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Dear Editor,

Thanks for the opportunity to prepare a revised version of the manuscript addressing comments from two reviewers. We have addressed most of the comments and have made changes accordingly in the revised version of the manuscript.

You can find point-by-point answers to all reviewers' comments below. These are the same answers that were already provided in the discussion forum.

Reviewer 1

This is a well written piece. The authored argued why carbon allocation should be based on GPP rather than NPP, as often done in most land surface or ecosystem models. These models assume zero residence time of respired carbon through autotrophic respiration, which contradicts the field measurements of the age of respired C from plants.

We thank the reviewer for recognizing the relevance of our manuscript.

However, there are several significant limitations with the proposed approach: (1) ecosystem GPP cannot be measured directly; where NPP can be easily measured in the field and with a large number of observations available globally;

Measuring GPP in the field is challenging, but so is measuring NPP. In a comprehensive review on the NPP concept and its quantification, Clark et al. (2001, Ecological Application 11:356) stated that 'Forest NPP cannot be measured directly; it must be approached by indirect methods'. These authors convincingly showed that there is a significant discrepancy between the concept of NPP and the way it is measured in the field. Most field studies provide only an approximation to the real value, very likely with an underestimation. Few studies quantify belowground NPP, and many only quantify one component of NPP, usually wood biomass production. Although GPP is also difficult to quantify from field measurements, eddy-covariance estimates of GPP are providing now a wealth of data from a large number of ecosystems world wide. Synthesis efforts such as Fluxnet and Fluxcom provide well-curated data-products of global GPP. In particular, Fluxcom combines remote sensing information with eddy-flux data to produce global gridded products of GPP at high spatial and temporal resolution, which could be of immense value for modeling studies. MODIS GPP is an additional product that has been very useful in providing approximations to ecosystem level GPP for a number of empirical and modeling studies. Therefore, we do not think that this concern from the reviewer is valid given that both NPP and GPP are difficult to obtain, and information from both can obtained from the literature for a large variety of sites.

(2) autotrophic respiration of individual biomass components are rarely measured, therefore allocation fractions of GPP to individual biomass components often are not available at ecosystem scale;

We agree with the review in that there are currently few observations of biomass and respiration for individual pools, which are rarely measured in field studies. However, we do not see this point as a limitation, but rather as an opportunity for new scientific studies that would combine pool-specific measurements with model development of respiratory processes in these pools. We think it is important to highlight the limitations of current models in our manuscript so new improvements are developed. This is better than pessimistically conclude that there is currently not enough data so models cannot be improved.

(3) to model the age of respired C in an ecosystem or land surface model using the proposed approach by the authors here, we will need to represent the total carbon in each biomass component using multiple pools with different availability for respiration and their responses to stresses, which will introduce quite a few additional, poorly constrained model parameters, and additional uncertainties in the model. On the other hand, most of the respired carbon is less than one-year old in leaf, and much younger than the mean age of woody carbon in stem. In my view, the authors should provide a more balanced view of the pros and cons of GPP-based and NPP-based approaches for carbon allocations, and their potential limitations.

Yes, the proposed approach would likely introduce a set of poorly constrained parameters. But the current approach of allocating carbon from NPP has also a set of poorly constrained parameters that cannot be optimized with the new set of data on the age of respired CO2. Our aim here is not to show what cannot be done with the current models, but rather to show what can be done in the future with models that are consistent with new data on respiration from specific pools and their radiocarbon content. There are large opportunities to develop new data-assimilation studies testing different allocation approaches based on GPP, and incorporating new datasets on respiration.

Although we showed that a large proportion of carbon is respired in less than one year, we also showed that a small proportion is respired several years after fixation. To understand the transit time of carbon in ecosystems we need to better understand both, processes occurring at fast timescales and processes occurring on longer timescales. However, the current approach of allocating carbon from NPP does not allow us to study these ranges of timescales.

We understand the concern of the reviewer in that our statements may be too strong, and we tried to balance our text in the new revised version. Nevertheless, we think it is important to clearly identify the limitations of current models, and also the opportunities for future studies.

Some detailed comments Figure 1. I disagree with the statement that majority of the models assume "a constant proportion of GPP". I know that ACCESS-ESM, GFDL, NorESM2, BCC are not. Not sure about other models. Because of the pool-size based approach, there is a negative feedback between respiration and pools, therefore the ratio of autotrophic respiration of GPP is rather constant when averaged over a year globally.

The reviewer is right on this point. The majority of models do not have a constant allocation scheme but rather show a relatively constant proportion when aggregating globally. We modified the text in this section to decrease emphasis on constant respiration ratios. We also eliminated Figure 1 for the same reason.
Nevertheless, we would like to emphasize that the issue is not that models have constant allocation coefficients. The issue is the source of carbon for allocation, NPP rather than GPP. We made changes in the manuscript to put less emphasis on the constancy of C allocation, and more emphasis on the source of carbon.

