Response to Editor

General comments

The two referees are the same as for the first version of the paper, with the same identification numbers.

Referee #1 writes that the authors have revised the paper taking into account his/her comments, and that the paper can be published as such.

Referee #2 is more critical. His/her criticisms do not bear on the science of the paper, but on what he/she considers as unsubstantiated claims, that need to be clarified. He/she gives specific examples. I ask the authors to revise their paper along the lines suggested by Referee #2, and to give a point-by-point response to each of those comments.

I also as Editor have a number of specific suggestions for modification (contrary to what is the case in Referee #2's comments, the line numbers below are those of the file npg-2024-26-ATC1.pdf, which shows explicitly the modifications made by the authors in their paper).

In case the authors disagree with a particular comment or decide not to follow a particular suggestion (whether from the Referee or the Editor), they must state precisely their reasons for that. I will be looking forward to a new revised version of the paper, which I may submit to further review.

[Reply to this comment]

We appreciate the editor's continued support and guidance throughout the review process. We sincerely thank the editor and both referees for their careful reviews and constructive feedback. We have revised the manuscript accordingly and addressed all comments in the point-by-point response below. We respectfully submit the revised version for your consideration.

Specific comments

R1. L. 137, (a), select only two ensemble members S and N? And what if all elements predict a regime shift? Go to the next time step, although action obviously seems to be required?

[Reply to this comment]

Thank you for your comment. We would like to clarify that this is not our proposed method. The selection of only two ensemble members (S and N) is part of the method introduced by Miyoshi and Sun (2022), which we use as a baseline for comparison. Since our goal is to fairly evaluate our proposed approach against their framework, we follow their original setting.

[Revised parts]

The following part has been added to **Section 2.2 on Page 5** of the revised manuscript.

"We adopt the above method as a baseline for comparison with other methods in the following tests. To ensure a fair evaluation, we follow the original setting described in their work."

R2. L. 248-250, Minimizing cost function (6) with condition (7) will tend to make the values of x_t negative, although it is positive values that are looked for (see ll. 314-317).

[Reply to this comment]

We apologize for the confusion. There was a typo in Equation (6): we missed a minus sign. It should be

$$J_{t,T} = \sum_{i=0}^{T} c(-x_t).$$

With the corrected equation above, the cost function will penalize negative values of x_t. Minimizing this cost encourages x_t to remain positive, rather than negative. This aligns with our intention to avoid negative x_t, as discussed later in the manuscript. We have clarified this point in the revised version.

[Revised parts]

Equation (6) has been revised as follows:

$$J_{t,T} = \sum_{i=0}^{T} c(-x_t).$$

R3. L. 302, Figure 3.a → Figure 4.a

[Reply to this comment]

We thank the editor for pointing this out. In the revised manuscript, we have corrected the reference accordingly.

R4 L. 302, Figure 3.b→ Figure 4.b. The same correction is to made later in the paper. Please check.

[Reply to this comment]

We thank the editor for pointing this out. In the revised manuscript, we have corrected the reference accordingly.

R5. Ll. 321-322, "*… the control success rate with incremental learning was never lower than without incremental learning for any combination of parameters T and D.*" And then (l. 328), This result indicates that MPCIL outperforms MPC in some cases … Only in some cases, or all (see also l. 332, … in certain scenarios.)?

[Reply to this comment]

Thank you for pointing out this inconsistency. We apologize for the confusion. Our intention in lines 321–322 (in the original manuscript) was to state that, in our experiments, the control success rate with incremental learning tended to be better than without it across the tested combinations of parameters T and D when the values of them are relatively small. However, since we cannot evaluate all possible combinations, we should use a more cautious phrasing, just like we did in line 328: "MPCIL outperforms MPC in some cases."

To avoid misunderstanding, we have revised the earlier sentence ("⋯ the control success rate with incremental learning was never lower than without incremental learning …") for consistency and clarity.

[Revised parts]

The following part has been added to **Section 4.3 on Page 14** of the revised manuscript.

"The control success rate with incremental learning tended to be better than without it across the tested combinations of parameters …"

Response to Referee #1

<u>General comment</u>

Accepted as is.

[Reply to this comment]

Thank you very much for your positive feedback and for recommending acceptance of the manuscript. I truly appreciate your time, valuable suggestions, and kind support throughout the review process.

