[Response to Reviewer’s Comments]

We are very grateful to the reviewers for their careful reviews and kindly giving us valuable and constructive comments and suggestions. Here, we provide our point-by-point responses whose P and L correspond to page and line numbers of the supplemental PDF file with trach changes.

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[Reviewer 1: General Comments]

This work addresses the mitigation of extreme climatic events through human interventions (e.g. cloud seeding to avoid torrential rainfall). The authors extend the classical model predictive control (MPC) framework with the introduction of a foreseeing horizon, which in a few words means that an extreme event can be foreseen with a longer horizon, and then prevented with an MPC optimization on a shorter "prediction" horizon in order to keep the computation time moderate. The capabilities of the proposed model predictive control with foreseeing horizon (MPCF) are extensively compared to those of MPC through experiments on the Lorenz-96 dynamical system.

As far as I can tell, the introduced method is quite original and promising. The presented experiments are convincing, showing a clear advantage over MPC and answering many (but not all) of the natural questions that arise from the presentation of the method. However, some statements made by the authors, although not crucial for their method, are at best misleading. Besides technical inaccuracies, there is still ample room for improving the general presentation of the paper.

I hereafter present a list of all the elements where I think that improvements can be made, ordered by their organisation in the paper rather than by their relative importances.

Response: We sincerely thank the reviewer for their encouraging and constructive feedback. We revised the manuscript following comments.

[Reviewer 1: Comments]

(1) L18 "Our results demonstrated that introducing the foreseeing horizon improved the [...] particularly when the control horizon is short"

-> at this point the foreseeing and control horizon are not defined, so a reader would have to guess what this is supposed to mean (especially the foreseeing, which is not classical). I suggest briefly explaining what these horizons are (and that the foreseeing one is novel) before this sentence, in order to improve the overall clarity of the abstract.

Response: Thanks for the constructive comment. Revised (P1L15).

(2) L20 "a comparison with the conventional method showed that the MPCF achieved success rates comparable to the conventional method with lower computational costs"

-> the repetition of "the conventional method" is a bit inelegant

Response: Revised (P1L19).

(3) L65-85: Some arguments related to data assimilation are quite unclear. First the authors say that "the EnKC, an approach based on a DA method, inherently calculates a control input only at the initial time of a DA window". This seems to suggest that all DA methods solve an optimization problem on the initial time only, which is false. Then, they say that "MPC is similar to variational DA methods in which the cost function is minimized within a certain time interval through iterative computations". Since variational DA is a subset of DA, this contradicts with the first statement.

Response: Thank you for pointing out this misleading statement. We revised the description (P3L68).

(4) Besides, some variational DA methods (hard-constraint 4D-var) minimize a cost on the initial state only while some others (weak-constraint 4D-var) minimize a cost on the whole trajectory. Yet the authors do not make this distinction, and only refer to "4D-var", which is in fact more often used to refer to the hard-constraint variant.

Response: Thank you for your comment. Our understanding is that the key distinction between weak-constraint and hard-constraint 4D-Var lies in whether the method accounts for model uncertainty. In the context of our study, this distinction was not central to the argument, and we therefore did not explicitly differentiate between the two in the manuscript. As such, we have not made any revisions regarding this point. However, if our interpretation is incorrect, we would greatly appreciate further clarification.

(5) L117 In equation (1), J_u and J_x are not explicited. Although the notations are quite intuitive, it would be preferable to include a brief explanation in the text, with some interpretation (e.g. J_x should be linked to the optimality criterion and J_u to the restrictiveness criterion).

Response: Revised (P4L119).

(6) L128 On a similar note, figure 1 makes no reference to the cost function, so it is difficult to understand the purpose of the control. The authors may consider making an "extreme value" appear like in figure 2 or simply modifying the legend of the figure.

Response: Revised (Figure 1).

(7) L135 "MPCF introduces the new concept" -> MPCF introduces a new concept

Response: Revised (P5L137).

