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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Calibrating satellite-derived carbon fluxes for retrospective and near real-time assimilation systems" *by* Brad Weir et al.

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Reviewer comments in red, responses in black.

This manuscript describes a new system for near real time analysis and forecast of global carbon fluxes. The aim is to allow a fast analysis of the actual state of the global carbon cycle in support of satellite data evaluation, allow for a rapid response to newly observed anomalies, prepare for targeted measurement campaigns, provide a reference for extended assimilation of data, etc. The first results indicate that the performance that is achieved is comparable to state-of-the-art inversions. In my opinion this is a rather sobering outcome, putting the inversion community with their feet

on the ground about what can be achieved. But I wonder also if it is fair given the focus on global or long-term mean fluxes in the performance evaluation. Furthermore, it is unclear whether the presented evaluation addresses the requirements of the system given its objectives. Without a specification of those requirements from the start it is very hard the judge how well the system is supposed to perform. Currently, the implicit assumption seems to be that it shouldn't perform significantly worse than state-of-the-art inversions and global and climatological means, however, without further quantification. The structure of the manuscript is a strange mix of method, results, and discussion. I found myself going backwards and forwards to make sure that I read all the parts necessary to understand what was done. Furthermore, I didn't find any clear conclusions in the conclusion section. From this I conclude that the purpose of the paper is mainly to document the first stage of the NRT data assimilation system, for which a journal like GMD would have been more appropriate.

The authors feel this work is well-suited to ACP since the journal has a long history of publishing important new results about carbon dioxide surface flux inversions. That literature forms the backbone of the findings in our research. As the reviewer notes, that LoFI performs so well compared to a modern flux inversion is a "sobering outcome, putting the inversion community with their feet on the ground about what can be achieved." In particular, this paper shows that adding a simple Northern Extratropical land sink to our a priori, "baseline" fluxes is enough to reproduce most of the skill of a modern flux inversion. We feel that this alone is an important scientific finding that would be outside the scope of a journal like GMD despite its impressive collection of Earth system model development literature.

SPECIFIC COMMENTS

Title: I have difficulty with the word "calibrating" here. The suggestion is made the method calibrates satellite measurements, which is really not what is done. Maybe something like "bias-correcting" would solve this problem.

The authors agree that the terminology is the title is imperfect, but feel it is technically appropriate. The use of "bias-correcting" may suggest to the reader that our correction is constant in time, while in fact it affects the inter-annual variability of the fluxes as well. In light of these considerations, we do not see a strong justification for changing the title, especially since keeping the current title allows for greater traceability.

2 The LoFi flux collection: The structure of this section is unclear to me. I had expected three sections, one for the "retrospective mode" on for the "forecasting mode" preceded by everything that is common for these modes. I thought the latter was the baseline, which confusingly enough is not exactly what it turns out to be (see my next point).

The first part of the section describes the components of the flux collection, and the second describes what we have to do differently in NRT. The baseline is simply the LoFI flux collection without the empirical land sink, as is noted in the paper on Line 91.

Line 91: If I understand well the baseline still requires the NOAA MBL CO2 measurements for the ocean flux, which would make it a "retrospective" type analysis. Some explanation is needed of the purpose of the "baseline" other than the notion that it doesn't include the empirical land sink. Initially I was assuming that it would be independent of the NOAA MBL CO2 measurements, which apparently is not the case.

You do understand that correctly. The main purpose of the paper was to evaluate the empirical land sink, so we kept all other products the same in the baseline package. While it would be interesting to test against an alternative baseline that does not use NOAA MBL CO2 measurements in any way, testing the importance of the MBL CO2 measurements, especially for constructing ocean fluxes, was not a primary objective of this paper.

Line 110 "Biofuel" and "Biomass burning": What prevents double counting when combining these components?

This is addressed in the paragraph immediately following the description of the com-

ponents. In the original manuscript it beings on line 149.

Line 124: "Estimates for the two ... Review of World Energy 2016" How is this done, by country, energy sector, or both?

This is done by country and fuel type. ODIAC, like CDIAC, is a fuel-based (e.g., oil, coal, etc.), not sector-based inventory. This is part of the ODIAC product that we use as an input and described in great detail in Oda et al. (2018). We've slightly restructured the two sentences here to make this clearer.

Line 135: "More information is available in Sections 3.1 A3": For the method, not really. Those sections point to evaluation results. In the case of A3 only a single sentence is about the ocean, which could easily have been included in section 3.1.

It could have, but we chose not to. In any case, Section A3 contains Figure A1, which provides more information about the ocean flux, but is not essential to the main text which is focused on the land sink.

Line 139: "This is designed ... spring and summer" What is the design? Is equation 1 only applied to the northern extra-tropics? Per model grid box? What is the spatiotemporal discretization of α? If it is only applied to the northern extra-tropics than what justifies the assumption that the residual land sink in CASA is fine elsewhere? Further details are needed here.