Response to Referee #2

General comment

I command the authors for publishing their codes and clarifying the methodological framework they are using. However, the current manuscript contains unsubstantiated claims in it and clarifying these would require additional analysis. On the other hand, dropping these claims would reduce the interest of the manuscript.

[Reply to this comment]

We thank the reviewer for the positive remarks regarding the publication of our code and the clarification of the methodological framework. With respect to the concern about the unsubstantiated claims, we acknowledge the importance of providing solid evidence. Accordingly, rather than removing these points, we have conducted additional analyses to substantiate them. These revisions are reflected both in the response letter and in the updated manuscript.

Major comments

R1. The authors now mention the nature run, however they still don't mention how this nature run is obtained, by running a separate independent copy of the L63 (so the same model but initialized with a given initial condition).

[Reply to this comment]

Thank you for the comment. As you insightfully pointed out, the nature run is generated by independently running the Lorenz-63 model with a fixed set of parameters but initialized with a given initial condition. This independent trajectory serves as the ground truth (real atmosphere) for evaluation purposes. We added clarification to this point in the revised manuscript.

[Revised parts]

The following part has been added to **Section 2.2 on Page 5** of the revised manuscript.

"... [nature run] which is generated by independently running the Lorenz-63 model with a fixed set of parameters initialized with a given initial condition."

R2-1. It is now clearer that the MPC is adding continuously perturbations to the NR (real atmosphere) at each time step, while the CSE is only perturbing if a regime change is detected in the ensemble. So, in short, MPC perturbation scheme is always on, while the CSE one is activated only if needed. Somehow, this is not very much acknowledged in the manuscript.

[Reply to this comment]

We thank the reviewer for this insightful observation. As correctly pointed out, the MPC framework applies continuous perturbations to the nature run at each time step, whereas the CSE strategy follows an event-triggered scheme, introducing perturbations only when a regime change is detected in the ensemble. This fundamental difference in perturbation mechanisms is indeed important and was previously underemphasized. We have now explicitly clarified this distinction in the revised manuscript.

[Revised parts]

The following part has been added to **Section 3 on Page 6** of the revised manuscript.

"Unlike the CSE strategy, which employs an event-triggered scheme and introduces perturbations only when a regime change is detected in the ensemble, the MPC framework applies continuous perturbations to the nature run at every time step."

R2-2. On the other hand, on lines 60-62, it is stated that "[MPC] can reduce the required control effort." Later, on lines 64-65, "[MPC] also reduces the required control effort in steering the system away from undesirable regime shifts." These claims are not substantiated later in the study. In practice, it should not be difficult to introduce an energy norm in the L63 model to estimate and compare the "energy consumptions" of both methods. Other metrics might also be introduced here to define more precisely what “effort” means. However, this has not been done by the authors, who also do not reproduce the CSE results of Miyoshi and Sun (as seen in the code), but simply copy-paste them into their tables and figures. Therefore, saying that one method requires “less effort” than the other one is unsubstantiated here, as maybe the fact that the CSE perturbation scheme is not always on saves a lot of “energy” in the end (even if MPC perturbations are smaller on average).

[Reply to this comment]

We thank the reviewer for pointing out the ambiguity regarding the control effort of MPC and CSE. We would like to clarify the following points.

First, we apologize for any confusion caused by the statement that “[MPC] can reduce the required control effort.” What we intended to emphasize is that, in our experiments, when the (bound of) magnitude of control input is relatively small, MPC achieved a higher success rate compared to CSE. This observation is clearly supported by the experimental results. As shown in Table 2, MPC outperforms CSE in terms of success rate when the control bound $D \leqslant 0.5$ and the time horizon $T \leqslant 188$. This finding is particularly relevant in practical scenarios such as weather control, where strong interventions are physically infeasible, and effective regulation must therefore rely on minimal and gradual inputs. Moreover, since long-term forecasts are inherently unreliable, the prediction horizon should be kept short to ensure robust and timely control. We have revised the manuscript accordingly to clarify this point.

Second, we agree that the concept of “control effort” should be more precisely defined to avoid ambiguity. In our study, the term refers to the instantaneous magnitude of the control input applied at each time step, rather than the accumulated energy over time. We will revise the manuscript accordingly to clarify this point and avoid potential misunderstandings.

