

We thank all reviewers for their comments. Our detailed response is provided below (in blue). We hope we have clarified all the issues raised. The main modifications to the manuscript made in response to the comments received was the addition of a new Table (Table 3) which provides an overview of the main methodological changes that have taken place between each carbon budgets published so far. We also added a column to Table 5 to specify the changes in the models that have taken place since the last published version of the carbon budget. Finally we provided the data for the carbon budget from year 1750, as shown used in the manuscript.

Compared to the version submitted to ESSDD, we modified the LUC CO<sub>2</sub> emissions. The submitted emissions were from an update of the Bookkeeping method published in last year's carbon budget. This update is no longer included in the revised paper, which uses the same LUC emissions published last year. This is because the underlying paper describing the update, in review at the time of submission, is not yet published. We revised all figures, tables and text accordingly. The largest change is the revision of the cumulative emissions, which have gone down from 590±75 PgC to 570±70 PgC. We also added one DGVM (ORCHIDEE, already published last year but not available at the time of submission) and made a small correction to one ocean model. The changes in the DGVM ensemble and ocean model have minor impact on the manuscript.

### **Interactive comment on “Global carbon budget 2013” by C. Le Quéré et al.**

#### **Anonymous Referee #1**

- 1) CDIAC - nice landing page, good information support; I like the thumbnail views of the graphs.. Version 2013 Budget v1.2; which one under review here? For this review, I looked at v1.2. (Two versions under one doi?. Not technically true, I suppose). V1.2 does explain, clearly and prominently, the changes to v1.1. But the dpi refers explicitly to v1.1. After using and reading the two versions, I can see how this works - to include the subsequent version on the dpi landing page for the original. I found everything I needed to review (and to use) the data. But, will v1.2 eventually have its own doi? Or, not until 2014v1.1?

An error has been found on the Australian emissions in versions 1.1 for years 2011 and 2012, which have been first removed (version 1.2), and then corrected (version 1.3). This was a minor but necessary correction. As the DOI leads to the landing page where both the original and corrected versions are available, one single doi is provided for submission. The doi will be updated for the revised version.

- 2) MS Excel files not, strictly speaking, open access? Would a .csv version, in addition, make sense in this case? But the .csv version would lose the careful formatting of the Excel version? Just sensitive to the casual use of proprietary software formats.

Most of the information published here is also available through the [globalcarbonatlas.org](http://globalcarbonatlas.org) web site, where it is available in csv form. We have now included a link to this Atlas in the text, as well as a link

to a freely accessible xls reader (<http://www.microsoft.com/en-us/download/details.aspx?id=10>). Although MS Excel is not strictly open access, it is widely accessible and has clear formatting suitable to the amount and diversity of data provided here. In contrast, all 10 individual spreadsheets would have to be saved separately with csv, losing the connection between variables (e.g. territorial versus consumption emissions) and clarity, and introducing much potential for miss-use of the data. We feel clarity is essential in this dataset and prefer to keep with the xls format, particularly with the availability of the free reader.

- 3) The authors do a very good job of explaining +/- 1 sigma and its % probability, and in several places explicitly discuss uncertainty as quantified here compared to uncertainty as quantified by IPCC. But the authors also use, in many places, the terms 'low', 'medium' and 'high' with respect to uncertainty, when discussing a sub-component of, for example, land sink estimates or a single paper on, for example, land-ocean carbon transport. I suspect the authors use these three terms deliberately and carefully, again in a manner consistent with IPCC or some other external protocol, but readers of this manuscript do not get the necessary quantitative or qualitative calibration to understand how and why the authors use those specific terms?

We have expanded the justification of the confidence level for the fossil fuel emissions, atmospheric growth rate, and ocean CO<sub>2</sub> sink, to clarify our assessment that confidence is a function of both the amount of evidence and their consistency. We also clarified in the introduction that this is an assessment of confidence level, and thus it is qualitative based on our own judgment.

