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).

1. 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 ?

2. 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).

3. L. 302, *Figure 3.a → Figure 4.a*
4. L. 303, *Figure 3.b → Figure 4.b*
The same correction is to made later in the paper. Please check.

5. 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*.) ?

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.