

Carbon Budget 2013

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?

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.

3) The authors do a very good job of explaining +/- 1 sigma and its % probability, and in several places explicitly discuss uncertain as quantified here compared to uncertainty as quantified by IPCC. But the authors also use, in many place, 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?

4) 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.

Page and line specific comments, suggestions:

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

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?

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

p694, line 3 - technically, I think the paper refers to net land and ocean sinks, but not to carbon fluxes (e.g. moles per m²)? 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.

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 O₂ 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?)

p695, last paragraph - very helpful, very important.

p696, line 24 - anticipating that many readers will not know its existence, but will find it useful, mention the (very cool) carbon atlas here?

p697, 2nd paragraph - very helpful!

p698, line 9 - for new readers, we have not defined ‘BP’ up to this point?

p699, line 2 - I understand what the authors intend here, but this phrase “hence count the carbon mass with CO₂ emissions” needs some clarification?

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?

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

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?

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').

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

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

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?

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?

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

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?

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?

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

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

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

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

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?

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?

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

p713, line 5 - Again, a very helpful description here about improvements (using newly-emerging annual CO₂ sink data from global models) and their impact on this paper and these data!

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 CO₂ data products (Table 5 and following paragraph)? Perhaps some more clear distinction would help?

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.

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?

p718, line 4 - “in that later study”: in that ‘earlier’ study?

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?

p718, line 17 - very useful and helpful section.

p718, line 24 - “incomplete fossil-fuel burning for CO and deforestation fires”. Something missing here?

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 CH₄ budgets, register above uncertainty, yet?

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?

p721, line 5 - Do not “question”? Rather, do not ‘impact’ or ‘disqualify’ or ‘contradict’?

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 CO₂”? 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 CO₂, 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_{atm}? 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?

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

p722, line 22 - decreased emissions are also reproduced.

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

p723, line 16 - punctuation missing?

p723, line 27 - define ‘PPP’?

p727, line 4 - “because they include an additional”

p730, lines 13 to 25 - emphatically agree!

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?

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?