Response to reviewer's comments 2 February 2018

We thank the reviewers for their comments on our manuscript, which has greatly helped to clarify the information presented. Please see the point-by-point response below.

The reviewer's comments are **in blue**, our reply **in black**, and the new text integrated in the manuscript **in red**.

Corinne Le Quéré, on behalf of the author team.

**Reviewer 1: B. Stephens (Referee) stephens@ucar.edu**

This paper represents the 2017 update to what has become an immensely useful annual synthesis of the global carbon budget. It represents a tremendous amount of collective effort by the authors as well as the broader community. As in past years the analyses are carefully done and improvements have been made as a result of community input and the authors' own initiative. Our ability to draw precise conclusions about the flow of carbon remains limited, and the authors do a good job of highlighting these uncertainties and making more clear statements and offering new insights where the results allow.

Several new or recent additions are particularly welcome, but also open for further improvement:

1) Adding a new term for the budget imbalance and making independent estimates of the ocean/land partitioning is in my opinion a great improvement and much more clear way to present the results. However, the great value in the global observational constraints is now underused. I agree with the decision to no longer adjust the ocean fluxes to match the O2/N2, ocean inversion, and CFC-based constraints, but these as well as the pCO2 estimates have a lot to say about the likely causes of the reported imbalance. I encourage the authors in this version to use the global observational estimates to make a more informed statement on the potential cause of the imbalance, and in future updates to make them a more integral part of the report.

We have rephrased a paragraph in the discussion to highlight more clearly that a source of carbon imbalance most certainly comes from the estimates of the two carbon sinks and to emphasize that this is in part due to the observational constraints. We will take on board the suggestion to use the observational constraints as a more integral part of the budget in the future, which we have done in part in a separate analysis (http://www.pnas.org/content/113/46/13104). A closer integration of observations will be key to reduce the imbalance and uncertainty in our analysis in the future.

Revised text: "There are also multiple sources of uncertainties in $S_{LAND}$, mostly related to the understanding and representation of processes as evidenced by the large model spread presented here. In $S_{OCEAN}$, multiple study based on observations have shown variability in ocean $CO_2$ sink larger than estimated by the models presented here, particularly related to representing the effects of variable ocean circulation in models (e.g. DeVries et al. 2017; Landschutzer et al 2015; Keeling and Manning 2014)."

2) The additional information provided on the cumulative budget, as well as the various multi-decadal budgets, including uncertainty estimates, allows for multiple ways of using and comparing to the GCP results, and is thus very helpful. If there is a more recent multi-decadal period (than 1959-2016) that provides more reduced uncertainty estimates, that might be a useful addition.

In principle, we would be happy to include additional periods if there is a specific request but we worry this table is getting quite large and increasingly difficult to interpret and therefore have not made changes. From 1959 the uncertainty is fixes or proportional, so there is no benefit in terms of constraints to using a shorter time period. The data is provided in full and users can calculate their own estimate easily if they wish.

3) I also applaud the addition of atmospheric inverse model results to inform on the latitudinal partitioning of terrestrial and oceanic sinks. However, given the spread in these estimates, robust conclusions will likely require more than 3 models and their comparison to posterior concentrations. I encourage the authors to consider this in future updates as robust latitudinal partitioning would greatly aid in our understanding of potential carbon-climate feedbacks.

Resolving the reasons for the latitudinal differences is indeed one of the ways we can improve the budget in the future. We are working on a more detailed assessment of the differences which we will detail elsewhere. We would like to have this analysis published before we increase the number of atmospheric inverse model results, so we have a better understanding of the additional constraints brought by these products. A second phase of the Regional Carbon Cycle Assessment and Processes (RECCAP) is also under discussion, which would help include additional constraints at the regional level.

One other item to consider for future updates would be the addition of an explicit river flux term in the budget equations and schematics. As it is presently, with the various adjustments to different flux estimates depending on their domain and method, it can be fairly difficult to keep track of what estimates can or can't be compared and what results have or have not already been adjusted.

Thank you we will consider this for future updates. For this year we added in Table 4c a comment on the river correction. The atmospheric inversions are the only product that need to be adjusted for the river flux.

