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Biogeosciences Discussions

Interactive comment on "An empirical spatiotemporal description of the global surface-atmosphere carbon fluxes: opportunities and data limitations" *by* Jakob Zscheischler et al.

Anonymous Referee #1

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Zscheischler et al. pull together a variety of surface to air CO2 flux estimates and ask the question "Do these add up to a globally balanced budget?" This is a worthwhile effort, and the authors are using state of the art estimates. As alluded to in the text, the primary goal of this work is to create a combined data product that can be used as input to future data assimilation efforts.

Unfortunately, there are vital errors the analysis. Large annual cycles of CO2 flux are taken into account for land, but entirely ignored for the ocean. The authors suggest that they are looking at the full "background" of natural CO2 fluxes, but only consider the anthropogenic perturbation in the ocean. To be correct and consistent with the statements of a full accounting for natural background fluxes, Table 1 and Figure 1

should have large fluxes in the ocean that are of the order of the GPP and TER on land.

Furthermore, just assembling these data-based estimates into one with global coverage is not sufficient for publication. The analysis here is too thin, and the findings are poorly presented. Based on how the independent products have been produced, no one should expect that they would add up to a balanced budget – this finding is no surprise. The authors do not do enough to explain what are the major sources of the uncertainty, nor do they do enough to make it clear how they estimate this uncertainty. They need to do a lot more with the products that they have before this manuscript is acceptable for publication.

Major Comments

1. The authors indicate that their goal is to not just address anthropogenic carbon uptake, but to also address the background carbon fluxes (Page 3). Yet their methodology is inconsistent across land vs ocean in this respect. While on land, they separate GPP uptake of CO2 from TER efflux, they completely ignore the comparable cycle in the ocean. See Figure 6.1 of Ciais et al. 2013 (IPCC WG1, Chapter 6) where it is clear that the naturally occurring cycle in the ocean creates an exchange flux of 80 PgC/yr out of the ocean and 78.4 PgC/yr into the ocean; this is comparable in magnitude to the GPP and TER (+- ∼100 PgC/yr), but the authors here simply ignore these ocean fluxes by only presenting their sum. They also appear to ignore these large fluxes in their assessment of uncertainty (though detail on how uncertainty is accounted for is so thin that it is hard for the reviewer to be sure on this point). The full background cycle in the ocean must be included in this analysis must be remedied in this analysis.

2. A coherent explanation for the large imbalance in the final "budget" is never presented, instead the reader is left is a laundry list (e.g. page 16) of possibilities and no clarity of what the authors have identified as the likely most important uncertainties. It seems quite likely that the large GPP and TER fluxes, or the comparable ocean fluxes,

are biased high or low. Their uncertainties are the only ones on the same order as the NCE uncertainty. This issue is even more obvious at the regional scale. This issue should be more directly addressed.

a. One clear place to do this would be on Page 12, where it is stated that in 13% of their runs, the global C source is consistent with the atmospheric growth rate. If this finding is meaningful, these 13% of runs need to be analyzed and presented clearly so that the reader can understand what is different about them. A simple explanation for the uncertainty in the budget could be that GPP is overestimated by 10%, and if all of these 13% of runs have GPP on the low side, then it would be useful to identify such a pattern.

3. There are many inconsistencies in the data products used here. For example, for the ocean flux the parameterization of gas exchange is Wanninkhof (1992) with ERA-interim winds, but for the shelf it is Wanninkhof et al. (2013) with CCMP winds. These differences could make a significant difference to the ultimate fluxes even though based on the same pCO2 database. On the one hand, this reviewer recognizes that these differences are due to choices made by the providers of these previously-published flux products, and cannot be easily changed by these authors. Nevertheless, some evaluation of these effects should be performed. One possibility for such evaluation could be in the overlap regions of the three products that go into the merged Marine flux field.

4. The text is difficult to follow, particularly in the discussion and conclusion sections. These sections read as a list of possibilities, without clarity as to what is really important. The authors need to do more to provide this needed clarity.

Minor comments

Page 2

- Line 15 “limitations.”

- Line 24 Which regions have the large net sink? The authors specify several regions with flux to the atmosphere, then the sum of all is large and negative, presumably due to the tropics. The reader should be able to better understand where this large negative is coming from geographically based on the abstract.

Page 3

- Line 15: "background CO2 fluxes over land and ocean," This analysis only accounts for background fluxes in the land, not in the ocean. Instead this analysis suggests there is no net background ocean flux, only the anthropogenic residual! In contrast to Figure 6.1 of IPCC WG1 (Ciais et al. 2013), this analysis ignores background, natural ocean exchanges. This is a major error that must be remedied.

Page 5

- Line 2 What is the meaning of “resampled”. Is this averaging of all points in a 1x1 grid? If data are at coarser resolution, what is done? Please be more specific. Show that global mean values of the variables considered are conserved by this method.

