Review of paper

Review article:
The future of the physics-based NWP methodology: a critical review on applications of catastrophe theory to the atmospheric science

Nonlinear Processes in Geophysics, paper egusphere-2026-2981

by

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I regret I cannot recommend acceptance of the paper, even after a possible revision. That is so not because I consider there is no scientific value in the paper (I think on the contrary that it contains material that may eventually lead to at least one publication), but because a large part of its content is largely exaggerated or even unjustified. For instance, the sentence in the abstract (ll. 30-31) *This* (meaning the increasing difficulty to enhance NWP models forecasting capabilities) *is caused largely by catastrophe/tipping phenomena in the atmosphere as well as the multiplicity of NWP equation solutions* is not supported by any objective evidence presented in the paper.

The paper, as I understand it, consists of two distinct parts. One part (Section 3, entitled *Literature Review*) presents a number of studies of 'tipping points' or 'catastrophes' in the atmospheric/climatic system (I will not here enter into the semantic discussion relative to the meaning of those two terms, and will also use the word 'bifurcation'). That presentation is however incomplete and unstructured. It is incomplete in that it does not mention possible tipping points that have been widely discussed in the literature, such as for instance the collapse of the Atlantic Meridional Overturning Circulation (AMOC, see, *e.g*., van Westen *et al*., 2024), or the dieback of the Amazon Rainforest (Melnikova *et al*., 2025). As for the structure of the presentation, ll. 360-367 of the paper list a number of illustrations of tipping points that are then discussed in some detail. That list is somewhat inconsistent, in that some items in it (1 and 6) refer to specific physical phenomena, while others (5 and 7) refer to much more general possible bifurcation processes in dynamical systems.

The content of Section 3 could be the basis of a general review paper on the broad question of bifurcations (tipping points, catastrophes) in the atmosphere/climate system. Such a review paper should, in addition to summarizing the status of knowledge on that broad question, and according to the NPG requirements, outline future directions of research.

The remainder of the paper (which I must say I find uselessly lengthy and repetitive) bears on the present limits of Numerical Weather Prediction. The authors start from the observation, supported by Figure 1, that the quality of NWP has not improved significantly in recent years. They conclude that there is a 'development bottleneck' that needs to be broken. Their point is that this is to be achieved by resorting to catastrophe theory (CT). Well, diminishing returns are observed in many fields of science and technology (not to mention economics ...), and are not necessarily proof of a methodological 'bottleneck'. They may be due to ultimate limits (for instance, of energy), or lack, or non-use, of resources (after all, the spatial resolution of the model of the European Centre for Medium-Range Weather Forecasts, from which most diagnostics shown in Fig. 1 have been obtained, has apparently not increased since at least 2018).

In order to make their point to support the use of catastrophe theory, the authors make a number of statements which are simply unfounded. In addition to the one I mention at the beginning of this review, I have noticed

Ll. 87-88, … *weather forecast failure happens frequently in case catastrophe/tipping phenomena emerge in the atmosphere*. What is the evidence ? Can you give precise examples of such forecast failures ?

The authors mention (ll. 99-103) a number of atmospheric phenomena, which they claim could be better described by catastrophe theory. That may be true, but these phenomena are simulated to an acceptable degree of accuracy by the present physical models (concerning for instance prediction of sudden stratospheric warmings, see Karpechko *et al*., 2018).

Ll. 631-632, *Numerous catastrophe phenomena have been observed in the Earth atmosphere, but many of these discontinuous phenomena still defy analysis.* Again, can you give precise examples to support the use of catastrophe theory in those situations ?

Similar comments could be made, for instance on the possible multiplicity of solutions of nonlinear equations (and on the use of ensemble forecasts in that context), or on the need for more explicit use of the second law of thermodynamics. But my main point is that the authors do not give any example, even highly idealized, of how what they advocate could be implemented in practice.

I certainly do not want to imply that what the authors say about the possible usefulness of CT for NWP is erroneous. I simply say that, as interesting as that possibility may be, what the authors write is unjustified at this stage. They write (ll. 614-615) that they will organize a separate paper to specifically address the question of a novel CT-based forecasting methodology. That is certainly what they must do now.

My recommendation is to reject the present paper, and to suggest to the authors to write a more complete and better structured review paper on the question of tipping points /catastrophes in the atmospheric and climatic system. And in addition, as they anticipate, to write another paper, with appropriate examples, on the application of catastrophe theory in Numerical Weather Prediction.

References

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