Response to RC3

We truly appreciate the reviewer for the careful reading and constructive comments on the manuscript "Impacts of anomalies in Arctic sea ice outflow on sea ice in the Barents and Greenland Seas during the winter-to-summer seasons of 2020". We would thoroughly consider all comments with meticulous consideration, and endeavor to incorporate all of them into the revised manuscript.

The followings are our preliminary responses to these comments:

Unfortunately, the manuscript as a whole lacks for clarity in my view. I have read this manuscript several times, and I am still not sure I can summarise the main findings. There is a lot of interesting information in the paper, but I find it hard to follow the *reasoning* - it was often not clear to me what one should make of the specific results that were presented, and I often had a hard time understanding on what basis the authors arrived at their conclusions. The discussion section currently consists in large parts of new sets of results from an extended analysis, and does not in my view do much to aid the reader in the interpretation of the study. Throughout the paper, there is a general lack of separation between qualified speculation, knowledge based on existing studies, and conclusions substantiated in the analysis.

There are many numerical quantities in this paper that are given importance (correlations, averages, anomalies etc). In my view, it is not always clear what these actually are - e.g., what is being averaged and over what domain, which variables are being correlated, how averages are computed, etc. It is important that such ambiguities are addressed in a final version of the manuscript (but I think this should be fairly straight forward to remedy).

I am hesitant to recommend the publication of this manuscript, and I would not recommend its publication in its current form. Yet, I believe that the authors have done some very interesting work here, and I think that this paper could be a valuable contribution to literature - but in my view, this would require a substantial effort in revising the paper.

I have included more specific comments below, but they should not be taken as an extensive list. My main recommendation to the authors would be to focus on making it much clearer to the reader what their key findings are and how they arrive at their interpretations.

Reply: Thank you for the constructive comments. With your suggestions, we will diligently address them in our manuscript revisions, primarily by 1) distinguishing between the results and discussion sections, and meticulously organizing the main findings. Any portion of the discussion section that bears resemblance to the analysis of results will be relocated to the results section, while speculations based on data and

literature in the results section will be appropriately transposed to the discussion section. 2) Elaborating on numerical quantities, such as correlations, averages and anomalies, etc., with detailed and clear descriptions. 3) Checking and revising any ambiguous or imprecise descriptions in the manuscript, and restructuring lengthy sentences that commence with related words, to ensure clarity and precision. In conclusion, we will improve the text so that the reader can clearly understand the main findings and interpretations of this manuscript.

MAJOR COMMENTS

There are many statements through the manuscript of 2020 being a year of very high sea ice exports through the gateways. The clearest example is perhaps L443, which states that area flux through the gateways was "extremely large compared to the 1988-2020 climatology". I find that this is an inaccurate description of the data as described. E.g. in figure 3: 2020 looks like a year of fairly high exports, but does not seem to particularly stand out; for example, area exports were higher in all three gates preceding year of 2019. Elsewhere, it is stated that the JFM 2020 area flux through the FS gateway was 1.2 times the 1988-2020 mean (L219) - this does not strike me as extreme given the large interannual variability (and there is a corresponding ~20% *negative* SIAF anomaly through the FJL-NZ gate in 2020). Similarly for "extremely low" chlorophyll concentrations in April (L20) - is this really substantiated by the analysis? I think it is important to be quite careful with language here; strong statements need to be supported by a clear justification based on underlying data. (Note: I do not think this season being less "extreme" means that it is not worth studying! For example, an observation that an extreme AO year only had a moderate impact on sea ice outflow would in my view be a valuable contribution.).

Reply: Thanks for the suggestion. According to Figure 3, the positive SIAF anomaly for 2020 is large but indeed not particularly prominent, so we will check the statement that 2020 is a very high year for sea ice export and revise this description to make it accurate. For the description of the "extremely low" chlorophyll concentrations, we will calculate a time series of spatially averaged chlorophyll concentration over the BGS for 2005-2020 to give an accurate description based on the data.

