Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-251-RC1, 2020

Atmospheric Chemistry and Physics Discussions

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

Received and published: 29 June 2020

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. 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!). 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). For these reasons, I unfortunately, cannot recommend that paper for further publication in ACP.

1 Major comments:

- The modeling setup is an evolution of the one used in Belikov et al., 2016. An ACP technical note should summarize "new developments, significant advances, or novel aspects of experimental and theoretical methods and techniques that are relevant for scientific investigations within the scope of the journal" (https://www.atmospheric-chemistry-and-physics.net/about/manuscript_types.html). If the developments introduced represent such a significant advance, I think it should be demonstrated (at least by a comparison with the old setup). The only results that are presented are model-observation mismatches (with assimilated and validation data), compared with those achieved by CarbonTracker. It's a useful diagnostic, but definitely not a result, and not a proof that an inversion performs better than another one (overfitting the observations with unrealistic flux adjustments is possible). If the improvements are limited to performance, then not only this should be more explicitly mentioned, but also I think ACP is not the good journal for this.

- I don't understand the possible interest of optimizing fluxes at a 0.1° resolution when 500 km correlation distances are used. With these correlations the flux adjustments patterns will span several hundreds of contiguous grid cells (500 km is almost 5° at the equator): this is just a wast of resources, the same flux adjustments can be achieved with a lower resolution inversion and proper accounting of the model representation error (or at least that's my intuition. I would be happy to be proven wrong, but nothing in the paper does that). So the whole setup seems to just add a layer of complexity (and potentially of biases), without clear performance or accuracy advantages. I could the need for NIES of an inversion system capable of assimilating satellite data at a high resolution, but this is not what is presented here. There is no guarantee that the inversion would still be feasible with shorter correlation length and a lot more data.

2 Minor comments:

p3, l9: A disadvantage . . . , is addressed . . . aggregated flux regions: there is something missing in that sentence

p3, l 15: thus → this

p6, l26: isn't it dangerous to base the uncertainty on GPP? (what about winter time, when GPP is near zero . . . there would still be uncertainty on the respiration term . . .

p8: If x is the optimized flux (as stated in line 6), then the minimum of the second term of Equation 1 is obtained for $||x|| = 0$ (the flux is minimized, not its distance to its prior). Or "x" is a flux correction, but then it is the first term of the equation that is wrong, Either way, I don't think that it's what the authors meant . . .

p9, l20: isn't it a problem to have all footprints stopping at 0 GMT, regardless of when they started? That means that some footprints will systematically span longer (I guess up to 4 days?) depending on their origin longitude?

p11, l16: What is the "implicit diffusion with directional splitting"? I think it's technical enough to be worth a more detailed explanation. The rest of the paragraph is dedicated to explaining the merits of that diffusion algorithm, so I assume it's an important part of the setup, but if I wanted to reproduce it, I would have no idea how to do it (based on what's written in the paper).

p12, l19: "our report is limited to technical development" ==> I don't think that it's the aim of ACP then . . .

p12, l23: Three-month → Three-months

p14, l23: the spatial resolution is roughly 500 km (i.e. the length of the prescribed correlations), not 0.1°. The system might technically be ready for inversions at 0.1°, but given the way it is setup, I doubt that the optimized fluxes would be any different if the optimization was done at a 1° or 2° resolution. Of course, I would be happy to be proven wrong, but the authors don't even try . . .

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