Response letter of egusphere-2024-781

Dear Editor,

Please find the final response to my paper “Ensemble Kalman filter in geoscience meets model predictive control”,

Comments made by the reviewers were highly insightful. They allowed me to greatly improve the quality of the manuscript. I described the responses to the comments.

Each comment made by the reviewers is written in *italic* font. I numbered each comment as (n.m) in which n is the reviewer number and m is the comment number. In the revised manuscript, changes are highlighted in yellow.

Sincerely,
Yohei Sawada

**Responses to the Anonymous Referee #1**

*(1.1) In this manuscript, the author propose to use a well-known data assimilation method, namely the ensemble transform Kalman smoother, for a model control problem. The method is illustrated using the three-variable Lorenz 1963 system.*

*The manuscript is well written and easily understandable. It presents a new method for model control. However, after reading the manuscript, I have a feeling of incompleteness both in the justification of several methodological choices and in the discussion of the results. I will try to explain this in a constructive way.*

→ Thank you very much for the evaluation of the paper. Please see the following responses to your comments.

*1 Major comments*

*1.1 Presentation of the EnKF*

*(1.2) In this manuscript, the EnKF is presented as a variational method (L 108-109 'EnKF aims to minimize the following cost function'). While it is true that under linear and Gaussian assumptions, the EnKF analysis provides the solution to a variational problem, the EnKF is not a variational assimilation method and the description provided in section 2 is partial and misleading. In particular there is no information about the ensemble update.*

→ This point was indeed unclear in the original version of the paper. I have added some explanations. I tried to make it short, since most of the potential readers in NPG do understand this point and may feel it is redundant to fully explain EnKF updates.

"Assuming that the observation operator is linear, and errors follow the Gaussian distribution, EnKF solution minimize Equation (4). It is not necessary to obtain a full covariance matrix $\boldsymbol{P}^b$ as well as the linearized observation operator since ensemble-based approximations are used to compute Kalman gain (see equation (1)-(10) in Houtekamer and Zhang 2016). There are several flavors of EnKF to transport each ensemble members. In this paper, I used the ensemble transform Kalman filter (ETKF; Bishop et al. 2001, Hunt et al. 2007) to obtain the analysis ensemble."

*1.2 Methodological choices*

*(1.3) Throughout the manuscript, strange methodological choices are made. These choices need to be justified. Retrospectively, my impression is that these choices are necessary because otherwise the control problem would not be solvable with an ETKS, but in the end, without justification it makes me believe that the ETKS is not an appropriate choice for the control problem.*

*Choice of controlling only the end of the trajectory As far as I understand, from a control perspective it would be more efficient to have the entire trajectory in the control horizon. For example, I think that the reason why in the numerical experiments the control fails for large values of Cr is precisely that the control is only applied at the end of the control window.*

→ This comment is related to the concerns of Referee #2. There are a lot of better control algorithms than mine in the literature in control engineering. However, to the best of my knowledge, most of them are computationally expensive and less flexible toward the application of geoscientific problems in which the degree of freedom is large. This is my motivation to propose this simple EnKF-based method. This point was indeed unclear in the original version of the paper. I have clarified this point in the Introduction of the revised paper. See also my response to the comment (2.1).

"Note also that my proposed method is suitable for controlling systems with extremely large degree of freedom, in which the size of state vectors is the order of $10^4$~$10^9$, such as atmosphere. In geoscience, many previous works recently proposed advanced methods to analyze these large-scale systems based on machine-learning such as recurrent neural networks (e.g., Tomizawa and Sawada, 2021), graph neural networks (e.g., Lam et al. 2023), and generative adversarial networks (e.g., Gagne et al. 2020). Some works used these advanced data-driven approaches to control water-related infrastructures (e.g., Tian et al. 2022). However, the application of these techniques to full-scale complex Earth system models is still in its infancy, so that the proposed EnKF-based method in this paper can be a feasible alternative to find optimal control perturbations for the modification of large-scale systems such as atmosphere."

