Response to M. Heimann

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

The authors integrate global carbon cycle data published elsewhere on fossil fuel emissions, land use change emissions, atmospheric CO2 increase and atmosphere-ocean and atmosphere-land CO2 fluxes into a common framework to estimate the annual components of the global carbon budget over the years 1959-2011. The paper describes the data sources and the methods and procedures to integrate the information into compact global annual numbers of the global carbon budget. It also provides a brief descriptive overview of the main features of the changing global carbon budget over the time period. The dataset is available online on the CDIAC data repository in an excel file and the authors promise to update it on an annual basis. This is clearly a worthwhile data compilation of wide interest for the scientific community and appropriate for publication in ESSD.

Major comments:
(1) Principally, I have some problem with the language in the text. The authors state in the introduction: "The "global carbon budget" presented here refers to the direct and indirect anthropogenic perturbation of CO2 in the atmosphere." This is clearly not correct for the annual budget numbers presented, which are dominated by climate variability (ENSO, Volcanism e.g. Pinatubo, etc.) driven interannual carbon fluxes which existed also preindustrially and are not affected by the anthropogenic perturbation. Even on decadal time scale natural variability contributes to the observed budget changes. This view is exacerbated by using consistently the term "sink" for the net air-sea or air-land CO2 fluxes. While this may be a matter of definition, the term "sink" has the connotation of a removal of CO2 for longer time periods. E.g. in the abstract the authors write "Estimates from four ocean models suggest that the ocean CO2 sink was 2.6 _ 0.5 PgC yr-1 in 2011, implying a global residual terrestrial CO2 sink of 4.1 _ 0.9 PgC yr-1." Clearly, these values for a specific year are not very relevant for an assessment of the anthropogenic perturbation of the global carbon cycle. In this vein, the numbers quoted in the abstract for the ocean and land "sink" should really be the decadal numbers. As in climate change detection and attribution studies, what is needed would be a separation of the observed carbon cycle fluxes into natural and anthropogenic (direct and indirect) components. Such a study, however, would go beyond the description of a data set in ESSD, hence I suggest only to revise the wording in the text. Perhaps use the term sink only for the decadal budget terms, and use net air-sea flux and net air-land flux when discussing annual numbers.

Indeed the carbon budget presented here includes also the effect of climate variability. This has been clarified in the introduction. We also included in the abstract the decadal values, which are more representative of the anthropogenic component, and the values for the last year available with a brief explanation for their cause and relation to natural climate variability. We would like to keep the term 'sink' as we have here in the text rather than introduce two additional terms (air-sea and air-land fluxes), because for all the values discussed, the air-sea and air-land are always positive (i.e. indeed always a sink). We have tried to address more generally the reviewer's comment about the important role of climate variability by increasing the text on the relation between the sinks and the natural variability, in Section 3.1 in particular. As the reviewer mentions, a detection and attribution study is outside the scope of this paper.

(2) In order to be as up-to-date as possible, the authors use more or less sophisticated extrapolation procedures for the years 2009-2011/12, because some of the underlying statistical data or model simulations do not cover the entire time period 1959-2011. It would be useful if the extrapolated values in all time series were specifically flagged

both in the excel sheet as well as in the graphics, since these will be adjusted in future releases of the dataset, when more information becomes available.

Thank you, this will be done in the database and in Figs. 3 and 4, where the components are shown individually.

Fossil fuel emissions:
(3) How are the BP based estimates for 2010 and 2011 merged with the CDIAC estimates up to 2009? The BP numbers are typically around 0.2 PgC/yr off from the CDIAC numbers. Does this then not introduce an artificial offset in the time series?

This has been clarified. We use the CDIAC numbers through 2009 and then use the BP values only for the year-to-year rate of change. The BP rates of change are used for an extrapolation of the CDIAC time series, where we never use the BP absolute values. We believe that the rates of change are less uncertain than the absolute values, and examination of the success of these extrapolations bears this out.

Land use change:
(4) How are the bookkeeping model fluxes combined with the annual satellite based emission estimates? The text is rather vague: "The bookkeeping model is used mainly to quantify the mean ELUC over the time period of the available data, and the satellitebased method to distribute these emissions annually" - what means "mainly"? Can this not be written rigorously in mathematical form?

The way the two datasets are combined has been clarified in the text. 'Mainly' is used here because the bookkeeping method includes annual data from changes in cropland and pastures, but not from changes in forest, which are every 5 or 10 years. Thus it is not a perfect separation of timescales between the bookkeeping and fire databases, but it is the best estimate we can produce that makes use of the available information.

(5) The error of the land use change flux is estimated from the simulations with different DGVMs using as drivers a common land use change are data base. The analysis gives the same error for the annual flux estimate as for the decadal means. Are the errors for the decadal means independent for each decade or are they correlated? This information is important when estimating the uncertainty of longer term trends in the land use flux estimates.

The error budget from the DGVM ensemble comes from 1) process uncertainty (e.g. not counting forest degradation in some models), which cause a bias that makes decadal estimates perfectly correlated (see Gasser and Ciais, 2012; Table 2), 2) common land cover change input data which cause a bias, but if a different input dataset is used each decade, dedacal fluxes from DGVMs may be partly decorrelated, 3) model structural errors, which cause bias that correlate decadal estimates. In addition, errors arising from unknown DGVM parameter values would be random and bias but they are not accounted for since no DGVM provided and ensemble of runs with perturbed parameters. Therefore, The errors are very likely to be correlated between decades. This information has been added in the text.

