

Dear Editor

We thank the reviewers for their comments on our manuscript. Please find a point-by-point reply below. The text in red is our response, the text in black is the original comments from the reviewers.

**Response to Topical Editor comments (26 Nov 2015):**

Overall, the authors have provided skilful and helpful answers, responses or changes to meet reviewers concerns.

Small concern remains about whether inclusion of the 2015 predicted emissions adds value to this data. The authors make a persuasive case that the historical and statistically reliable data enable such a projection, that such a projection adds potential interest and relevance to a larger audience, and that these projections have served a useful prior purpose within the annual budgets and across the sequence of budgets. This editor accepts the authors' reasons.

**We are grateful to the topical editor for his comments. We hope the responses outlined below are to his satisfaction.**

The as-published paper will require a tested and working doi pointing to the data on the CDIAC site.

**We are in the process of resolving this issue. We will supply this information as soon as it is available. The Topical Editor is aware of our efforts to rectify this matter.**

Either in final up-load or during type setting and proofing, a small series of typos need correction:

- p9 L2: "minising"

**Corrected.**

-p9 L20: "totals will be to be slightly"

**Corrected to 'totals will be slightly'**

- p21 L7: Remove "boundary conditions"? The example given (whether fire management is included or not) is not a boundary condition but a question of definition.

**Removed.**

- p22 L18: Replace "some of" by "a fraction of"

**Replaced.**

- p32, 37 and caption to Figure 8 (and perhaps elsewhere): There is a sign error in the formulae?  $E_{LUC}$ ,  $S_{LAND}$  and  $S_{OCEAN}$  are all positive (equation 1). Hence what is meant here is  $S_{LAND} - E_{LUC}$ , or on p37 L6 and in caption to Figure 8:  $S_{LAND} + S_{OCEAN} - E_{LUC}$ .

**The sign error has been corrected. For consistency the affected formulae have been corrected for sign and reordered, i.e.  $S_{LAND} - E_{LUC}$  and  $S_{OCEAN} + S_{LAND} - E_{LUC}$ , throughout the manuscript.**

- Table 1: Ballantyne et al. 2012 actually use 2.124 for the conversion of ppm to GtC. The calculations were done with this factor and are just reported here with rounded numbers? However the GtCO<sub>2</sub> to GtC conversion factor is not rounded... Perhaps better to state here the unrounded ppm to GtC factor.

**Given the level of uncertainty here we thought it was more appropriate to use 2.12, e.g the uncertainties do not warrant using 4 significant digits. Note b underneath the figure has been amended from 'Using a factor of 2.12 makes the approximation that the growth rate of**

CO<sub>2</sub> in the stratosphere equals that of the troposphere on a yearly basis.' to 'Using a factor of 2.12 makes the approximation that the growth rate of CO<sub>2</sub> in the stratosphere equals that of the troposphere on a yearly basis and reflects the uncertainty in this value.' to include this information

- Excel spreadsheet: Would help the user of the spreadsheet if the meaning of the red numbers in the carbon budget spreadsheet were explicitly mentioned in the description.

The meaning of the red numbers in each worksheet of the global carbon budget spreadsheet is now explicitly stated on each individual worksheet where red values appear.

- p49 L16: "in press. in press."

Corrected to 'in press.' in two different references.

#### **Response to Referee #1: Prof Albertus J (Han) Dolman**

The suggestions have been incorporated into the manuscript, specifically:

1) 2.1.4 Pg16 |14 - 'because they are unrelated' has been deleted

2) 2.2.4 - 'Commentary on' has been deleted.

#### **Response to Referee #2**

Overall the manuscript and spreadsheets present data from a wide mixture of scientific and political sources. The authors do a first-class job of compiling, describing, assessing and explaining. They provide a logical and useful structure and excellent documentation, with careful and explicit attention to errors and uncertainties throughout. The two spreadsheets represent models of clear and effective data presentation. Even if this represents the 10th version and now the third or fourth published in ESSD, the topic and the budget calculations remain absolutely essential. I applaud the authors and the overall GCP team for the effort and for the quality of the effort. Prompt publication will serve the project and represent a good effort by the journal.