Performing the control perturbations only at the end of trajectory (i.e. at the beginning of control horizons) is also a compromise ground to avoid the iterative integration of computationally expensive Earth system models. It is generally expensive to directly optimize the equation (2), which I believe is infeasible for geoscientific applications such as weather modification. I tried some heuristic methods to apply the intervention at all timesteps in the control horizon. I divided $x_0 - \overline{x_0^a}$ obtained in Step 3 by the number of timesteps of the data assimilation window and applied them to model trajectory at all timesteps in the window. Although this method is highly heuristic, it can achieve the control objective under a sufficiently small $\boldsymbol{C}^r$.

I believe that this issue can also be addressed by the recent growth of the extremely frequent observations and data assimilation of them. Whenever observations are obtained, the control program can be updated. For instance, Miyoshi et al realized 30-second EnKF updates using Phased-array weather radars. In this case, my control method can gradually change the intervention every-30-second looking at the observations, which may be actually too frequent considering the potential technologies of weather modifications. These points were indeed unclear in the original version of the paper. I have discussed this point in the revised version of the paper.

"There are several limitations of the proposed method against conventional model predictive control methods. First, the proposed method performs a control intervention only at the beginning of control horizons, which is apparently sub-optimal to minimize equation (2). Note that it is infeasible to directly optimize equation (2) for geoscientific applications such as weather modifications since dynamic models are computationally expensive. I tried some heuristic methods to apply interventions at all timesteps in the control horizon. I divided $x_0 - \overline{x_0^a}$ obtained in Step 3 (see Algorithm 1) by the number of timesteps of the data assimilation window and applied them to model trajectory at all timesteps in the window. This method can achieve the control objective under a sufficiently small $\boldsymbol{C}^r$ (not shown). Since recent advanced observation systems enable rapid (30 seconds ~ 10 minutes) updates of data assimilation (e.g., Miyoshi et al. 2016; Sawada et a. 2019), the proposed algorithm can frequently change the strategy of interventions, which also mitigates this disadvantage of the proposed method."

*(1.4) Choice of the ETKF/ETKS With a time interval between observations of 0.08 model time units (MTU), the dynamics of the system is mildly nonlinear, and therefore using the ETKF for the assimilation step totally makes sense. On the other hand, the control horizon is 300 × 0.01 = 3 MTU, which means that the dynamics will be strongly nonlinear throughout the control window. In a strongly nonlinear regime, the choice of a Kalman smoother with its inherent linear and Gaussian assumptions is questionable. Furthermore, from a control perspective the goal is to control the dynamics of the system, which is why a nonlinear optimisation method would make much more sense than the ETKS here.*

→ This comment is essentially similar to the comment (1.3). I totally agree that a nonlinear optimization method is always better if it is feasible for the problem. Again, I believe that the proposed method is the reasonable and effective compromise to solve the control problem in geoscience whose models have the extremely large degree of freedom. I performed the sensitivity test of the control horizon and found that the proposed method is more effective with shorter control horizon. This point was indeed unclear in the original version of the paper, and I have included this point in the revised version of the paper.

"Figures S1 and S2 show the sensitivity of control horizon $T_c$ to the performance. When a sufficiently small $\boldsymbol{C}^r$ is chosen, the proposed method with relatively shorter control horizons can also successfully achieve the control objective. While the trajectory stays in the smaller region $(6.5 < X < 10.5)$ when $T_c$ is 300 timesteps, it stays in larger regions with shorter $T_c$. This may be an advantage of choosing shorter control horizons. However, the magnitude of the perturbation, $D$, increases when $T_c$ is shorter. This is because the timing when the extended forecast (Step 2 in Algorithm 1) detects the risk of moving to the other wing of the butterfly is delayed with shorter lead times. In this case, larger perturbations are necessary to adjust the trajectory to stay in the current wing of the butterfly. I also found that when $T_c$ is 500 timesteps, the simulation of the Lorenz63 system gets unstable and the computation cannot be completed. Strong nonlinearity of the dynamics in the long control horizon prevents the successful control."

