Response to G. McKinley

(review comments are shown in blue, reply comments in black)

ESSD review 27 January 25, 2013 The authors present a comprehensive synthesis of the global carbon budget, with a focus on updating numbers for 2011 over estimates made by the same group for previous years since 2005. They use the Kaya Identity to estimate 2012 emissions based on already available data for GDP and estimates of the fossil fuel intensity of the economy. In this paper, they detail the datasets used, and the analysis performed to arrive at the global budget. The companion dataset consists of a single excel file with multiple sheets that offers the user direct access to the pieces of the budget. The authors propose also to make "living reviews" to this budget in coming years based on this methodology. Thus, it is critical that this manuscript be exceptionally clear and complete.

Overall, this is an well-written manuscript that summarizes a significant effort by many authors, and should be published. There are several points that do need to be addressed in a revision, as outlined below.
MAJOR COMMENTS

A. The authors state prominently on line 15 of page 1110 that it is only anthropogenic fluxes that they consider, and they do not discuss natural variability revealed in their annual budget results in any detail. However, there is significant variability evident in Figures 3,5,6 that does draw the reader's attention. The fact that this is driven by non-anthropogenic climate variability, and not anthropogenic forcing, should be more clearly noted.

Indeed the carbon budget presented here includes also the effect of climate variability. This has been clarified in the introduction.

B. Overall, the figures have a lot of information, but are hardly discussed. There are some significant features, such as the large dip in Ocean Sink that gets to its lowest point in 2002/2003, that deserves discussion, particularly in the context of the previous literature by many of the same authors (e.g. Canadell et al. 2007, LeQuere et al. 2007). If the authors do not intend to discuss these features, I suggest showing only the mean flux values and reserving these timeseries figures for subsequent publications.

We have included additional discussion in section 3.1 to highlight the variability of each of the components of the carbon budget, and include comments on the prominent features such as the 2002/2003 ocean minimum and the Pinatubo effect on the land sink.

C. The ocean sink calculation is convoluted, and it not clearly presented in the dataset and in the text. This directly determines the terrestrial sink estimate, amplifying its importance.

We have clarified Section 2.4 and the spreadsheet.

C.1. It is not clear why the authors choose to calculate the ocean sink using older models (from LeQuere et al. 2009) for 1959-2008, and then to switch to other models for 2009-2011. Why not simply use the newer four for the whole period and remove potential for discontinuity and the need to show so many results in the dataset? This choice would also simplify the corresponding sheet in the dataset. As highlighted prominently in the first paragraph of the Discussion, reassessments of previous estimates are being used in this budget for other components. This should be done here as well. If for some reason, the authors feel they cannot use only the newer models, they need to

quantify what would be the impacts on the final estimates for this component and the land sink would be.

We have now simplified the computation of the ocean CO_2 sink. We included only the latest version for each model, thus allowing each modeling groups to update their own model as new and better models become available. We also kept one of the models that was available until 2008 only, to increase the model ensemble to five. For all models, we used the anomalies compared to the 1990s only, where observations are available. This simplifies the calculations, and minimizes the risk of major revision depending on single models.

C.2. Starting on line 17 of page 1128, it is stated We do not recompute the 1959–2008 trend to avoid introducing annual changes in the trend that are associated with the model ensemble rather than with real progress in knowledge or in the number of models available. Instead, we compute the average model anomaly compared to the average of 1999–2008, the ten-year period immediately preceding the end of the trend previously estimated and add this to the estimate presented in Le Que ÌA˛ re ÌA˛ et al. (2009). Which models make up the "average model anomaly " for this calculation? It was stated before that four models are used, but perhaps this is now multiple realizations of four models now? This confusion should be addressed by simply using four consistent models throughout.

See above, we simplified the ocean CO_2 sink estimates as suggested.

C.3. The dataset appears to be the pre-correction numbers from the models, as the mean for the 1990s is not 2.2 PgC/yr. The path from the numbers in the dataset to the numbers in the manuscript should be made explicitly clear in the dataset itself. Also, the numbers in the columns 2008 to 2011 do not clearly average to the mean column. How was this calculation done?

We have simplified the calculation and clarified the spreadsheet.

C.4. Regarding the text presentation, when it is discussed outside of section 2.4, the authors state that this estimate is based on four models. In fact, it is based on a previous data-based mean estimate for the 1990s from the IPCC report which scales more than four ocean models for variability. It is important to be clear about the scaling by data throughout.

Thank you, the use of ocean observations has now been made explicit.

D. Fossil Fuel and Cement Uncertainty. A +-5% uncertainty is used throughout, but on page 114, it is stated that this uncertainty is actually growing because of less accurate inventories from Non-Annex B countries. This could be as large as +-10% at the 1-sigma level fro China. Variable uncertainty needs to be incorporated in the budget.

The assessment of time-variable uncertainty is underway, but unfortunately it is not yet available to use here. Without a detailed peer-reviewer paper explaining how the uncertainty evolves with time, we can only refer to the general comments above.

E. CO and CH4 contribution to budget.
E.1. The discussion on page 1126 on natural emissions is unclear because everything is being wrapped into the residual-estimate of the land sink. But in the end results in the statement that "none of the CO and CH4 sources above are included in the budget" . Is this previous discussion needed?

To our knowledge, this is the first time that the CO and CH_4 contribution to the global carbon

budget is estimated. Although we neglect this contribution in the end because it is small, it is still useful to quantify why we do this, and to show that we have thought about it.

E.2. For CH4 from the FF budget, it also states that these are not in the budget, but then goes on to state a "negligible impact". How negligible? Please give percentage. Further, if it can be quantified, why not go ahead and include it since every small bit counts.

We have now quantified the value. We prefer to keep the carbon budget in its simpler version, than to add a lot of small corrections for which we don't really have an uncertainty yet. The case is not strong enough for adding such corrections at this stage.

E.3. Overall, the title of this section should be reworded to "Neglect of CO and CH4 contributions in the CO2 budget". OR the paper could just state upfront that it is only about CO2 and that this entire section could then be left out. I favor the latter choice.

We changed the title to 'Assessing the contribution of anthropogenic CO and CH4 to the global anthropogenic CO2 budget' and now make it clear in the start of the section that we do not include CO and CH4 in the budget, but that this section aims to estimate their negligible contribution.

F. Dataset
F.1. Uncertainty, as it was used in the budget calculations, needs to be reported in the dataset.

thank you, now clarified in the database.

F.2. When more data is included in a sheet than actually goes into the budget, shading to identify "Budget Values" vs. "Comparison Values" would be useful. For example, in the case of the DGVM vs other LUC estimates.

This was clarified in the database.

MINOR COMMENTS
Abstract: In the Abstract and on Line 14 of page 1133, the value is 0.9 +- 0.5 PgC/yr. But in Figure 1 and Table 4 the LUC number here is 1.0 +- 0.5 PgC/yr. From the dataset, it seems the 1.0 number is correct.

The table shows decadal mean value and the abstract the value for 2011. We now mention both in the abstract.

Abstract: ": : :four ocean models, WITH A MEAN 1990 SINK CALIBRATED BY DATA BASED ESITIMATES, .." (see comment C4)
Clarified in the abstract.

Page 1130 and 1133: Sland uncertainty is stated as +-0.8 on pg 1130, and +-0.9 on 1133. Please clarify.
Now clarified.

Figure 2: poor resolution
Resolution will be improved.

Dataset: In the tab "Country Territorial Emissions" , please define what countries are included in each of the "Rest of : : :." columns.
Ok.