Comments from Reviewer 2:

We appreciate the Reviewer's comments and agree that our manuscript was lacking a proper consideration and discussion of the two key questions he raised. We have addressed those points in our revision as detailed below and believe the revised manuscript is clearer and significantly improved as a result.

General comment

This is a study on the effect of sea ice on the carbon exchanges in ice-covered seas, based on a rather conceptual approach and an offline ESM application. I find the paper reasonably well presented, yet insufficiently developed and subject to important methodological ambiguities.

The origin of the latter could be that the paper has not taken full benefit of the literature, ignoring key processes (subduction of carbon below the mixed layer) and progresses in the definition of the sea ice carbon pump.

There is space for a conceptual study of the seasonal cycle of air-sea carbon exchanges in ice-covered seas, however important aspects (including basic calculations and experimental design for model experiments) would need to be reworked, in my opinion, before the paper represents significant progress against state-of-the-art.

More detailed comments

The key question, as presented in the abstract « how the storage of carbon in sea ice affects air-sea CO2 flux and quantify its dependence on the ratio of TA vs DIC in ice », was central to two contributions on the topic by Grimm et al (2016) and Moreau et al (2016).

Both studies are cited in the submitted paper, however two key progresses that these made were not considered in the submission.

First, both author teams conclude that the subducted fraction of the carbon anomaly generated in the mixed layer during sea ice growth is a key player in the intensity of the sea ice carbon pump (SICP). Rysgaard et al assumed a 100%-efficiency of carbon subduction below the mixed layer (leading to maximum efficiency), whereas both Moreau et al and Grimm et al suggest the subduction efficiency could be less than 5%, which would drastically decrease the intensity of the SICP compared to the large numbers of Rysgaard et al 2011. This finding is not mentionned and not accounted for in the conceptual model presented in the submitted paper, which **I find problematic as subduction efficiency is a key player and source of uncertainty**.

Response: We agree with the Reviewer that subduction efficiency is a key player for determining the ultimate fate of atmospheric carbon taken up by the ocean and its long-term storage, because subduction provides a mechanism for transport of carbon into the deeper ocean, out of reach from atmospheric influence. We now state this more clearly in the manuscript (l. 44) where we have added:

"and the long-term fate of the carbon taken up is controlled by subduction processes, including advection of water masses to depth (Bopp et al., 2015; Karleskind et al., 2011)."

However, an investigation of subduction is beyond the intended scope of this study, which addresses the effect of the sea ice carbon pump on the air-sea flux (as stated in the abstract) and considers seasonal to annual time scales. We are confident that the efficiency of subduction is not a strong determinant of the influence of carbon storage in sea ice on air-sea flux. We further note that the current Arctic Ocean is **strongly undersaturated** in CO_2 with regard to the atmosphere. Given the undersaturated state of the Arctic Ocean, even if subduction was nonexistent, the sea ice carbon pump would still affect air-sea flux on the seasonal to annual time scales we are considering. Indeed, by amplifying the pCO_2 seasonal cycle in phase with the ice concentration, the sea ice carbon pump generates a higher seasonal uptake than without the carbon storage in sea ice, even if its mean value were to remain constant or increase slightly (i.e. if subduction was inefficient). This would potentially mean a faster "catch-up" of the surface Arctic Ocean pCO_2 values toward atmospheric values if subduction is as low as suggested by Moreau et al. and Grimm et al.

As shown in our study, the choice of vertical resolution impacts model-derived estimates of the supplementary carbon uptake; we believe that the relatively coarse resolution used by Moreau et al. and Grimm et al. raises the questions about uncertainties in their estimates about subduction.

In the case of the ACCESS-ESM1.5 model, subduction processes are accounted for in the initial outputs, but an estimate of how this subduction would act on the supplementary carbon pump would require a full run with the ESM with a sea ice biogeochemical component (we note that we do not have access to the ESM model). Our methodology inherently assumes that all the supplementary carbon is subducted, which we didn't mention in the initial manuscript. We have corrected this omission as follows (l. 304):

While subduction processes are simulated in the initial outputs, our offline methodology does not correct mixing and advective carbon transport for the supplementary carbon due to the sea ice carbon pump. Therefore, an inherent assumption to our methodology is the subduction of all the added carbon."