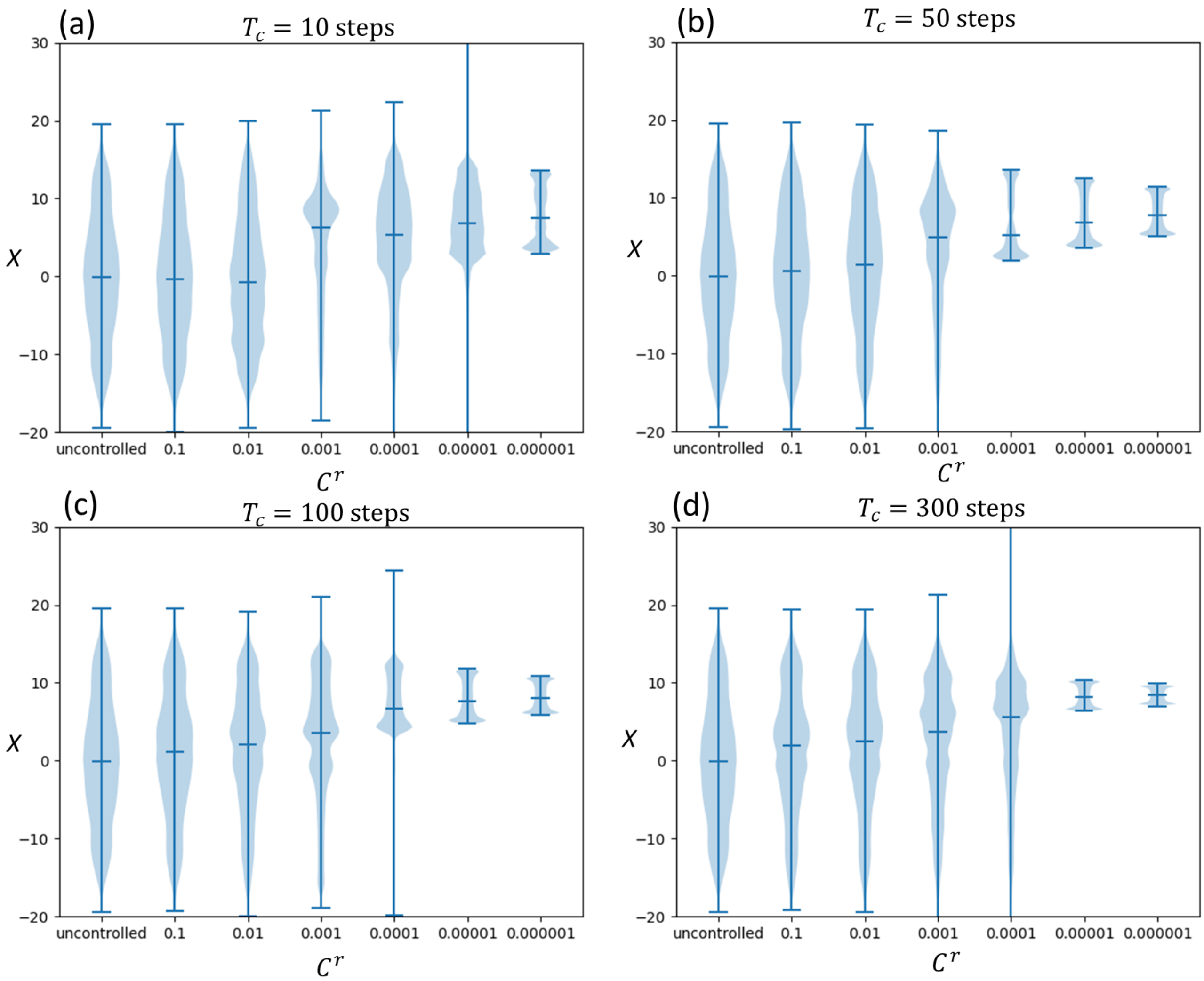


**Figure S1**. Same as Figure 3a but for control horizons set to (a) 10 timesteps, (b) 50 timesteps, (c) 100 timesteps, and (d) 300 timesteps. Note that Figure S1d is identical to Figure 3a.

