Full Reply on RC2

In the text below our response components are expressed in the form of *italic Times New Romans.*

RC2: 'Comment on egusphere-2026-2981', Anonymous Referee #2, 14 Oct 2026
Thank you for inviting me to review paper: “Review article: The future of the physics-based NWP methodology: a critical review on applications of catastrophe theory to the atmospheric science.”

This is one of the most difficult papers I have had to review, as I’m not sure some of the claims are justified. At the same time, the manuscript may contain profound suggestions worthy of detailed assessment by future researchers. In many places, I could not fully understand the key points. In that context, the best I can do is set out my interpretation of the paper below, in three main bullet points. These notes can then be used by the authors to help get their ideas across more clearly.

We are grateful that you spend valuable time to review our MS and provide uncommon comments that improve our MS fundamentally.

(1) As I understand it, catastrophe theory was a major new idea in the 1980s, when researchers began investigating nonlinear ordinary differential equations. In essence, the idea is that a forcing could change by only a small amount and, in some cases, cause a jump to another state. Furthermore, there could be hysteresis, so reversing the forcing parameter does not cause a return to the same state. Now, in 2026, almost everyone is aware of climate change and the risk of climate “tipping points”. Mathematically, tipping points are expressed through changes to bifurcation parameters, which are in turn related to summary features of climate change (e.g. global warming). Can the authors confirm, or explain any differences, between catastrophe theory and the more modern concept of tipping points? And what specific bits they believe are missing in current NWP models?

Yes, to our knowledge, the main ideas of catastrophe theory are captured substantially in the relevant comments above, with the notion about small forcing leading potentially to large changes in the internal variables as well as hysteresis in the cusp catastrophe type particularly emphasized by you.

As mentioned in our MS, in mathematics, CT is a branch of bifurcation theory in investigation into dynamical systems, which studies and classifies phenomena characterized by sudden shifts in behavior arising from small changes in circumstances, analyzing how the qualitative nature of equation solutions depends on the parameters that appear in the equation and change gradually. Small changes in certain parameters of a nonlinear system can cause equilibria to appear or disappear, or to change from attracting to repelling and vice versa, leading to large and sudden changes in the behavior of the system represented by a state variable. Rigorous studies of partial differential equations (PDEs) can often lead to the elementary catastrophe (see the relevant subsections in our revised MS for further details).

The popular emergence of "tipping point" before their adoption in climate change discourse is around 2005, as mentioned in (e.g.) "The tipping point trend in climate change communication" by Russill and Nyssa (2009). Actually, aside from the rhetorically striking character of "tipping point", tipping is the mathematically well-defined generalization of the bifurcation treated in the autonomous dynamical systems to the non-autonomous setting.

As a result, in the context of dynamical systems theory, tipping should be associated with bifurcation and, both would have a catastrophe connotation. On one hand, the idea is spreading like a contagion, and on the other hand, "this is old wine in new bottles" (both views were published in Nature and in 2006 (Nature, 2006; Walker, 2006). Since that's the case, CT, as a rare weapon to overcome benighted ignorance, can then be employed as an existing methodology to handle the issues related to tipping points as well (Nature, 2006; Lucarini and Chekroun, 2023) with innumerable great achievements expected.

On what specific bits missing in current NWP models

In the revised MS, we have made many modifications according to your guideline, and here, we might particularly mention the following points: (1) From the multiple timescale perspective, the multiple equilibria existing has been revealed early in 1970s by say Charney and De Vore (1979), and so there distinctly exist multiple solutions at least on a "lower timescale", which is the basis of catastrophe (an abrupt temporal change or sudden changes) occurrence but not yet properly treated in current NWP models where e.g. step-change features are smoothed out via averaging, or sudden changes are regarded as outcomes of computational instability etc.; (2) more significantly, the existence and uniqueness theorem of the Navier-Stokes equation that is the physics-based NWP methodology's leading fluid mechanics equation is not settled till now；（3） the Lipschitz condition is a commonly used sufficient condition that guarantees the local uniqueness of solutions to initial value problems and, we wonder whether it has been made a pre-assessment in current NWP models or not； (4) in case multiple solutions come out it is required that we need to employ the so-called "selection criterion" for fixing the more realistic physical solution based on an additional restriction condition such as "Entropy Condition" that is not covered by the original equation, and, (5) the others (including the principle of the 2nd law of thermodynamics has not been incorporated into current NWP models in a great extent). For further details please see the text of the revised MS, esp. the relevant part to the future directions of research in its final section. Thank you!