Finally, regarding the use of CSE results from Miyoshi and Sun’s paper, we would like to emphasize that our decision to directly use the data reported in their original study was intentional and driven by fairness. Specifically, our goal was to ensure a consistent baseline for comparison between the two methods. Re-implementing the CSE scheme independently might introduce discrepancies due to differences in implementation details. By directly using their published results, we aimed to avoid such inconsistencies and ensure that any observed differences in performance were attributable to the control strategies themselves, not to experimental variability. We have revised the manuscript to make this point clearer.

In summary, we highlight that the MPC framework outperforms the CSE strategy in terms of control success rate when the control effort (instantaneous magnitude of the input applied at each time step) is limited and the prediction horizon is short. This conclusion is supported by experimental data and is particularly relevant to practical scenarios such as weather control, where strong interventions are physically infeasible and long-term forecasts are unreliable.

[Revised parts]

The following part has been added to **Section 1 on Page 3** of the revised manuscript.

"The results suggest that, when the control effort (instantaneous magnitude of control input applied at each time step) is limited and the prediction horizon is short, the MPC framework outperforms the CSE strategy in terms of control success rate.

… …

This finding is particularly relevant in practical scenarios such as weather control, where strong interventions are physically infeasible, and effective regulation must therefore rely on minimal and gradual inputs. Moreover, since long-term forecasts are inherently unreliable, the prediction horizon should be kept short to ensure robust and timely control."

The following part has been added to **Section 1 on Page 3** of the revised manuscript.

"… when the control effort (instantaneous magnitude of control input applied at each time step) …"

The following part has been added to **<u>Section 4 on Page 9</u>** of the revised manuscript.

"We adopted the results from Miyoshi and Sun (2022) as a baseline to avoid discrepancies that might arise from re-implementing the CSE scheme independently. This ensures that observed performance differences are due to the control strategies themselves, not implementation variability."

R3-1. The summary and future work section is problematic, again because of many unsubstantiated claims: First, the authors state that their findings "revealed that while the CSE control strategy exhibited high effectiveness for relatively long prediction horizons, its control success rate significantly decreased when the prediction horizon was short." The authors should not attribute to themselves these findings since the results are coming directly from the Miyoshi and Sun paper. Also, effectiveness is here with respect to MPC, not in absolute terms. The sentences from line 323 to line 327 should be rephrased.

[Reply to this comment]

We thank the reviewer for the insightful comments regarding the attribution of findings related to the CSE method and the interpretation of its effectiveness. In what follows, we would like to clarify these two points.

First, regarding the attribution issue, we acknowledge that our original phrasing may have caused confusion. It was not our intention to present the observation regarding the performance of CSE over different prediction horizons as a novel finding. Rather, our aim was to summarize this behavior in the context of our own comparative experiments. To clarify this, we have revised the relevant sentences to explicitly acknowledge that the trend (the high effectiveness of CSE for longer prediction horizons and its reduced success rate for shorter ones) was originally observed in Miyoshi and Sun (2022), and that our results are consistent with their findings within our experimental setup.

Second, as the reviewer correctly noted, the notion of "effectiveness" in this context should be interpreted relative to MPC, not in absolute terms. We have updated the manuscript accordingly to ensure this comparative perspective is clearly conveyed.

[Revised parts]

As suggested by the reviewer, the **first paragraph (lines 323 to 327 in the original manuscript) of Section 5** has been revised accordingly. The updated version can be found on **Page 15** of the revised manuscript.

"In this study, we conducted a comprehensive evaluation of the control strategy employed in the CSE by comparing it with MPC using temporal deep unfolding on the Lorenz-63 model. The findings reported by Miyoshi and Sun (2022) indicate that the CSE strategy performs relatively well under longer prediction horizons, whereas its success rate diminishes when the horizon is short. In contrast, our results show that MPC achieves better control performance under short prediction horizons, maintaining the system's state within the desired regime."

R3-2. Line 328: The temporal deep unfolding was already present in the Kishida and Ogura 2022 paper, and also in Aizawa et al. 2024, so this cannot be proposed as a key contribution of the manuscript.

[Reply to this comment]

We thank the reviewer for this comment regarding the role of temporal deep unfolding in our manuscript.

We agree with the reviewer that the temporal deep unfolding methodology itself was originally introduced and developed in previous works, notably by Kishida and Ogura (2022) and Aizawa et al. (2024). We have appropriately cited these seminal contributions in our manuscript to acknowledge the origin of the technique.