L121. CABLE does not assume a fixed fraction of GPP being respired by plants. See Wang et al. (2012), GEOPHYSICAL RESEARCH LETTERS, VOL. 39, L19405, doi:10.1029/2012GL053461.

We apologize for this inaccuracy. In our original review, we used an earlier description of CABLE to determine the type of allocation scheme (Wang et al. 2010, Biogeosciences 7:221), and overlooked the more recent model description. We updated now the text based on this more recent reference.

P9, the last two lines below Figure 3. "the median transit time is 0 yr, because the autotrophic respiration flux, which corresponds to 50% of GPP..". That statement may be specific to the model of Emanuel et al. (1981) and that model is more than 40-year old! In most land surface models, Ra/GPP is not constant, particularly at daily or seasonal time scale. The median transit time would have to be weighted by the carbon flux, then the median transit time will not be zero!

The statement is indeed specific to this model, and the specific value may change for other models. However, we noticed a potential misunderstanding in this comment. In the majority of models, allocation is done from NPP and Ra is subtracted immediately from GPP as we show in the manuscript. It does not matter if the median transit time is flux corrected because the age of the respired carbon is still zero (i.e., age_C_in_Pool * respired_flux_from_pool = 0 * respired_flux_from_pool = 0). Also, we used here the model of Emanuel et al (1981) because it is a useful model to make our point clearly. It has a minimum of complexity to show the differences between allocation schemes without additional details that would be irrelevant and would obscure our point with extra complexity. The model may not be appropriate to make specific numerical predictions, but it is very useful as a reduced complexity model that helps to express ideas clearly.

Reviewer 2

In this manuscript, Sierra et al. proposed that carbon allocation should be from GPP instead of NPP in ecosystem models since NPP-based models assumed autotrophic respiration only consumed fixed carbon immediately without transit time. They introduced the conceptual development of carbon allocation from NPP, reviewed 18 ecosystem models, and analyzed the distributions of carbon transit time between the two types of modeling schemes. The authors also showed that the NPP-based model conflicted with empirical evidence on plants' age of respired carbon. Overall, I enjoyed reading this paper. The logic of the manuscript is clear, and the presentation of the ideas and perspectives is precise and neat. I agree with the authors that there are many advantages to modeling carbon allocation from GPP. From a modeler's perspective, I found that some statements in the current version of the manuscript may need more discussion. From the perspective of an empirical ecologist, there are still some limitations to using GPP instead of NPP for carbon allocation. Please find my major and specific comments below.

We thank the reviewer for the accurate description of our manuscript. We followed these recommendations in a revised version, trying to balance the modeling challenges with the available observations.

Major comments: GPP and carbon allocation are simulated with different time steps in many process-based ecosystem models. For example, GPP is commonly simulated during the daytime with a time step from half to three hours, but carbon allocation is updated daily. If the models adopted the GPP-based carbon allocation scheme, they have to improve the representations of diurnal changes in many processes related to plant growth and carbon allocation. These improvements could dramatically increase the complexity of the canopy module in the model. Some additional discussions on this issue could help modelers better understand the challenge of adopting the GPP-based carbon allocation scheme.

Thanks for the suggestion. We mentioned the differences in time-steps among the different processes involved, but did not mention the challenges for representing allocation at a timescale compatible with the representation of GPP. To address this issue, we added a paragraph in Section 2.2 adding more detail on these time-step differences, and discussed this challenge in the recommendations section.

Figure 1 showed a constant Ra/GPP ratio in most CMIP Earth system models. This pattern has also been reported in terrestrial ecosystem models (e.g., https://doi.org/10.1002/2016JG003384). As mentioned by the authors, a critical question is how to improve the modeling of autotrophic respiration in the models. Unlike leaf photosynthesis, the Ra scheme varies greatly among current ecosystem models. For example, in the CLM4.5 model, the growth respiration (Rg) is calculated as a factor of the total carbon in new growth on a given timestep, based on construction costs for a range of woody and non-woody tissues. The maintenance respiration (Rm) in CLM4.5 is a temperature function based on a base rate of Rm. However, in the JULES model, Rm is simulated from a moisture and nitrogen function based on dark leaf respiration. Rg in JULES is further calculated as a fraction of the difference between GPP and Rm. The authors have reviewed Rm in different models in section 2.1. It would be better if they could provide some details of the modeling of Ra in some specific models.

Reviewer 1 correctly pointed out that the pattern presented in Figure 1 emerges by aggregating the data globally, but in fact most models represent a dynamic allocation pattern. However, we would like to point out that the emphasis of our analysis is not that allocation is a constant proportion of GPP. This has been shown in other analyses as in the reference provided by Reviewer 2. The emphasis of our analysis is in the source of the carbon used for allocation, independent of whether allocation is a constant fraction of GPP. Therefore, we decided to reduce the emphasis in the text on this aspect, removed Figure 1, and give now more relevance to the source of carbon for allocation.