(8) L150-168 The authors describe the content of figure 2 by introducing what looks like an algorithm, yet they do it with a rather informal description that lacks the clarity that would be expected from an algorithm. Typically, the fact that step 3 contains a condition like "if step 2 was skipped" is quite disturbing. I recommend taking some time to think about potential improvements of this presentation.

Response: We revised Figure 2 following the comment (Figure 2).

(9) L165 "The process proceeds" does not sound very good.
Response: Revised (P6L170)

(10) L170 Figure 2: the subfigures a, b, c, d are not directly indicated on the figure. While it is obvious which is which, it makes the overall figure less readable.
Response: Revised (Figure 2).

(11) L181-183 it is really not necessary to introduce both variables n = 40 and k = n-1. Classically only n (the number of model variables) is introduced, and I recommend the authors follow this convention.
Response: Revised for entire manuscript.

(12) L187 writing "5 days" instead of "5 d" would be clearer in my opinion.
Response: Revised (P8L191).

(13) L189-190 the subscripts are inconstitent, I guess the authors wrote "u_k" where it should have been "u_i".
Response: Thanks for finding this typo. Revised (P8L193)

(14) L193 "Multiplying this value by dt is equivalent to the value of the control input in the case of direct addition to the state" is wrong in general. This statement only holds under the approximation of an infinitesimally small dt, or when using the Euler integration scheme with a time step of dt, while the authors use the Runge-Kutta 4 scheme in this study.
Response: Thanks, you are right. We updated the description (P8L197).

(15) L203 Equation (4) -> have you considered using U_max = 0 (i.e. directly penalizing the squared norm of the control)? You would certainly have to used a reduced penalty cost in this case. Perhaps a L1 norm would be a better choice too. I would like to see some comments on these considerations. Some associated experiments would be greatly appreciated.
Response: Thanks for your comment. We do not consider the case of U_max=0 in this study because this would significantly penalize the control inputs, which is expected to be significantly difficult for mitigating extreme events.

(16) L207 "The scalar U_max ¥in R is the value that the norm of the control input is constrained to"
-> the formulation is rather misleading, as it seems to suggest that one wants the norm to stay close to U_max. Yet in principle a lower norm of the control would also be acceptable, although figure 3 seems to show that in practice the norm always remains close to U_max.
Besides, it is not even an inequality constraint, since in theory the norm of the control input could also go

above U_max. The fact that this does not happen in practice is certainly due to a high value of w, which should be underlined in the text. Ideally, experiments analysing the influence of w would be a good addition to the manuscript.

Response: Thank you for insightful comment. We added descriptions on these points (P9L215 and P12L291). In addition, we determined the tunable parameters (w and W) through preliminary experiments, which is described in the revised manuscript (P9L221).

(17) L265 The indications of the tested values of T_p are contradictary between the first paragraph of section 4.2 and the corresponding row of table 2.

Response: Thank you for finding the typo. Revised (Table 2).

(18) L324 For figure 5, I believe that the sets of hyperparameters used to produce the points are not clearly described, outside of the fixed value of T_c. For instance, why are there 3 points for MPC and 12 points for MPCF? If only one hyperparameter varies, then it might be interesting to use the color of points to give this hyperparameter information, and to differentiate between MPC and MPCF with the shape of the scatter points instead.

Response: Thank you for your helpful comment. We agree that the description of the hyperparameter settings used to generate the figure (now Figure 7 (a) in the revised manuscript) was insufficient. In the revised manuscript, we have clarified that this figure is a summary of the results presented in Figure 4 (P18L388). Also, we updated Figure 7 to differentiate the hyperparameter (Figure 7).

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[Reviewer 2: General Comments]

(1) In this paper, the authors applied model predictive control (MPC) to Lorenz96. They proposed triggering MPC based on long-term and large ensemble model prediction which detects the emergence of extreme values. Their "foreseeing horizon" method improves the efficiency of MPC, which has a potential to contribute to mathematical optimization of weather control. Although the paper is within the scope of NPG, I believe this paper does not have a publishable quality. The issues raised below may not be addressed in a short period of time. Therefore, I recommend rejecting this paper.

