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 #2

Reviewer comment: *The manuscript by Kirschbaum and others is a well-written summary of existing estimates of small C fluxes that should not be excluded from global C syntheses, as the authors demonstrate. I feel that it is publishable after the authors consider a number of minor points for clarity and a few more major revisions regarding deposition pathways. Namely, some dry and wet deposition terms are attributed to a flux to the ocean but in reality go to both land and ocean. In a few instances the authors appeared to be overly critical of existing budgets without justification in my opinion.*

Response: We like to thank the reviewer for the favourable overall assessment of the manuscript. We have addressed the specific points of criticisms in our itemised responses below.

Reviewer comment: *The paper would also very strongly benefit from a table of abbreviations (especially equation 1!).*

Response: We have general sympathy with the notion that it can often be useful to provide easy access to abbreviations used in any paper. However, in this specific paper, almost all abbreviations are used only once – for specific equations – and then immediately described in the text adjacent to the respective equations. The only abbreviations used on more than one occasion were ΔB and ΔB_{incLUC}. We have renamed these symbols now into ΔB_{phys} and ΔB_{act} to stand for biomass changes due to physiological factors and actual changes, respectively. We regard these terms as much more intuitive. We expect that with this change, it is no longer warranted to include an additional table of abbreviations.

Reviewer comment: *Figure 1 is nice but doesn't link pools and fluxes with the abbreviations used in the text.*

Response: We are unsure how to respond to that comment. Figure 1 gives the very abbreviated form of the global carbon cycle, with only five identified fluxes (fossil fuels, cement manufacture, land use change, ocean uptake and residual terrestrial uptake). The main purpose of our paper was to present a more differentiated picture and add additional pools and fluxes to the budget. So, the very essence of our paper is that these additional pools are not included in the simplified version of the global budget. Showing that difference is the essence of our paper. Thus, there is little correspondence between the pools and fluxes in Figure 1, and our more complete list of pools and fluxes. It would thus be impossible to do what the reviewer is asking us to do.

Reviewer comment: *In section 2, 'The shallow ocean is too small for significant carbon storage, but the deep ocean has a huge carbon-storage capacity' seems inconsistent with the goal of the paper to quantify small C fluxes*

Response: We see no inconsistency between there being a 'huge capacity', yet there being only a relatively small annual flux into that reservoir. The relatively small annual flux is still large and important in relation to the anthropogenic disturbance of the system even though it is small relative to the potential magnitude of carbon storage in the deep ocean. It just means that the flux into the reservoir has virtually no feedback effect on subsequent fluxes into that reservoir. The flux, is instead controlled by other factors. As we see no inconsistency between these statements, we have made no changes to the text.

Reviewer comment: *'As these organisms are eaten by larger organisms' is true, but small organisms also die.*

Response: We have modified that statement to include the possible extra carbon fate.

Reviewer comment: *Regarding 'However, we believe that a more explicit representation of this pool would be desirable for greater transparency.' Yes, everyone does, but writing it as such doesn't make it clear if this will be addressed in the paper.*

Response: We have an extra part to this sentence to make it clear that such quantification is part of the present paper.

Reviewer comment: *'However, under anaerobic conditions, breakdown effectively ceases completely' and 'never breaks down' are slight elaborations. Over meaningful time scales to the contemporary climate system perhaps. (See also Table 1 'permanently'. Readers with a long view of time may disagree.)*

Response: The text tries to make that assessment within the context of the contemporary carbon cycle, which is the relevant focus of the present paper. The statement is not meant to refer to a geological context. We have therefore modified that sentence to indicate that permanence refers only to a time frame relevant for carbon management.

Reviewer comment: *Wording can be simplified in many places. For example, 'Forbes et al. (2006) estimated this flux to be only small at less than 10 MtC yr–1. Could lose 'only small at'.*

Response: We have worked through the text once more and further tightened and simplified any text where appropriate.