- Line 25 “For NCE estimates, we randomly combined all datasets, using a single realization of each flux, to generate an estimate of NCE.” What is the meaning of “randomly combined all datasets”? How is this random if all datasets are used? If the “random combination” applies only to the 2 fluxes (ocean and LUC) that have multiple sources according to Table 1, then the result here is an incomplete estimate of uncertainty. More explanation is needed here so that the reader can have confidence in the uncertainty estimate being made.

- Overall it is hard to understand how the uncertainty is propagated. Bits and pieces are mentioned under each of the flux products below, but a coherent picture is not made clear. Perhaps this lack of clarity could be partially remedied with a schematic figure that clarifies how many different realizations of each flux and how the sampling across them is performed.

Page 6

- Line 16 “schused” is a typo

Page 8

- Line 15 If the same FLUXCOM product is being used to separate the gpp and ter, the ocean fluxes out vs in should also be separated (Figure 6.1, Ciais et al. 2013). It is inconsistent to take different approaches with land vs ocean, and skews the reader impression of the magnitude of local fluxes and their uncertainty.

Page 10

- Line 8: A reference for EDGAR is likely warranted.

- Line 24 "Not all inversions were available till 2010." What is done if this is the case?

Page 11

- Line 6: That this imbalance is not real, but an artefact of the uncertainty of the data should be made explicitly clear here; not just left for section 4.

- Line 12: “whereas in fact many errors might be correlated as this is clearly the case for GPP and TER.” The same statement will almost certainly be appropriate in the case of uncertainty in ocean fluxes, once they are appropriately accounted for.

- Line 22: “Due to the small contribution of the oceans, absolute uncertainties are barely discernible." Comment: This will probably be different once out vs in is considered separately.

Page 12

- Line 14 "We use the land cover map of 2005 from the European Space Agency (http://www.esa-landcover-cci.org/) to identify tropical forests (all pixels where broadleaved evergreen trees dominate). " Why use satellite product, when FLUXCOM model is what your estimate is based on. The FLUXCOM land cover product should

be used.

Page 13

- Line 3-11: This section overstates the level of agreement with Ciais et al. (in revision). It suggests that Figure 3 illustrates “good agreement” except for a few regions, but when one looks closely, only 5 land regions agree, including Australia that is basically zero, while 4 do not. Overstatement is exemplified by this sentence “Given that Ciais et al. (revision) rely on an independent method, this demonstrates that a good understanding of net C fluxes exists for non-tropical areas, North America excluded.” This section should be written more carefully to acknowledge that lack of agreement is as common as agreement.

Page 14

- Line 6: NEP should be defined again as its not been defined for many pages.

- Line 23-end: This reads as it may be one hypothesis of many. Or is it a leading one? The reader needs the authors to be more clear.

Page 15

- Line 8: "often not too far off but given that different top-down studies using different atmospheric models provide conflicting information on the adjustments needed to align modelled concentrations with measured ones, this information cannot be used to provide clear uncertainty ranges.” This is not understandable to someone doesn’t work with these models or know this jargon.

- Line 13 “To better constrain C exchange on a monthly basis, however, the seasonal cycles of those fluxes would be necessary." Comment: Awkward phrasing.

- Line 23 “at similar latitudes.”

Page 16

- The degree to which the unaccounted fluxes (list) could be large enough to impact the global “budget” should be discussed. Each of these fluxes should be quantified to the best degree possible so as to put in context with the overall budget. The laundry list approach is not helpful to the reader, particularly when so many of the proposed fluxes are left entirely unquantified, and the authors do not discuss the list at all after it is presented. What is the reader to meant to conclude?

Page 17

- Line 14: Regions such as the North Atlantic (Schuster et al. 2013, Biogeosciences) should also be noted as having large uncertainty at seasonal timescales and beyond.

Section 5 overall:

- This section is also poorly organized. It reads as a listing of issues largely already mentioned prior. It needs to be rewritten to focus on the key findings of this work – What are the take-home messages that the reader should be getting?

Table 2: - should note that negative is from the atmosphere.

- If the label is –GPP then GPP should be 108.29 not -108.29

- A consistent number of significant figures should be used, unless the authors can justify the greater precision of the numbers with 5 significant figures (GPP) as opposed to those with only 2 or 3. This is important because it is uncertainty in GPP that drives most of the NCE uncertainty. The GPP numbers is clearly not actually known to 5 significant figures.

- All numbers should have the same fontsize, or if the different sizes have a meaning, it should be noted

- The full decomposition of the “marine” should be noted in this table, so as to be consistent with Figure 1

- The natural fluxes of the ocean need to be accounted for in a manner comparable to

GPP and TER.

Figure 1

- The units on the 815 are presumably PgC. This should be noted explicitly on the figure or in the caption.

- The ocean should have two arrows, one in and one out. The picture from this figure should be consistent with Figure 6.1 of IPCC in that both the ocean and the land have a large background, natural cycle on top of which the anthropogenic is superimposed.

Figure 2

- The colorbar in panel a is mislabeled as “%”

Figure 3

- The x-axis needs a label

Figure 4

- What are the circles? Presumably outliers? Clarify in caption.

- The regions indicated by each acronym should be noted in the caption.

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