I submit a short set of comments, questions and suggestions. I also mark a few typos because apparently many of those persisted through prior proof reading and in at least one case they affect the intended meaning.

General (and throughout the manuscript): Assuming publication of this manuscript before the end of calendar 2015, the authors and this journal will then have two carbon budget papers cited as Le Quere et al. 2015 (with likely slight differences in the long lists of authors). The manuscripts will have separate dois, of course, and good, but can the authors or publishers think of a convenient way to designate 2015 first and 2015 second?

This is a difficult point for us to handle, and probably it will be down to the people citing the paper to make sure they use the correct citation. The papers are differentiated in their titles, i.e. Global Carbon Budget 2014 and Global Carbon Budget 2015.

P4, line 4 - Typo: "growth in EFF will be near of slightly below zero". 'or', not 'of'?

Corrected.

P7, line 13 - Typo (?): "Finally we provide the total or cumulative emissions from fossil fuels" Does the 'or' in this case indicate that 'total' equates to 'cumulative' or should this read 'total of cumulative'?

To avoid confusion 'the total or' has been removed from this sentence. It is cumulative emissions that are presented.

P8, line 24 - Comment: "emission estimates are verified by the UNFCCC". How does the UNFCCC verify? Against some other data source not available to this group?

The protocol for verification by the UNFCCC involves a team of experts visiting each country and scrutinizing the totality of their reports following their self-established methodology for which we have no detail. We modified the text to clarify that this is an audit process, and expanded slightly to say: "emission estimates are periodically audited for each country through an established international methodology overseen by the UNFCCC."

In the National Carbon Emissions spreadsheet, the (green) UNFCCC reports for Canada and Germany (I only looked at those two countries) look higher than the CDIAC estimates. Systematically true, e.g. for all reporting countries? Impact?

We have expanded the text describing the differences in the manuscript. There is no impact on global emissions and we now mention this explicitly. The impacts at the country-level are generally small for the numbers mentioned in the text, and do not affect the ranking of the top four countries.

We added: "Our emissions totals for the UNFCCC-reporting countries were recorded as in the UNFCCC submissions, which have a slightly larger system boundary than CDIAC. Additional emissions come from carbonates other than in cement manufacture, and thus UNFCCC totals will be to be slightly higher than CDIAC totals in general, although there are multiple sources for differences. We use the CDIAC method to report emissions by fuel type (e.g. all coal oxidation is reported under 'coal', regardless of whether oxidation results from combustion as an energy source), which differs slightly from UNFCCC. "

and "Thus the comparison of global emissions with previous annual carbon budgets is not influenced by the choice of UNFCCC national reports."

finally we changed all instances of 'emissions from fossil fuel combustion and cement production' as a descriptor of  $E_{FF}$  into 'emissions from fossil fuels and industry' to reflect the inclusion of non-combustion fossil processes (which are also included in CDIAC).

P11, line 12 - Question: "China's emissions could be overestimated". Those of us who follow the climate blogosphere, and no doubt several of these authors, now know that China under-reported emissions for 2015 (and perhaps prior years). I do not request or even encourage the authors to chase down every small or large update - to manage annual accounting they must set and adhere to firm cut-off dates. But in view of the importance of this particular correction, and noting that the authors admit later, in the discussion (P42, line 29) "Our approach also depends on the reliability of the energy and land cover change statistics provided at the country level", this reviewer wonders whether they can or should correct the China national data?

Such a correction in EFF would of course have a knock-on effect on SLand for 2014 and on projections for 2015 as well? Apparently the authors accessed the particular Chinese data sources as recently as October 2015? I leave it to the authors to decide their possible and plausible options at this late date. Perhaps 15% to 20% change for a single country, even for China, remains within the uncertainty limits in any case? They might at least need to modify the sentence about overestimation then leave more accurate accounting and any corrections to a subsequent budget?

