Comment on the manuscript:

“Unsupervised neural network for dynamics control under chaotic regime”

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This manuscript introduces the use of an unsupervised machine learning algorithm, the Cellular Neural Network (CeNN), as a data assimilation (DA) strategy for controlling a dynamical system. The study is based on the experiments in Miyoshi and Sun (2022). Using the Lorenz-63 (L-63) framework, the authors compare the control performance of the CeNN and the Ensemble Kalman Filter (EnKF). They find that the CeNN performs better than the EnKF, especially on shorter assimilation windows. The result could provide an interesting perspective of its potential for control of chaotic systems and DA applications.

I find that the method proposed is interesting; however, I have several concerns about the scientific context, experimental design and the presentation and interpretation of the results. Therefore, I recommend major revision. I hope my comments help to improve the quality of this manuscript.

Scientific context and motivation:

The authors provide some context in the increasing use of machine learning (ML) for control systems (and DA), and identify a methodological motivation for the study. But the introduction does not provide of enough scientific context and is not clear in the scientific gap that the manuscript is trying to address. The abstract could benefit from this too.

For example, the authors should consider to discuss why ML-based approaches, particularly the CeNN, provides an advantage over other conventional approaches for system control. They should comment on the limitations of the other existing methodologies. At the present state, it is unclear what is the main challenge in system control that the authors aim to overcome with this method, the motivation behind the choice of the CeNN method and what is the novelty of this approach.

Experimental design and methodology:

The experimental design and evaluation need improvement too. The EnKF implementation, for example, is not sufficiently described. For example, the authors do not explain why the ensemble size is set as N=3 (as it is my understanding). Small ensemble sizes result in low-rank estimation of the background error covariance matrix and can introduce significant sampling errors, degrading the assimilation. This, in my opinion, is important since the EnKF is used as a benchmark of performance for the CeNN. The authors should justify this configuration and further discuss its potential implications while comparing the two methods.

Much of the methodological choices are scattered across different sections. Organizing the information in one section would benefit the clarity of the manuscript.

Finally, it would be useful to clarify if the performance evaluation is done only for the ability of the method to control the chaotic system, or if it considers the DA. The metric or criteria for evaluation should be identified and clearly indicate if its for control evaluation or for the DA (if it is discussed and done).

Presentation of results:

The presentation of the results also need of improvement. The main results (Figures 1-4) could benefit of better quality in the labeling and on the orientation of the axis. That would make the results easier to interpret. In the current form it is not possible to see the information, for example Figure 2. Some of the information on the tables should also be included as figures, or include them as supplementary material.

Discussion and conclusions:

The results, discussion and conclusion should be strengthened to provide a better interpretation of their results. Particularly (but not exclusively), provide a discussion on the mechanisms and characteristics of the methods that may explain the performance differences.

The main finding is that the CeNN performs better "*overall*" (line 213) than the EnKF for shorter assimilation intervals, while their performance becomes similar when the assimilation interval increases. However, as mentioned before, the ensemble size used for the EnKF very limited, which could affect its performance. Therefore, it would be useful for the authors to explain or discuss whether this advantage would remain if the ensemble size were larger. Ideally, the authors should assess the sensitivity of the results to the EnKF ensemble size. This should help to establish if the observed performance difference can be attributed of the CeNN characteristics rather than to the particular EnKF configuration.

Additionally, the results seem to suggest that CeNN performance is also sensitive to the assimilation frequency. The authors further mention in the conclusions that this particular aspect of the results differs from the findings presented in Miyoshi and Sun (2022). Could the authors elaborate on this apparent dependence and discuss the possible reasons for the differences between the two studies?

Finally, the discussion mentions that "… *the CeNN approach offers more options to be included in the dynamics control*" (line 218). However, the authors do not elaborate on what are these options or provide examples on how they can be implemented. This point deserves attention, since it can be important for applicability beyond the L-63 framework.

References :

Miyoshi, T. and Sun, Q.: Control simulation experiment with Lorenz's butterfly attractor, Nonlin. Processes Geophys., 29, 133–139, https://doi.org/10.5194/npg-29-133-2022, 2022.