Overall, a well organized and intensely valuable data set and data description. Very encouraging to see the authors continue the annual updates and bring the data into open access through ESSD. In a few specific sections below I have questioned the authors (and the journal editors) about when and how to identify specific changes and improvements from prior versions, but overall I think the authors have done a very good job of identifying, documenting and justifying changes and improvements. My experience suggests that the living data processes as alluded to several times does not, perhaps will never, obviate the need for a careful reading and review. Very positive overall, please see the many small comments that follow as suggestions for improvements from the point of view of future readers and users.

Thank you for your comments.

Page and line specific comments, suggestions:

- 4) p692, line 9 - a comma missing here?: "to previous estimates consistency within"

Comma inserted as suggested.

- 5) p692, line 21 - "All uncertainties are reported". All 'annual' uncertainties, or all the annual, decadal and long-term mean uncertainties? Later, p695, I read that this applies only to annual estimates?

All uncertainties are reported the same way throughout the paper. To avoid confusion, we removed the 'annual' from p. 695. This way it is clear in the abstract that we are referring to all the uncertainties

presented.

- 6) p692, line 27 - "assuming and ELUC of" 'an', not 'and'?

Corrected.

- 7) p694, line 3 - technically, I think the paper refers to net land and ocean sinks, but not to carbon fluxes (e.g. moles per m<sup>2</sup>)? I suppose a value for Socean of 2.9 GtC / yr does in some sense represent a temporal 'flux', but really instead a net growth / increase of that reservoir, distinct from actual atmosphere to ocean flux(es)? Some terminology issues here? Later the authors label these land and ocean processes as "perturbation" and "uptake", terms I think most readers will understand. But a change in the net annual ocean sink due to, for example, change in nutrient supply to the biological pump in some region, does not, strictly speaking, represent a change in the air-sea flux terms, at least not as, e.g. CLIVAR, defines air-sea turbulent fluxes? Perhaps a net flux vs a specific flux? Perhaps a terminology difference among the carbon and heat or momentum communities? Later on this page the authors define fluxes more carefully, e.g. lateral fluxes, evasion fluxes, net air-sea fluxes. In these later examples, readers can clearly understand the processes as described.

We have removed the word 'flux' which was causing confusion and clarified the terminology as follows: 'the resulting changes in the *storage of carbon in the land and ocean carbon*'.

- 8) p695, line 5 - I have to think about this statement. On first reading, it seems correct and logical. But, if a net lateral flux, land to coastal seas, of, e.g P or N (separate from a C transport) causes increased O<sub>2</sub> depletion zones, which in turn perturb the coastal ocean C sink - I have to think about in which direction - then even for an unchanging land-to-ocean carbon flux, a change in non-carbon terms of the land to ocean nutrient flux would have a carbon sink impact? Thus the "complementary" ways argument does not pertain? Perhaps these local processes have minor impact on global C budgets? (Later - the discussion in section 2.6.2 helps in this regard, at least about the impacts of these LOAC processes, but does not directly address this question of complementary processes?)

It is correct that some land-to-ocean fluxes of nutrients other than C could cause a change in  $S_{\text{OCEAN}}$  with no corresponding change in  $S_{\text{LAND}}$ . However the sentence on line 5 refers strictly to equation (1), which is the balance of carbon only. In the case of the transport of nutrients, it is not so much that equation (1) is not entirely exact, but rather that some of the anthropogenic influence (in your example of the impact of anthropogenic nutrients on the ocean C sink) are not quantified through our method because the ocean models used to quantify the ocean sink do not include changing river nutrients. We have clarified in Section 2.4.2 that 'Models do not include the effects of anthropogenic changes in nutrient supply.'

- 9) p695, last paragraph - very helpful, very important.
- 10) p696, line 24 - anticipating that many readers will not know its existence, but will find it useful, mention the (very cool) carbon atlas here?

Thank you. We have added some context and a link to the Global Carbon Atlas.

11) p697, 2nd paragraph - very helpful

12) p698, line 9 - for new readers, we have not defined 'BP' up to this point?