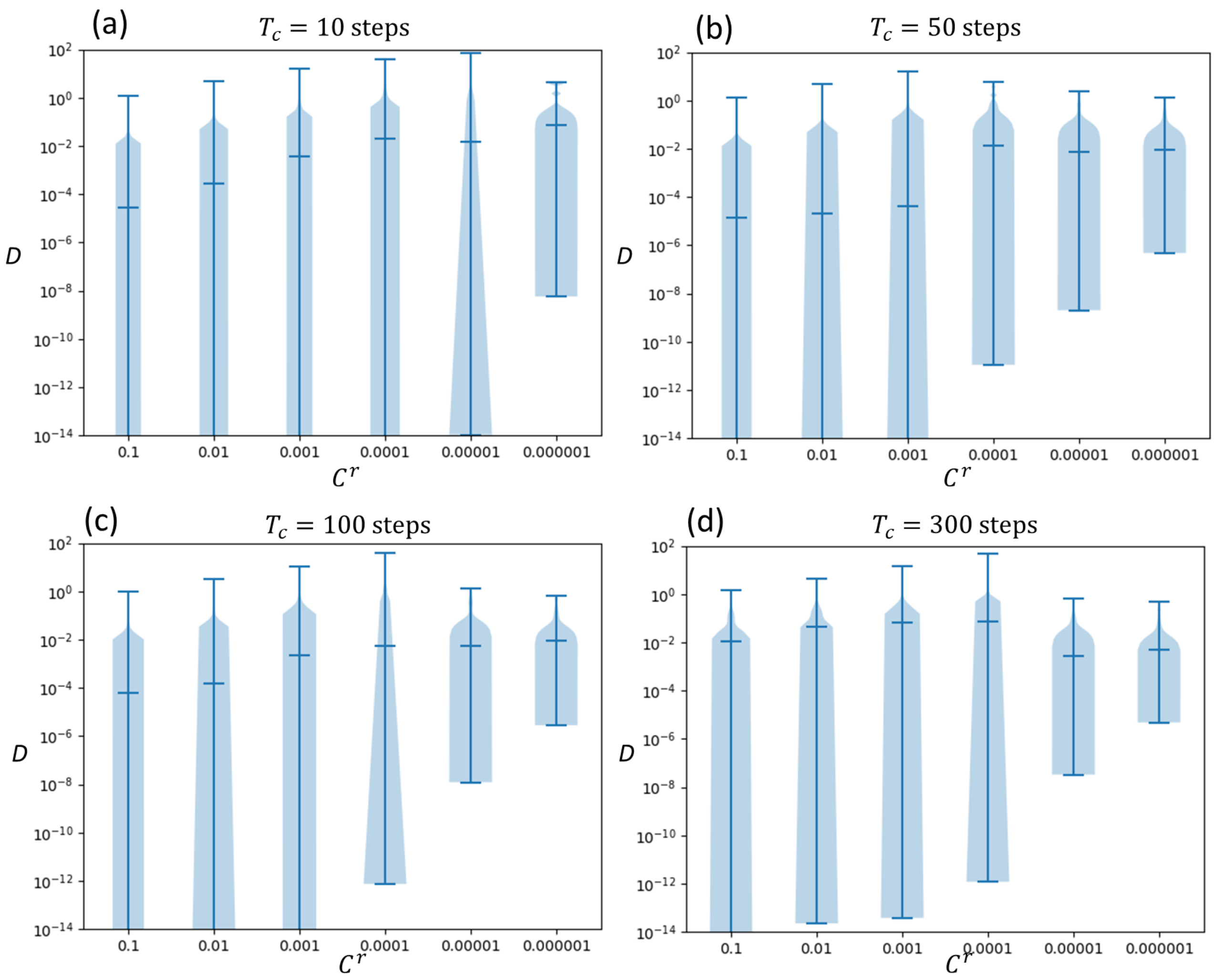


**Figure S2**. Same as Figure 3b but for control horizons set to (a) 10 time steps, (b) 50 time steps, (c) 100 time steps, and (d) 300 time steps. Note that Figure S2d is identical to Figure 3b.