Since the physical circulation remains unchanged by the sea ice carbon pump, we could expect mixing and advective processes to be linearly dependent on the DIC and alkalinity gradients, hinting towards a proportional increase of the additional carbon uptake by subductive processes, but this remains to be proven. We have introduced another sentence in the manuscript to briefly discuss the ACCESS-ESM1.5 case, at l. 393:

"The output from the ACCESS-ESM1.5 model account for subduction, but the fate of supplementary carbon estimated here cannot be determined without running a new configuration of the model. It is therefore unknown whether carbon flux driven by advection and mixing would proportionally increase and export the supplementary carbon or whether the latter would saturate the surface mixed layer, leading seawater pCO_2 to catch-up with atmospheric values faster than without accounting for the sea ice carbon pump."

We believe that the sea ice carbon pump is an important enough process that deserves investigation and probably shouldn't be neglected in ESMs as it currently is. We acknowledge that the points raised by the Reviewer should have been clearer and, in addition to the changes described above, have added the following sentences:

- l. 388: *"On top of that, a proper representation of subduction, included in the Grimm and Moreau studies but beyond the scope of the present one-dimensional study, would be important to more fully understand the long-term fate of carbon in the global ocean. Yet, in an undersaturated ocean, the amplification of the pCO_2 seasonal cycle can in itself explain an increased seasonal carbon uptake. Without any subduction, this would then lead the Arctic Ocean to reach equilibrium with the atmosphere faster than without accounting for the sea ice carbon pump, eventually saturating the surface ocean and reducing the carbon uptake."*
- l. 465: *"including the role of mixing and advective processes on the fate of the added carbon. A high vertical resolution would be crucial to properly resolve the shallow Arctic summer surface mixed layer and the carbon subduction"*

Second, Moreau et al and Grimm et al decomposed the sea ice carbon pump into several sub-components. The decomposition of the SICP has important implications on the requirements for what the reference control case should be in model experiments, in order to draw proper conclusions on the sea ice carbon pump. Both aforementioned studie acknowledge that the SICP results from mostly three groups of processes. (i) Sea ice growth, which implies an uptake of freshwater from the ocean to the ice, (ii) brine rejection, which proportionally decreases the uptake of solutes in sea ice, and (iii) active biogeochemical processes, which modify the TA/DIC ratio in sea ice.

Response: We appreciate this comment which has prompted us to state the assumptions in our model experiments more clearly and contrast them directly with the assumptions in the previous publications. We have added the following sentences:

- l. 57: *"The sea ice carbon pump is considered to result mostly from three groups of processes: (i) sea ice growth or melt, which implies a freshwater flux (upward or downward) from the ocean to the ice, (ii) brine rejection, which proportionally decreases the uptake of solutes in sea ice, and (iii) active biogeochemical processes, which modify the alkalinity to DIC ratio in sea ice. Most, if not all, Earth System Models (ESMs) lack a representation of biogeochemical processes within sea ice and are therefore unable to account for (ii) and (iii), but encompass (i) by dilution and concentration of tracers similar to the handling of precipitation and evaporation. In the present study, we do not distinguish between (ii) and (iii) and instead consider that the carbon cycling in sea ice encompasses both aspects. We also consider our reference point (later referred to as CTRL) to be that of current ESMs, i.e., they include processes (i) but not processes (ii) and (iii)."*
- l. 78 (additions in bold): *"[The supplementary carbon flux] is quantified here as the difference in air-sea CO_2 flux between a **reference** situation where there is no ice-ocean carbon flux, **i.e including aforementioned processes (i) but not (ii) nor (iii),** and situations where ice-ocean carbon flux occurs**, i.e including (i), (ii) and (iii)."***

In this context, depending on the question of interest, the reference point for « no sea ice» could be several things, and basically two points of view are used in the literature. A first point of view (#1) is to assume no freshwater and no solute uptake/release associated with sea ice, wherease point of view #2 assumes no solute uptake, but some freshwater uptake associated with sea ice. #1 (no fw and no solute uptake) was generally adopted by Moreau et al and Grimm et al and implies that there is virtually no change in surface concentrations due to sea ice at all in their CTRL experiment. #2 assumes full rejection of solutes and would lead to the strongest effect of sea ice on surface concentrations.