'BP' is the name of the company. It is no longer an acronym (formally British Petroleum).

13) p699, line 2 - I understand what the authors intend here, but this phrase "hence count the carbon mass with CO<sub>2</sub> emissions" needs some clarification?

We have modified the text as follows: 'will soon be oxidized to CO<sub>2</sub> in the atmosphere and can be accounted with CO<sub>2</sub> emissions.'

14) p700, line 6 - from the data for 2012, bunker fuels represent 0.3 Gt out of a global total of 6.7 Gt, so only 5%. But, from the same table, the 0.3 Gt bunker fuels exceed total emissions of some regions? What does col HX ('Statistical Difference') mean in the terrestrial emissions table? I would need to go to the Boden et al. citation to learn the answer to that question?

There may be a misunderstanding here. The global total for 2012 is 9.667 GtC, so the bunker fuels are 3% rather than 5%. The bunker fuel estimate is global only, so it could well exceed emissions for some regions. We have clarified in the territorial spreadsheet that 'The statistical difference presented on column HX is the difference between the world emissions and the sum of the emissions for each countries and for the bunker fuels.'. The text presents an explanation for the difference (p. 700 top paragraph).

15) p700, line 23 - how does "medium confidence" assigned here relate to the +/- 1 sigma (68%) mentioned earlier?

We added: 'Thus there is only limited and indirect evidence for emissions, although there is a high agreement among the available estimates within the given uncertainty (Andres et al. 2012), and the emissions estimates are consistent with a range of other observations (Ciais et al. 2013).'

We use the IPCC guidelines in our assessment of the confidence. We clarified in the introduction that this assessment is qualitative (new text in red): 'we have introduced a **qualitative assessment of confidence** level to characterise the annual estimates from each term based on the type, amount, quality and consistency of the evidence as defined by the IPCC (Stocker et al., 2013).'

16) p701, final paragraph (continues to p702) - versions and improvements. For some reason, perhaps stimulated by this phrase ("incorporate several improvements"), I compared this paragraph closely to the equivalent paragraph of the prior paper. Of the words and sentences in this paragraph, 95% (estimate, NOT an accurate count) replicate exactly the prior version. Of course, no surprise, and very appropriate - a reader of the current paper will want all the details at hand, without having to open the prior version as well! But how will that reader appreciate the three or four subtle changes/improvements here: annual update to GATP data, inclusion of the transportation data in the global totals, errors in the trade shares data now restricted to one country (Taiwan) rather than to two as previously (Taiwan and Netherlands)? I see the living data process at work here, with

improvements small and large incorporated throughout. But does a reader need to have some better way to find and evaluate those changes? I do not know how the authors, editors or publisher should distinguish or otherwise highlight those changes - certainly the authors have already given a useful summary of substantial changes in an earlier paragraph. But somehow this paragraph, which perhaps typifies more subtle but very necessary improvements and updates, caused this reader to wonder about a better way to itemise those changes. Perhaps a question more for the journal than for the authors. I re-read the 'main changes' paragraph (2nd paragraph on page 697) with appreciation, but perhaps a sentence there that also explicitly mentions many small changes and updates throughout? Or perhaps the phrase "update of Le Quéré et al. (2013)" should have provided me with sufficient notice? I like the process, I urge the authors and the journal to make it work for both the data providers and the data users, but somehow this paragraph exposed this 'living data' issue in an acute manner; a careful reader might want / expect something more?

We now provide a table that includes the main differences with the previous carbon budget updates (new Table 3). This table includes the most important changes only, but it will allow the readers to have a quick view of what main differences were introduced with time. We feel that smaller changes (e.g. the trade shares in the Netherlands) can remain in the text only as they will be of interests to a minority of readers.

- 17) p702, line 23 - interesting use of the word 'needs' here: "movement of carbon across the Earth's surface in response to human needs (both physical and economic)". How much carbon consumption (and transportation) meets legitimate needs (food, heat, etc.) and how much satisfies more ephemeral 'wants' (entertainment, recreation, discretionary travel, etc.) seems to cloud this issue. Perhaps 'human activities' represents a more careful description? Only a suggestion. (A few sentences later, by the way, I like the careful use of the word 'unharmonized').