This correction is applied everywhere and α is a constant. We have added some additional text here to make this clear. The adjustment "focuses" itself in the Northern Extratropics (NE) because outside of the Spring and Summer there, the term $\Delta^+ T_m$ will be very close to zero. That CASA should and can be adjusted in the NE in this way to better agree with inversions is the subject of this paper.

Line 148: "about the construction and evaluation of the empirical sink, see Sections 3.2 A3" Fine to put details in A3 (even though I only found information about the ocean and biomass burning there), but evaluation section 3.2 should not deal with the construction of the empirical land sink.

The purpose of the evaluation section is to evaluate our fluxes, notably the empirical land sink. So it's unclear why it shouldn't discuss the construction, when and where it does well, etc.

Line 152: ", yet the sink due to the corn and soybean harvest ..." This suggests that the midwest is the sink accounts for the global emission of short cycle fuels.

It very well could, but this question is beyond the scope of this paper.

Line 176: "in the Niño 3.4 region" Either this region needs to be defined, or a reference should be given where this information can be found.

We've defined the region in the text now.

Line 215: "our ocean exchange fluxes produce a sink that is generally consistent with the inversion ensemble" Whether or not this result is consistent enough depends on the requirements. I would agree that the average sink is in good agreement, however, the trend is not. There is no discussion whether or not this is important, but it seems that a NRT projection or forecast would quickly divert from the uncertainty range.

The trend is discussed in the last sentence of that same paragraph. For the 15-year period considered in this paper, our global ocean flux trends do stay within the window of the inversion ensemble. For longer time periods, using a linear pCO2sw of 1.5 muatm/yr is indeed not appropriate. However, since this growth rate is used in the construction of the pCO2sw climatology of Takahashi et al. (2009), it's unclear that simply choosing an exponential growth rate would fix the problem. Since the focus of this paper is on the land sink, we chose to leave this topic for future investigation.

It's unclear to the authors why one would expect an NRT projection or forecast to diverge quickly from the uncertainty range. Recall that the years 2016 and 2017 are forecasts and they fall within that range.

Section 3.2 The empirical land sink: According to the components specified in section 2 this does not include biomass burning and biofuel. Yet in the description of this section numbers are provided for NBE. This should be made consistent.

See line 155 from the original text:

NBE = NEE (from CASA) + empirical land sink + biomass burning + biofuel

We've made this explicit in the revision.

Line 230: "the sum the sum"

Noted and corrected.

Line 267: "adjustments to HR or NPP are both ... we look for in the empirical sink." Given this conclusion from the preceding discussion, what is it that justifies the current treatment of the empirical land sink?

The preceding discussion emphasizes that CASA, which has a neutral biosphere by design, likely lacks a sink in the Northern Extratropics during the Spring and Summer. This is why the empirical sink has the temperature increase term. We then multiplied that term by HR instead of NPP, recall NEE = HR - NPP, because we expect NPP to be better constrained by the CASA methodology.

Line 294: "This suggests that ... diagnostic fluxes with a similar skill as running a formal inversion system based on MBL data." I do not agree with this for two reasons: 1) the agreement between CT2016 and NOAA MBL sites would have been much better when using its native transport model, 2) Table 1 suggests that only MBE and Ocean fluxes from CT2016 are used. If these are combined with different anthropogenic fluxes then this would add further inconsistency. It would have been fairer to use the CarbonTracker optimized concentrations in this comparison. In particular, because the empirical land sink didn't suffer from the same transport inconsistency.

See line 281 of the original manuscript. Our run uses CT2016 fluxes for all components. We have stated this explicitly in the revision. We have also stated explicitly that the use of a different transport model likely disadvantages CT2016 in the comparison, but that the goal of a flux inversion is not to find surface fluxes that are appropriate for only one model. Although we cannot yet cite it here because it is a work in progress, the OCO-2 Model Intercomparison Project has confirmed that what we see in this initial evaluation holds in a much broader context: LoFI is comparable in skill with modern in situ inversions when evaluated against independent in situ data and TCCON retrievals. Those flux inversions were all run with their native transport models, etc.

3.4 Growth rate forecast: The authors indicate themselves that they could have extended the retrospective mode until 2018. It is not clear why this has not been done. It would have significantly strengthened the evaluation of the skill of the NRT mode (I mean by doing both modes for the 2016 – 2018 time window).

The authors feel that the evaluation is sufficient to support the scientific claims made in the paper.

Line 362: “When necessary, fluxes are downscaled to a higher resolution” It is not clear if this is done, or whether it is only a general possibility. It is also unclear which fluxes would require this step. If it is not used in the current setup then I recommend deleting this part.

It is used to do temporal downscaling from monthly to daily for NEE and spatial downscaling for pCO2sw. We agree that this was unclear and have adjusted the text accordingly.

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