I regret I cannot recommend acceptance of the paper, even after a possible major revision. This is so because I consider that, in spite of the fundamental interest of the questions considered by the authors, the scientific content of the paper is not of sufficient novelty or originality to warrant publication.

The authors write that they have *demonstrated the possibility of controlling the real atmosphere by solving inverse problems and adding small perturbations to atmospheric states* (last sentence of abstract). They have not shown how those perturbations could in practice be added to atmospheric states. They consider in their numerical experiments perturbations of the product $\rho\theta$ of the density and the potential temperature, and of the specific humidity q_v, but they do not explain how these quantities could be perturbed in practice.

The authors are fully aware of the problem, and write (p. 2, l. 5) that the first of three remaining difficulties they have identified is that *there is no practical intervention technique for quantitative weather control at the present day*. They nevertheless write (same page, first sentence of third paragraph) *This paper overcomes the above three difficulties with a suitable problem setting*. There is no justification for that, at least certainly not as concerns the first of those difficulties.

There are similar statements at other places in the paper. Any claim by the authors that they have shown how to perturb the atmosphere for controlling weather is unfounded. The paper is only a numerical study of the sensitivity to initial conditions of the output of a Numerical Weather Prediction (NWP) model. If methods are found some day to actually perturb the atmosphere for controlling weather, this type of sensitivity study will of course be absolutely necessary. But I do not think anything more can be said at this stage.

But even as a pure numerical sensitivity study, the present paper is not in my opinion of sufficient scientific novelty or interest for publication. The authors write (p. 2, ll. -6-5) *To the best of the authors' knowledge, this is the first method to compute control inputs that achieve quantitative specifications for weather control, all based on an NWP model*. That may be true as concerns specifically weather control, but controlling inputs of NWP models in order to achieve quantitative specifications for outputs has been done for a long time. I mention two examples.

- *Singular modes* are defined as initial perturbations that lead to the largest forecast error over a given forecast period. They are used, among other purposes, for defining the initial perturbations to be used in ensemble prediction (Diaconescu and Laprise, 2012). In a linear setting, these modes are independent of their amplitude. They have been extended, by M. Mu and colleagues, under the name of Conditional Nonlinear Optimal Perturbations (CNOP), to nonlinear situations, in which a constraint on the initial amplitude of the perturbations is then included (Mu *et al.*, 2010).

- *Variational assimilation* is intended at defining the model state at a given time that leads to the model solution that fits most closely a given set of later observations. Variational assimilation has been used in operational NWP for a long time (see *e.g.,* Rabier *et al.*, 2000). Actually, determining initial conditions that fit later observations or that lead to specific conditions in the ensuing forecast is essentially the same problem.

I do not think that the possible purpose of weather control should lead to specific methods for the definition of initial perturbations. In any case, nothing in the present paper, as concerns the numerical method to be used, seems specific to weather control and any other method could *a priori* be used as well.

But even concerning the purely numerical aspects, the present paper does not in my opinion bring anything that is instructive enough for publication. The authors use a strictly linear method for identifying the required initial perturbations (Eq. 7). From what I understand, if the initial perturbations achieved what they are meant to achieve, the deviations in, *e.g.*, Figure 2d should be zero. They are not (and are large enough, in comparison with the results shown on Figure 2c, not to be due to round-off errors). The authors do not discuss that aspect. An obvious explanation could be that the basic model is nonlinear, and that the linear approximation of Eq. 7 is not sufficient to determine the optimal intended initial perturbations.

I mention that, in the two examples given above, the numerical optimization can be (and is often) exactly performed (at least to computer accuracy) with a full nonlinear model. That is made possible by the use of the *adjoint method*, which allows to compute economically the gradient of one scalar output of the model with respect to all inputs (Courtier and Rabier, 1997). The gradient is introduced an iterative minimization process, which can actually be described as an example of what the authors mention in their conclusion as *successive linearization and optimization of perturbations*. My point is that this approach has been implemented for a long time.

Sensitivity of future weather to initial conditions is a fundamental and very important question, and I do not want to look dismissive of the work of the authors. I encourage them in their interest in that question. But the present paper is not sufficiently instructive for publication. One point in the paper that has arisen particular interest for me is the comparison that the authors make between the l_1 and l_2 norms. The l_2 norm is typically used in this kind of optimization problem, for the simple reason that quadratic functions are the easiest to minimize. Further study of the specific qualities and appropriateness of various norms, both from the physical and numerical points of view, may be instructive, but is not sufficiently developed in the present case.

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