Reviewer comment: *'any transfers to the ocean' in section 9 could also be transfers to land to the extent that NMVOCs create aerosols and cloud condensation nucei that are subsequently deposited to the surface at some point. Later in the section dry deposition (can also be wet deposition) is mentioned. This needs to be integrated more strongly with the material above. Figure 7 also needs to be modified; dust, NMVOCs, charcoal and the like also land on land.*

Response: We are well aware of the facts mentioned by the reviewer, and these factors have been properly included in our analysis:

- **The dust deposition used in our calculations based on the work of Mahowald et al. (2005) specifically refers to dust transfer from land to oceans.**

- **The estimate of charcoal transfer of Forbes et al. (2006) specifically referred to charcoal transfer to the oceans**
- **For the transfer of NMVOC-derived compounds, we explicitly estimated the proportional deposition over land vs the oceans. This is described in detail in Supplementary Materials: "For species that are subject to dry and wet deposition, we partitioned the ocean flux as follows. We used the modelled global distribution of dry deposition fluxes to the Earth's surface of each species and accounted for deposition to the ocean using the model's land-sea mask information. Total dry deposition to the land and the ocean were then calculated by integrating the respective fluxes over the land and the ocean. In the model version used here, wet deposition fluxes were output as zonally averaged 2-dimensional fields. Therefore, we needed to partition the global wet deposition fluxes to the ocean using 3-dimensional global distributions of the species and weighted them by the global distribution of total precipitation rates."**

So, we fully agree with the reviewer's position by the reviewer and are well aware of the importance of separating ocean and land deposition, and we believe that throughout the paper, we have used the appropriate data sources for estimating fluxes to the oceans.

Reviewer comment: *Section 12.1 for some reason dismisses a large body of literature demonstrating that 'older' forests can take up substantial amounts of carbon, e.g. https://www.nature.com/articles/nature07276.*

Response: Section 12.1 stated: '*Forest growth tends to be highest in young stands and decreases as stands age'*. That position is well-supported by the general forestry literature. Even the nature article by Luyssaert et al. (2008) cited by the reviewer agreed with that statement and showed that net ecosystem productivity of younger stands was about twice as high as that of older stands and trended towards carbon neutrality for the oldest stands in their data set.

At the same time, we also agree with the reviewer's point that 'older forests can take up substantial amounts of carbon'. We make no statement that would contradict that position. We simply state that younger forests can have a higher net ecosystem productivity than older forests.

So, our statement is well supported by a large body of forestry literature, including Ryan et al. (1997) and Kurz and Apps (1999) that have been cited in our paper, and are not contradicted by the Luyssaert et al. (2008) paper referred to by the reviewer. We, therefore, believe that this criticism is not justified, and the reviewer criticises statements that are not actually made anywhere in our paper. That makes it difficult to know how we could respond to that criticism.

Reviewer comment: *This sentence is an overly-harsh critique of the hard work that goes into global carbon budgeting: However, the global carbon budget in its currently used simplified form is incomplete and, therefore, does not provide appropriate guidance on the way anthropogenic and natural processes interact to lead to the observed increases in atmospheric concentrations.*

Response: We do not mean to be harshly critical of the global carbon budget. We recognise that overall, it provides timely and relevant information on the key carbon fluxes. Nonetheless, as we are trying to point out in our manuscripts, there are additional tweaks through inclusion of additional fluxes that would make the budget even more accurate, and that this tweak would have important consequences for our overall understanding of the current role of the biosphere,

in particular. So, we think the essence of our statement is correct, but we have reworded it to make it sound less critical of the valuable ongoing work on the global carbon budget.

Reviewer comment: *Table 2: waterway is one word.*

Response: That has now been corrected throughout the manuscript

Reviewer comment: *Simultaneous red and green should be avoided in Figure 5.*

Response: We have redrawn the figure to avoid that colour conflict.

Reviewer comment: *Figure 6 is somewhat underwhelming.*

Response: Our paper is trying to communicate with a wide range of potential readers with varying levels of background knowledge. Figure 6 probably contains little information for the expert reader, but we do believe that for a reader with less expert knowledge, the figure provides a useful short summary of the relevant fluxes and the main compounds contributing to those fluxes. We have, therefore, retained that figure.

References

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