

Anonymous Referee #1

General Comment

This is a nice paper that updates previous analyses of sea ice changes in the Pacific sector and the contributions of melt/growth and advection processes. The paper is well-written and thorough. The methodology appears sound and is explained well and the results are clearly presented. There are a few small questions/concerns, noted below. After addressing these, I find the paper acceptable after minor revisions.

Response: We appreciate for the comments and suggestions and revise the manuscript accordingly.

Specific Comments (by page and line number):

P2, L4: “de” is non-standard and somewhat confusing for “decade”. I would use either “dec”, “d”, or “decade”.

Response: Based on this comment and suggestions from Reviewer 2# , we use unit “/yr” to take place of “dec” throughout the manuscript to ensure a unit consistency. Still many thanks for the reminding that “de” is not an appropriate word to be used in scientific paper.

P2, L11: what is the NSIDC report? Need to reference (if it’s a webpage, I think it’s fine to just include the link)

Response: The associated webpage link is added. (P2, L14 in the revised manuscript with a edit-tracking format. Hereafter, if not stated, the P and L correspond to the page and line numbers in the revised manuscript with a edit-tracking format)

P2, L15: Could provide a more updated reference than Maslanik et al., (2011): Tschudi et al., Remote Sensing, 2016

Response: The updated reference Tschudi et al., 2016 is added in the revised manuscript. (see P2, L16)

P3, L17: Should provide the full citation (and include in the reference list) for the data, not just the website. NSIDC provides guidance on proper citation on their product website.

Response: the relevant reference is added, see (Tschudi et al., 2017) in the revision.

P3, L17: Note that a new version of the product will soon be released. At this point, I wouldn’t expect your analysis to be redone and I don’t think the new version would substantively change your results, but noting so that it is addressed in the final paper. Also, there is a new reference, submitted to The Cryosphere, documenting the changes: Tschudi, M. A., Meier, W. N., and Stewart, J. S.: An enhancement to sea ice motion and age products, The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-40>, in review, 2019.

Response: I have to admitted that this is a timely and fantastic work since the NSIDC SIM and age products have been widely acknowledged in various studies. We found that the updated version 4 product is now (Access date: 2019-04-08) is in a process to be further improved based on some error reports. We keep in mind that if the newest version (v4.0) data is available, the final paper will use it for a further analysis. The associated reference by Tschudi (2019) introducing the new improvement regarding the product is added in the revised text (See P4, L6)

P3, L18-19: The first letters in the SSM/I, SSMIS, and AMSR-E written out sensor names should be capitalized.

Response: The first letters of the sensor name as referred is capitalized in the revision. (see P3, L26-29).

P3, L22-23: There are a couple erroneous statements here. The SIM product is on the EASE-Grid, which is an equal-area projection – it is not polar stereographic. Also, some AVHRR images were removed due to errors, but AVHRR sources are still used for the 1979-2000 period. Likewise, a few buoys were removed, but buoys are used throughout the product. Maybe there just needs to be a rewording of the sentence to be clear.

Response: Yes, the SIM product is available on the EASE-grid and we corrected accordingly. The sentences are reconstructed and reworded to make it clear that AVHRR and buoy measurements are used in the composition of SIM and some error sources have been excluded. Please see P4, L2-6.

P3, L27: As for the SIM, the full citation should be provided for the SIC product. NSIDC provides guidance on proper citation on their product website.

Response: the associated reference (Comiso, 2017) is added. (See P4, L11)

P4, L4-5: Again, an error here in the grid/projection info. The SIC is on a polar stereographic grid, but it is not equal area. I assume that this is just a miswording. However, if an equal area is assumed for the polar stereographic grid, that will give incorrect sea ice extent and area estimates. Each cell in the polar stereographic grid has a different area, so when summing for extent, you have to account for the specific area of each cell. If this was not done, then I would say that the analysis needs to be redone. NSIDC provides grid cell area files for the polar stereographic grids.

Response: The misleading information is corrected and the SIC projection is presented as a polar stereographic grid. See the concerning texts in the revision (P4, L18).

P5, L3: I can understand the focus on the summer, but it's important to note that the quality of both the SIC and SIM fields lower during summer due to surface melt water. I think reasonable estimates can still be obtained, but the lesser performance should be noted.

Response: This is true. Surface melting causes ambiguous observational signals which are really a big problem to retrieve a real sea ice motion or sea ice concentration. Following the suggestion, we remind the author that summer performance may be less good as that in winter. (Please refer to P5, L15-16)

P5, L11: While the reference is provided, the uncertainty value is important and should be given explicitly here in the paper. A reader shouldn't have to dig up a reference for what is salient information. And as noted above, summer uncertainties are higher.

Response: The specific uncertainty values are summarized and explained in the revised manuscript. For example, the uncertainty of the daily sea ice motion data during winter is obtained as 2 cm/s (i.e., 1.70 km/day) (Ivanova et al., 2014; Sumata et al., 2015) (Sumata et al., 2015). Also, the uncertainty in summer SIM data is presumed to larger than that that used in winter. For more detail, please refer to P6,

L4-11.

P6, L10-11: What is the source for the DA index in terms of the data? I assume it's based on a reanalysis? That needs to be documented. Even if the values were provided directly by a colleague, the source that the colleague used needs to be cited.