*(1.5) Choice of the control operator You chose to use a logistic function as control operator. This is a nonlinear operator and hence makes the ETKS step more difficult. Beyond this considerations, using 1 as pseudo observation is roughly equivalent (since the logistic function is invertible) to having X = ∞ as observation which seems extreme. From a control perspective, it would make much more sense to have in the cost function a term that would penalise negative values of X (since the objective is to avoid these negative values).*

→ It is generally difficult to deal with binary criteria (i.e. observation) in the framework of data assimilation. I fully agree that the cost function should have a penalize term from a control perspective. However, it is not straightforward to solve the minimization of the cost function in the conventional data assimilation with computationally cheaper way for geoscientific models. So this comment is essentially similar to the comment to (1.3). I have already discussed the limitation of the linearity of the proposed method in the response to the previous comment, so that I have decided not to change the paper responding to this comment.

*(1.6) Choice of the error covariance matrix for the perturbation increment For simplicity, Cu is set to P a, but this choice does not make sense at all. Indeed P a measures the uncertainty in the analysis (which varies over time depending on the current observation errors and the position in the attractor in particular), while Cu measures the cost of applying a given perturbation increment (economic cost). There is absolutely no relationship between both quantities. Without further knowledge on the economic constraints, I would suggest to use a diagonal Cu.*

→ The description of the original paper was indeed unclear and misleading. I believe that using Pa is effective to effectively find the perturbation to achieve the control objectives from the information of the ensemble. Note that Pa includes the information of correlations between state variables while a diagonal Cu does not. Also, it is more straightforward to use Pa since we can directly use the results of EnKF to perform the control step. I did not choose Pa just "for simplicity". I have clarified this point in the revised version of the paper.

"In the proposed method, $\boldsymbol{C}^u$ is set to $\boldsymbol{P}^a$ which is the analysis error covariance matrix."

"By setting $\boldsymbol{C}^u$ to $\boldsymbol{P}^a$, the ensemble estimated by EnKF can directly be used. In addition, it is expected to effectively use the information of correlations between state variables to estimate effective interventions to achieve a control objective."

In this case, I could not explicitly consider the economic cost, which is a major limitation of the proposed method. A simple countermeasure is to scale ensembles to follow the specified variance (i.e. economic cost). Also, it is heuristically effective to scale an estimated control perturbation to a specified norm as Miyoshi and Sun (2022) did. I have clarified this limitation and the potential countermeasures in the revised version of the paper.

"Second, the proposed method cannot consider the economic cost of interventions while the original model predictive control methods can explicitly describe it in $\boldsymbol{C}^u$ in Equation (2). A simple countermeasure is to scale ensembles to follow a specified variance (i.e., cost). Also, it is heuristically effective to scale an estimated control perturbation to a specified norm, which users believe is economically reasonable, as Miyoshi and Sun (2022) did."

*1.3 Discussion of the results*

*(1.7) Overall, I have the impression that the manuscript lacks a proper discussion of the results. In*

*particular, the following points are left unanswered.*

*Objective of the experiment In the end, what is the real objective in the experiment? Is it to ensure X > 0 at all economic cost? Is it to minimise the economic cost to ensure X > 0 all the time or at a fixed percentage of the time? Is it something else? In connection with this question, I would have like to see, for each experiment, the averaged economic cost (as a complement to figure 3b). Furthermore, I think that a baseline method must be included in the experiments must be compared to the proposed method. The last paragraph of the manuscript is not sufficient in that sense.*

→ I fully followed the work done by Miyoshi and Sun (2022). It says "The goal of the control is to stay in a wing of the butterfly attractor without shifting to the other. It is essential that our prediction and control system is blind to the NR and takes only the imperfect observations." It does not have any specific percentage of the time nor economic cost, although it primality pursues the relatively small perturbations. Since some works have been done in this testbed, I would not like to add any new constraints. In the revised version of the paper, I would add the results of the control methods of Miyoshi and Sun (2022) (i.e. original control simulation experiment) and compare the averaged economic cost (i.e. averaged perturbation norm).