Response: First of all, thank you very much for carefully reviewing our manuscript and providing valuable comments. We understand your concerns and believe that some of the issues you raised were caused by insufficient explanations in our initial manuscript. In the revised version, we have improved the descriptions to clarify our contributions and avoid misunderstandings. We still believe that our proposed approach has sufficient novelty and scientific value, and we would greatly appreciate your reconsideration based on the revised manuscript. Detailed responses to each comment are provided below.

(2) First, the proposed method is not novel enough to be published in my opinion. Although the authors

did not mention it, what the authors proposed can be recognized as event-triggered MPC in which the control process is applied only when a prescribed condition is met. The designed trigger of this paper is the predicted extreme variables above the prescribed threshold, and these extreme variables are detected by large ensemble prediction whose horizon is longer than the prediction horizon that the subsequent MPC process used. Generally, it requires a priori knowledge of the problem to design event-triggered policy. So, it looks to me that the authors proposed the original event-triggered policy suitable for controlling the Lorenz96 model, which is not a real-world problem. The authors guess that the similar approach is effective to weather modification, but it is a speculation and has not been verified. The paper provides neither generally applicable mathematical methods nor heuristic solutions to real geoscientific problems.

Response: We appreciate your comment pointing out the relationship between our method and event-triggered MPC. As you indicated, our method can be interpreted as an event-triggered MPC framework. However, the novelty of our approach also lies in how the MPCF exploits the intrinsic nature of chaotic dynamical systems, where some trajectories lead to extreme events while others do not. By detecting extreme events earlier with a longer foreseeing horizon than the prediction horizon and guiding the system toward safe trajectories within the chaotic attractor, the MPCF enables efficient mitigation of extreme events compared to simple extension of prediction horizon as the conventional method (Figure 4, P14L316). While our study is still in the conceptual stage using the Lorenz 96 model, we believe that these ideas provide meaningful insights for controlling chaotic dynamical systems. We added these points in the revised manuscript (P3L89, P5L139, P6L145).

In addition, we carefully revised the manuscript to avoid excessive speculation regarding the application of the MPCF to weather control. However, the Lorenz 96 model has been widely used in the development of data assimilation methods for numerical weather prediction, and many insights gained from it have led to successful applications in real-world weather forecasts. Therefore, we believe that the findings on extreme event mitigation in the Lorenz 96 system represent an important step toward the practical realization of weather control (P20L463).

(3) Second, the experiment design is flawed. The authors performed an experiment “14d before the time of the target extreme event”, so that in their experiment, the method “knows” that the extreme events will happen. In all experiments, it is necessary to perform MPC, and the authors do not need to worry about false alarms of their ensemble prediction in foreseeable horizons. This is unfair. Considering that the prediction gets rapidly worsened in the longer lead time, the authors’ foreseeing horizon may provide an adverse effect to mistakenly let controllers intervene the system for nothing.

Response: Thank you for this critical and constructive comment. We believe that this concern was raised by the insufficient explanation in the previous version of the manuscript. Actually, as shown in Figure 3, false alarms can occur even with the current experimental design. We emphasized this point in the revised manuscript (P11L279).

Furthermore, to verify this concern, we conducted a new set of experiments under conditions where the occurrence of extreme events is uncertain. Even in this more realistic setting, the proposed MPCF method consistently outperformed the conventional MPC in terms of reducing the number of extreme events. These results reinforce the usefulness of MPCF as a practical and effective approach for mitigating extreme events (Sect. 5.2 and Table 3).

(4) Also, I guess the authors quantified the computational efficiency of their proposed method based only on the cost of MPC optimization processes, and they did not include the computational cost for prediction in foreseeable horizons. If so, it is unfair. Their large ensemble extended forecast in foreseeable horizons is very costly especially in the weather modification context, although it can be run in parallel. I think that their primary claims of the advantage of their proposed method have not been fully supported by the experiments in the current form of the paper.