Thank you for the suggestion. We have discussed the option of updating China's emissions to the latest statistics and think it is too early for this. First the Liu et al publication is quite new and others have not yet had a chance to react to it. Second, by "under-reported emissions for 2015", we assume the reviewer refers to the upwards revisions in October this year of official energy consumption statistics for 2000-2013, mentioned in several news

reports. These revisions were already taken into account in the BP emissions growth rate data we use to extrapolate CDIAC data from 2012 onwards. Our growth rates since 2012 are therefore already aligned with the new revisions, although absolute values would be changed somewhat. We cannot at this point include the revisions further without risking errors and inconsistencies. But we have added language to address the existence of the revisions and to suggest what their effect would be.

Finally, the Chinese revisions, while large for China, end up only having a minimal (1%) effect on global emissions. This is because the global total is calculated using the reference approach rather than from the sum of national-level apparent consumption, and while China's reported consumption of energy has been revised upwards markedly, their reference approach (supply-side data-driven) consumption has been revised up much less, narrowing the large gap between the two. Effectively, our global statistical difference (difference between sum of countries and global total) would reduce if we incorporated China's revisions. So while one might expect that a 10% upwards revision in a country that is about 25% of global emissions might increase global emissions in the order of 2.5%, it's actually much less. The reason the reference approach is used at the global level (by CDIAC) is that it reduces the effect of the many problems in energy trade data.

We added in the text: "Note that the growth rates we project for China are unaffected by recent upwards revisions of Chinese energy consumption statistics [CESY 2014], as all data used here dates from after the revised period. The revisions do however affect the absolute value of the time series up to 2013, and hence the absolute value for 2015 extrapolated from that time series using projected growth rates. Further, because the revisions will increase China's share of total global emissions, the projected growth rate of global emissions will also be affected slightly. This effect is discussed in the results section."

and in the results section: "Finally, China revised their emissions statistics upwards recently, which would affect the absolute value of emissions for China (but not the trend). With a slightly higher global contribution for China, our projection of global emissions "growth" for 2015 would decline further from -0.6% to -0.8%, a small difference that falls within our uncertainty range."

P15, line 2 - Question: "In both cases, the projection of a decrease is consistent with the realised change." I do not understand this sentence. It refers, evidently, to projected vs. reported coal use (-3.2% to -2.9%) and projected vs. reported cement production (+3.5% to +2.3%). Because the values for coal (negative) and cement (positive) differ in sign, I do not understand how both can show a projected decrease? Perhaps the authors mean that both projections had the correct sign (trend direction) even if they missed the magnitude of change slightly?

Text changed to 'In both cases, the projection is consistent with the sign of the realised change.'

P21, line 23 - Typo: "The methods implicitly assumes instantaneous loss or gain". Either 'method implicitly assumes' or 'methods implicitly assume' ...

Corrected to 'methods implicitly assume'

P22, line 11 - Typo: "These estimate are" 'estimates are'?

Corrected to 'estimates are'

P25, line 24 - Comment: We just saw, in past days, updated version of SOCAT v3? Does the updated version include some or all of these additional data records? (And then the authors could eliminate the long version of Appendix 1?)

Additional data listed in Appendix are NOT in SOCAT-V3.

P26, line1 - Comment: "discrepancies". I have not read Rödenbeck et al. 2015 but evidently the authors refer to more or broader discrepancies than addressed in that paper? From Table 3 we know that the authors used 3 ocean data products in 2013 but only two in this paper. Do we need more explanation? At least as guidance or advice to other potential users? Again, I leave this decision to authors.

We have expanded on this as follows: "Several other data-based products are available, but they show large discrepancies with observed variability that need to be resolved. Here we used the two data products that had the best fit to observations, distinctly better than most in their representation of tropical and global variability (Rödenbeck et al. 2015).

We also complemented the introduction to explain that we have set criteria for the selection of data products (and also DGVMs, which we had done but not explained explicitly).

P35, line 7 - Typo: "ensemble mean ... also reproduce the observed mean" 'reproduces'?

Corrected to 'reproduces'

P36, line 29 - Question: Here again the acute dependence on accurate emission numbers from China "lower growth in emissions than anticipated in China". See comment (P11, line 12) above. Would the revised emission numbers have come closer to the authors projections? As above, authors to decide if and how to use any revised numbers.

