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# *Interactive comment on* "$CO_2$ flux over young and snow-covered Arctic sea ice in winter and spring" *by* Daiki Nomura et al.

**Anonymous Referee #1**

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General comments

Nomura et al present an interesting analysis of rare data capturing CO2 fluxes between sea ice and the atmosphere in Arctic winter, spring and summer as part of the N-ICE project. The methods are robust, the data are of high quality and significant value, and the arguments laid out in the paper will be of wide interest amongst the sea ice and CO2 communities.

However, the manuscript comes across as a little rushed in its current form, and I believe it would be improved significantly by adding more detail and explaining more clearly the key points. I recommend acceptance for publication after moderate revisions.



Results are presented in summary tables. In general, I find that not enough information is presented for the reader to easily follow the arguments made in the paper, and I think some may even be misleading. For instance, based on Table 3, you argue that Fice is greater than Fsnow and thus make the argument that snow cover reduces flux magnitude. From the table, it appears that this is only demonstrably true for two out of the seven first-year ice stations. Two of the stations appear to have negative fluxes, but this is not addressed in the text at all, but seems to me to be quite important. These factors should be discussed in much greater detail in the text. Given the variability in your results, I think it is necessary to present the actual data, rather than just summary data. This would probably be best as figures, to accompany the summary tables. On a similar note, you have the number of measurements listed for F-snow and F-ff in table 3, but why not F-ice. Please include this information and error estimates.

It is also quite difficult in general to follow the flow through and between the different tables, for example discussion of the relationship between flux magnitude and snow thickness or water equivalent. The text needs more detail to guide the reader's understanding and some more figures would certainly help.

Specific comments

Introduction: it would be useful to include a little more information about what we know about ice-atmosphere CO2 fluxes in the context of ocean-atmosphere fluxes overall in the Arctic, and how they may change in the future. That would set the scene nicely for your statements at the end about ice-atmosphere fluxes being important in the context of a changing Arctic and the broader implications of your work. The final paragraph (line 107) could also be much stronger and punchier.

Line 125-127: state specifically which stations you are referring to. I assume "young ice", but this should be explicit. That might also help the descriptions of relationships between variables in the discussion, as mentioned in "general comments".

Line 155-157: does this not contradict your argument that snow provides insulation?

Perhaps it would help to mention timescales of T change/stability.

Line 162: I think you have air and ice surface the wrong way round.

Line 172: I think you should distinguish between stations where snow was cleared and where the sea ice surface was naturally snow-free. Given your arguments about the effects of snow cover, I assume this is significant.

Line 185-187: clarify when temperature was measured.

Line 192-193: why was carbonate chemistry only measured at these four stations? This should be explained. It also means that table 2 looks like there is a lot of data missing; perhaps there is a better way to present these data?

Line 220: I think this should be Guildline PORTASAL salinometer Model8410A

Line 239-240 and 239-250: this strongly suggests that the constants are not valid for your conditions. The following clearly attempts to justify its use, but it is not clear why the 40 % uncertainty does not apply to your data, which would mean that none of your calculated values would have statistically significant differences. Please clarify.

Line 253-254: please give enough information for the reader to understand this calculation, without having to dig out an old reference.

Methods: please include information about how atmospheric pCO2 was measured. It comes later as a footnote to a table, but should be included here.

Line 275-276: state which stations you are referring to. This would help in general in various places in the text.

Line 279-280: I think it would help to demonstrate this point if you plotted air temperature on figure 4, so that the relation is clear.

Line 285-286: can you highlight on figure 4b which measurements are from frost flowers?



Line 292 and table 2: you present data from the top 20 cm, which presumably means your top two 10cm slices. Why do you only present the top 20cm when most cores are longer? Would it be better to present profiles to show downcore variability? If not, please justify presenting only the top 20 cm and provide error/uncertainty estimates from averaging of values from two core slices.

Line 322: “except for station OI1”. Should this also say YI1 as it does in section 3.2?

Line 324: “..and in cases the thick insulating snow cover”. Does not make sense. In certain cases? In cases where. . .?

Line 355-358: this statement is only true for FI5, FI6 and YI1. Same comment for line 372-373.

Line 357: Where you state that one value or group of values is lower than another, please provide relevant statistical details (e.g. t-test, z-test etc.)

Line 372-382: This paragraph is an example of where a lot more detail is required to demonstrate your points. Flux direction, magnitude and relationships between variables all need to be discussed for the different stations.

Line 380: reference to table 3. You need to be specific about what you are referring to that shows that flux is reduced by the presence of snow. If you compare FI5 and FI6, FI6 shows a much greater potential flux but actually has a greater snow thickness and water equivalent than FI5. This should be incorporated into your comparisons.

Line 396-399: How will footprint size make such a big difference? If it arises from small-scale heterogeneity in time and/or space, this should be stated. Are there any other reasons worthy of mention?

Line 401-406: your fluxes are at the lower end of positive values – this should be stated, and elaborated on to discuss negative fluxes as well as positive ones (as per my earlier comment).

Line 406: should be “up to +11.8” or somehow make it clear that this is the maximum value.

Line 432-461: this section emphasises the importance of the temperature gradient in modifying fluxes and gives the impression that this is the most important variable. In fact, the correlation between temperature difference and flux is less strong than the correlation with pCO2 difference between the ice and atmosphere (given in line 310). This would be much clearer and more reflective of what the data show, if both variables were discussed here in terms of their relative importance overall and such a strong emphasis on temperature dampened. I also think it would help to add to figure 5 a panel which plots pCO2 difference vs. flux, to show the two relationships directly.

Line 458-459: “for young sea ice likely the frost flower conditions”. Does not make sense.

Line 468-473: from the data presented in table 3, not all stations can be described as showing CO2 sources. Some clearly show sink behaviour (negative fluxes), and for a number of others, the uncertainty on flux estimates cannot confidently be described as a source, e.g. when flux = $0.1 \pm 0.1$. This is particularly the case given that you state the detection limit as 0.1. This also needs to be considered in your discussion.

Line 476-477: This should not be presented as a conclusion.

Line 485-488: I think you undersell the importance of your work here, and you could make more compelling statements about the role of sea ice in CO2 fluxes in a changing Arctic.

Figure 3. should this cite Hudson et al., 2015?

Table 2. Consider adding an extra column for $\Delta$pCO2 (air-sea difference) to aid understanding.

Table 3. The key thing that jumps out for me is that natural flux is much higher for frost flowers than snow. I would have thought that’s worth highlighting in your discussion.



Technical corrections

In general, the manuscript is well-written, the technical language is appropriate, and the standard of English is good. However, there are a couple of points to check throughout the text: use of the definite/indefinite article; singular/plural nouns and their following verbs e.g. frost flowers, was/were.

Line 84: should be transport by molecular diffusion

Line 218: remove hyphens

Line 377: I think 0.0 is a mistake.

Table 3: brackets in the top line are confusing.

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