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Interactive comment

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

Anonymous Referee #1

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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

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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.

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.

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).

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.

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

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

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.

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 spatio-temporal 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.

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.

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.

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.

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.

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.

Line 230: ‘the sum the sum’

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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?

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.

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).

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.

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