

Interactive comment on “Variability of North Atlantic CO₂ fluxes for the 2000–2017 period” by Zhaohui Chen et al.

Anonymous Referee #1

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Summary

The authors present an improved atmospheric inversion data assimilation model (GCL) and apply it to the investigation of mean, variability, and trends of North Atlantic air-sea carbon fluxes. Specifically, the advancements made to the inverse model within involve multiple representations of prior ocean fluxes as well as sensitivity experiments assessing the different priors and related flux uncertainties from three different schemes. Additionally, comparisons are made to previous estimates of North Atlantic carbon fluxes as well as estimates from observation-based pCO₂ products and global ocean models. Overall, I found this manuscript well organized, concise, and novel. I would support its publication but have a few suggestions that I believe that would improve the overall strength of the paper.

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Main comments

I highly suggest including additional observation-based products in your analysis (Figure 3 specifically). You use Takahashi et al 2009 in the uncertainty analysis section but cant utilize it for long term mean/variability because it is of course only a climatology. You include 2 products (one by Landschützer and one by Rödenbeck) but there are more available and I highly suggest including them in the comparison to improve your message. Given that your ensemble of inverse models and of GOBMs are much larger than 2, it is worth making the effort to include more pCO₂ products as well. See Denvil-Sommer et al. 2019, Gregor et al. 2019, Iida et al. 2015, and Zeng et al. 2015 for starters. Additionally, if Landschützer's product is used as the prior for the GCL inverse method, is it fair to use it as an independent comparison? If the data-assimilation method is trying to "fit" or "correct" the GCL inverse model to the pCO₂ from that product then I would not consider it an independent comparison.

Section 2.5 could use more discussion/explanation. To my understanding, while the inverse model itself is not new, this method of specifying prior CO₂ fluxes and using them to create more robust flux uncertainties seems to be a major improvement described in this manuscript. I'd be keen to see more explanation and discussion on that in this section.

Lastly, I think the title could be more descriptive of the actual work you are presenting. Specifically mentioning inverse models or uncertainty or a comparison between approaches.

Minor comments

The end of the introduction could improve from a motivation statement. Why do this work? Who will use this? How will it impact the community and what are the broader impacts? Clearly your improvements on the uncertainty estimates would be beneficial to the community as a whole so make that case more clearly.

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Why is year 2003 selected for sensitivity tests on the prior flux uncertainty? Is three years of spin up sufficient? Is 2003 an anomalous year at all? With the dynamics at play in the North Atlantic basin it is important to consider how the selection of one year of focus can influence your analysis.

It could be clarified that when you move on from Section 3.1 you will only be using the U3 approach to specify uncertainty. Additionally, the same for your selection of Landschützer et al. 2017 as the prior for the GCL model as you transition to analysis in Section 3.2.

Figure 2: I find it very interesting that the CTE is so anomalous in the NA subtropics but the CT model is more anomalous in the subpolar regions. It jumps out at you from this figure and you barely notice anything else. Would be worth further discussion as to why those are so different in their mean, IAV, and decadal variability. What do these other inverse methods use as prior flux inputs?

Figure 3: could be cleaned up and simplified by reducing the y-label axis ticks and tick labels. Additionally, on this figure, if the trends in subplots e and f are not significant, consider making the filling color gray or something else to distinguish. They should be noted on the figure as well as in the table. Currently you only note that one of the GCL trends is significant and one is not but need to do this for all inverse models, pCO₂ products, and GOBMs as well.

Throughout the paper, where you mention Figure 3, please add the subplot letter so that the reader can easily navigate to which subplot you are referencing (e.g. Line 268: “Fig 3e, Table 4”). Also, in the Table 2 caption you could reference “Figure 3 a,b” rather than just “Fig 3”.

Section 3.2.2. should include further discussion and references explaining why the GOBMs have such low IAV as compared to the other products and inverse models.

Line 225: Your first comparison is to Schuster et al. 2013 but that work is looking at a

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very different time period. While it can still be referenced and mentioned, highlighting comparisons that focus on the same decades of analysis is more appropriate.

Your summary statement on beginning on Line 303 could be expanded on. How is it more “robust”? Is it just smaller uncertainties and significant trends? Perhaps tie in reference to Figure 1 to discuss further.

References

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