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Interactive comment on "Technical note: A high-resolution inverse modelling technique for estimating surface CO_2 fluxes based on the NIES-TM – FLEXPART coupled transport model and its adjoint" *by* Shamil Maksyutov et al.

Anonymous Referee #1

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I initially suggested rejecting the paper, since I thought it was just a minor evolution of a previous work from the same team (e.g. Belikov et al., 2016) and because of the insufficient quality (and quantity!) of the results presented. The authors have clarified in a reply that the difference with Belikov et al., 2016 was larger than what I had understood. I thank them for this clarification and apologize for the confusion.

Even though the sections 1 to 4 (introduction and methods) are well written, it remains difficult to give good gradings to paper (especially for the "scientific quality" and "presentation quality" criterias) as the results presented are absolutely insufficient to prove

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that the model is working as expected, and more generally to support some of the main claims of the conclusions (p15, l5-7). I am however willing to change my recommendation to major revisions, in the light of the clarifications provided by the authors.

Detailed reply to author's comment

> The review comment states: "The paper is a technical note, reporting on minor improvements in the setup already described in Belikov et al., 2016. The improvements seem to be exclusively technical (some improvements in the memory management,and the use of a different tool to derive the adjoint code, which would fit better in GMD than in ACP). Furthermore there is not demonstration that it achieves any better results of performance compared to that setup or to comparable inverse models."
>
> Author's reply: There is some misunderstanding about developments made since the mentioned paper. It should be noted that in a paper by Belikov et al., (2016), there was no attempt to do the inversion, instead, it focused on development of forward coupled model (at lower resolution of 1 degree), its adjoint, the adjoint accuracy and performance. In this study, (1) the Lagrangian model resolution was increased to 0.1 degree, and necessary prior fluxes were developed; (2) Flux covariance operator was developed specifically to handle the challenges of operation at high spatial resolution; (3) Iterative optimization technique was implemented and multiple (time consuming) inversion trials were performed before achieving reported results.

Ok, noted. But then I would suggest to make this much clearer in the paper. The paragraph starting at line 15 on page 3 is particularly confusing.

The review comment: "In fact, the only results presented are a series of model-data mismatches, which do not demonstrate much, beyond the fact that the model is indeed able to improve the fit to observations (the contrary would be very worrying!). "Author's reply: Still, do demonstrate that the technical development is valid, and the inverse model does work, showing the fit to the observations is desirable.

What is your criteria to say that "the inverse model does work"? If the criteria is just to "improve the fit to the observations", then indeed it works, and you demonstrate it. But the aim of doing an inversion is to find the optimal value for the optimized parameters, in you case the CO2 fluxes (given the information from the prior and from the observations), which you formalize as finding the vector that minimizes J(x) in Equation 1:

1. How can I be sure that the posterior fluxes indeed minimize J(x)? There are certainly vectors that improve the fit to the observations but increase the value of J, which your system could find if it malfunctions). Even if it works properly, how do you know that 45 iterations is enough?

2. And how can I be sure that the value of x that minimizes J is indeed closer to reality than the prior? (if your transport model is strongly biased, or if the uncertainties matrices are not adapted, you can totally end up with posterior fluxes that are worse than the prior).

Even if you can't provide a formal answer to these points, you can make steps towards addressing them! And at the very least show the posterior fluxes and how they differ from the prior!

The review comment: "Finally, I don't think that the setup is adequate for what it aims to achieve (it makes no sense to optimize fluxes at a 0.1°

> resolution with covariance lengths of 500 km)."Author's reply: Using the same resolution in inversion as in transport is achieved in our case with a minor additional computational cost (due to efficient covariance operator).

Could you be more specific? What is the cost of a NIES-TM-FLEXPART inversion, compared to a NIES-TM inversion? And how different are the posterior fluxes (and how better are they)?

> The covariance scale is a tunable parameter, it can be set according to information content available in the observations. Many inverse modeling studies (eg Chevallier et al, 2010) do not assume the current observing network provides enough information to constrain the land biosphere fluxes globally at a higher resolution than 500 km. It is mentioned (Chevallier et al, 2010) that with shorter covariance scales the model may take more iterations to converge. Accordingly, the transport model resolution is often higher here than the effective resolution of the inverse model. The rationale for using higher resolution inversion in comparison to lower resolution, such as using large regions, is to reduce aggregation error (Kaminsky et al., 2001).

I am not questioning the benefit of solving the fluxes at a higher resolution, just the adequacy of that combination of covariance scales and transport/optimization resolution. If your observations limit you to an effective resolution of 500 km, then I think that you don't need such a high resolution transport model. And you had enough observations to require such a high resolution transport (if you have enough obs. to use covariance scales of e.g. 50 km), would your inversions still be feasible (it would require much more iterations).

If you can demonstrate that you get much better results (fluxes, not just observation fits!) with 500km/0.1° than with e.g. 500km/1°, then it could be a strong incentive from

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other groups to implement a similar coupling, so I think that it would be really interesting to do that comparison properly.

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