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Biogeosciences Discussions

Interactive comment on "Variability of North Atlantic CO_2 fluxes for the 2000–2017 period" *by* Zhaohui Chen et al.

Anonymous Referee #2

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The authors have studied the CO_2 annual fluxes in the North Atlantic during an 18-yr period with an atmospheric inverse modelling approach. They show some agreement with other estimates and present a sensitivity study with respect to the prior ocean flux constraint. The topic is obviously of great interest but the actual paper is rather deceiving, with little scientific depth. I am listing here important questions that are fully in the paper scope but that seem to be left open:

- How significant are the presented sensitivity tests for the inversion community? Despite a subsection and an appendix devoted to it, the description of the data assimilation system is unclear on what matters in practice. My interpretation of l. 95 is that the elementary assimilation window of the LETKF is of four weeks, a period which is too short (given mixing time scales in the atmosphere) to al-

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Discussion paper

low a clear distinction between the uncertainty in the prior initial state of atmospheric CO_2 and the uncertainty in the prior surface fluxes, when assimilating atmospheric measurements. The authors should therefore not separate the two. However, Incidentally, in the legend of Eq. A1 in the appendix, we understand that the uncertainty in the initial state of atmospheric CO_2 has been neglected. This rough simplification makes it hard to interpret B, officially the flux covariance matrix, in these terms.

- Similarly, the authors do not discuss spatial correlations in the prior errors, leaving the impression that they have neglected them as well. How credible is this hypothesis, e.g., among the prior ocean flux products tested here?

- How are the ocean flux results presented here affected by the leakage from the land fluxes noted in l. 48? The statement in l. 156 suggests there is none of significance, but without any justification.

Additionally, a number of points of various important need clarification:

- L. 27: the 20% value is rather artificial given the fact that the global ocean uptake is made of both sources and sinks.

- L. 57-8: bad example; the studies mentioned here are not for the same year and therefore should not use the same uncertainty budget for a frozen prior flux distribution anyway, given existing trends in the real fluxes.

- L. 91: this is Appendix A, not A1.

- L. 121: what is the rationale behind the 60% and 120% values? The authors should relate them to their knowledge of the quality of their prior fluxes, while they make it look arbitrary (except if indeed matrix B is just an ensemble of tuning factors and not an error covariance matrix; see above).

- L. 127: what is the value of K? I get the impression that only 3 flux products are used here: no standard deviation can be estimated from just three members.
- L. 140-1: why would the three prior ocean flux distributions have the same uncertainty statistics?
- L. 170: flexibility is not the question. The question is about well modelling the prior uncertainty.
- Table 2: if the numbers behind plus/minus signs for the mean values across studies are standard deviations, how can they have been computed on 6, 3, or even 2 members only?
- L. 339: what is the value of L?

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