Peer review report on “Impulse response functions as a framework for quantifying ocean-based carbon dioxide removal” by Elizabeth Yankovsky, Mengyang Zhou, Michael Tyka, Scott Bachman, David T. Ho, Alicia Karspeck, and Matthew C. Long

1) General comments:

I was asked to review the responses to Reviewer 1's concerns; therefore, my main suggestions are based on that request.

From a physical perspective, how can we show that the LTI (Linear Time-Invariant) assumption is a reasonable approximation? Another question I have is: how realistic is it to use alkalinity pulses that last for one month and cover several hundred square kilometers? I wonder how closely this setup reflects real-world applications. Moreover, the biological non-linearity is ignored on the basis of complexity. As a result, the IRF framework is supported only by the LTI assumption from the chemical perspective, which raises concerns about its overall suitability for OAE and mCDR approaches in general.

I believe Reviewer 1’s main — that the manuscript “is sufficiently miss-marketed and occasionally overstated that I believe it needs to be rewritten to be shorter, more focused, simpler, and more straightforward in its aims” — has not been fully addressed. While I cannot completely assess Reviewer 1’s reasoning behind the recommendation to shorten the manuscript, I agree that presenting this work as a general proof of concept for MRV applications may be too ambitious at this stage. Therefore, I believe additional effort is needed to make the manuscript more digestible and to the point, in line with Reviewer 1’s detailed comments.

That said, the authors have, in many cases, provided sound reasoning for the statements questioned and/or made appropriate revisions. I, therefore, recommend the acceptance of the manuscript after the remaining points raised by Reviewer 1 have been more fully addressed.

2) Specific comments:

Here, I focus only on those specific comments from Reviewer 1 that I believe were not fully addressed by the authors.

1-5: I understand why the authors chose to focus on OAE; however, I agree with Reviewer 1 that it may be confusing to introduce it so abruptly. The first part of the abstract could be revised to incorporate some of the background information currently presented in the introduction (as the authors mention in their response to this comment). Additionally, including a clarifying sentence at the end of the abstract could help bridge the abstract with the title and reinforce the points raised by the authors in their reply.

10-15: I believe Reviewer 1 was referring to the statements: "*We find that the IRF prediction can typically reconstruct the carbon uptake in continuous-release simulations within several percent error. Our simulations elucidate the influences of oceanic variability and deployment duration on carbon uptake efficiency*." While this is addressed in the manuscript, I agree that the abstract should make it clear that these findings only refer specifically to the model used by the authors.

39: I agree with Reviewer 1 that the mixing of timescales is problematic. If the main point is that OAE aims to accelerate rock weathering to ultimately enhance CO_2 uptake, then I concur with the reviewer that if the added alkalinity sinks, that objective is not achieved. I understand the authors' intention with the "thermostat" analogy, but I also agree with the reviewer that this could be omitted to maintain focus on the primary goal of OAE—enhancing CO2 uptake at the ocean surface.

106: The authors state that they have modified the text to clarify their point, but they have only added "*h*" to that sentence. They could strengthen the manuscript by expanding on this point in more detail—similar to how they do in their response to Reviewer 1.

Figures 3 and 4: The authors have added new text to the discussion section in response to the comment about how different Figures 3 and 4 would be if atmospheric pCO_2 were not fixed. However, this addition only partially addresses Reviewer 1's question. There is no clear answer provided regarding how different the figures would actually be. Instead, the authors mention that including an interactive atmosphere and terrestrial carbon pools "*may be important*" future considerations, as this could reduce the sensitivity of the biological pump to changes in carbon uptake. I believe that simply acknowledging this known limitation, without offering any estimate or indication of how the final results might change, does not fully address the reviewer's concern. One possible solution would be to add a note directly to Figures 3 and 4, flagging this limitation for readers who may focus on the figures without reading the full discussion. However, I still think the authors should include some numbers to their estimation of the error introduced by not having interactive atmosphere and terrestrial carbon pools.

Figures 6, 7, and 8 do not include subplot labels, although they are referenced in the text using subplot letters.