

Author Response to Interactive Comment on “Towards a more complete quantification of the global carbon cycle” by Kirschbaum et al.

Miko Kirschbaum and co-authors

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Response to Reviewer #1

Reviewer comment: This is a novel and interesting paper that should stimulate discussion around this important topic. It brings together a quantification of many relatively small elements of the global carbon cycle that when combined could make a substantial reduction in the “residual sink” that has typically been assigned to the terrestrial biosphere. The paper further
10 *makes explicit some aspects which had previously been implicit in the budget – as the authors state, this improves clarity. Overall, the implications for vegetation modelling could indeed be substantial as there is an implication that current global vegetation models (which simulate an imbalance within the uncertainty of the residual sink; Le Quéré et al., 2018) may be overestimating the carbon sink provided by the biosphere (but see comment below about how this discussion is presented).*

Response: We would like to thank the reviewer for this positive overall assessment. The comment clearly summarises
15 **what we had intended to do with this paper.**

Reviewer comment: Many of the estimates included have been published elsewhere or are novel contributions but very provisional. This paper will certainly not be the last word on those numbers, however the important thing that this paper does is to bring them all together in a consistent format and set them in the context of the global carbon budget. Careful attention has been paid to whether the fluxes considered are omitted or considered implicitly in the Global Carbon Budget as presented
20 *by Le Quéré et al. (2018). I recommend publication subject to addressing the concerns below*

Response: Again, we thank the reviewer for this positive overall assessment. We also acknowledge that our paper will not be the last word on these numbers. For that reason, we have entitled it ‘Towards a more complete quantification of the global carbon cycle’. Global carbon budgets are continually evolving to reflect changing real-world fluxes, advancing scientific understanding, and the conceptual terms used to summarise observed or inferred fluxes into
25 **quantities that are deemed to be relevant to the scientific and policy-making community. Our paper aims to contribute towards that process of continual improvement.**

Reviewer comment: Major comment My only substantial concern relates to Section 12. The results presented by Kirschbaum et al. potentially tie in with very active discussion over the extent to which CO₂ fertilisation of leaf photosynthesis is propagated through to ecosystem-level increases in carbon storage (e.g. Körner, 2017, 2006; Luo et al., 2004; Medlyn et al., 2015). It is
30 *relevant to mention this however I find Section 12 generally a step too far. For instance, in section 12.1 it is stated “any carbon*

uptake by forests is likely to be largely due to their disturbance history”. This is a valid and highly-relevant hypothesis, but it is only a hypothesis. We currently do not know the relative contributions of CO₂ fertilisation versus forest demography with any certainty. This should be reflected in the discussion.

Response: It had not been our intent to provide a conclusion on that ongoing debate about the various contributing factors. The specific statement in question that ‘any carbon uptake by forests is likely to be largely due to their disturbance history’ was meant to primarily refer to the pattern in individual stands for which the normal growth cycle presumably over-rides any other growth-promoting factors. We had not intended it to be seen as directly applicable to global forest carbon balances.

We have therefore changed that section now primarily by removing that offending sentence. We have also further restructured that section with some additional minor wording changes. We hope this rectifies the concern expressed by the reviewer.

Reviewer comment: Similarly, Arneth et al. (2017) is cited relating to the importance of biophysical drivers (pg. 12, line 19), but a key conclusion of Arneth et al. is that because the land-use and management change emissions may be systematically underestimated in the budget, this implies that the terrestrial “residual” sink may have previously been underestimated. Thus, it may be that the calculations presented by Kirschbaum et al. do not imply an overestimation of the carbon sink in global vegetation models, but instead account for a missing portion of the budget that balances previously underestimated land-use and management change emissions. This possibility should be explicitly laid out.

