We thank Dr Else as well as the other two reviewers for their reviews. We respond to the comments of Dr Else in italics, in line with his review below.

First of all, my apologies for not submitting a second review of this paper in a more timely fashion.

I thank the authors for considering all of my previous comments carefully. This manuscript now applies a more cautious interpretation of results, which I believe is appropriate. This does not diminish the importance of the measurements - these are exceptionally difficult to obtain, and the paper will make a substantial contribution to the field of knowledge. I still think the results will be widely used in the modelling community, but hopefully this more cautious phrasing will lead to more nuanced applications of the results. The removal of the sea ice chemistry results was probably a good idea, as they did distract from the main results of the paper.

I offer only a few minor comments for consideration prior to publication:

Line 257 (revised manuscript): consider mentioning the overfilling of bottles BEFORE mentioning the Hg poisoning. I assume (or hope!) that was the order of operations.

Changed as suggested to clarify the order of operations (nb line number is 227).

Lines 420 - 425 (revised manuscript): I'm sorry that I didn't bring this up in the previous review, but I think a bit of clarification is needed in this paragraph. The two comparisons (1: chamber flux to surface measurements & 2: chamber flux to 8m depth measurement) provide two different insights. Comparison 1 shows that there is probably a physical effect of sea ice on gas transfer velocity. This adds to the body of literature discussing the impact of sea ice on gas exchange. Comparison 2 shows that biases arise when ocean chemistry measurements are not made near the water-air interface. This adds to the body of literature discussing biases that arise when trying to apply the bulk flux equation in the Arctic. Since this is a discussion section, these points can be made within the paragraph. I would recommend adding a sentence after each comparison that explicitly explains the insight that these comparisons provide. This will make the relevance of these observations much more clear, and untangle an otherwise slightly confusing paragraph.

Changed as suggested.

Line 433: Small point: please say "the U10-normalised ice-based measurements APPEAR to have a dependence on lead width" (or something to that effect), as no statistics have been applied to determine if this relationship is statistically significant. While I agree that the data hint at a possible relationship, the number of samples and the uncertainty necessitate a cautious interpretation.

Changed as suggested.

Line 467: I don't love that this -55 Tg CO2 yr-1 number is still in here... It no longer appears in the abstract or conclusion, which is good. If it must stay, may I recommend the following wording: "Therefore, while we can estimate an annual CAO air-sea flux of -55.0 Tg CO_2 yr^{-1} by multiplying the average daily flux by 365 days, this result is highly uncertain".

We have made the suggested wording change.

Line 497: "...but are approximately 35 times smaller than aircraft-based observations". When discussing this discrepancy, the authors omit the obvious third option: the aircraft fluxes are erroneous. I leave it up to the authors to determine if they want to include that in their paper, but at least it has now been noted in the open review.

We have added a comment noting this possibility to the manuscript.