The Results and Discussion sections in my view need reorganisation. There are many instances where some fairly strong claims which are not based on the present analysis are included in Results. Conversely, much of the Discussion section consists of the presentation of whole new sets of results and analysis that have not previously appeared in the manuscript. I strongly suggest reorganising the manuscript such that the Results section is reserved for the presentation of the outcome of the analysis (including that of

SST and chlorophyll), and the Discussion section for the authors' interpretation of the data/comparison with literature/qualified speculation etc. Given the complex topic and the many different datasets involved, the Discussion section of this paper is a great opportunity to carefully guide the reader through each key argument with reference to results from the different analyses.

An example of discussion in the Results section is found in ~L288 - L293. Here, there are some fairly broad statements about stratification which are not actually substantiated in the data. Similarly with e.g., L233-L235 / L239-241 / L299-L301 etc. These are important points but they require explanation and are not in my view obvious from the data alone.

An example of results in the Discussion section is the correlation analysis in 4.1. This reads very much like new results to me. Same with the SST/chlorophyll analysis that follows.

Reply: Thanks for the constructive suggestions. We examined the results and discussion sections of the manuscript and, as you say, they appear to have blurred boundaries and need to be reorganized so that the reader can easily understand. For the Results and Discussion sections, we will carefully check and place them in the appropriate section. We will place the correlation analyses of 4.1, SST and chlorophyll in the Results section. We will move speculations and statements made in the Results section of 3.1-3.4 based on data or literature (L288-L293/L233-L235/L239-241/L299-L301, etc.) to the Discussion section, and provide detailed explanatory notes for these statements.

If I have understood correctly, the atmospheric/sea ice area/chlorophyll analysis was done using averages across the area defined on L82 and shown as black polygons in Figure 1 BGS (if this is *not* the case, I suggest making it clearer which regions are used). The GS and BS boxes cover a vast area spanning quite different climatic environments and quite different ecosystems. I would guess that about half of this area is more or less never ice-covered in the present epoch; if chlorophyll in the southeastern part of the domain is influenced by sea ice inflows it may be in a rather indirect way. Likewise, if Figure 7 shows surface heat budget for the entire BGS area, I am not sure how meaningful they are. The authors need to justify this choice of a study area and discuss the implications of an analysis spanning widely different domains (or, if I have misunderstood, I would suggest that they clarify their methods..).

Reply: Yes, we used the averages within the black polygons in Figure 1 for the atmospheric/sea ice area/chlorophyll and surface heat budgets analysis. We have identified an irrational division of the study area and will change the boundary between the east and west sides of the study area to the coastline in the revised manuscript. The implications of this analysis will be discussed in the context of the fact that there will be ice-free areas within the study area. For chlorophyll, in conjunction with the

recommendations that follow, we will remove this part of the discussion. For surface heat budgets, we will recalculate surface heat flux anomalies and their associated changes in sea ice thickness, not across the entire BGS area, but in the ice-covered areas within the BGS area.

I would recommend going through the manuscript in general for clarity. In quite a few instances, the meaning of a sentence is ambiguous or, I suspect, not in line with the intended meaning. I've included some examples below under "technical/language", but they do not constitute a complete list.

Reply: We will revisit the manuscript to avoid ambiguities and incorrect meanings. Thanks for the reminder.

I found the discussion of ecological conditions/phytoplankton to be severely lacking. First, it looks like the discussion is based on satellite chl-a concentrations from areas not directly influenced by sea ice (spring blooms in southern BS, eastern/southern GS). It was unclear to me whether the authors believe that conditions were favourable (L398) or unfavourable (L19) to biological production. Repeated statements about extremely low chlorophyll-a in April 2020 seem to refer to sea ice-covered areas where there are in fact no satellite measurements (Fig 9a). The statement that "phytoplankton seeding" and "residue of marine nutrients" were responsible for high primary production (L399-L400) is neither explained nor substantiated at all. I would suggest removing the chlorophyll discussion from the manuscript unless it is completely overhauled.

