

Interactive comment on “Satellite-observed sea ice area flux through Baffin Bay: 1988–2015” by Haibo Bi et al.

Anonymous Referee #2

Received and published: 28 August 2018

General Comments:

In reference to the review criteria provided by The Cryosphere (https://www.the-cryosphere.net/peer_review/review_criteria.html), I submit the following general commentary on this work.

The originality (novelty) of this work is Fair. It does not represent substantial progress beyond the current scientific understanding of sea ice flux through Baffin Bay or any other area of the Arctic Ocean.

The scientific quality of this work is Fair. The purpose of the work is articulated, however the objectives laid out in the second-to-last paragraph of the introduction section do not strike this reviewer as testable hypotheses. They are rather statements resulting

[Printer-friendly version](#)

[Discussion paper](#)



Interactive
comment

in the paper becoming something of a data report. The analyses presented are somewhat perfunctory, and potentially these points indicate a lack of understanding of the physical atmospheric and/or oceanic processes acting on the surface (ocean or sea ice) within Baffin Bay. The first mention of the hemispherically significant North Water Polynya does not occur until Line 6 on page 23 of this work. In this vein, it's not really explained that the sea ice in Baffin Bay may be as a result of import from the Lincoln Sea as a result of motion through Nares Strait, Kane Basin, and then Smith Sound before it might either enter Baffin Bay, or encounter an ice-bridge blocking inflow to Baffin Bay from the north. The annual and seasonal presence/absence of this very important ice-bridge feature and the amount of MYI imported from the North is not explicitly investigated in this work. First-year stages of development of sea ice in Baffin Bay may then also have been grown in place within the North Water and exported southward, depending on the formation of the ice-bridge, and the amount of ice in the Bay, which has been imported from the Canadian Arctic Archipelago (CAA), mostly through Lancaster Sound. Since the authors chose a position for their northernmost 'Gate A' south of Lancaster Sound, the flux of sea ice from the CAA in the west, or from Smith Sound in the north, cannot be differentially discerned. Other important considerations that occur to this reviewer include sea ice melt during transport southward within Baffin Bay, especially considering that new and young stages of development (<30cm thick) may be grown and exported from the North Water Polynya. Finally, it might be that the authors have not accounted for the presence of fast ice around Baffin Bay in fall, winter and spring, especially on the Greenlandic (east) ends of their passages A, B, and C. All of these points above make the comparison of these presented data in Baffin Bay to the data presented from Fram Strait a bit of a stretch to this reviewer. I appreciate that Kwok 2007 makes a comparison of the annual volume export he calculated for Baffin Bay and Davis Strait to the annual export of sea ice through Fram Strait, but it's my opinion that Kwok made that comparison in his 2007 work to simply highlight the amounts of sea ice exported southward in the two regions, and not to compare the processes or sea ice stages of development that typically are exported in the two areas



which are not physically similar.

The significance of this work is Poor. I think especially a re-worked version of this paper could improve our understanding of sea ice flux form this important region, but as it stands this work falls short of improved scientific understanding of the region and its relevant physical oceanographic, atmospheric, and/or sea ice processes. The analyses are comparative rather than investigative, and their presentation is in the style of a data report.

The presentation quality of this work is Fair. The figures are too numerous, and each conveys too little information. The authors convey too much data in Tables, while the text does not explain either the figures or tables more than superficially. This work is way too long, and clarity of direction is missing from the objectives onward through the results and discussion sections. The reason for the decadal period break at 2000/2001 is not obvious to this reviewer. It seems a break of convenience rather than scientific reasoning. Little in the way of conclusions is presented in that section of the text, though "...A comparison shows that our SIF estimates are reasonable consistent with previous results" (page 25 Line 8) is encouraging, it's not a conclusion. There are two more conclusions stated in Lines 9-11 of Page 25, but their validity is brought into question for this reviewer by the changing definition in the work (Oct or Nov to May depending on the section I read). The conclusions based on the three defined Gate locations make this reader interested in why their locations were chosen (especially given my previous note on the position of the northernmost Gate)? The last two paragraphs of the conclusions section are statements, which cannot be concluded as a result of the new work presented here.

Specific Comments: P1. L12: This sentence should make some reference to sea ice melt? L14: Why three passages, this really isn't useful information in the abstract given there is no geographical reference to their actual positions. L20: Causation is not shown in the work. Could the decline in SIM be a result of the fact that the SIM data are calculated in part from the SIC data? L24: Unclear what you mean here.

TCD

Interactive comment

[Printer-friendly version](#)

[Discussion paper](#)



L28FF: The data exist to determine if the sea ice is in free drift in Baffin Bay and Fram Strait, but quantification is not attempted. Not sure why the authors insist on acronyms, especially for Fram Strait?

P2. L7: What is the potential impact? L9: Does outflow imply melt?

P3: L1: “passages” is a poor word choice, my opinion only. L2: Is there an effect to the trends/forcing? L5: What is the point of the comparison to Fram Strait? L7: “preferred...” Whose preference? Why? L28: I wonder what causes the discrepancy in the SMMR and SSM/I records with respect to sea ice motion data?