*(1.8) Is the experiment successful? For small values of Cr, it seems that the method is indeed able to enforce X > 0. However, there is an important side-effect: X is now restricted to values between 6.5 and 10.5. This must be discussed. In particular, to what extent does this side-effect provoke damage and can it be avoided? In the end, this raises the following question: can we really consider that the experiment is a success?*

→ This adversarial effect can be mitigated by setting the control horizon shorter. If the control horizon was set to longer, the proposed method provides many "too preventive" perturbations, which leads that X is restricted to the smaller boundary. I have added this results in the revised version of the paper. Please see my response to the comment (1.4)

*2 General minor comments*

*(1.9) L 164 '1600 data assimilation cycles'. Why so few cycles? For such a small system, it is easy to make experiments with hundreds of thousands of cycles (e.g. Bocquet 2011).*

→ I have performed the longer cycles (16000 cycles) in the revised version of the paper. It did not change our conclusions.

*(1.10) L 253-254 'as humans cannot (and should not) undertake large-scale alterations of the Earth system' The first part of this sentence is questionable: by changing the climate of the Earth, humans*

*do alter the Earth system at large-scale. Perhaps you should use 'rapid, large-scale alterations' instead. The second part of this sentence 'and should not' is subjective and hence out of subject in a scientific article.*

→ I have fixed this point following the reviewer's instruction.

"In the context of weather modification, it is of paramount importance to minimize the magnitude of perturbations introduced to the Earth system, as humans cannot undertake rapid and large-scale alterations of the Earth system."

*(1.11) Mathematical notation Please use the journal conventions for the mathematical nota- tion. They can be found here: https://www.nonlinear-processes-in-geophysics. net/submission.html#manuscriptcomposition.*

→ I have fixed this point following the reviewer's instructions.

*3 Technical comments and suggestions*

*(1.12) 4D-Var '4-D variational' The form of this acronym is highly unusual. The full name is usually 'four-dimensional variational method' and the associated acronym is '4D-Var'.*

→ I have fixed this point following the reviewer's instruction.

*(1.13) L 46-47 'the duration of an data assimilation window' this is usually called 'the data assimilation window length'.*

→ I have fixed this point following the reviewer's instruction.

*(1.14) L 52-53 'to predict the future behavior of the controlled system' Is 'predict' to right term here? Shouldn't it be 'control'?*

→ Thank you for finding the fatal typo. I have fixed it.

*(1.15) L 58 'where ut is control inputs at time t' Please reformulate.*

→ I could not understand how to reformulate this phrase. If the reviewer clarifies this point in the next round, I'll modify it.

*(1.16) L 108 'EnKF aims to minimize' → 'the EnKF aims to minimize'.*

→ I have fixed this point following the reviewer's instruction.

*(1.17) L 113 I would recommend here to also cite Hunt et al. (2007), who provided a highly simplified and efficient implementation of the ETKF (alongside a localisation technique) compared to the original one.*

→ I have cited Hunt et al. in the revised version of the paper following the reviewer's instruction.

*(1.18) L 114-115 'assuming that H is linear and the ensemble members are Gaussian-distributed' In a sense, this is redundant since the EnKF in general, and the ETKF in particular, make these assumptions (even though they can be relaxed to some extent).*

→ I have simply deleted this part following the reviewer's instructions.

*(1.19) L 130 Please provide a citation for the ETKS.*

→ I added a reference following the reviewer's instructions.