We like the suggestion of the reviewer of giving more detail on how some models represent carbon allocation. Although we do not want to give the impression that one or two models are representative of the allocation schemes of all models, we try to give some more details on CLM4.5 as suggested by the reviewer.

I agree with the authors that ecosystem models need to incorporate the non-structural carbon (NSC) pool dynamics. Adding the NSC pool into the equation (1) or (2) could affect the solution of carbon transit time because it changed the pool-flux structure in fig. 2. I'd like to suggest the authors discuss whether and how adding the NSC pool can influence the distributions of carbon transit time. Also, if we have enough data to parameterize the age of the NSC pool in the models?

As suggested by the reviewer, we added a section discussing in more detail the addition of a non-structural carbon pool. In particular, we make reference to the ACGCA model of Ogle and Pacala (2009), as well as some of our previous work (Trumbore et al. 2017, Ceballos et al. 2018, Herrera et al. 2020) where we modeled a NSC pool with an associated distribution of carbon age. This previous work shows that NSCs used for allocation and respiration can have ages as old as a decade. The NSC reserves can support metabolism during periods of no or little photosynthetic inputs, so they play an important role for representing potential threats due to droughts and extreme events. The distribution of ages for the NSC compartment helps to explain observed ages in respired CO2, and can be used to constrain parameters of specific allocation functions in models. However, the number of available observations of ages in NSCs and respired CO2 are still very low, but we hope that more studies will report them in the future, hopefully motivated by what we discuss in this manuscript.

There are some benefits to using NPP-based carbon allocation, especially in global models. First, the NPP-based scheme consists of more measurable parameters than the GPP-based scheme. The increasing observations of plant traits can be helpful in constraining those parameters. This advantage could be important for those non-woody ecosystems, in which the carbon allocation can be approximated by the annual growth of different plant tissues. Second, because the GPP-based scheme may need to increase the complexity of the canopy process, the computation cost could increase dramatically for data assimilation. Third, GPP itself is unmeasurable, so that the GPP uncertainty could propagate to the carbon allocation.

Reviewer 1 had a similar opinion on the measurability of GPP and the increase in complexity in GPP-based schemes. Again, we believe both GPP and NPP are extremely difficult to measure and there is no single approach that can provide accurate estimates of both variables. Available measurements of NPP only make a crude approximation of the many different components that are often not measured (Clark et al 2001). With the new generation of GPP products such as those provided by Fluxcom, a wealth of information is now available on GPP at the global scale. Nevertheless, we understand the concern that many available observations on traits may not be used if there is a change to allocation based on GPP. We think this concern can be reduced if traits such as allometric relations are used in the development of new allocation functions. For instance, the Allometrically Constrained Growth and Carbon Allocation (ACGCA) model proposed by Ogle & Pacala (2009) allocates carbon from GPP, has NSC pools to store carbon before allocation and respiration, and uses allometric constrains to obtain biomass growth. This modeling approach, or a variant of it, would be a good candidate for implementing in new GPP-based schemes, and does not imply aadditional complexity in modeling canopy processes.

I also agree with the authors that radiocarbon data is helpful for improving the model. However, the measurements of radiocarbon are expensive in many countries. Maybe some introductions or discussions of available radiocarbon data from the ISRaD database are helpful for the readers.

We added more discussion on the use of radiocarbon. Although radiocarbon measurements are indeed expensive, they are now being collected more often and in many more studies as previously. One main development in the previous years has been the introduction of the Mini radioCarbon Dating System (Micadas), which has reduced costs and increased sample throughput in many laboratories around the world. Unfortunately, the International Soil Radiocarbon Database (ISRaD) only contains radiocarbon measurements from soils, and does not include measurements from plant parts and their respiration flux. However, there is potential that in the future a radiocarbon database for plants becomes available.

Minor comments: (1) It is better to give basic information about the function $f_a(\tau)$ *as described in Metzler et al. (2018). Some new readers could be unfamiliar with the matrix equation and its solution.*

We added more details on this equation as suggested by the reviewer.

(2) Fig.1: Please add a few sentences to briefly describe those Earth system models.

We removed this figure from the manuscript, so model descriptions are no longer needed.

(3) P13, L253-254: This statement might be too strong.

It may be strong but it is true. An NPP-based model cannot use radiocarbon data from respiration for model-data assimilation.

(4) The word "model" is used in different ways in the main text, such as ecosystem model, ecosystem carbon model, coupled carbon-climate model, land-surface model, carbon allocation model, etc. It is better to reduce the diversity of model types in the text.

Thanks for the suggestion. We adopted only one single description of model, ecosystem model, and made more specific descriptions according to the context.

We hope you find this version of the manuscript suitable for publication.

Best regards,

Carlos A. Sierra, on behalf of all coauthors