P4. L5-9: this whole section is worded like the authors actually did this processing? L10: Maybe use the whole words? Especially in a Section Heading? L16: Citation required? Figure 1: The position of Gate A is too far south to allow for quantification of ice flux from Lancaster Sound. I’m not really sure of the point of Gates B and C unless there’s some quantification of melt? There’s no scale for the magnitude of the vectors displayed on the figure.

P5. L8-10: What is the mechanism for ice motion through Baffin Bay with the NAO atmospheric patterns? Especially considering the height of the Greenland Ice Sheet that separates the Icelandic low from Baffin Bay? Does it even make sense that the NAO should drive ice drift in Baffin Bay? Some justification of this use of the NAO should be made? L17-24 and Table 1: Why order the Gates A, C, B, in the explanation of their positions? Why are all their lengths different in the text and the table? Seems like those lengths should be consistent?

P6. L2-3: Is this assumption valid? L11: Maybe call the Gates “North”, “Mid”, and “South” if you’re going to continue using three so their positions are immediately apparent to the reader? L14, L16: Here’s where I first noticed that the months used for “winter” change constantly through the work. Nov-May vs. Dec-May? There’s got to be a consistent set of winter months used I think? This really reduced my trust in the analysis presented. L18: Where is the Cuny Gate in relation to Gates B, or C?

[Printer-friendly version](#)

[Discussion paper](#)



P7. Figure 2: The lines, colours are too hard to discern. Now winter is Nov-May. L12: Now winter is Oct-May? There are a bunch of typos in this paragraph. L13: It'd be nice to see the sea ice concentration data? L14: Where is the wind forcing data?

P8. Figure 3: The previous figure was in cm/s, now we've changed to km/day, and we have a legend for the magnitude of the quivers. Maybe add the Gates to the figure? It would be nice to see the actual sea ice concentration? Delineate the fast ice? What portion of the sea ice motion in Baffin Bay is driven by ocean currents? L3: Doesn't the data exist to determine this? Even the magnitude of the gradient? L6: Could the pattern not be visible due to your use of monthly averages? Seems like the higher the ice concentration the more likely that ice motion events might be temporally discrete due to the magnitude of the forcing required?

P9. L5: Why 4c before 4b? Figure 4: I don't understand why the comparison to Fram Strait. Probably should specify the Gates in the figure caption? L16-17: Why not quantify this? Also now winter is Dec-May?

P10. L7: There seems no scientific reason to break at 2000? L9: Now December is in the autumn? L15: What use the sci. notation? Why not just write $-21\text{km}^2/\text{day}$? L31: Not really a decadal change, it's a change between two decades.

P11.L12: Now winter is Nov-May

P12. L10-11: This is not a result of this work and is left unsubstantiated.

P13. L4: Does this mean that the trend is not significant? L6: Shouldn't the "looseness" of the ice pack, that is it's internal stress, be quantifiable with the data here? L8: I don't know where this statement comes from. It's unsubstantiated by the work presented.

P14. Table 1: There's a better way to graphically display the pertinent/important parts of this Table. You're asking the reader to do too much work to understand your analysis.

P15-16. Figures 8, 9, 10: These figures are to small, to hard to read. Why use the



three different significance levels? Figure 10: the y-axis keeps changing, makes it hard to compare within the columns.

P17. L3: The increasing sea ice motion trend is not caused by a positive sea ice flux trend; you've put the cart before the horse here. L12: now winter is from Oct-May

P18. L3: What's the point of this section? L4: What does this sentence actually mean? Maybe this is some reference to melt within an area between two Gates?

P20. L15FF: What about the source of the exported sea ice? Smith Sound? Grown in the Northwater? From the CAA through Lancaster Sound? Surface winds in Baffin Bay are tricky to model because of the elevation of the Greenland ice sheet and the CAA islands? How well do these model winds actually represent reality? L16: What do all these acronyms mean? L20: Surface winds towards the southeast are northwesterly winds.

P21. L1: Winter is Oct-May now.

P22. This whole page seems like conjecture, it should all actually be borne out by some analysis. Kind of seems like the authors are listing possibilities rather than elucidating processes.

P23. L16-17: isn't the NAO calculated from the pressure difference between these two atmospheric phenomena? Hurrell 1995?

P24. L16-18: The internal stress of the sea ice pack should be able to be approximated at least. This sentence seems like conjecture as is.

P25. Figure 17. Any actual information in these panels is undiscernable due to their size.

There's no mention of the North Water Polynya in the conclusions section. L15-20: I think the statements in this paragraph remain unsubstantiated by this work. What about fast ice extent? The last two sentences are not conclusions of this work. I don't really

[Printer-friendly version](#)

[Discussion paper](#)



understand why you've listed surface winds and sea level pressure because they're two sides of the same coin, same goes for SAT and SST?

P26: These are not new results. As indicated in part by your reference to Kwok's papers. The last sentence of this paragraph is not a conclusion that is supported by the work presented.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-136>, 2018.

TCD

Interactive
comment

[Printer-friendly version](#)

[Discussion paper](#)