"It is straightforward to recognize this minimization problem as ensemble Kalman smoother (EnKS; e.g., Cosme, 2014)."

"Cosme, E.: Smoothers, Advanced Data Assimilation for Geosciences, Oxford University Press, https://doi.org/10.1093/acprof:oso/9780198723844.003.0004 , 2014."

*(1.20) L 136-150 This could be included in a dedicated algorithm environment.*

→ Although I could not fully understand this comment, I guessed that it is recommended to provide the algorithm table, which is added in the revised version of the paper.

"The proposed EnKF-based control algorithm, called Ensemble Kalman Control (EnKC), is outlined in Algorithm 1."

| **Algorithm 1**. Ensemble Kalman Control |
|---|
| Step 1. Perform an ETKF analysis step using forecast ensemble $\boldsymbol{x}_t^{b(i)}$ and actual observations to get analysis ensemble $\boldsymbol{x}_t^{a(i)}$ |
| Step 2. Compute $\boldsymbol{x}_{t+T_c}^{a(i)} = M(\boldsymbol{x}_t^{a(i)})$. |
| Step 3. Perform an ETKS analysis step using ensemble from extended forecast $\boldsymbol{x}_{t+T_c}^{a(i)}$, the operator $H^c$, and a reference vector $\boldsymbol{r}_{t+T_c}$ as pseudo observations. |
| Step 4. Add the perturbation $x_0 - \overline{x_0^a}$ obtained in Step 3 to the real nature. The same perturbation is also added to all analysis ensemble members at time *t* to accurately estimate the modified nature. |
| Step 5. Compute $\boldsymbol{x}_{t+T}^{b(i)} = M(\boldsymbol{x}_t^{a(i)})$ to get forecast ensemble. Go back to Step 1. |

*(1.21) L 137 'Perform ETKF' → 'Perform an ETKF analysis step'*

→ I have fixed this point following the reviewer's instruction.

*(1.22) L 161 'The data assimilation window was set to 8 timesteps'. Since you are using a filter and not a smoother, it seems weird to use the term 'data assimilation window'. I would rather speak of 'time interval between observations'.*

→ I have modified this sentence following the reviewer's instructions.

"The time interval between observations was set to 8 timesteps."

*(1.23) L 161-162 'Observation error was set to √2, and it was assumed that all variables X, Y , and Z were observed.'. Is this the variance or the standard deviation? In addition, are the observation errors for all three variables independent to each other? Please be accurate.*

→ This is the standard deviation. And they are independent. I have clarified these points in the revised version of the paper.

"Observation error (standard deviation) was set to $\sqrt{2}$, and it was assumed that all variables *X*, *Y*, and *Z* were observed. The observation errors for all three variables are uncorrelated."

*(1.24) L 163 'Observation was generated from the nature run by adding Gaussian noises.' For consistency, I think you should use plural everywhere here ('observations were' and then 'noises') and not singular.*

→ I have fixed this issue following the reviewer's instructions.

*(1.25) L 179-180 Please use scientific notation for the values of Cr (e.g. 10−1, 10−5, etc.).*

→ I would fix this issue following the reviewer's instructions. Note that figures in this rebuttal are not fixed, but I would fix this issue when submitting the revised paper.

*(1.26) Figure 1 3D plots are very hard to read in general and I would advise to avoid them. In the present case, the attractor could be visualised used 2D projections.*

→ I added 2D plots following the reviewer's instructions.

*(1.27) Figure 2 The figure would be easier to read with a grid. Is panel (a) representing the true X or the analysis? In the first case, I would suggest to plot all the 0.01 time steps to avoid ugly spikes For D, you could potentially use only markers (again to avoid ugly spikes). For both panels, I would recommend to remove the white space at the left and right of the panels because it gives a false impression that the experiment starts at cycle 620 and ends at cycle 700.*

→ I have fixed this issue following the reviewer's instructions.

*(1.28) Figure 3 I would recommend to show violin plots instead of box plots and to use an horizontal grid. Also, it would be good to use a scientific notation for Cr.*

→ I have fixed the figure following the reviewer's instructions.