As suggested, we replaced 'needs' by 'activities'.

- 18) p703, line 22 - "so-called Kaya (also called IPAT) identity": we need a definition or at least a citation for 'IPAT'?

We have provided a definition for IPAT.

- 19) p704, line 8 - which equation divided by which other equation?

The equation numbers have been corrected.

- 20) p704, starting from line 20 - my ignorance of LUC exposed here, but a) again the term "fluxes" and b) doesn't the term 'deforestation' includes processes such as logging and shifts in cultivation? The legend for Table 4 seems to imply the same, that the explicit processes listed in col 1 can contribute to the large overall deforestation change?

The term 'fluxes' is appropriate here because it refers to exchanges between the atmosphere and ocean that can go either direction, in response to changes on land.

We use the terminology 'LUC' here to include all processes that affect carbon storage on land from deliberate land management. Deforestation is one of those processes, specifically referring to the transformation of forested areas into pastures or cropland. Logging and shifts in cultivation differ because they refer to activities that modify the land, but do not change its primary function. All models include the processes of 'deforestation, afforestation, and forest regrowth' (Table 4), but differ in their representation of the other processes.

21) p705, line 17 - here the reader finds an explicit description of improvements to the prior version. Very helpful, and somewhat in contrast to comment above about the paragraph in the consumption emissions section?

[See response to comment 16.](#)

22) p706, line 28 - why does this sentence link to Table 3? Table 3 says nothing about recent satellite data? Perhaps for the FAO references?

[We corrected the reference to Table 2.](#)

23) p707, line 1 - LUC data "non-spatial"? I think the authors mean not resolved to specific national or vegetation type boundaries? But certainly, if aggregated at regional basis, the fundamental data must have some spatial basis? E.g as opposed to purely temporal basis? The following sentence appears to confirm my observation?

[We clarified the text to: 'The LUC country data are aggregated by regions.'](#)

24) p707, line 15 - "legacy emissions such as decomposition from on-ground debris or soils are missed by this method": something awkward about or missing in this phrase?

[We changes 'missing' to 'not included'.](#)

25) p708, line 1 - "Burned area from (Giglio et al., 2010)": something wrong with parentheses here?

[Reference corrected.](#)

26) p708, line 4 - "CASA biogeochemical model": definition, citation, link to Table 5? (But, not listed in Table 5?)

[The bookkeeping model and CASA model are now added to Table 5 \(now Table 6\). CASA is now defined in the text.](#)

27) p708, line 14 - assumes that all land management activities ?over that time period? ?did? not vary ....(?)

[Clarified in the text: 'Our comparison of LUC CO<sub>2</sub> emissions from fires with other methods assumes that all land management activities apart from deforestation do not vary significantly on a year-to-year basis for the overlapping period. ' \(note that the time period is given two sentence above\).](#)

28) p708, line 19 - "Trendy": I found this easily with a Google search. Do we need an explicit url here? ESSD or Copernicus policy?

We added the URL.

- 29) p710, line 28 continuing to p711 - “definition issues when calculating ELUC from the difference of simulations with and without LUC, which cause a bias compared to the bookkeeping estimates that makes decadal uncertainty estimates perfectly correlated! (Gasser and Ciais, 2013);”: I don’t understand what the authors intend to say here? Do the definition issues from the DGVM models cause the bias which in turn causes the perfect correlation of decadal variation in the bookkeeping estimates? Or, do the DGVM definition issues result in a bias relative to the bookkeeping estimates because the latter have inherent perfect decadal correlations? I thought we wanted to understand how uncertainties in the DGVM decadal ELUC estimates impact our ability to use those DGVM- derived estimates to ‘calibrate’ the bookkeeping estimates? Instead, the paragraph seems to focus on internal correlations / de-correlations within the DGVMs themselves? How does this influence or help the overall uncertainty analysis for ELUC?