However, we would like to clarify that the contribution of our study does not lie in the invention of temporal deep unfolding itself, but rather in the adaptation and implementation of temporal deep unfolding within the stabilization of the Lorenz-63 model inspired by CSE.

Nevertheless, we appreciate the reviewer's attention to the positioning of our contributions and revise our manuscript accordingly to clearly state the novelty of this work related to temporal deep unfolding lies in the adaptation and implementation of this technique rather than in proposing the core methodology itself.

[Revised parts]

The following part has been rewritten in **Section 5 on Page 15** of the revised manuscript.

"First, it presents an adaptation and tailored implementation of temporal deep unfolding for controlling the Lorenz-63 system within a control framework inspired by the CSE strategy."

R3-3. Lines 331-332: "the results highlight the broader applicability of MPC in tackling practical challenges associated to chaotic dynamics." Why can the authors say that? Why does MPC have broader applicability than CSE? Again, this looks like an unsubstantiated claim.

[Reply to this comment]

We thank the reviewer for pointing out this issue. Our original wording may have unintentionally implied that MPC has broader applicability than CSE in general, which was not our intention. What we intended to emphasize is that our work extends the application of MPC to a new context, chaotic weather-like systems such as the Lorenz-63 model. We have therefore rephrased the sentence to clarify that our study demonstrates the effectiveness of MPC in this specific setting, rather than making general claims about its broader applicability compared to CSE.

[Revised parts]

The following part has been revised in **Section 5 on Page 15** of the revised manuscript.

"While this study primarily focuses on theoretical and numerical analyses, it broadens the applicability of MPC by demonstrating its effectiveness in addressing practical challenges associated with chaotic dynamics."

Minor comments

R4. Lines 59-64: Sentences seem to be redundant here. Please clarify.

[Reply to this comment]

We thank the reviewer for pointing out the redundancy in lines 59 -- 64. To address this, we have revised these sentences to improve the logical flow and remove redundancy.

[Revised Parts]

As suggested by the reviewer, lines 59 to 74 in the original manuscript have been revised accordingly. The updated version appears in the **sixth paragraph of Section 1 on Page 15** of the revised manuscript.

"The results suggest that, when the control effort (instantaneous magnitude of control input applied at each time step) is limited and the prediction horizon is short, the MPC framework outperforms the CSE strategy in terms of control success rate.

… …

This finding is particularly relevant in practical scenarios such as weather control, where strong interventions are physically infeasible, and effective regulation must therefore rely on minimal and gradual inputs. Moreover, since long-term forecasts are inherently unreliable, the prediction horizon should be kept short to ensure robust and timely control. We have revised the manuscript accordingly to clarify this point."

R5. Table 1: The step size units are in "sec" (second), but time units in L63 are dimensionless, so typically one writes "timeunits" for the units or nothing. This is a mistake coming originally from the Miyoshi and Sun paper.

[Reply to this comment]

Thank you for pointing this out. We agree that the Lorenz-63 model uses dimensionless time, and the unit "sec" is inappropriate in this context. We have now corrected it in our revised manuscript.

[Revised Parts]

In **Table 1** of the revised manuscript, we have removed the unit "sec".

R6. Table 1: T_a is also every 8 steps, so it should be T_a = 8 steps, as above for the prediction horizon. Also "Observation period" looks wrong to me and should rather be "Observation frequency". Please check.

[Reply to this comment]

We thank the reviewer for pointing out the inconsistency regarding the description of T_a. In response, we have revised the manuscript accordingly.

We also thank the reviewer for the suggestion related to "Observation period". However, in our case, "Observation period" is the intended term, as it refers to the time interval between two consecutive observations. "Observation frequency" would instead denote how many observations occur per unit time, which is the inverse. Therefore, "Observation period" is more accurate in this context.

[Revised Parts]

In **Section 2.2** and **Table 1** of the revised manuscript, we have corrected the expression for the assimilation interval T_a to explicitly state T_a = 8 steps, in line with the notation used for the prediction horizon.

R7. Line 138: I would say "new observations are obtained" rather than "new observations are performed". This matches the step 1 formulation.

[Reply to this comment]

We thank the reviewer for the suggestion to improve the wording. In response, we have replaced the phrase "new observations are performed" with "new observations are obtained" throughout the manuscript.

[Revised Parts]

We have replaced the phrase "new observations are performed" with "new observations are obtained" in both **Section 2.2 and Section 3.3** in the revise manuscript.