(2) I really want to better understand the main idea proposed by the authors, namely that step-change features in the climate system are not captured in weather forecast models (and, by association, climate models). It is true that much of the atmosphere and ocean system is governed by smooth fluids, so standard NWP discretisation is appropriate. But there are many "on-off" components that are treated rigorously in NWP/GCMs. For instance, phase changes that enable

rainfall from vapour, the simulation of dynamic ice cover and its interactions with local climate. As many NWPs have been converted into Earth System Models (ESMs), they introduce even more nonlinear switches. For instance, the global carbon cycle and vegetation components correctly simulate edges between biome types, as seen in data such as EO retrievals. The authors may need to explain more precisely where they believe NWP deficiencies lie. What are the specific quantities, forcings and circumstances in which "hard-wiring" local tipping is expected to enhance forecasts?

Yes, catastrophe phenomena seen in either observational data or model outcomes have been concerning and discussing in the various context, using terms such as step-change features in the climate system or the computational instability in computational fluid mechanics. As is well known, continuous or smooth processes in the atmosphere are usually easier to predict than those with sudden change (catastrophes) or ones with step-change features by using the NWP models (by association, using climate models; cf. e.g. North, 2012; Meehl et al., 2013; Lu et al., 2014 below) since the former is most likely a linear process and the latter is largely nonlinear.

As for the issue on "where they believe NWP deficiencies lie", the main aspects may be found in the section of ***Summary and suggestions for future research directions*** *in the revised MS.*

Thank you!

(3) There remain some concerns in NWP. Although weather forecasts now operate at very high spatial resolutions, it remains necessary to parameterise even finer-resolution processes. Some of these processes are strongly nonlinear (convection, cloud formation, rainfall phase changes). Spatial averaging probably smooths out "switching" (i.e. localised catastrophic effects), so some standard discretisation is likely valid. Hence, I could imagine that some nonlinearity that is present in the real system becomes overlooked. This suggestion feels like the nearest I can think of to where the authors believe NWP is unable to capture strong nonlinearity? If this is the case, then there is a lot of literature in the meteorological journals that could be cited – and the authors need to more clearly state this?

We are sorry that we are not very familiar to the issues about the relevant smoother or discretization to spatial averaging above-mentioned, but we do meet the problems related to (say) the so-called computational instability in computational fluid mechanics. When we employ a discretization scheme for computing, the averaging approach is needed to treat localised catastrophic effects in the case of a system coming to a bifurcation point. Surely, there possibly exist more than a single solution in such a circumstances, we can however only output a single solution as the forecast outcome so that averaging is needed. As a result, we are afraid, the case you mentioned above is not involved in the nonlinearity caused by catastrophe attributable to coexistence of multiple equilibria.

I do want to be helpful, and I would be happy to see another version of the manuscript if NPG gives the authors permission to resubmit. There is always a concern that a paper that is radically different from the norm is not accepted because reviewers do not fully understand it. I do not want to be that reviewer, so I think we should give the authors a second opportunity with this manuscript.

Many thanks for your kind assistance and we have made tremendous efforts to revise our

original MS for providing a better version as much as possible. Thank you again!

More Minor Things:

The authors have targeted an English-language journal. I am sorry to say that the paper needs major improvement in its writing style. In many places, the word choice is inappropriate, likely selected with very good intentions from a thesaurus, but the context is very wrong. AI should never be used to generate text that is copied, but it is broadly acceptable to run parts of a paper through a grammar checker such as Grammarly. I think that software can also check context.

We would show our apology for the poor English writing in the MS, which caused your reading trouble so much. We have paid much attention to it when revising the original draft and hopefully it will be more satisfactory this time. Thank you!

The figures need work to improve their presentation to meet the standards for a peer-reviewed journal. For instance, Figure 1 uses a mixture of font sizes, ranging from very large to extremely small. The tick labels are so small that, when I started this review from a paper copy, I simply could not read the numbers. Similarly, the axis labels are impossible to read. Standard format should have the unnecessary horizontal and vertical lines removed.

Thank you very much for your comments and, we suppose, the modified figure version would be of quality.

The paper feels long and very "text-heavy". The authors might consider reducing the word count.

Yes, we reduced the word count. Thank you!

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Citation:

Lu, J., A. Hu, and Z. Zeng (2014), On the possible interaction between internal climate variability and forced climate change, Geophys. Res. Lett., 41, doi:10.1002/2014GL059908.

Meehl, G. A., A. Hu, J. M. Arblaster, J. Fasullo, and K. E. Trenberth (2013), Externally forced and internally generated decadal climate variability
associated with the Interdecadal Pacific Oscillation, J. Clim., 26, 7298–7310.

North, G. R. (2012), Apportioning natural and forced components in climate change, Proc. Natl. Acad. Sci. U.S.A., 109(36), 14,285–14,286,
doi:10.1073/pnas.1212724109.