Nonlinear Processes
in Geophysics

Discussions

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Interactive comment on "A Novel Approach for Solving CNOP and its Application in Identifying Sensitive Regions of Tropical Cyclone Adaptive Observations" *by* Linlin Zhang et al.

Anonymous Referee #2

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General comments: The authors study the computation of perturbations with optimal non-linear growth over forecast time determined with the mesoscale meteorological forecast model MM5 in the context of tropical cyclone forecast. The underlying computational problem is a very high dimensional global minimisation problem. In order to find viable alternatives for using an adjoint, the authors test a combination of two other search algorithms, "particle swarm optimisation" and "wolf search" on a reduced dimension state space with 50 dimension and test their performance against a reference method called "the ADJ method". However, it does not become clear, whether this reference method is used to solve the same problem, which should give identical results provided that all methods find the global minimum. Instead, the optimal perturbations

found are quite different. Also, solving a 50-dimensional problem with 200 (resp. 420; see swarm size from table 1) model integrations at each solver step in 20 to 30 steps (Fig. 2) does not look like a dramatic improvement over conventional methods, and no direct comparison to those is offered. "The ADJ method" is used as a benchmark, but it is ambiguously defined and no attempts on parallelisation are made, not even in the case of multiple starting points, which supposedly can be parallelised trivially. Also the article leaves the impression that "the ADJ method" is run on the full state space, rather than the 50 dimensional PC space. In summary, the comparisons in terms of computational performance are not convincing.

The experiments with the reduced amplitude CNOPs are hard to follow. I had difficulties to understand section 4.3., which is the motivation for the verification and forecast experiments.

Specific comments: In the presentation of the resulting CNOPs, the surface pressure patterns are neither shown nor discussed. No information on the vertical structure of the CNOPs is given. Moisture, an important energy source for tropical cyclones, is not included in the state vector and no justification for this omission is given. The authors do not address the the role of the fixed PC space dimension (and basis?) when comparing patterns at different resolutions. No information on how the excitation of numerical modes is avoided, both in the computation of the CNOPs and when making perturbed forecasts.

Technical comments: Many formulations in the abstract and the article are confusing on a language level, to name only a few: "...suggest that the use of an ocean coupled model needs to be conscious,..." (page 2, line 13), "the mutual affection of binary typhoons" (page 2 line 14), "[wolf search] ... takes long consuming time." (page 4, line 6). Language editing is encouraged.

page 5, formula 6: What is the update for u_i if neither of the two conditions is satisfied?

page 8, formula 10: Is this using the same energy norm as formula 10? If not, how are

the different variables combined?

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