Reply: Based on chlorophyll concentrations obtained from satellite remote sensing products, we quantified and analyzed the relationship between chlorophyll and sea ice area over the BGS. Due to the limitations of the data, we attempted but could only do some simple analyses and we were unable to substantiate and explain further mechanisms and linkages, resulting in a serious lack of discussion on ecological conditions/phytoplankton. We will therefore accept your suggestion to remove the discussion of chlorophyll from the manuscript.

I would say something along the same lines about the section on surface heat fluxes (L304 - L323). First, there is the issue of (apparently) using integrated fluxes over areas including huge ice-free areas of the north Atlantic to assess ice melting in the small northern/westernmost portions the domain (I would expect there to be large heat losses from the ocean to the atmosphere in the southern BS and over the West Spristbergen Current in winter, for example - how do they affect this estimate?). Second, the values that are given for estimated ice thickness change spans a huge range (1 to 41 cm). Lastly, I think it would be quite helpful to plot the actual delta h alongside the actual fluxes to clarify what we are actually looking at.

Reply: 1) We realize that it is not reasonable to assess the effect on sea ice melting by the surface heat flux anomalies across the entire BGS area, as this incorporates heat losses from the ocean to the atmosphere. We will change the calculation area for surface heat flux anomalies to the ice-covered area in the corresponding year to assess the impact of surface heat flux anomalies on sea ice melting. 2) We will then recalculate the estimated reduced ice thickness and superpose it on the surface heat flux anomaly graph, i.e., Figure 7.

MINOR COMMENTS

It might be nice to show time series of AO/CAI for context since these are pretty central to the paper. It's not strictly necessary, but I would suggest adding this as a small figure, at least in the supplementary.

Reply: Following your suggestion, we will add the AO/CAI time series to the appendix.

It is stated in the abstract (L14) and elsewhere that "the variability of.. total SIAF was dominated by changes in ice motion speed", listing a high and significant correlation. In the text I could not find any details about how you actually calculated this, and given that this is a key point in the abstract I think the authors need to elaborate on actually how this quantity is computed, e.g.:
Is this a correlation between monthly values of a) SIAF across all three gates and b) mean speed across all three gates? Over what time period?
How are no-ice instances counted (for SIM and SIAF)? As zero, or are they not included?

Reply: We will add a description of the calculation of this correlation in the method section. 1) Actually, this is about the correlation between the sum of monthly SIAF for all three gates and the monthly mean SIM speed for all three gates. Seasonal mean SIAF and SIM speed were calculated based on winter (JFM) and spring (AMJ) to further quantify the correlation between them seasonally. Correlations are calculated based on data from 1988 to 2020. 2) NSIDC SIM speed is only available when the SIC is greater than 15%, and SIAF will be negligible when the SIC is zero.

Along similar lines, it should be clearly stated whether "sea ice thickness anomalies" include "thickness" from ice-free periods. (Fine either way, but "thinner ice" and "less frequently ice-covered waters" are different things, for example).

Reply: Thanks for the suggestion. We will add sentences to the section on sea ice thickness anomalies to clarify whether such anomalies include "thickness" from ice-free periods. In the calculations, "sea ice thickness anomalies" do not include the "thickness" from ice-free periods.

I think it should be discussed whether comparing trajectories from one single day (e.g. 31 May 2020) with a climatological mean vector field (e.g. May 1988-2020 mean drift) may (or may not) influence the analysis. Is a trajectory along an average field different from a time average of several trajectories? Does the long-time average introduce a low speed/short trajectory bias? (I don't really know - but I think it warrants at least a brief comment in the paper given that this is an important point in your study).

Reply: This is an important point. Yes, this may be a misunderstanding due to the lack of clarity in the labeling of Figure 4, which will be revised to clarify its meaning. The trajectories in Figure 4 all refer to the backward trajectory from a particular day. Figures 4a-c all refer to the backward trajectory from a specific date in 2020 back to January 1. The 1988–2020 mean trajectories in Figures 4d-f do not refer to the temporal averaging of multiple trajectories, but rather the average SIM vector field in 1988–2020.

SIAF is basically the integrated product of SIM and SIC, so if it were not SIM that controlled SIAF variability it would presumably be SIC (or am I misunderstanding?). You should therefore probably include the corresponding correlation between SIAF and SICs somewhere in the paragraph at L236 in support of your statement that SIM controls SIAF.

