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# *Interactive comment on* "Global Carbon Budget 2017" *by* Corinne Le Quéré et al.

**B. Stephens (Referee)**

stephens@ucar.edu

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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.

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.

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.

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.

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' 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.

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 also note the supplement to this comment:
https://www.earth-syst-sci-data-discuss.net/essd-2017-123/essd-2017-123-RC1-



supplement.pdf

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