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Interactive comment on "Resolving ecological feedbacks on the ocean carbon sink in Earth system models" *by* David I. Armstrong McKay et al.

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Thank you for your comment on our paper. Here we will respond in brief to your comments and describe how we will subsequently revise the paper in response.

You are correct to note that DIC and ALK are not identical across our four configuration baselines, which is the result of prioritising calibrating equivalent POC & PIC export (the logic of which we explain in response to Reviewer #2, and will make clearer in the revisions) and the difficulty of exactly calibrating multiple variables. The uncalibrated ECO configuration has significantly different surface carbonate chemistry than BIO+FPR (with much lower [CO3] of ∼70-80 μmol/kg and higher DIC of ∼2280

μmol/kg), which we endeavoured to calibrate as closely as to the BIO+FPR configuration possible to minimise its impact (while prioritising POC & PIC export, hence the remaining difference). We also note that despite these differences ECO+TDR has a greater carbon sink reduction than ECO+FPR relative to BIO+FPR in most scenarios despite the larger mismatch in surface [CO3] of ECO+FPR, and similarly that ocean carbon sink capacity under ECO+FPR increases relative to BIO+FPR in three scenarios despite its lower buffer capacity, which indicates that surface [CO3] is not the primary factor in the relative ocean carbon sink differences for at least the ECO configurations. However we recognise that the remaining differences still introduce a confounding factor to our ocean carbon sink results. In our revisions, this needs to be acknowledged and constrained. Reviewer #2 brought up a different issue with the calibration process (relating to POC remineralisation parameters) which in response to we will revisit uncalibrated or recalibrated configurations to assess the impact of different calibration choices. For example, the initial BIO+TDR configuration has surface [CO3] very close to BIO+FPR (∼105.55 vs 105.53 μmol/kg) despite higher POC export (∼8 PgC), and so we can use simulations with this uncalibrated configuration to constrain the impact of surface [CO3] on our BIO+TDR results.

We recognise that our wording on the importance of the biological pump for climate feedbacks and the implications of our results (as "critical") gave the wrong impression. We of course accept that the solubility pump is the dominant factor in the ocean carbon sink (as stated on line 54) with biological processes are of second order importance, and that our carbon sinks results show relatively minor changes (<∼1%) in comparison to the biological pump (<∼10%). We also do not believe this study to be the first to propose more complex biogeochemistry in ESMs or to assess the inclusion of temperature-dependent remineralisation or plankton size traits in an ESM or EMIC (although we do believe we provide novel insights on the interaction of these factors). Our focus on CMIP relates to their dominance in ocean carbon sink and climate feedbacks discourse, but in our revisions we will expand the Background to include more recent biological pump research in EMICs and ESMs as well to put our study in a wider context.

An important aspect of this study is the separation and the nonlinear relation of the biological impact and the ocean carbon sink, but we recognise this could be made clearer in the Background. In the revisions we will further clarify that the biological pump is not itself a carbon sink, but that a transient shift in the remineralisation depth can change the partitioning of carbon within the ocean and so indirectly affect the ocean carbon sink (as per e.g. Kwon et al, 2009: Nat. GS). We will also clarify in our revisions that the nitrogen cycle, redox-dependent feedbacks, and ballasting may also have significant impacts on the biological pump, and their exclusion in this study is therefore an additional limitation on our results.

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