Reply: We will calculate the correlation between SIAF and SIC and state their correlation in the corresponding text.

L45: I would revise the statement that sea ice outflow "contributes" to deep water formation in the north Atlantic. Please clarify what is meant here (presumably that high sea ice export -> high fw input -> increased stratification -> *inhibited* dw formation - or am I missing something?). I also can't see that Lemke et al. 2000 is a great reference here - they show a large variability in sea ice export but don't actually look at DW formation as far as I can tell?

Reply: Good suggestion. We will revise this sentence to clarify the effect of sea ice outflow on the deep water formation in the North Atlantic. And we will remove this literature and cite more relevant references to support our statement.

Figure 1 is excellent. The authors could consider adding mean SIC or similar (not necessary but might give some additional context to readers unfamiliar with the region). Reply: Considering readers unfamiliar with the region, we will add the average SIC for January-March 2020 as a background to Figure 1.

The switch from CS2SMOS to PIOMAS seems to warrant a (quick) comparison of the two in the overlapping period. Should add at least a sentence about this somewhere around L123.

Reply: We will compare these two SIT products in the overlapping period and explain this in the data section of SIT.

Which temperature does the oiSST give for fully ice-covered waters? What about for partial ice concentrations? Should be included in the presentation of this product somewhere after L124, and may be relevant for the interpretation of the SST data.
Reply: Thanks for the suggestion, we will add the corresponding details in the data section of the SST.

L159: I believe that there is no SIM vector for SIC<15% in this product? I don't believe that is a significant problem for your methods, but it is probably worth a mention here.
Reply: We will add specific notes in the methods section.

L161: Was one trajectory computed from each grid point on each gateway?
Reply: In fact, we divide a total of 400 points from the location of the three passageways and then calculate the backward trajectory from these points. The number of points varies for each gate, from 200 points (with a distance of ~ 448 km) for the FS, 100 points (with a distance of ~ 284 km) for S-FJL and 100 points (with a distance of ~ 326 km) for FJL-NZ.

L169: This seems to imply forward propagation, I would suggest reordering the equations to show backward propagation if that is what you do. Also, if delta t is negative as suggested in the preceding paragraph, I think this equation is in fact incorrect (wrong sign of second term)?
Reply: Thank you for the reminder. We will check and revise the formulas to show backward propagation.

L223. If you attribute the 20% positive anomaly at FS to the AO anomaly, it seems strange to say that 20% negative anomaly at FJL-NZ means that flow across this gate was "not sensitive". Could it not be a similar-magnitude response of opposite sign? Please elaborate.
L226: See above. This seems like a huge negative anomaly, and the idea that positive=response and negative=insensitivity needs an explanation at least. I also have a hard time seeing the 85% negative anomaly in Figure 3c - is this because the absolute values are small?
Reply: 1) As mentioned in the result section, positive AO anomaly promotes sea ice transport to the BGS, so we assume that a response to positive AO anomaly implies an increased SIAF, which would be large compared to the 1988-2020 climatology. In contrast, the FJL-NZ gate showed a large negative SIAF anomaly, which we believe

that the SIAF through the FJL-NZ does not respond significantly to the positive AO anomaly. We will revise the statement about the response being sensitive or insensitive. 2) L226 refers to the fact that the spring cumulative SIAF through the FJL-NZ gate is only 14.1% of the 1988-2020 climatology, while Fig. 3c is the difference between the monthly SIAF and the 1988-2020 climatology. so it is difficult to see the 85% negative anomaly there.

L230: The phrasing here is a bit ambiguous - should make it clear what exactly these percentages are. (I assume that they are the fraction of SIAF through all three gates that went through FS - but it can read as the percentage of the *anomaly*). Same for L13.
Reply: We will revise the phrasing to clarify the meaning, these are the SIAF through a single gate as a fraction of the SIAF through all three gates.

L233: In my view, this is a strong statement that needs substantiation. What is your evidence that the low Arctic SIA was a result of increased outflow? Probably also a better fit in Discussion.
Reply: We will quantify the correlation between Arctic SIA and SIAF and discuss it in an updated discussion section.