New text in caption of Table 4c: "Atmospheric inversions see the full $CO_2$ fluxes, including the anthropogenic and pre-industrial fluxes. Hence they need to be adjusted for the pre-industrial flux of $CO_2$ from the land to the ocean that is part of the natural carbon cycle before they can be compared with $S_{OCEAN}$ and $S_{LAND}$ from process models."

For this version, I only have one detailed concern that I would like to see addressed, which is with the calculation of uncertainty on the decadal atmospheric growth rate (reported as +/-0.1 GtC/yr). The values used as the basis for this are annual differences between the NOAA MBL estimate and the WDCGG estimate (0.35 ppm), applied as a random error estimate at either end of a decade. However, these differences result because WDCGG uses continental data in there global mean estimate whereas NOAA do not, and they are fairly constant in time (annual 1-sigma 0.1 ppm). Because this is a systematic and stable difference with a well-understood cause, it is not really suitable for treatment as a random error on a trend. A better estimate of uncertainty might come from comparing the decadal trends estimated by NOAA and WDCGG, which for the past 10 decades ending in years 2007-2016 gives a standard deviation of 0.18 ppm. However, this likely overestimates the uncertainty in the NOAA product, as variations in continental fluxes or mixing, or high frequency events at continental sites, will lead to greater variations in the WDCGG estimate that may or may not impact the global representativeness of the NOAA estimate. The metric that really matters in the GCP context is the total atmospheric CO2 mass balance, and I suspect the largest uncertainty in estimating this is the use of surface data, which does not account for tropospheric mixing, or strat-trop exchange, as acknowledged by the authors but not quantitatively estimated. A fairly straightforward way to estimate this component of the uncertainty would be to compare global MBL estimates from model output extracted at observing stations to that from the full 3D model field. I understand that such a calculation is underway using the NOAA CarbonTracker system in co-author Tans,Äô group, so it may be possible to report on the results here. Either way, I suggest not using the 0.35 ppm figure as is currently done and trying to be more explicit about the uncertainty that matters and how one might best estimate it. While this is a minor term in the GCP error budget it actually has potential use as a valuable test of atmospheric inverse models, if the uncertainty is well supported.

Indeed the uncertainty for the decadal trends needs revising in light of this comments. We have identified three sources of uncertainties, first the reproducibility from gas standards, second the changes in network composition, and third the conversion from ppm/yr to GtC/yr. Unfortunately we are able to quantify only the first two sources of errors at the moment, but we are working on the third and hope to have an estimate available for the next issue of the global carbon budget. We have therefore modified the explanatory paragraph in Section 2.3.1 to explain the choice and its limitations, but we have not changed the value of the uncertainty as we do not have a full analysis to provide a quantitative number yet.

The new text reads: "The uncertainty around the atmospheric growth rate is due to three main factors. First, the long-term reproducibility of reference gas standards (around 0.03 ppm for 1σ from the 1980s). Second, the network composition of the Marine Boundary Layer with some sites coming or going, gaps in the time series at each site, etc (Dlugokencky and Tans, 2017). The latter uncertainty was estimated by NOAA/ESRL with a Monte Carlo method by constructing 100 "alternative" networks (around 0.1 ppm; NOAA/ESRL 2017; Masarie, and Tans, 1995). Third, the uncertainty associated with using the average $CO_2$ concentration from a surface network to approximate the true atmospheric average $CO_2$ concentration (mass-weighted, in 3 dimensions) as needed to assess the total atmospheric $CO_2$ burden. In reality these will differ, especially owing to the finite rates of vertical mixing and stratosphere-troposphere exchange. Preliminary estimates suggest this effect would increase the uncertainty, but a full analysis is not yet available. We therefore maintain an uncertainty around the annual growth rate based on the multiple stations data set ranges between 0.11 and 0.72 GtC $yr^{-1}$, with a mean of 0.61 GtC $yr^{-1}$ for 1959-1979 and 0.19 GtC $yr^{-1}$ for 1980-2016, when a larger set of stations were available as provided by Dlugokencky and Tans (2017). We also maintain the uncertainty of the decadal averaged growth rate at ± 0.1 GtC/yr as in our previous assessments based on previous IPCC assessments but recognising further exploration of this uncertainty is required."

I have made a number of minor suggestions as inline comments in the attached pdf that I hope the authors find useful and consider including.

Please see our reply in the pdf document. Where the reply is ticked we have addressed the comment as suggested or equivalent.