

## Review global carbon budget ESSD-2016-51

Overall a very strong product with substantial scientific and political impact. All data well organised and easily and freely accessible. I applaud the global carbon project for this continuing effort to deliver an accurate annual account of the carbon system. Apparently a good fit to the journal as well.

I note author name and sequence changes from prior (2015) description. Assume changes and updates in references as well. None of these changes highlighted in red? Assume editor(s), journal publishers and authors will sort this? To check to ensure full proper author list for citation and confirm fidelity of references list to references cited in narrative?

This reviewer has a small conceptual worry about the automatic attribution of all annual variations to, by definition,  $S_{\text{LAND}}$ . Any annual budget calculates two emission terms, measures the atmospheric concentration, and calculates with reasonable accuracy the ocean sink,  $S_{\text{OCEAN}}$ .  $S_{\text{LAND}}$ , as the residual and least tightly constrained (plus / minus 0.9 GtC, compared to at worst 0.5 GtC for any other term) factor automatically sweeps up all the annual variation. Thus a statement such as in the abstract lines 31 and 32 “is expected to be near record-high because of the smaller residual terrestrial sink ( $S_{\text{LAND}}$ ) in response to El Nino conditions in 2015-2016” becomes a bit circular? In crude terms, if we assign all unexplained variability to  $S_{\text{LAND}}$  as a default, then  $S_{\text{LAND}}$  necessarily always becomes the preferred reason for annual variability? I suppose the authors argue correctly that reliable and persistent uncertainty estimates on the other four terms of the budget justify this assignment to  $S_{\text{LAND}}$ , but this reviewer wonders if we have missed something, or if we conveniently rarely ask the question of what else we might have missed based on the convenient option to always assign an imbalance to  $S_{\text{LAND}}$ ? The authors correctly provide several hints about other factors (fugitive CH<sub>4</sub> emissions or lateral fluxes across coastal boundaries mentioned on page 5, nutrient-dependent changes in ocean carbon cycling mentioned on page 26, etc.) but seem confident in discarding those factors. Later statements highlighted this concern, especially that the authors report medium confidence for both the ocean sink and land sink but assign all explained variability to land sink. I note that section 2.7 does a very good job of addressing these issues, but strangely again ends mostly in affirmation of prior assumptions. In addition to the quantitative summaries of uncertainties in the overall budget terms, we need some small positive or cautionary sentence or two about what we know and where we remain most vulnerable to what we don't know?

Page 14, line 27: “assuming a 2% improvement in coal energy content”. Do the authors mean a revision in the thermodynamic energy produced by coal consumption, e.g. kJoules per kg burned, or a change in the carbon intensity of the Chinese economy as discussed earlier in this paragraph, those changes due to socioeconomic factors?

Page 38, lines 8 and 9: “to improvements in energy content of coal at the top of the range.” Same issue as above? Does this imply a change, perhaps a deliberate change, in the thermodynamic energy content per kg of coal burned, or a social change in the efficiency of using that coal energy? I do not understand “at the top of the range”?

Section 3.2.2, especially page 39: As I remember Betts et al. 2016 (cited elsewhere in this paper but apparently quite relevant here as well), who primarily relied on CO<sub>2</sub> extrapolations based on global average SST rather than the careful budget accounting as reported here, estimated 1 ppm (with a large uncertainty?) increase on top of the 2.1 ppm expected annual CO<sub>2</sub> increase assignable to ENSO conditions and processes. But later this paper reports increase of 2.1 ppm for just the first 6 months of 2016? A final accounting of increased  $G_{\text{ATM}}$  for 2016 will certainly hit 3.0 but plausibly might hit closer to 3.5 ppm? Can we really assign that all to change in land surface sources and sinks caused by ENSO? For example Betts et al mention large-scale fires in Indonesia having an impact of ‘only’ 0.2 ppm. Should we, would we not have already observed a land disruption of sufficient magnitude to cause a CO<sub>2</sub> increase 1 or 1.5 ppm?

Page 41, line 7. Up to this point the reader has encountered many CO<sub>2</sub> concentration reports as ppm or GtC, always on annual or even decadal time scales. Here for the first time we encounter a 6-month estimate. The authors could help the readers with a small adjustment to make that point earlier and clearer in the sentence? E.g. "*in the 6 month period* between December 2015 and June 2016 was already 2.1 ppm (Dlugokencky and Tans, 2016) after seasonal adjustment".

Page 41, lines 10 to 17. This represents a good and necessary discussion! But in this section the authors have abandoned all confidence limits or uncertainty estimates. Perhaps a concluding sentence here, about the medium or low confidence assigned to this assumption (that all 2016 changes will occur due to land surface process and none to ocean or other uncertainties) would satisfy my earlier concern about where the cumulative uncertainties leave us vulnerable to mis-interpretation. E.g. the substantial uncertainties in net land and ocean CO<sub>2</sub> fluxes in NH in recent years (Figure 8)?