L237: Please explain exactly what this correlation is.
Reply: A detailed explanation of this SIAF and SIM correlation calculation will be added in the method section.

L251: " the anomalies of sea ice volume outflow.." This is a bit confusing. You don't have these numbers, right - is it that you would *expect* the volume export anomaly to be more pronounced than the area export anomaly? Rephrase for clarity.
Reply: We will rewrite this paragraph for clarity.

L267-L268: This seems true for the FS, but not for the two other gates?
Reply: We will revise the sentence to state that it only applies to the Fram Strait.

Figure 4: I suggest using much thinner lines to show that these are trajectories and not a continuous scalar field - I think this figure is nice, but it took me quite a while to figure out what was going on.. Given that trajectories don't run past January, I don't think the colour by date actually adds that much, so I wouldn't be worried if thinner lines show the colours less well. Should also change the labels in abc from e.g. "April 2020" to "30 April 2020" if you indeed only show back-trajectories from one single day in these. (You should also explain why you chose to compare one day per month with monthly climatological means).

Reply: Thank you for your suggestion. 1) We will use thinner lines to show the trajectories, these dense lines are because we used 400 points located at three gates as endpoints. 2) The trajectories in Figure 4 all refer to the backward trajectory from a particular day. Therefore, we will change the labels of Figure 4 to specific dates for easier understanding. Specifically, figures 4a-c all refer to a backward trajectory from a particular date in 2020 back to January 1. The average 1988-2020 trajectory in Figures 4d-f does not refer to the temporal averaging of multiple trajectories, but rather the average SIM vector field in 1988-2020.

L298: It's not clear to me from Figure 6a that there was a positive SIT anomaly in the BS - please explain.
Reply: The positive SIT anomaly in BS in Figure 6a is small and not very obvious, and we will revise the description of this sentence.

L301: Similarly, it does *not* look to me like an overall positive anomaly in the GS in Figure 6d. And it seems strange not to address the east-west pattern in the GS here. And, as mentioned elsewhere, I think you also need to say something about whether the positive anomalies are a result of anomalously high sea ice *extent* or anomalously thick ice since this could impact your argument about increased import of thicker ice from the central Arctic (probably depends on how you compute these means).
Reply: We will revise the phrasing of this sentence and discuss the east-west pattern of the SIT anomalies that occur in GS. We will also clarify how SIT anomalies are calculated and explain the meaning of positive anomaly results.

L337-8: Is it surprising that you did *not* find a significant correlation between SIAF and AO in months other than February? Seems to warrant at least a mention.
Reply: We will add sentences stating that no significant correlation was found between SIAF and AO except for February.

L340: What about R?
Reply: The correlation coefficient R is shown in Table 2 and we will add the description of R to this sentence.

L345-351. I found it difficult to see this in Figure A1ab, please explain how you arrive at this conclusion. To me, it looks like there is little difference except perhaps that the drift is stronger in the negative AO phase (A1b). It also looks like these are the trajectories of ice arriving at the gate in mid-summer - so is it actually representative of AO anomalies in winter? Please clarify.
Reply: 1) This can be seen at the end of the backward trajectory in Fig. A1 ab (blue

trajectory). The western edge of the blue trajectory in Fig. A1a extends westwards, while that in Fig. A1b moves closer to the prime meridian. This suggests that under the positive phase of AO, sea ice originates further west, i.e., the TPD shifts westwards, thus reducing the spatial extent of the BG. Rigor et al., (2002) mentioned that such a pattern favours the transport of sea ice from the central Arctic Ocean to the BGS. The results in Fig. A1 ab are consistent with the westward alignment of the TPD under the positive AO in Figure 4. 2) These represent the backward trajectory of ice arriving in the Fram Strait on 30 June back to 1 January, and we focus on the location of the winter (JFM) trajectory which is also the source location of the sea ice arriving in the Fram Strait. We have chosen years with winter AO anomalies to check whether the January-June backward trajectory of sea ice arriving in Fram Strait in June is affected by winter AO anomalies. We will revise this paragraph to clarify.

