Report on the manuscript "Bridging Data Assimilation and Control: Ensemble Model Predictive Control for High-Dimensional Nonlinear Systems" by Kenta Kurosawa , Atsushi Okazaki , Fumitoshi Kawasaki , Shunji Kotsuk

March 18, 2025

The paper investigates several control strategies derived from a new ensemble predictive control (EnMPC) method. Inspired by ensemble methods in data assimilation (DA), the model predictive control problem is reformulated in a way to be solved using an ensemble of model simulations instead of the standard adjoint method. In addition to the treatment of non-linearities, a strong advantage is the breaking of the sequential character of the numerical procedure, which can be crucial for real-time control or operational applications. Based on this idea, all variants of ensemble methods can be adapted to MPC, and the paper compares different classes such as ensemble Kalman filter/smoother, ensemble 4D-Var and particle filters. The comparisons are performed on the Lorentz 63 benchmark system.

I think that the study has a strong potential impact, and the comparison is novel. It is really interesting to make this comparison in order to choose one of the methods for further applications in light of their respective behaviour. However, I believe that the methodological description needs significant improvement in order to fully understand what is performed. In particular, the new EnMPC is formulated as a data assimilation problem, rather than a control problem. I suspect that this has some consequences for the results presented, which may affect their interpretation. In addition, the context and positioning of the study could be significantly improved to be fully convincing. Below are more detailed comments and questions, to be addresses before I can recommend the paper for publication.

General comments

I begin with comments and questions about the technical content.

- In my point of view, there is one main issue that affects the whole paper, and it is the following. In the presentation of EnMPC in section 3, the control problem is formulated as a data assimilation problem. On the one hand this simplifies the adaptation of ensemble methods to MPC, but on the other hand it leads to some inconsistencies. It is more than a simple presentation problem, because at best it requires some clarifications to understand what is really being done, and at worst it induces some inconsistencies in the results.

  First, the definition of pseudo-observations is confusing. Usually in control (often in robust optimal control) an objective output variable plays this role [see for instance Sipp and Schmid, 2016]. It would be clearer to speak of "objective output" rather than "pseudo-observations" and change the notations accordingly. This is just a matter of presentation.

  The second point is the fact that in the control problem, the variable sought is the control $\boldsymbol{u}(t)$. In equation (16) the cost functional is a function of the initial condition (as in data assimilation). In addition, the penalty term is associated with the initial condition, which is associated with the analysis error covariance matrix. Apparently, instead of penalising the control value, the initial condition anomaly is penalised. Is this just an analogy or is it really the implemented cost functional? Is it assumed that the control is a direct forcing on the variable x_i and $\mathbf{P}^a$ is used as the covariance for the control $\boldsymbol{u}$? There is a hint l.239, which states that the control inputs act only at the initial time, but it is not clear how restrictive this is. As discussed in the results, the definition of the penalty term has a strong impact and explains the better performances compared to conventional MPC.

This requires clarifications. If the control is penalised, this does not correspond to what is written, and justification of the use of $\mathbf{P}^a$ would be welcome. If there is no control penalty, this should be better justified; this would explain the RMSE performance improvement, but I do not understand the lower control values.

- How control ensemble members is sampled? Similarly as the initial conditions in the data assimilation problems? This is not clearly explained, but I think that it will become clearer after answering the first comment.
- The presentation of classical methods implemented in the results is not fully self-containing. For instance it is not told how the MPC problem is classically solved (using adjoint method I guess). The Kalman smoother variant is left to search in the cited references. Similarly, the resampling for particle filters is not explained.
- At first read, it is difficult to understand the interest of considering $T_c < T_p$ in MPC. After reading the EnMPC formalism, we understand that a 1 time step is performed in the results (See l. 239). This could be announced to avoid a pending question. Moreover, it is not clear how much the 1 time step control is required for keeping consistency in the formalism (see first comment).
- Equation (9), $\delta\boldsymbol{x}$ seems to be the ensemble anomaly, while it has been defined as the increment in incremental 4D-Var l.132.
- l.197, the presumed similarity between eq. (13) and the MPC penalty is not restricted to particle filters but all presented DA techniques. After taking into account the first comment, the similarity may be less obvious.
- l.207, It is not clear to what procedure is referred the "adaptive selection of control outputs".
- Equation (17): is the observation error $\mathbf{U}_t^y\boldsymbol{v} - \boldsymbol{d}_t^p$ or $\mathbf{U}_t^y\boldsymbol{v} - \boldsymbol{y}_t$?
- I believe that a factor 2 is missing for the gradient in equation (18).
- l.295: "choice of data assimilation". The DA are an ETKF for all tests, and it is the MPC method which is chosen.
- Interpretations l.413 about the reasons why ensemble-based linear transformations lead to smaller control is not fully clear. I believe that this interpretation should be improved in light of the first comment.

Here are questions and comments concerning the context and positioning of the study.

- In the title, the part "Bridging data assimilation and control" and the related positioning in the text is overstated in my point of view. Indeed, the data assimilation problem is related in the control community to design an estimator. The full information control problem and the full control problem are dual [Zhou et al., 1996, p. 423] and it is not surprising that they have the same structure. Afterward, they have to be coupled. In the present paper, the bridge between DA and control lies in the use of this structure similarity to solve the control problem, and in the coupling section 4.a.
- The word "High dimensional" in the title and the related positioning in the text is also exaggerated. Even if ensemble methods are designed for high-dimensional systems, the methods are applied in the paper to the Lorentz system, whose dimension is 3. Even if it has been designed for

convection cells in the atmosphere, it results from a Galerkin projection on 3 modes. In particular, the success of particle filters is not guaranteed as the dimension increases, since it is known (as mentioned in the introduction) that they are subject to degeneracy for high-dimensional systems.

- The weather control application is very restrictive compared to the potential impact of the present study. It makes feel as an ad-hoc justification to solve a control problem in the geophysical fluid dynamics community.
- An advantage of ensemble methods compared to adjoint method is that the latter requires sequential iterations between direct and adjoint model, while the former can be straightforwardly parallelised, which is very interesting for real time control and operational applications. If the authors agree, I believe that this could be mentioned.
- A better review of ensemble MPC would be welcome. A quick search lead me to the preprint [Yamaguchi and Ravela, 2023] and related conference communications. I believe that EnMPC is very new and a hot topic, with apparently few groups working on it and the review can be almost exhaustive. Moreover, similar names of EnMPC apparently refer to something else and a clear distinction may be welcome. I believe that it would be beneficial to perform a rigourous review.
- Full information control is presented in section 3, since the initial conditions are assumed to be known (up to clarifications of the first remark). The coupling between the data assimilation and the full information control is presented in section 4, but presented as "Experimental setting". This distinction may not be clear for readers not familiar with control theory. I suggest adding some remarks, or even reorganising sections to highlight the coupling, which is a very interesting feature of the paper.
- In the conclusion, the step between the Lorentz 63 system and operational systems is big. There is a range of intermediary steps and I believe that it may not be presented as a simple "extension" (l.544), but a nice mid/long-term perspective.

Typos

- l.75: "control objectives are unclear". To reformulate.
- l.331: space after the semi-colmn.
- l.334: "psuedo" $\mapsto$ "pseudo".
- l/356: "calculated" $\mapsto$ "applied".

References

D. Sipp and P. J. Schmid. Linear closed-loop control of fluid instabilities and noise-induced perturbations: A review of approaches and tools. *Applied Mechanics Reviews*, 68(2):020801, 2016.

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K. Zhou, J. C. Doyle, K. Glover, et al. *Robust and optimal control*, volume 40. Prentice Hall New Jersey, 1996.