurate when accounting for any temporal autocorrelation in forcing. Ditlevsen and Johnson (2010) illustrate, using such a model, that standard EWSs based on critical slowing down should be considered trustworthy only if both variance and autocorrelation increase. The difficulty of understanding equilibrium states that may be far from contemporary observations is shown by such a cubic dynamical system (Dakos and Kefi, 2022). Associated with tipping points is the mathematics of related EWSs that additionally entrain spatial variation. Clarke et al. (2026) consider cubic models at multiple locations with spatial connectivity, finding that, in some circumstances, accounting for spatial variation can yield more refined predictive statistics for distance to tipping. *Hence, the form of Eq. (1) in the literature of large-scale environmental change is well established, and our aim here is to make its application to further Earth system components easier when the values of key underlying defining attributes are known”*

However, we also take your advice, which is specific to reduced-complexity models of AMOC. Towards the end of the manuscript, we now add:

“*There are already extensive and valuable analyses of potential tipping points in the literature, focusing on behaviour at the time when a major change in a climate component might be expected. Such literature includes examples using the “cubic” form (see Section 2.1), while also noting in particular the historical development of dynamical systems to represent AMOC changes (e.g. Stommel, 1961; Rahmstorf, 1996; Hawkins et al., 2011).*”

The model discussed is a first order, nonlinear differential equation. I like simple models, but the possible dynamics of a system like this is extremely limited (and if one goes back to Einstein there caution should be exercised about models that are too simple). This is textbook dynamical systems theory, and indeed while long series of calculations are presented nothing in the manuscript provides an indication that the dynamics represent the true Earth system.

> Thank you for this comment. We agree that low-order dynamical models have inherent limitations and are not intended to capture the full complexity of the Earth system. To clarify the intended scope of our framework, we now write at the end of the Abstract:
>
> "*In some instances, a basic equation may be too simple to capture the dominant qualitative behaviour of a large-scale environmental system. In those instances, a more complex model from any hierarchy of suggested simulations of climate components may be appropriate. However, should a system be amenable to representation by a dynamical system, we hope the framework we present offers a simple-to-use equation structure that may quantify tipping points in the climate system. Such a framework may illustrate a more complete description of behaviours both before and after tipping, and depending on the post-tipping forcing scenario, it may involve substantial hysteresis*."

Also, it is worth mentioning that for anyone coming from a physics background it will be very strange to discuss inertia in a first order equation. In classical mechanics, when the order of newton's equations is reduced from second to first order, this is precisely the inertia-less limit. It is never made clear why this should be different here.

> We note that we use the word "inertia" in a broad sense, to refer to resistance to change rather than as a resistance to a specific force. We amend the Abstract, to write:
>
> "*Most advances to date have focused on understanding whether tipping events can be avoided if systems possess substantial inertia, i.e. respond over long timescales, allowing climate change to temporarily exceed thresholds that might otherwise trigger major nonlinear change*."

The primary issue, and I believe this has been climate textbook material for decades, is that different aspects of the climate have different timescales (i.e. the ocean is often thought to have much longer time scales) associated with them. The assumption of the present model is that a single bifurcation parameter and a forcing based on the mean state can accurately represent the system dynamics and in particular, the inertia in it. However, for the actual Earth System, it is the ocean that can provide inertia for changes and in the end I am unconvinced that the model presented has enough realism to provide much utility.

> We make clear in the paper that the proposed model may be considered for application to a range of Earth system components, and that the "cp" term is likely to be process-specific.
>
> However, this comment prompts us to note that, for some specific processes, it may be necessary to consider multiple timescales and, potentially, to use equations in terms of more than one state variable. Thank you - we now write:

"In some instances, a large scale process may be represented by a dynamical system, but it has to be more complex, potentially including more than one timescale (via parameter c'_p) or more than one state variable, x, suggesting coupled equations. *In some particular cases, the equations may contain switches, with behaviour conditional on state variable value for forcing level*."

This new text above also responds to the request below.

Finally, no information is given on how model complexity can be built up. In the AMOC box modeling literature there are one equation models, two equation models, three equation models, models that include switches (and hence bring in impulsive differential equation theory) and many more that I am likely overlooking.

Thank you. Please see the new text added in the response above, which also covers the points made here.

I think this manuscript likely belongs in a mathematics journal where "inspired by applications" is good enough to motivate the mathematical analysis. I can imagine a version appearing in NPG (and I commend the authors on a clear writing style) but it would take a considerable amount of reworking.

We are pleased that the writing style is clear, and our aim is to present an unambiguous mapping between a component of the climate system, for which some key parameters are known, and a potentially descriptive "cubic" dynamical system.

The manuscript has now undergone two main iterations. The first moved the bulk of the algebraic derivations to a new Appendix, leaving only the essential equations required for the mapping in the main part of the manuscript. In this second revision, particular emphasis has been placed on embedding the mathematics within a more comprehensive literature review of existing applications of dynamical systems to components of the Earth system. Our preference, therefore, is that the paper remain with NPG, which is associated with understanding the geosciences, rather than a purely mathematical journal.