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# *Interactive comment on* "The Climate Benefit of Carbon Sequestration" *by* Carlos A. Sierra et al.

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We thank the reviewer for taking the time to review and comment on our manuscript, and regret that he finds this manuscript 'not a meaningful contribution to the climate literature'. We understand that in the current version of the manuscript, the link to state-of-the-art Earth system models (ESMs) is not completely clear. However, we would like to clarify that the concepts we introduce are very general and can deal with models of any level of complexity, working at different scales, and developed for different purposes. The compartmental approach we use works for simple 'outdated' models as well as for highly complex models. We have written extensively in the past about this generalization approach (Sierra and Müller, 2015; Metzler et al., 2018; Sierra et al., 2018; Ceballos-Núñez et al., 2020), and similar ideas are also well developed in papers by Dr. Yiqi Luo's group (e.g Luo and Weng, 2011; Xia et al., 2013; Luo et al., 2017). Unfortunately, we do not have much space in this manuscript to demonstrate the



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generality of the compartmental framework, but we have done exactly that in previous publications. Similarly for the limitations of the concept 'residence time', for which the reviewer asks why we do not show the limitations of such a concept. We also have written a number of publications about issues with this concept and the common methods to compute it (ratio of stock over flux). We developed new mathematical approaches to advance on this subject (Metzler and Sierra, 2018; Metzler et al., 2018) and this manuscript is a step further in the application of the new methods. Again, we feel this manuscript is not the appropriate venue to elaborate on the 'residence time' issues and the new methods. Instead, we provide a presentation on these ideas, show with more detail some of the formulas in the appendix, and provide appropriate references, but can't go in more detail. Also, we want to point out that the problem on how to account for time in carbon sequestration has been a long-standing issue, with important debates in forestry (e.g. Fearnside, 1995; Fearnside et al., 2000; Sedjo and Sohngen, 2012), ecological economics (e.g. Moura Costa and Wilson, 2000), and ecosystem management (e.g. Neubauer and Megonigal, 2015). We think we provide here a relevant contribution to those previous debates. This may not be very obvious for researchers currently working on climate feedbacks, but it is a topic that touches on different disciplines and we think it is a meaningful contribution to the overall topic of carbon sequestration in natural and anthropogenic sinks.

Below, we provide specific answers to the main issues raised by the reviewer (in *italics*), with a description of corresponding changes in the manuscript to address those comments.

Answers to specific comments

- *To justify the development of this metric, the authors would need to make a compelling case for where existing, widely-used related metrics of carbon sequestration (such as carbon residence time and net ecosystem productivity) fall short,*





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*and why this new metric is superior (or at least complementary). However, there is hardly any mention of these existing metrics in this manuscript.*

We discuss in our manuscript the limitations of the use of global warming potential (GWP), which is the most common metric to assess climate consequences of carbon management. Limitations of the concept of residence times have been already published in Sierra et al. (2017), where we elaborate on the need to distinguish between the concepts of system age and transit time. The present manuscript is a further development of the concept of transit time to show that it can be used to quantify the climate benefit of having carbon stored in ecosystems during the time it remains there. Therefore, we focus not in showing the limitations of the ambiguous concept of residence time, but rather in showing the power of the transit time/age framework. The manuscript did not elaborate on limitations of the concept of net ecosystem production (NEP) to assess carbon sequestration, although its limitations are somehow intuitive based on Fig 1 and the text in the introduction. NEP provides a net flux between the carbon exchanged between ecosystems and the atmosphere, without accounting for harvest exports. It does not tell you for how long the carbon in the output flux stayed in the ecosystem. Two different ecosystems with similar NEP values could have very different carbon storage values and transit times, so this concept is not very useful to assess carbon sequestration, particularly for long time scales.

To address these issues, we added text in the introduction briefly mentioning our previous work on the ambiguity and limitations of 'residence time'. We also elaborate more on the limitations of studying either gross (e.g. GPP) or net (NEP) fluxes to assess carbon sequestration.

We also would like to mention that our CBS concept just tells something different that other metrics do not tell. It combines in a single metric the amount of carbon that enters a sink and the time it remains there. Previous metrics simply do not provide this information in an integrated form.

- *For that matter, the manuscript's application of CBS to terrestrial C cycle models focuses only on dated and/or simplistic models and makes no mention of recent syntheses of terrestrial carbon cycling and associated climate feedbacks by modern land surface models—for instance, Friedlingstein et al. 2014 (https://doi.org/10.1175/JCLI-D-12-00579.1) or Heinze et al. 2019 (https://doi.org/10.5194/esd-10-379-2019)—which makes it difficult to draw meaningful conclusions from those results.*

  We chose a 'dated' model because, to introduce a new complex metric, we believe a simple model is more effective and transparent than a complex Earth system model. With an ESM, potential users of this framework would not have the opportunity to test results if they do not have access to the source code of the ESM and to a supercomputer. A simple model allows readers to test the framework with very simple code. We believe this is a more transparent approach for a paper that introduces a new concept. Future applications can of course be implemented in large ESMs, but that is beyond the scope of this manuscript.

  However, a simple model like the one we used does not have any feedbacks with other components of the Earth system. Nevertheless, these feedbacks can be part of the atmospheric impulse response function. In fact, the IRFs of Joos et al. (2013) include all feedbacks that are part of the ESMs of the CMIP5 generation. If one uses these IRFs for the computation of the CBS, it is not necessary to include feedbacks in the terrestrial carbon model as long as simulations do not deviate much from the original simulations used to create the IRF. Alternatively, one can use an ESM and compute the CBS directly from the net biospheric fluxes and the particular IRF of the model. This would require a complex simulation setup, that again is beyond the scope of this manuscript. More importantly, we would like to emphasize that our aim is not to produce state-of-the-art calculations of the CBS for the terrestrial biosphere, but rather to introduce the concept and the mathematical theory behind it. For this reason, and also based on the comments





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from reviewer 2, we decided to remove some of the examples and give more emphasis to the development and explanation of the concepts. After all, this is a theoretical paper and not a state-of-the-art analysis of the actual climate benefit of sequestering carbon in the terrestrial biosphere.

- *Ultimately, I cannot recommend this manuscript for publication beyond the Discussion format. I would encourage the authors to carefully read a recent review of terrestrial C cycling and its climate implications and consider how CBS fits into that context*

  We will check the recent carbon cycle literature to see if we are missing something. We have coauthored a good number of papers on the carbon cycle, including reviews. But indeed there might be something we are missing. We would have hoped the reviewer to point out more clearly what exactly are we missing. Without a clear indication of what particular concepts are problematic in our framework, it is impossible for us to guess them.

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