Response: We agree with this description. Moreau et al. conducted three different experiments: CTRL which corresponds to no freshwater and solute uptake (point of view #1), PHYS which has freshwater uptake as well as some solute uptake proportional to salinity (so doesn't really match point of view #2), and CARB which includes a modified alkalinity to DIC ratio. They then compare PHYS – CTRL, and CARB – PHYS. The approach adopted in our study is closer to this latter experiment, although it doesn't perfectly match it.

Grimm et al. have a similar distinction, with their "control" run matching point of view #1 and their R0 experiments corresponding to point of view #2. Our CTRL experiment matches R0, instead of their "control" experiment, which could potentially lead to confusion.

Our rationale is that we wish to use the current state of the art in ESM as the reference point.

We have attempted to clarify this by adding the following clarifications to the manuscript, on top of the changes described above:

- l. 112: *"[and a control (CTRL) case, where storage is not considered]* **and ice growth or melt only leads to a freshwater exchange**"
- l. 197: *"[Phys includes the advective and dispersive transport terms]* **as well as dilution and concentration due to sea ice melting and freezing or due to precipitation and evaporation**".
- l. 225: *"In both configurations, sea ice growth and melt generates a freshwater flux that concentrates or dilutes tracers at the surface ocean."*

Both points of view can be justified depending on what one is looking for. However, the issue with the submitted study is that, in my understanding, points of view #1 and #2 are adopted in different parts of the paper without notice, which I view as problematic because the conclusions drawn are be completely different if one or the other point of view is adopted.

Response: Actually, we have consistently adopted point of view #2. Please cf. below.

Section 2 (conceptual study) assumes point of view #2. In this context, comparing CTRL and ICE differentiates between the effects of full rejection of solutes minus brine rejection (which I find not so useful scientifically, but possibly useful as a technical stage meant to fully decompose things).

Response: We agree.

In Section 3 (1D model), I'm not really sure which point of view is adopted. Table 1 says TA=DIC=0 (point of view #1), whereas the text says no ice-ocean carbon flux (line 235), which would correspond to point of view #2. As far as I understand it, the default approach in PISCES assumes point of view #2. Finally, regarding the ACCESS-ESM application, where the analytic perturbation is applied, there is no

mention of what is assumed in that ESM for TA and DIC concentrations in sea ice, hence we don't know the point of view adopted. Clearly, applying the perturbation would require point of view #1, for consistency with the analytical calculations. However, it could well be that model developers would have adopted point of view #2 (I know of many ESMs which adopt point of view #2). In that case, the ESM outputs and the diagnosed perturbation on carbon fluxes would be incompatible, and any finding based on their association would be wrong, I'm afraid.

Response: In this section as well, we adopt point of view #2, for the exact reason mentioned by the Reviewer: we need to be comparable with ESMs, which account for tracer dilution and concentration due to sea ice melt and growth but lack a sea ice biogeochemical component. We argue for the need for the modelling community to include BGC in sea ice, so we make sure our reference point matches the current state of ESMs, hence the discrepancy between our CTRL reference and Grimm et al. and Moreau et al.'s. We hope some of the previously mentioned revisions (e.g. l. 57) clarify this perspective.

The values given in Table 1 reflect values of DIC and TA in ice: point of view #1 would require TA and DIC in ice to have the same values as in seawater, which is not the case here: we have TA=DIC=0 in ice, which means no ice-ocean carbon flux (cf. equations line 192-193) but does not prevent dilution or concentration of tracers due to freshwater fluxes.

We acknowledge that this was not explained precisely enough in the manuscript and hope that the new version is clearer, with some of the previously mentioned changes and the following addition:

- l. 301: "[*This model, as any ESM, does not include*] **any carbon storage in sea ice, although the freshwater flux between the ocean and sea ice is accounted for**"

This ambiguity on the relevant reference point of view cast doubt (on my side at least) on the findings presented. Properly defining the sea ice carbon pump, translating that into a fit-for-purpose experimental design in all sections of the paper, and making appropriate connexions to concluding statements on the sea ice carbon pump should be clarified before anything else, I reckon.

Response: Again, we appreciate the comments and believe the manuscript has benefited greatly from the resulting clarifications and revisions.