We have clarified the intention of this paragraph, which is to quantify the uncertainty in the decadal mean estimates. Since we think the error is correlated between decades, we can apply the uncertainty we derived from the annual estimates. We have clarified the point flagged above. The paragraph now reads:

‘The uncertainties in the decadal mean estimates from the DGVM ensemble are likely correlated between decades, and thus we apply the annual uncertainty as a measure of the decadal uncertainty. The correlations between decades come from (1) system boundaries (e.g. not counting forest degradation in some models); (2) common definition for the calculation of  $E_{LUC}$  from the difference of simulations with and without LUC; (3) common and uncertain land cover change input data which cause a bias, though if a different input dataset is used each decade, decadal fluxes from DGVMs may be partly decorrelated; (4) model structural errors (e.g. errors in biomass stocks). In addition, errors arising from uncertain DGVM parameter values would be random but they are not accounted for in this study, since no DGVM provided an ensemble of runs with perturbed parameters. ‘

- 30) p711, line 23 - If I remember, ESRL uses the marine boundary layer sites exclusively to avoid land sites influenced by high and/or recent Eff? Does this deliberate selection deserve mention here; e.g. does it have relevance to the budget calculations?

We added ‘sites with well-mixed background air’ to make the point suggested by the reviewer.

- 31) p712, line 16 - I think the authors mean Table 7 here, not Table 6?

Corrected as suggested.

- 32) p713, line 5 - Again, a very helpful description here about improvements (using newly- emerging annual CO<sub>2</sub> sink data from global models) and their impact on this paper and these data!
- 33) p715, line 19 - “two data-based products”: a bit confusing because the reader has just finished a section (2.4.1) on three data-based estimates (of the long term mean ocean sink) but now here reads about 2 data-based ‘products’ that in fact refer, I think, to the two ocean CO<sub>2</sub> data products (Table 5 and following paragraph)? Perhaps some more clear distinction would help?

We tried to clarify the terminology by using 'data-based products' only when referring to the interannual estimates.

- 34) p715, line 24 - "uncertainty of  $\pm 0.4 \text{ GtCyr}^{-1}$ ": where did this come from? Does it represent a rounding of the std dev among models (e.g. in line 14, above)? Later in the same sentence we see a std dev among models of  $\pm 0.3 \text{ GtCyr}^{-1}$ ; a rounding of the 0.29 value from line 22? Where do these numbers come from? Confusing to this reader.

We clarified in Section 2.4.1 that the uncertainty provided by the IPCC and used here comes from the spread in estimates based on observations. We remove the estimate of the standard deviation across models shown on line 14 to avoid confusion and because it was not needed. We finally changed 0.3 into 0.29 to make clear that this is the same number.

- 35) p716, top paragraph - here we find additional uncertainty numbers, including a 0.2 value, supposedly from equation 7 but nowhere mentioned in the equation 7 paragraph. I dislike to ask for additional tables, but readers need some better-organised tool to help digest and understand these interlinked and inter-compared ocean uncertainty estimates?

We have simplified and clarified the text to highlight that this paragraph refers to the confidence in our estimate, rather than to the uncertainty. We hope the revised text is sufficient. We would prefer not to include a table with this information, because it is a detailed assessment and we would like to avoid overloading the reader with information. The revised text is:

'We examine the consistency between the variability of the model-based and the data-based products to assess confidence in  $S_{\text{OCEAN}}$ . The interannual variability of the ocean fluxes of the two data-based estimates for 1990 – 2009 (when they overlap) is  $\pm 0.34 \text{ GtC yr}^{-1}$  (Rödenbeck et al., 2013) and  $\pm 0.14 \text{ GtC yr}^{-1}$  (Park et al., 2010), compared to  $\pm 0.20 \text{ GtC yr}^{-1}$  for the model mean.'

- 36) p718, line 4 - "in that later study": in that 'earlier' study?

Corrected as suggested.