Response: To capture the point made by Arneth et al. (2017), we have added an extra sentence to Section 12.1: ‘*subtler disturbance related effects on woody biomass are difficult to capture fully at the global scale and may have led to past underestimation of land-use change related carbon emissions (Arneth et al. 2017)....*’

Reviewer comment: Finally, the soil organic carbon section (12.4) is extremely speculative and doesn’t really fit in the framework of the manuscript. Yes, a change of 0.4

Response: The reviewer’s comment ended abruptly, and we are not sure what (s)he intended to say to complete the review point. At the same time, we agree with the reviewer of the speculative nature of this Section, but that is precisely the point it was trying to make. Changes in soil carbon constitute the largest unknown contribution in the global budget. We may be able to improve the quantification of various flux by 100 MtC yr⁻¹ or so, but at the same time, soil carbon may change by 1 GtC yr⁻¹ in one direction or another without anyone being able to quantify it. We need to remain conscious of the uncertainty in our budget estimates when soil-carbon changes alone have such a large level of uncertainty. We, therefore, believe that this is an important section of the paper and have retained it.

Reviewer comment: Overall, in my opinion this section needs to be much more balanced, laying out the various competing hypotheses, so as to reflect a review, rather than an opinion piece.

Response: We are unsure what ‘section’ the reviewer is referring to here. If the reviewer is referring to Section 12.4, we see little ‘opinion’ in that section as we merely point out the existing uncertainty. If the reviewer refers to the sum-

total of Sections 12.1 to 12.4, we aimed to do exactly what the reviewer has asked us to do: we very briefly summarised the main fluxes that could contribute to an enhanced global terrestrial sink. We tried to avoid any conclusive statement as to our view of the contributing components but simply summarised the existing literature. We are unsure what else the reviewer might want us to do to those sections.

5 *Minor comments*

Reviewer comment: Pg. 1, line 38. “net additions”? “the oceans overall are”?

Response: Changes made as suggested.

Reviewer comment: Pg. 2, line 5. The budget is based on terrestrial biosphere models (TBMs) run offline, not Earth System Models.

10 **Response: Change made as suggested.**

Reviewer comment: Pg. 4, line 11. Ro or Rd?

Response: It should have been Rd. Change made to correct that.

Reviewer comment: Pg. 5, para 2. Wood product pools are included in many, if not all, of the TBMs used in Le Quéré et al. (2018). Stocks have rarely been published, which unfortunately does not facilitate a comparison, but this flux has not entirely been neglected. This should be recognised in the text.

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Response: We were aware of that inclusion of wood products in past budgets and referred to it in the original text on three separate occasions:

Page 2, line 34: This flux [wood products] has already been included in net land-use change calculations (Le Quere et al., 2018), ...

20 **Page 4, line 30: Le Quere et al. (2018) included a simple term in the calculations of net land-use change that accounted for harvested-wood products.**

Page 9, lines 8-11: For greater transparency, it would also be desirable to explicitly include harvested-wood products and landfill pools. The associated carbon flux is already included under the net-land-use calculations (Le Quere et al., 2018). Inclusion of a harvested-wood-products pool, therefore, would not affect the size of the residual sink, but it would require a corresponding adjustment of the net land-use-change flux.

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We believe that three mentions of that inclusion of wood products in prior budgets is adequate, if not excessive already, and believe it would not be warranted to refer to its inclusion yet another time.

Reviewer comment: Pg. 7, line 8. “some extra inputs mineral weathering” – does not seem to make sense. Please rephrase.

Response: This sentence needed an extra ‘from’ to say ‘some extra inputs from mineral weathering’. That has now been corrected.

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Reviewer comment: Pg. 7, line 10. Cole et al. 2009 or 2007 (cf. Table 2)?

Response: Thank you for spotting that inconsistency. It should have read ‘2007’ in all references to ‘Cole’. That has now been corrected.

Reviewer comment: Pg. 7, line 18/19. Repetition of material from two paragraph previously.

Response: This partial repetition stems from the initial mention in a context where it simply listed all river related fluxes and storage items, while the second mention relates it to the fluxes and quantities that are relevant to the global carbon budget. We, therefore, regard some repetition as appropriate because the contexts are slightly different.

5 **However, we have shortened both sections to reduce the extent of that repetition.**

Reviewer comment: Table 2 is not the easiest to follow. Use of vertical lines for grouping into sections and bold text to highlight the values being carried forward would help readability.

Response: To improve an understanding of the flow and grouping of the table, we have bolded our resultant estimate to indicate the numbers being carried forward. We have also omitted some of the vertical lines so that the retained

10 **vertical lines now indicate the logical grouping of some of the values.**