*References*

*Bocquet, Marc (20th Oct. 2011). 'Ensemble Kalman filtering without the intrinsic need for inflation'. In: Nonlinear Processes in Geophysics 18.5, pp. 735–750. doi: 10.5194/npg-18-735-2011.*

*Hunt, Brian R., Eric J. Kostelich and Istvan Szunyogh (June 2007). 'Efficient data assimilation for spatiotemporal chaos: A local ensemble transform Kalman filter'. In: Physica D: Nonlinear Phenomena 230.1-2, pp. 112–126. doi: 10.1016/j.physd. 2006.11.008.*

**Responses to the Anonymous Referee #2**

*(2.1) Indeed, the Author is correct that the origins of data assimilation are in control. There are journals, textbooks, that consider control of complex nonlinear dynamics, stochastic and otherwise. The state of the art in this field is fairly advanced. Current papers consider ML methods, such as adversarial networks, Bayesian learning networks, recurrent networks, etc. Apparently none of the ML or dynamics literature is familiar to the Author. To consider the cite paper by Henderson et al pioneering is an incredible stretch.*

*The Author proposes an algorithmic tweak that I've not seen, but in the end the outcomes are incredibly modest and thus the work is incremental. In the analyses side of things, variance minimizing or L2 minimization is very old and mature, so at the very least I would have expected a complete analysis of the methodology, but all the paper provides is proof by figure.*

*I do not see a way to salvage this paper by revising it.*

→ Thank you very much for your critical comments. I fully agree that the recent ML growth can positively influence the field of control engineering, and it also substantially impacts to geosciences. Although there are so many approaches to analyze and simplify the complex geoscientific dynamics such as recurrent networks, graph neural networks, and adversarial networks, these approaches are still in its infancy considering the application of the extremely large degree of freedom of the real Earth system. Therefore, I believe that my EnKF-approach, although it may look "old and mature" for those who are interested in ML approaches, is useful specifically for weather modification. This point was indeed unclear in the original version of the paper. I have included this point in the revised version of the paper.

"Note also that my proposed method is suitable for controlling systems with extremely large degree of freedom, in which the size of state vectors is the order of $10^4$~$10^9$, such as atmosphere. In geoscience, many previous works recently proposed advanced methods to analyze these large-scale systems based on machine-learning such as recurrent neural networks (e.g., Tomizawa and Sawada, 2021), graph neural networks (e.g., Lam et al. 2023), and generative adversarial networks (e.g., GagneII et al. 2020). Some works used these advanced data-driven approaches to control water-related infrastructures (e.g., Tian et al. 2022). However, the application of these techniques to full-scale complex Earth system models is still in its infancy, so that the proposed EnKF-based method in this paper can be a feasible alternative to find optimal control perturbations for the modification of large-scale systems such as atmosphere.

Gagne, D. J., Christensen, H. M., Subramanian, A., and Monahan, A. H.: Machine Learning for Stochastic Parameterization: Generative Adversarial Networks in the Lorenz '96 Model. Journal of Advances in Modeling Earth Systems 12, e2019MS001896, https://doi.org/10.1029/2019MS001896, 2020.

Lam et al.: Learning skillful medium-range global weather forecasting, 382, 1416-1421, https://doi.org/10.1126/science.adi2336, 2023

Tian, W., Liao, Z., Zhang, Z., Wu, H. and Xin, K.: Flooding and Overflow Mitigation Using Deep Reinforcement Learning Based on Koopman Operator of Urban Drainage Systems. Water Resources Research 58, e2021WR030939, https:// doi.org/10.1029/2021WR030939, 2022.

Tomizawa, F. and Sawada, Y.: Combining ensemble Kalman filter and reservoir computing to predict spatiotemporal chaotic systems from imperfect observations and models, Geosci. Model Dev., 14, 5623–5635, https://doi.org/10.5194/gmd-14-5623-2021, 2021."