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Geoscientific Model Development Discussions

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_2 inversion" *by* Yosuke Niwa et al.

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RC

This paper describes the application of the POpULar minimization scheme within the 4dVAR analysis of CO2 fluxes. The topic is worthy of publication as a number of modelling issues are raised, however the use of an identical twin experiment with synthetic observations has its limitations. While such an approach is appropriate for testing a system, care needs to be taken regarding the strength of the conclusions. There are a

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number of corrections and clarifications that should be dealt with prior to final acceptance. Some revision is warranted prior to final acceptance.

AC

We really appreciate the reviewer for taking her/his time to review our paper and giving us many valuable comments and suggestions. Described below are our replies to the reviewer's comments with page and line numbers of the attached supplementary manuscript, in which the changes from the original manuscript are colored in blue and red.

RC

Specific issues: 1. A number of choices were made in the construction of the identical twin experiment that could unfairly favour one of the configurations over the other systems tested. These choices include: - Using the same initial concentrations in the true and experimental run - Having the true and prior land fluxes represent emissions with the same sources. The effect of these choices on the conclusions should be assessed.

AC

Differently from NWP, the initial condition (concentrations) does not strongly affect observed spatiotemporal variations of CO_2 concentrations, but the boundary condition (surface fluxes) dominantly affect those. This is because it has much longer assimilation window, i.e., 1-year-long, while NWP usually has a several-hours- to 1-day-long assimilation window. Therefore, we think the same initial concentrations in the true and experimental run is reasonable. Nevertheless, in an actual case, initial concentrations may have some errors. To cope with it, we will disregard optimized fluxes in early part of an assimilation window that have "absorbed" the errors of the initial concentrations. A corresponding description as below has been added in the revised manuscript.

"Optimizing only the surface fluxes is reasonable since these fluxes are the main driver of the atmospheric CO_2 concentration variability. In "real" inversion analyses, there are errors in initial concentrations, but we can reduce their impact by disregarding the optimized fluxes during the early part of an assimilation window." **[Page 9, lines 4-6]**

Although the true and prior fluxes have the same sources (fossil fuel emission, terrestrial biosphere, air-sea exchange, and biomass burning emission), the data themselves have very different spatiotemporal patterns as shown in Figs. 7-10 (previously 6-9). Such deviations of the prior flux from the true flux are also likely in an actual case due to lack of our knowledge about CO_2 fluxes. As Reviewer 1 suggested, we have thoroughly modified the flux descriptions of Section 2.2.1. Furthermore, we have added a description in **Page 10 lines 3-6** ("General spatiotemporal variations . . . show distinct differences") that corresponds to the above statement.

In summary, these experimental settings chosen in this study will not be serious concerns in an actual inversion analysis.

RC

2. Initial concentrations are held fixed, and only fluxes are adjusted. This choice needs to be justified, especially if this system is to be cycled in an assimilation-forecast mode.

AC

As described above, the initial concentrations do not affect CO_2 concentration variations so much, which justifies the same initial concentrations. Furthermore, at this stage, we aim only at a surface flux inversion, not at an assimilation-forecast mode like NWP.

RC

3. The background error variances are derived using the same truth as the experiment, Eq. 24, but in general the variances would also contain error, and probably significant errors since the method as described has no method of updating these.

AC

We think that the prior (background) error variances we set in this experiment are too optimistic. However, we wanted to simply validate the optimization scheme as a first step and also to clarify the adjoint effects, which might depend on the setting of the prior error. We think the "true" prior error could provide a sort of benchmark result. We touched on the weakness of the prior error in the first manuscript **[Page 11, Lines 6-10]**, but we have added more in the revised manuscript to draw readers' attention **[Page 1, lines 6-7; Page 4, lines 13-15; Page 10, Lines 23-27; Page 11, lines 23-25; Page 18, lines 2-3]**. As stated earlier, we do not consider an assimilation-forecast cycle, therefore, there is no need so far for a method of updating the errors.

RC

4. To better understand Fig 3 (Global RMSEplots vs. iteration) there should also be lines showing, cost function and its gradient. This is important for a number of reasons: - All iterative schemes are run for 60 iterations – the importance of this is not established. Is the iterative process for each configuration close to convergence. - Each configuration will have a different cost function value, so it is important to see how the different configurations compare in terms of cost functions. - It would also be useful to check the standard Jmin diagnostic, given that the covariances are reasonably well known (by construction in an identical twin experiment)

AC

We appreciate the reviewer's suggestion and agree the benefits of showing the cost functions. Accordingly, we have added the figure that shows the cost function vs. iteration for each experiment (Fig. 3). The added figure shows that, after 60 iterations, the off-diagonal cases are sufficiently converged and the diagonal cases are close to convergence **[Page 13, lines 9-11]**. Furthermore, we have added the discussion about the Jmin diagnostic, which is also suggested by Reviewer 1 **[Page 14, line 34 – Page 15, line 10]**.