- 37) p718, line 14 - "The sum of ELUC and SLAND is better constrained than their individual components." Very provocative, to my view. Discussed later in this paper, or elsewhere?

We removed this sentence as it was not relevant here. The discussion already includes sentences on the confidence on the various terms of the carbon budget.

- 38) p718, line 17 - very useful and helpful section.

- 39) p718, line 24 - "incomplete fossil-fuel burning for CO and deforestation fires". Something missing here?

We clarified the sentence by removing 'for CO' after 'fossil-fuel burning'.

- 40) p719, line 28 - "uptake by photosynthesis on continental and long time-scale (e.g. decadal or longer)": continental and long time scales? But does that make sense? Does CH4 from submarine (Siberian)



permafrost, arguably a potential anthropogenic impact on Global C and CH<sub>4</sub> budgets, register above uncertainty, yet?

Indeed this paragraph talks about 'anthropogenic' sources, so a balance by natural processes does not make sense. We have changed the sentence for: 'Other anthropogenic biogenic sources of CO and CH<sub>4</sub> from wildfires, biomass, wetlands or permafrost changes are similarly assumed to have a small effect on the CO<sub>2</sub> growth rate.'

41) p720, line 25 - Table 6 represents a good reference here, but perhaps Table 7, because it shows all the other terms, represents a better link?

Changed to Table 7 as suggested, this link gives more context.

42) p721, line 5 - Do not "question"? Rather, do not 'impact' or 'disqualify' or 'contradict'?

Changed to 'contradict'.

43) p722, line 9 - Because the land sink by definition represents the residual of the other four terms, I wonder about the statement here about "important interannual variability in the atmospheric growth rate caused primarily by variability in the land CO<sub>2</sub>"? Carbon must go into land processes, and we understand how large seasonal patterns of land vegetation NPP and of soil respiration must drive seasonal variation in atmospheric CO<sub>2</sub>, and we can model reasonably well the seasonal cycles in DGVM, but can we actually then invoke those land processes as the definitive drivers of interannual variability of G<sub>atm</sub>? As written earlier, land sink contains all the missing terms and errors of the other factors, so some caution here seems advisable? E.g. the sentence at the top of p729. The results paragraph at the bottom of p725 seems to give a more circumspect assessment of land sink and uncertainties?

We have changed the sentence to read 'with important interannual variability in the atmospheric growth rate and in the land CO<sub>2</sub> sink', which does not include any cause and effect but is a simple description of our results.

44) p722, line 16 - Some punctuation or other words missing here?

Edited for clarity.

45) p722, line 22 - decreased emissions are also reproduced.

Changed to 'are' as suggested.

46) p723, line 9 - large apparent? decadal variability on land?

We added a reference to Table 8 where this information comes from. We changed 'large' into 'significant', as the decadal variability on land exceeds the uncertainty.

47) p723, line 16 - punctuation missing?

Punctuation inserted.

48) p723, line 27 - define 'PPP'?

'PPP' was defined as 'purchasing power parity'.

49) p727, line 4 - "because they include an additional"

Changed to 'an' as suggested.

50) p730, lines 13 to 25 - emphatically agree

51) p752, Table 7 - if the authors provided a reason for lower uncertainty of ocean sink in the 1990-1999 decade (0.4, compared to 0.5 in all other decades), I don't remember reading it? The ocean sink page in the spreadsheet does not include the error numbers by which to check this. A typo?

We have added a note to the Table to explain why the value for 1990-1999 is different – "The uncertainty in SOCEAN for the 1990s is directly based on observations, while that for other decades combines the uncertainty from observations with the model spread (Section 2.4.3).

52) p757, Fig. 3 - I searched back through the document for reference to figure 3, but did not find text that would support the final statement in the fig 3 legend: "The black dots in panels (a), (b) and (e) show the values extrapolated from original data as explained in the text." Perhaps I missed something?

We clarified the caption text of Figure 3 "The black dots in panels (a), (b) and (e) show values for 2011 and 2012 that originate from a different dataset to the remainder of the data, as explained in the text".