Below we have pasted the Reviewer's comments in their entirety interspersed with our responses in blue font.

Comments from Reviewer 1:

We are grateful for the Reviewer's comments and suggestions, and are glad that they enjoyed reading this manuscript. In our revision, we have addressed the Reviewer's comments and suggestions as described below and hope we have clarified the points raised.

The manuscript by Richaud et al., "Underestimation of oceanic carbon uptake in the Arctic Ocean: Ice melt as predictor of the sea ice carbon pump" investigate how the oceanic carbon uptake is strongly modulated by sea ice. They base their work on previous studies showing that the ratio of alkalinity to dissolved inorganic carbon in sea ice is higher than in the underlying water and previous suggestions that this storage amplifies the seasonal cycle of sea water pCO2 and leads to increased carbon uptake in the ocean. They have two independent approached; a theoretical framework and a simple parameterization of carbon storage in sea ice in a 1D physical-biogeochemical ocean model. Sensitivity simulations show a linear relationship between ice melt and an amplified seasonal carbon uptake. In addition, they estimate a 30% increase in carbon uptake in the Arctic Ocean compared with no ice amplification. Applying this ice melt parameterization to future scenarios of an Earth System Model suggest that the Arctic Ocean carbon uptake is underestimated by 5 to 15%.

Overall comment:
The paper provides new and valuable results for our understanding of the biogeochemical processes in sea ice and how sea ice modulate the air to ocean carbon transfer in the Arctic Ocean and ice covered seas. The paper is well structured, well written and the results highly interesting to a broader scientific audience interested in global warming. Therefore, I will recommend the publication of this work if the authors consider the minor comments below.

Response: We appreciate the positive assessment.

Specific comments:
Line 37. Suggest to provide an additional reference to Rysgaard et al. 2013 (doi:10.5194/tc-7-707-2013) where the link between ikaite crystals trapped within the sea ice matrix and the distribution of alkalinity are shown for winter ice conditions.

Response: Agree. Done.

Line 42. After DIC ratio, I suggest to provide a reference to Rysgaard et al. 2012 (doi:10.5194/tc-6-901-2012) where ikaite dissolution is shown for melting sea ice and how this affect pCO2 and pH levels in Arctic surface waters.

Response: Agree. Done.

Line 89. I'm not sure DIC and alkalinity are homogeneous in sea ice. They are probably more C shaped. However, it is a fair assumption considering the few existing observations in different forms of sea ice.

Response: We absolutely agree, alkalinity and DIC are likely to be vertically variable. However, their homogeneity is a necessary assumption to analytically derive and solve the differential equation. Furthermore, if values are used that are representative of the freezing and melting ice over a seasonal cycle, we believe the assumption is reasonable. We added a brief discussion of this assumption in the manuscript (l. 402):

"[Our parameterization of the alkalinity-to-DIC ratio] may be overly simplistic. First, the vertical profiles of alkalinity and DIC in sea ice, assumed homogeneous here, might be C-shaped to follow salinity profiles, though observations do not necessarily support a vertical heterogeneity (e.g. Miller et al., 2011 ; Rysgaard et al., 2009). As long as the parametrized values are representative of the freezing and melting ice over a seasonal cycle, we believe that the vertical homogeneity assumption is reasonable. Second, the alkalinity-to-DIC ratio is known to increase over time.[...]"

Line 188-190. I am surprised the biological terms had a negligible impact on carbon uptake. Could you elaborate a little more why that is?

Response: We understand the confusion and found this to be a difficult point to explain concisely. Note that we do not mean to say that biology has a negligible impact on carbon uptake in general, but only on the **supplementary** carbon uptake due to the presence of alkalinity and DIC in sea ice. In other words, calculating the difference between a CTRL run <u>without</u> biology and the corresponding ICE run <u>without</u> biology yields a similar supplementary carbon uptake as the difference between the CTRL run and ICE run <u>with</u> biology. The supplementary carbon uptake is not driven by biological processes but by the chemical properties of ice and sea water.

We have attempted to clarify this by adding the following text (l. 199):
"[the biological terms have a] similar impact on carbon uptake regardless of whether the carbonate system inside sea ice is represented or not, and thus yield a [negligible impact on supplementary carbon uptake]"

Line 325. The assumption of a constant mixed layer is a good beginning. However, I expect leeds and polynyas (ice fabrics) could elevate the carbon uptake. I'm aware that this will require very high-resolution modelling, but could be very interesting thing to look into after your present work. Looking forward to a follow up study later.

Response: Absolutely agree, the spatial heterogeneity of sea ice concentration and mixed layer conditions would be interesting to investigate. Several papers have observed intense carbon uptake in leads (e.g. Else et al., 2011, https://doi.org/10.1029/2010JC006760). But as mentioned by the Reviewer, the model requirements for such a study are completely different from what we have used here.

Line 355. Here you state that models without the ice pump parametrization may underestimate carbon uptake over seasonally ice-covered areas by 10-15%. In the abstract this number is 5 to 15 %.

Response: Indeed. When reporting this 10-15% number, we exclude the scenario SSP8-5.5, but we included it in the abstract numbers to be conservative. We have clarified this in the manuscript (l. 370):

"Without it, the ACCESS-ESM-1.5 model could be underestimating carbon uptake over seasonally ice-covered areas by ***5*** *to 15 %,* ***or 10 to 15 % if we exclude SSP5-8.5****."*

Line 360. I'm happy to see that your estimated supplementary carbon flux is consistent with numbers provided by Rysgaard et al 2011. Do your model also include the Southern hemisphere and would it be possible to provide a number for sea ice Antarctica? Could be a really interesting follow up study after this work.

Response: It would indeed be interesting to look at the Southern Ocean. Conditions there are significantly different and might provide a different linear relationship with ice melt. Unfortunately, our forcing data set does not include the Southern hemisphere, preventing us from easily extending the analysis.

Line 370. Your statement regarding the importance of high vertical resolution in the model to represent the shallow mixed layer is an important one. In order for the carbon pump to work, the CO2 released from ikaite production in sea ice only has to go below a thin mixed layer to prevent (or greatly reduce) exchange with the atmosphere in the Arctic Ocean due to an impermeable sea ice cover (autumn, winter and spring). As this cold water below the mixed layer meets warmer and saltier Atlantic water on its way out of the Arctic Ocean it will sink in the Denmark Strait. At the same time melting sea ice in the summer will be in contact with the atmosphere and result in dissolution of ikaite and release of excess alkalinity to surface waters and hereby stimulate CO2 uptake from the atmosphere. Could be interesting to look into regional differences in air-ocean CO2 uptake.

Response: That is our understanding as well. Moreover, the vertical resolution conditions the resolution of the mixed layer, which in turn has an important impact on the volume in which the ikaite can dissolve in summer and therefore on the lowering of pCO_2 and strength of the supplementary carbon uptake. We have emphasized this in the manuscript by adding (l. 465):

"*A high vertical resolution would be crucial to properly resolve the shallow Arctic summer surface mixed layer and the carbon subduction.*"

Line 395. Polynyas and leads. Interesting and I would love to see more on this modelling in the future.

Response: We appreciate this comment and, following the previous addition, we now mention those studies explicitly (l. 467):

"*Modelling studies dedicated to leads and polynyas would also help to qualify and quantify the sea ice carbon pump in those areas of intense mixing, as well as providing guidelines on how to parametrize those mesoscale ice features in low resolution ESMs.*"

Summary: I really enjoyed reading this study.

Response: We really appreciate this comment and are grateful to the Reviewer.