I apologize to the authors for this delayed response to their own response to the two referees' comments.

As Editor, I do not recommend to the authors to submit a revised version of their paper, but rather to withdraw it and to submit, as explained below, a new paper.

The authors have responded in detail to the referees' comments, and make a number of specific proposals for modifying their paper along those comments. They stress what they consider as potential advantages of convex optimization (*convex optimization provides greater flexibility in handling a wide range of cost functions and constraints, including not only quadratic functions but also absolute values, maximum values, and general l_p norms*).

From the referees' comments, and from my own evaluation, the main weakness of the paper is that the basic linearized optimization problems in eqs (4) or (7) do not solve the corresponding, physically significant, nonlinear problems. The authors are fully aware of that, and write *There is a mismatch between the linear model for optimization and the highly nonlinear NWP model for validation*. Because the linearized optimization produces modifications in the model fields that have some good consistency with the intended purpose, the authors write *The linearity assumption was validated in the nonlinear simulation*.

That is not in my mind sufficiently original or instructive for warranting publication of the paper, which does not contain really original results, either in terms of atmospheric dynamics or numerical algorithmics. What the authors actually propose seems to be simply inclusion in their paper of additional comments and discussion of specific points. But they do not propose inclusion of new fully nonlinear results, which would really validate their approach. A fundamental reassessment of the paper, with in particular proper solution of the basic nonlinear optimization problems, is necessary. That will of course require implementation of appropriate algorithms for solving these problems, and will go beyond a revision of the paper. Nonlinear optimization problems have already been solved with fully nonlinear numerical models of the atmospheric flow (see in particular references by Reviewer 2).

In addition to performing fully nonlinear experiments, I also suggest to the authors to put less emphasis on weather control (in particular in the title of their paper). It is true that sensitivity studies of the type considered by the authors can be useful (actually necessary) in the perspective of weather control. But weather control, at least of the type considered by the author, is at this stage a very remote possibility. Numerical experiments of the kind considered by the authors, beyond their theoretical interest, can have practical usefulness, for instance for numerical optimization, for predictability, for the definition of observing systems, or for assimilation of observations (assimilation is actually a form of control problem, intended at leading a numerical model in a state of compatibility with known observations).

A new paper could of course include a detailed presentation and discussion, with possibly numerical results, of what the authors consider as being the specific advantages of convex optimization.

I thank the authors for having thought of *Nonlinear Processes in Geophysics* for submitting their paper, and encourage them to submit a new paper along the lines suggested above.