Ocean fluxes:
(6) The authors state in the abstract and in the documentation in the excel sheet, that the ocean flux is determined from an ensemble of 4 different ocean models. Only inside the manuscript text one learns that actually the magnitude of the ocean uptake is tuned to an observational value for the 1990's. By putting the observations under the rug, the authors do not provide a service to the ocean science community, which has done a huge effort to come up with observational estimates of the ocean uptake. This clearly needs to be reworded, else any funding agency will get the impression that further

ocean measurements are not needed, since the scientific community relies on 4 ocean carbon models. Only by going into the literature one finds out that the observations used here are the CFCs, the O2/N2 method and a combination of models + the global anthropogenic DIC inventory. I am also astonished that the compilation by Takahashi et al. is not included here.

Thank you, we clarified the abstract to say that the mean ocean CO_2 sink over the 1990s was based on observations, and the anomalies were estimated using an ensemble of ocean models. We used the 1990 value of the mean ocean sink as estimated by the IPCC AR4. In that report, the Takahashi value is not used as one of the three estimates used because the uncertainty in the ocean sink from pCO_2 measurements is very large (1.0 PgC/yr, compared to about 0.4 PgC/yr for the other data-based estimates). Here we simply use the IPCC value, and have not provided a new assessment of the mean ocean carbon sink.

(7) By looking at the numbers in the data file, the atmosphere-ocean time series of the models were shifted by a fixed amount in order to match the observational value of 2.2 PgC/yr during the 1990's. Does this not create an inconsistency with the assumed equilibrium preindustrial atmosphere-ocean flux in the model simulations? Would not a scaling by a factor be more appropriate on the grounds that the longer-term ocean uptake is expected to be on first order a function of the excess atmospheric CO2? Indeed, if this is done e.g. to the "updated model" time series, their inter-model standard deviation decreases substantially, especially in the earlier part of the record.

We have now corrected the ocean models using a scaling factor rather than a fixed amount as suggested. This is in better agreement with our understanding of the processes, as pointed out by the reviewer.

Residual land fluxes:
(8) The authors show, that the inter-model standard deviation of the normalised DGVM simulations (0.9 PgC/yr) is similar to the estimated error of the residual sink (0.8 PgC/yr). The authors state: "However (1) they confirm that the sum of our knowledge on annual CO2 emissions and their partitioning is plausible, (2) they suggest that the uncertainty of _0.8 PgC yr1 for SLAND estimated from Eq. (7) is an appropriate reflection of current knowledge, and (3) they enable the attribution of the fluxes to the underlying processes and provide a breakdown of the regional contributions (not shown here)." I do not think that (1) and (2) are really tenable, since the 0.9 PgC/yr spread of the DGVM simulations refers only to how they simulate interannual variability, while the estimated error of the residual sink also refers to the longer term carbon uptake, and e.g. provides a global constraint on the CO2 fertilisation effect.

Indeed (1) and (2) cannot be inferred from the DGVM inter-model standard deviation. We have removed (2), and rephrased the sentence and expanded the text. We think (1) can still be argued. In fact one of the main reason for doing this comparison is to check if we can reproduce the residual terrestrial flux with models, and thus if we have a process understanding of the full carbon cycle. As mentioned in the discussion, the correlation r = 0.48 between the budget residual and the DGVM results and the good correspondence of the mean sinks justify the statement that 'the sum of our knowledge on annual CO_2 emissions and their partitioning is *plausible*'. Note that the adjective 'plausible' has been carefully chosen in the text to reflect the correlation level.

Minor comments: p 1110, line 9: is the industrial revolution the same as the industrial era?
Yes. Both words have been harmonized to industrial era.

p 1129, line 13: The missing sink term was used already before the particular studies listed

here.
We removed this sentence.

p 1132: The 2011 emission numbers for China and the EU are reversed.
This has been corrected.

p 1133, line 6: I presume that PgC/yr is meant here.
Yes. Corrected.

p 1133, lines 27/28: The impact of ENSO on global atmosphere-land fluxes has been documented in the literature well before the cited papers by Peylin et al. and Tian et al.
We replaced the reference to Tian et al by a reference to Keeling, C.D. et al., Nature 1995. We kept the reference to Peylin et al as to our knowledge it is the first to provide a detailed comparison of atmospheric inversions based on observations and process models showing the land/ocean partitioning from different methods.

p 1136,
lines 12-14: have we really seen rapid changes in the biophysical world over the last decade? The annual increase in fossil fuel emissions are somewhat larger than in the previous decades, but is acceleration the correct word? Is there any evidence for globally significant impacts from global climate phenomena? I'd tune this down or provide appropriate literature references.
We modified the text in parenthesis to tone down the statement and change the word 'acceleration' into 'increase'.

Comments to the excel spreadsheet Sheet 2
(global carbon budget): Is co-author Ralph Keeling really happy the way the Scripps CO2 Program is referred to here? "The atmospheric CO2 growth rate is estimated directly from atmospheric CO2 concentration measurements by the US National Oceanic and Atmospheric Administration Earth System Research Laboratory (NOAA/ESRL). http://www.esrl.noaa.gov/gmd/ccgg/trends/global.html 1959-1980 are based on Mauna Loa and South Pole stations as observed by the CO2 Program at Scripps Institution of Oceanography and other institutes. " I do not think that the early Mauna Loa and South Pole data were "measured by NOAA" or are from "other institutes". I'd at least cite the website of the SIO program.
We modified this citation to clarify that the early data from MLO and SPO were from Scripps only, and included a citation to the website.

Sheet 7 (ocean sink):
The column labelled "Mean" is not clear: I presume it is the mean of the "old" LeQuere et al, 2009 ocean models. However, there are numbers fro 2009-2011 - where are they from? Also, the values for the years 1959 and 2008 are not the mean of the "old" models. This needs to be fixed and properly described.
We clarified and simplified the ocean sink calculation to reflect comments by G. McKinley, and clarified the spreadsheet.