Response: DA is retrieved from NCEP reanalyzed SLP product. The concerning information about DA is provided in the revision and a reference by Wu et al. (2005) is given for more detail about how the DA index is obtained. (See P7, L11-19)

P8, L2: I find the color scale for Figure 4 somewhat ambiguous – it's hard to tell where the 0 anomaly is exactly and which small values are positive or negative. I would suggest perhaps a gradient two-color anomaly scale with white denoting 0.

Response: We redraw the Figure 4 with a white color pointing to zero. (See Figure 4, for example, Feb 1979). Also, following suggestions by Reviewer 2, we added the annual average of the anomaly in the bottom row of the Figure.

P9, L11: Earlier in the paper (e.g., in the abstract), “yr” is used, but here “a” (I assume for “annum”). You should be consistent in usage, so choose one or the other (I prefer “yr”, but “a” is perfectly acceptable).

Response: For consistency in the use of time, “yr” is used through the paper.

P10, 3: For Figure 5, it might make things too busy looking, but I think it could be helpful to overlay the flux gates. Especially for January, it's hard to tell if the vectors are advecting ice across the Pacific gate or not.

Response: For a clear look about the sea ice movement around the fluxgate, the gate line is overlaid on Figure 5.

P11, L2: What are the “*”s next to the trends? I presume an indication of significance? That should be included in the caption.

Response: Yes, the label “*” denotes for significance. ‘*’, ‘*,*’, ‘*,*,*’ correspond to the significance level at 90%, 95%, 99%. This notion is given in the caption of Figure 6 (P12, L11-12).

P13, L4: The “summer season” should be defined here. It's noted further down on the page (June – September), but it should be specified when the term is first used.

Response: The definition of summer is presented in P14, L7.

P16, L10-14: I'm curious why AO shows such low correlation, but NAO shows higher. My understanding (which may not be complete or totally up-to-date) is that the two are very similar and that the NAO can be thought of as a regional expression of AO. Given that, I would've thought that AO would have higher correlation because it's hemisphere whereas the NAO is focused on the Atlantic sector. In other words, why would the Atlantic sector variability have much more effect on the Pacific sector than hemispheric variability?

Response: The relevant explanations have been given in P21, L11-19. In addition, for a clear analysis, climatic variables (SLP, SAT, and PW) related to AO trending changes are presented in Figure 14 (see Figure 14d, h, and i).

Since AO shows a negligible trend (Figure 13a), the 38-yr climatic changes related to AO are insignificant (Figure 14d, h and i) and smaller in magnitudes compared to the DA and NAO-associated changes. The NAO pattern is conventionally deemed as a regional index, representing parts of the broader AO pattern. However,

NAO-associated SLP changes (Figure 14g) show a stronger gradient across the fluxgate than that of AO-associated SLP (Figure 14h), which would favor more sea ice outflow from PA to AA sectors. In comparison with NAO (Figure 14g), the AO-associated SLP distribution shows a much weaker gradient across the Arctic Ocean (Figure 14h), although it may contribute to the sea ice advection from the Pacific side to the Atlantic side. As a result, lower correlations between AO and sea ice melting and advection processes are expected, with $R = -0.28$ and 0.14 (Table 3). However, these small overall correlations do not necessarily imply that AO plays no role in causing sea ice variations. For instance, throughout the examined periods, AO exerted more influences on sea ice changes for the earlier two decades (1979-1998), with $R = 0.57$ and 0.46 (Table 4).

P16, L23-25: This sentence is really difficult to follow. I'm not a big fan of the "positive (negative)" way of saying two things, but here it is especially tough because you use "positive (negative) NAO" for summer and then switch to "negative (positive) NAO" for winter. Trying to keep these straight is difficult! I would use "positive (negative) NAO" (or the reverse) for both cases and word the rest accordingly.

Response: The sentence is reconstructed for a clear expression. (P19, L4-6)

P17, L3: There is a lot of variability in yearly values, even when the correlation is fairly high. Did you test for significance of the trends? I would guess that none of them may be significant. I think that would be good to include here and in the discussion.

Response: The linear trends of NAO and DA indices are significant at the 95% significance levels according to the t-test method, while the AO index reveals an insignificant trend. This is noted in the caption of Figure 13. (P19, L9-10).

P19, L23: Here again in Table 4, I'm curious about how different the NAO and AO correlations are. The differences are quite stark. In particular, during the P4 period, the NAO and AO are even opposite signs. And while the AO has a higher correlation than the NAO through 2008, the NAO has had a much higher correlation since then. While earlier, you note that overall the AO correlation is weak and thus you focus on the NAO and DA, I think further discussion of the AO is warranted, particularly in terms of comparison with the NAO.

Response: The relevant explanations have been given in P21, L11-19. In addition, for a clear analysis, climatic variables (SLP, SAT, and PW) related to AO trending changes are presented in Figure 14 (see Figure 14d, h, and i).

Technical Corrections

P4, L17: Use "length" instead of "distance".

Response: Corrected as suggested (P5, L9).

P5, L28: typo, "does"

Response: Corrected as suggested (P6, L26).