Response: As the reviewer correctly pointed out, the current definition of MOOT does not include the computational time required for ensemble forecasts over the prediction or foreseeing horizons. However, while ensemble forecasts can be parallelized, the multiple forecasts over the prediction horizon required during the tens of iterations within the optimization for identifying control inputs cannot be parallelized. In addition, the optimization requires iteratively calculating the gradient of the cost function. Therefore, the computational cost of ensemble forecasts is considered to be relatively small compared to the optimization and is not expected to have a significant impact on MOOT. We discuss this point in the revised manuscript (P10L261).

[Reviewer 2: Major Comments]

(1) L35-40: These three key characteristics should be met in any control problems. For instance, any control problems should provide a solution in real time. I believe that the authors would like to say something different.

Response: Thank you for your comment. We revised the text to clarify our intention (P2L35).

(2) L63-64: Here the authors mentioned that the difference-based approach does not have an optimization process. But what is the optimization process specifically? Also, I think the optimality (or accuracy) of the solution is important, and the existence of the optimization process is not important. The difference-based method may not perform optimization, e.g., iterative evaluation of a cost function, but if the solution is very good, I'm going to be satisfied with it. I believe the authors would like to say something different.

Response: Thank you for your insightful comment. Our original wording was misleading. What we intended to highlight was not the absence of an optimization process itself, but rather the difficulty of explicitly constraining the control inputs in the difference-based approach. We revised the manuscript on this point (P2L63).

(3) L117: I think it is better to explain more about Equation (1). J_u and J_x are not explicitly defined.
Response: Revised (P4L119).

(4) L196: 200-member ensemble forecast for the 40-dimensional Lorenz96 model is apparently infeasible when it is translated into the real-world atmospheric model. Since the authors mentioned the advanced rapidity of their methods, I believe that the authors may evaluate their method with a feasible ensemble size. Maybe the authors think that MPC may not be able to be applied to the conventional atmospheric model, and they prepared the methods for future advanced AI weather models which are computationally efficient. Even in this case, the authors may explicitly explain if the large ensemble is absolutely necessary or not.
Response: Thank you for this important comment. The present study uses a 200-member ensemble to verify the concept of the MPCF without the need for parameter tuning, such as localization. The revised manuscript describes the reason of the large-ensemble experiments (P8L200).

We agree that a 200-member ensemble forecast for the 40-dimensional Lorenz 96 model is not directly feasible for physical-model-based operational numerical weather prediction models. However, as described in the revised manuscript, we anticipate future applications involving AI-based weather forecasting models, which have much lower computational costs than traditional physical models and are well suited for large ensemble forecasts (P21L469). Moreover, the revised manuscript also discusses that the required ensemble size for real-world weather control problems is expected to be significantly smaller than the full dimensionality of atmospheric models, as suggested by previous work (Kondo and Miyoshi, 2016). Thus, while a large ensemble is not absolutely necessary, it was used here to isolate and evaluate the core concept of MPCF in an idealized setting without confounding factors such as localization. We added discussion on this point (P21L475).

(5) L310-315: I think it is more beneficial that the authors indicate the number of iterations in the optimization loop of MPC in addition to computational time. If the number of iterations is found, readers easily infer the potential computational cost in the real-world applications.
Response: We added further analyses and discussion on this point (Section 5.1).

[Reviewer 2: Minor Comments]

(1) L10 & L28: Personally, I do not like mentioning a specific funding project in the paper. I believe that the authors can easily connect their work to the broader academic context without mentioning the specific project which does not last longer than the academic time scale. I'm not asking the authors to follow the instructions, this is just my subjective opinion.
Response: Thank you for your thoughtful comment. Following your suggestion, we have removed the

mention of the Moonshot Research and Development Program from the abstract to maintain a broader academic perspective (P1L10). However, we believe that providing the context of this ongoing national program is important for clarifying the practical background and motivation of our study. Therefore, we have retained a brief mention of the program in the main text.

(2) L267: I think it is better to separate “Results and discussion” into two sections (i.e., “Results” and “Discussion”). In most cases, in the discussion section, the authors discuss their work, comparing it with the other work, and mention their limitations. It is a bit difficult for me to find the “discussion” part of this “Results and discussion” section.

Response: Thanks for your comment. We separated into two sections as suggested (P3L97).