

## ***Interactive comment on “A 4D-Var inversion system based on the icosahedral grid model (NICAM-TM 4D-Var v1.0): 2. Optimization scheme and identical twin experiment of atmospheric CO<sub>2</sub> inversion” by Yosuke Niwa et al.***

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This paper further develops and tests the inverse system introduced in part I of the paper. It contains interesting information, of which the POpULar system is particularly interesting. Unfortunately, the ability to provide complex correlation structures is not tested in the manuscript. Moreover, the study uses only artificial observations to test the system, which is OK for the current paper.

I have a few major/minor remarks, which need to be addressed before the paper can move on the GMD.

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First, I have problems with the conclusion that the “non-exact” adjoint outperforms the linear model that uses the exact adjoint to calculate the derivative of the cost function. I think the result may be driven by the fact that the pseudo-observations have been produced by the model version with the flux-limiter activated. Thus it comes as no surprise that the non-exact flux-limited version of the model finds the solution more easy (the authors use a fixed number of iterations of 60). The problem with a non-exact adjoint appear when you want to estimate error-reductions, for which you to the best of my knowledge need an exact derivative (to approximate the Hessian). Anyhow, it would be interesting to include the views of the authors on the quantification of the error reduction and posterior co-variance calculation. As a note, I am not claiming that the non-exact adjoint produces erroneous results, but I am afraid the authors want to propagate the use of non-exact adjoint, which comes with disadvantages also. It would be instructive to repeat some experiments with pseudo-observations calculated with the flux-limiter turned off.

The language needs refinement, preferably by a native speaker. Now, at many places, the optimal wording is not used to correctly phrase what the authors (in my opinion) want to say. I made many suggestions in the attached pdf. Especially sections 2.2.1 and 2.2.3 need to be clarified, since the strategy with respect to the biomass burning emissions remains unclear to me. Why is GFED left out of the true fluxes?

What also should be clarified is the fact to the error setting of B is such that you assign larger errors at places where the flux sets differ. This leads to (artificial) good performance, because the system will adjust fluxes where differences are present, and will not adjust sources where no differences between the flux sets are present. I guess your statement: “Nevertheless, not including the biomass burning(s) in xtrue would help us to elucidate the ability of the inversion system to find large CO<sub>2</sub> flux anomalies” gives a clue, but the description is simply messy, and needs improvement. To be more convincing, it would be good to repeat the inversion with a “proportional to emission” error, as commonly done in inversions, or using the “French” approach to

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set the error based on the emissions in neighbouring grid cells.

Please also note the supplement to this comment:

<http://www.geosci-model-dev-discuss.net/gmd-2016-232/gmd-2016-232-RC1-supplement.pdf>

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