

## ***Interactive comment on “Leveraging the signature of heterotrophic respiration on atmospheric CO<sub>2</sub> for model benchmarking” by Samantha J. Basile et al.***

### **Anonymous Referee #3**

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Basile and colleagues compare three model formulations of heterotrophic respiration in their predictions of CO<sub>2</sub> generation to the atmosphere, and compare the predictions with observations of atmospheric CO<sub>2</sub> concentrations from a series of oceanic observations. I found the direct comparison of model formulations to be important and timely, given that the authors compared a CENTURY-like traditional formulation to more recent “mechanistic” models that explicitly simulate microbial processes.

In some ways, this is a well-written manuscript. The text has the crisp precision is a hallmark of good scientific writing. However, the manuscript is also challenging to understand and follow, in part because it uses jargon as well as many symbols and

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acronyms. I suggest that the authors embed summary sentences at the end of some paragraphs throughout the results and discussion section to sum up the meaning of the results for the reader, without using acronyms (e.g., “These results suggest that the preponderance of the CO<sub>2</sub> production driving the seasonal cycle of atmospheric CO<sub>2</sub> originates in the southern tropical region”).

Line 60. I agree with the central message of this paragraph, but I would further emphasize that HR is exceptionally challenging to measure, even at the local scale. Separating soil respiration into autotrophic and heterotrophic components is possible, and it has been done well in a few places where isotopic techniques are possible on intact soils, but it has also been done poorly or with significant limitations in other places. This is, in part, because of the intrinsic linkage between microbial decomposition and root activity (i.e., exudation, allocation of carbohydrate to mycorrhizal partners). I encourage the authors to acknowledge the uncertainty in estimating HR from soil respiration fluxes, similar to their statement regarding NEE measurements (~line 64).

Line 80. I appreciate this text directly comparing models like CENTURY to the newer, “more mechanistic” models that explicitly simulate microbial processes. Directly comparing these modeling frameworks is timely and important.

Line 239. I am somewhat concerned by the lack of treatment of ocean CO<sub>2</sub> fluxes, which are quantitatively large relative to the other fluxes listed here. I appreciate the following sentence, which at least partially addresses my concern. The authors might consider specifically state the assumption they are making by ignore these fluxes, which is that ocean CO<sub>2</sub> fluxes are constant at seasonal and interannual timescales. This assumption is challenging to swallow, particularly given that the atmospheric CO<sub>2</sub> observations were made in areas surrounded by oceans.

I found the ordering of the results to be challenging to understand. I first wanted to see an assessment of the model simulations relative to the data at the two temporal scales of interest here (seasonal and interannual). I did not find Figure 2 or it's associated

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text at the beginning of the results section to be useful in aiding my understanding. I am likely missing something. However, I would find the results to be structured more understandably (for me) if the current figures 3 and 4 became the first figures presented as results. That is, the authors may consider omitting figure 2, or moving it down.

I was surprised by the relative lack of direct comparisons across these three models in the discussion section. I was hoping for more explicit “unpacking” of the particular model formulations, with direct recommendations as to which model components are most justifiable given the observed data. I found that much of the discussion amounted to throw-away sentences such as line 457-460, in which little of consequence was said regarding how we should model HR.

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