RC

Figures: When comparing fields, it is much more instructive to compare show differences rather than full fields. Increments/differences are shown in Figure 4, but in 5, 6 7 only the full fields are shown, so it is quite possible to miss some important differences.

AC

According to the reviewer's suggestion, we have changed Figure 6 (previously, Figure 5) from the full fields to the difference fields for the prior and posterior fluxes. For Figure 7, we made a figure that shows the difference fields as below, but found that the full fields look better. It is because the differences between the prior and true fluxes have magnitudes comparable to those of the true flux, which may mislead readers. The same goes for Fig. 8.

RC

Figures 8 9: Not sure why the lines need to be so thick. I would find it clearer if thinner lines were used – and easier to actually read values.

AC

As suggested, we modified the figures with thinner lines.

RC

Minor corrections: Page 1, line 11: ". . .difficult decomposition of a matrix. . .". The standard decompositions used in variational assimilation are generally well-established not especially "difficult" in comparison with the development of other models described in this paper. The constraints on matrix structure imposed by the decompositions can be a problem – and so the key point is in the following sentence regarding the freedom *to use a more general error covariance structure. Unfortunately the paper uses a simple Gaussian correlation function, with one length scale for land and another for ocean, and no cross-correlation so the advantage is not investigated - My underlying concern is that having the freedom to more generally specify error covariances is still some way from constructing a more general matrix and showing that it adds value – both in terms of accuracy and computational cost. There are a number of further potential problems that are not explored, and these may interact with the comparisons of the two adjoints explored in this paper*

AC

We have deleted "difficult". **[Page 1, line 14]**

We agree that we have just introduced the optimization scheme and its advantage has not been explored. However, exploring the advantage of the optimization scheme is not a trivial task because, as the reviewer mentioned, a complicated error design may need a huge computational cost. The study for designing the flux error covariance is still in progress; therefore, we leave it for another paper. Corresponding descriptions are added in **Page 11, lines 23-25, and Page 17, lines 27-30**.

RC

Page 2, line 17: While 4D-Var was implemented in operational NWP earlier than most other geophysical models, the theoretical basis goes back further, and so to say the 4D-Var was originally developed for NWP is incorrect. There were also 4D-Var ocean systems developed at a similar time.

AC

We appreciate the reviewer's thoughtful comments. Accordingly, we have modified the text as follows: "The 4D-Var method has been employed in numerical weather prediction (NWP), in which a weather model's initial state of the atmosphere is optimized, using observations, to improve weather prediction in many leading operational centers (e.g. Dee et al., 2011; Kobayashi et al., 2015). In NWP, the 4D-Var method is performed over successive time windows. The 4D-Var method has also been used in oceanography for a long time (e.g. Smedstad and O'Brien, 1991; Stammer et al., 2002; Usui et al., 2015)." **[Page 2, lines 23-28]**

RC

Page2, line 22: "..and need to be resolved" is redundant

AC

As suggested, we deleted it. **[Page 2, line 32]**

RC

Page 3, line 2 3: It should be pointed out that nearly all global NWP systems also employ some level of ensemble covariances. These are considered important to: - Capture some variation in time and space of background error variances and correlation - Provide analysis error estimates. Also while the error covariances are diagonal, *these are in transformed variable space, which allows for some non-linear balances. The error covariance matrix of untransformed (physical) variables has off-diagonal elements. These latter variables are more akin to the fluxes being examined here.*

AC

We now understand that this discussion was shallow. For simplicity, we have deleted this discussion, not pointing out the details of the NWP situation. **[Page 3, lines 14-15]**

RC

Page 3, line 5: There are problems with using a spectral decomposition for surface fluxes, but not allowing variables to be mutually correlated when they are close by is not one of them. Indeed the error correlation used here can be handled spectrally. The points that follow are valid, however the construction of useful, general correlations can add computational cost that is not explored here – and the effect of non-linear vs linear calculations could potentially be different.

AC

We appreciate the reviewer's valuable comment. The discussion about the error correlation is elaborated as follows. "In fact, Chevallier et al. (2012) found that the biosphere model they used to construct the prior CO_2 flux has positive error correlations within certain spatiotemporal ranges, suggesting that the error correlations introduced in the prior error covariance matrix could propagate observational information to estimated flux values more effectively. Furthermore, introducing the flux error correlations reduces the degree of freedom and this may provide positive influences, e.g. reduction of noises, in a flux estimation, especially when observational networks are sparse." **[Page 3, lines 15-22]**

As already stated, we agree that the construction of useful, general correlations may add an extra computational cost. We think sophisticating the design of the prior error covariance is not a trivial task and we leave it for a future study. This statement is added as

"As described above, we use idealized variances and simply define covariances to construct the prior error covariance matrix. In fact, the configuration of the prior error covariance significantly affects the performance of the inversion and a careful evaluation needs to be conducted, which we leave for a future study." **[Page 11, lines 23-25]**

Also, the computational discussion is also inserted in Conclusion of the revised manuscript as follows.