See response above.

P58, Table 2 - Question: Here and again in Table 4 the authors provide very useful listing of and access to "individual components" or "each component" of "global" budgets. But the country data do not appear here, e.g. the 'General' or 'National' data from China. Those sources do occur in the reference list. Because of their prominent use in this particular version of the budget, this reviewer does not understand why some data sources appear in both the tables and the reference list but others only in the reference list? Not a big issue, only a curiosity.

We have tried to clarify Table 2 as it mentions already how to cite the national data. We added a clarification to table 4 on  $E_F$  (that the data is used by CDIAC only).

P60, Table 4 - Typo: For GATM, the NOAA ESRL data source should reference 1980 to 2015, not 1980 to 2014?

Yes, indeed. Now corrected to 1980 to 2015

P62 & 63, Table 6 - 'Not applicable' in this table indicates that first use of the particular model occurred in this paper, e.g. that no prior use occurred so that accordingly no change could occur?

Yes, we have changed to 'Not applicable (first use of this model)' in the table.

P62, Table 6, row starting with "CCSM-BEC" - Typo: "small difference in the mean flux are caused by". 'differences'?

Corrected to 'differences'

P64, Table 7, line 9 - Typo: "\* Estimate are not corrected" 'Estimates'?

Corrected to 'Estimates'

P72, Figure 5, line 5 - Typo: "emissions projection for year 2014 based on GDP projection (red dot)". Surely 2015 rather than 2014?

Corrected to '2015'

P74, Figure 6, Panel C - Comment: In this panel and in a few other figures as well, consider using a different colour palette to accommodate colour-blind readers? E.g. [http://geog.uoregon.edu/datographics/color\\_scales.htm](http://geog.uoregon.edu/datographics/color_scales.htm).

We have changed the colours of Fig. 6, 7 and 8 to take into accommodate colour-blind readers.

P80, line 4, Typo: "year when the budget was first release." 'released'? (And this designation will get more confusing in future years with two '2015' citations? See top comment.)

Corrected to 'released', and see response to top comment.

#### Response to Referee #3

The authors present an update of the "global carbon budget" through 2014 primarily based on the methodology used in the assessments in the previous years. Clearly the main dataset provided by this study is robust, consistent and valid, as witnessed by relatively minor changes in the main budget terms when comparing the present updated compilation with the past updates. It is an important data resource to the Earth system science community and a timely update. I just have a few minor comments regarding presentation, clarification and documentation as given in the specific comments below.

My main concern in the present update lies with the forecast of the fossil fuel emissions for the year 2015. Even the emission data for 2013 and 2014 are already based on preliminary data and needed various adjustments to make them compatible with the more consolidated emission data that are available only up to 2012 in this iteration. As the authors concede, these preliminary estimates in past assessments had to be substantially revised in the next assessment; sometimes beyond their previously assigned, perhaps too optimistic, uncertainty bracket. Extrapolating the consolidated emissions of UNFCCC/ CDIAC beyond 2011/12 for 2013 and 2014 makes sense, since they are confronted by the observational data from the atmosphere and the modelled ocean sink for these years and are thus in support of Earth system science as a preliminary assessment of the global carbon budget for these years. However the forecast of the fossil emissions for 2015 is just standing there without any connection to Earth system science; what is its purpose? Is it included for political reasons, because the forecast based on the data up to August 2015 indicate a tantalising levelling of the emissions forecast for 2015? But if so, is ESSD really the right outlet for this? Who reads ESSD? Should not this forecast, if reliable, be much more prominently announced, perhaps as an opinion piece in Nature or Science? The methodology could then also be properly reviewed by socio-economists; I do not have the expertise to do this given the short 7-day period allowed for making this review.

Socioeconomic datasets are not listed specifically in the subject areas of ESSD. I suggest to remove this forecast and keep in ESSD the robust natural science, as in the past.