"However, it will not be a trivial task to appropriately design the prior error covariance in terms of accuracy and computational cost. A complicated flux error structure may require a considerable computational resource, even though POpULar requires only the matrix-vector multiplication with the prior error matrix. A further research effort in this direction is required." **[Page 17, lines 27-30]**

RC

Page 3, line 27: Guo and Sandu found that the results varied between tests using ideal- ized data and real data. This should be a flag that care must be taken with interpreting results from the identical twin experiments conducted here.

AC

We have detailed the result of Gou and Sandu (2011) a little bit more as

“However, when real observations were used in assimilation, they found that both the adjoint models showed similar performance, but the discrete adjoint performed slightly better than the continuous adjoint.”. **[Page 4, lines 7-9]**

Furthermore, a discussion of our result against their result is also added in Conclusion as

“The similar optimization performance of the continuous and discrete adjoint models achieved in this study is consistent with the assimilation result obtained by Gou and Sandu (2011) with real ozone observations.”. **[Page 17, lines 14-15]**

RC

Page 3, line 31. It is not obvious to me where there was significant investigation into how the observation network could be better exploited – apart from the suggestion that the current network can provide sub-continental scale information, which seems a rather obvious comment to me.

AC

Accordingly, we have modified the text as, “. . .we also investigate how the current observation network could optimally constrain surface flux estimates.” **[Page 4, lines 16-17]**

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Page 4, line 11: ". . .x represents the increment. . ."

AC

Accordingly, we have corrected. **[Page 4, line 28]**

RC

Page 11, line 19: typo: quadratic

AC

We have corrected. **[Page 13, 29]**

RC

Page 13, line 2: "Upon closer inspection. . ." would be better

AC

As recommended, we have changed. **[Page 15, line 27]**

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Other trivial changes

Because the words “off-line”, “on-line”, “non-linear” are changed to “offline”, “online”, “nonlinear”, respectively, in the previous accompanying paper (Niwa et al., 2017) through the publication process, we have changed the text of this manuscript as well.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/gmd-2016-232/gmd-2016-232-AC2-supplement.pdf

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Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-232, 2016.

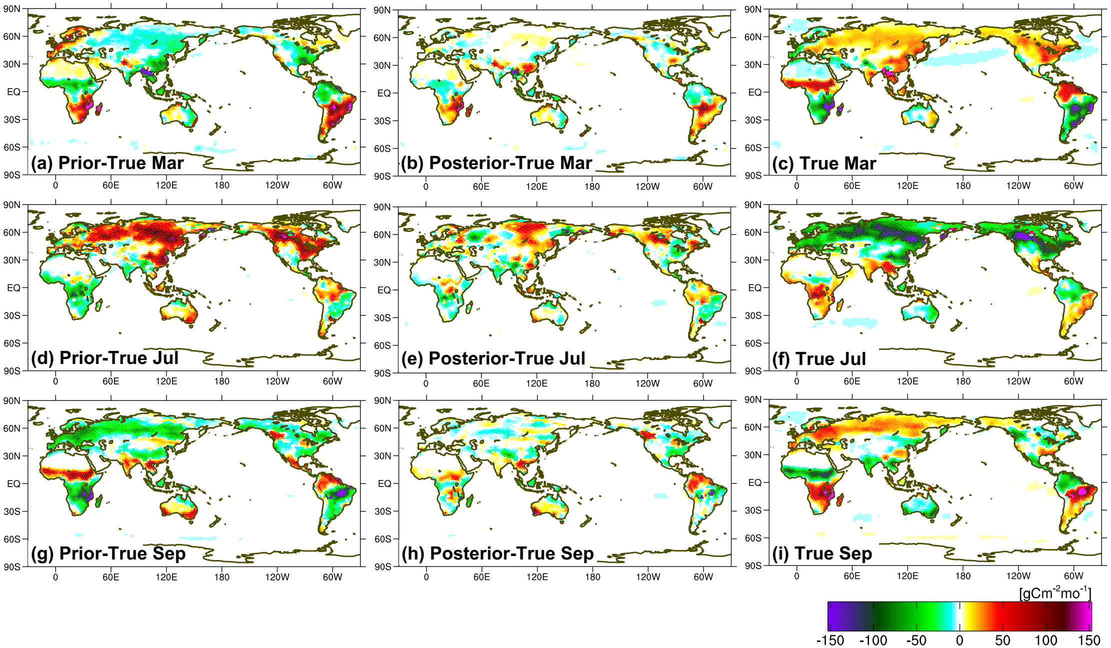

Fig. 1. Same as Figure 6 of the previous manuscript, but showing differences between the prior/posterior and true fluxes.

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