Thank you for your comment. The 2015 forecast was submitted separately in a Commentary to *Nature Climate Change*, where it has been peer-reviewed and accepted. This ESSD paper provides much more detail regarding the source of the data and the context in which the 2015 projection is made, than we could include in the NCC Commentary. The two papers are thus complementary. We provide the 2015 projection because this information has high policy relevance, and answers questions posed by policymakers, journalists and members of the public who want to interpret the implications of what is happening in the world. The analysis we do is thorough and to the best of our knowledge. Although the projection made last year was not realised, the previous five projections had been useful and we have changed our methodology accordingly. We feel this is an important part of our annual carbon budget analysis and would quite like to keep it as part of the manuscript where the methodology can be traceable year-on-year.

Specific comments:

p11, L11 ff: Liu et al make a case for overestimating CO<sub>2</sub> emissions from China. However a recent news item e.g. in the NY times (<http://www.nytimes.com/2015/11/04/world/asia/china-burns-much-more-coal-than-reported-complicating-climate-talks.html>) based on the reported emissions in China's Energy Statistics Yearbooks comes to a quite different conclusion. 600 million tons of coal more consumed in 2012 is a large number; exceeding the reported uncertainty on fossil emissions of 5-10%. While probably an issue of data analysis/conversion factors etc. can this not be addressed more clearly here? I guess this is touched upon in section 3.2.1, but without any clear statement on which of these different estimates can be trusted.

These revisions in Chinese energy statistics affect the years 2000-2013, and were already taken into account in the BP emissions growth rate data we use to extrapolate CDIAC data from 2012 onwards. Our growth rates since 2012 are therefore already aligned with the new revisions, although absolute values would be changed somewhat. We cannot at this point include the revisions further without risking errors and inconsistencies. But we have added language to address the existence of the revisions and to suggest what their effect would be. See also response to Reviewer 2.

p21, L16 ff : what means "boundary conditions" in this context? This whole section 2.2.4 has many jargon terms which are not explained (e.g. "IPCC Tier 1 type approach", "FRA data" etc.). These terms should be defined in order to make the section readable without going to the original cited literature.

We now provide an example of boundary conditions and have clarified the definition of Tier 1 approach.

p35, section 3.1.3, and legend to Figure 8: The term "surface CO<sub>2</sub> flux" is ambiguous. Better to replace it by "surface CO<sub>2</sub> sink" or perhaps more accurate "non-fossil fuel atmosphere-surface CO<sub>2</sub> flux". Especially since the fossil emissions (which are also a surface-atmosphere flux) are not included here. It's also somewhat questionable, why the land-use flux is included here...

We replaced 'surface CO<sub>2</sub> flux' by 'Atmosphere-minus-surface CO<sub>2</sub> flux'. The land-use flux is included because it is seen by the atmospheric inversions.

Table 8 and Figure 2: perhaps a minor point, but my calculator gives for the error in the residual land sink in the 2005-2014 budget a value of 0.87 PgC/yr, which, if properly rounded, gives 0.9 PgC/yr, not 0.8 PgC/yr as shown here.

We have checked with the original data and the uncertainty calculation comes out at 0.844 PgC/yr before rounding off so we have made no change.

Figure 6 and Figure 8: I understand that the authors would like to use the same y-axis scale in the shown 3 panels. But this hides a lot of the detail of the displayed curves. Why not use the same ratio of y-units (GtC/yr) per cm, but have min- and max value different in the panels? This would allow for showing more detail while still preserving the correct size of the displayed variability for comparison among the panels.

We have modified the figure as suggested.

Spreadsheet:

Explain the red colours: do these denote the preliminary extrapolated values or are they changes since the last update of the global carbon budget? If the former, why are the values for 2012 in the fossil table red, while in the main text the UNFCCC/CDIAC data go all the way

to 2012? Or if the latter, then the corresponding numbers in the ocean and land sink (e.g. for 2014) should also be coloured.

Red colours denote preliminary values (see cell B7 - Territorial Emissions CDIAC worksheet). CDIAC values only go to 2011, whereas UNFCCC data runs from 1990 to 2012 and is denoted in green when it appears in the spreadsheet (see cells B6 to B8 - Territorial Emissions UNFCCC worksheet).