Reference:

Rigor, I.G., Wallace, J.M., and Colony, R.L.: Response of sea ice to the Arctic Oscillation, J. Clim., 15(18), 2648–2663, https://doi.org/10.1029/1999gl002389, 2002.

L365: Are these SST anomalies integrated over the whole area? Also, are the maps in Figure 8 really anomalies from the climatological mean for the month? To me they look like anomalies from the SST average across the entire season or something (+4C anomalies in the southern BS in July and -3C anomalies in the northernmost range in April both strike me as weird). I could certainly be wrong - but it might be a good idea to double check.

Reply: Thanks for the reminder. These SST anomalies were obtained by performing calculations within the study area, and we will check the process of calculating SST anomalies and redrawing Figure 8.

L389 onward: Again, it seems strange to me to do this sort of analysis across this very large area - you at least need to discuss the implications of 1) including large perennially ice-free areas, and 2) there being no satellite measurements of chlorophyll in ice.

Reply: Thank you for your suggestion. In light of the major comments related above, we acknowledge that the discussion of chlorophyll is indeed severely lacking. Given that only satellite remote sensing chlorophyll concentration products were used, we will follow your suggestion and remove the chlorophyll discussion section from the manuscript.

I found the Conclusions section to be quite clarifying and well-written. I would encourage the authors to use a similar style in an updated Discussion section.

Reply: We will use a similar style in the revised discussion for clarity.

TECHNICAL/LANGUAGE

L13: "77.6%": I gather from the conclusion that this is the fraction of the total flux through the three gateways that goes through the FS gate. That was not clear to me when reading the abstract alone.

Reply: We will revise the sentence to clarify that 77.6% is the proportion of the total flux through the three gates that is through the FS gate.

L51: "More primary productivities" - please use more precise language.

L55: I would suggest reordering this sentence for clarity.

L63: "Relatively extreme" - please use more precise language.

Reply: We will revise these phrases using more precise language and, for sentences, we will reorganize them for clarity.

L69: "Thereby.. ..the BGS." - Meaning of this sentence is unclear.

Reply: This sentence will be rewritten to clarify the meaning.

L82: Studies -> Study?

L107: "abnormal" - "anomaly"?

L124: "Could be used as the best proxies for.." - please rephrase. "We use SST and chl as proxies for.."?

L137: Suggest replacing "a significantly more advanced" with "an advanced" or similar ("more" relative to what?).

L143: "According to" - "In accordance with" or similar?

L146: These directions aren't really S/N? Maybe replace "southward" with "toward the BGS" or something.

Reply: These grammatical mistakes and inappropriate expressions will be revised as suggestions.

L185: "with the values maintaining the top three.." - This phrasing is a bit unclear (here and elsewhere in the manuscript).

Reply: We will revise all similar phrasing in the manuscript to clarify its meaning.

L215-L217: Suggest moving this sentence to data/methods (and adding e.g. "relative to the 1988-2020 climatology" after "June 2020" in L218).

Reply: We will follow your suggestion to move the sentence and add "relative to the 1988-2020 climatology" in the original position.

L238: Maybe "monthly SIM speed" to avoid confusion.

Reply: We will revise the phrasing to avoid confusion.

Reply: Thanks for the reminder. We found that the geographical location of the confluence is not visible in Figure 4. We will add the location of the marginal sea and the central Arctic Ocean to Figure 4.

Reply: This sentence is used to describe that the backward trajectories obtained from the S-FJL gate are more curved than that obtained from the FS gates, and we will revise this phrase to make it clearer.

Reply: We will rewrite this sentence for clarity.

Reply: For this section, we will reorganize and rewrite it to ensure that this section is clear and easy to understand.

Reply: This refers to the correlation between the sum of the SIAF through the three passageways and the SIA of the BGS. We will rephrase it to clarify the meaning based on your suggestion.

Reply: We will correct misspellings and inappropriate phrases based on your suggestions and rephrase inappropriate expressions for clarity.

Reply: We will check and revise the entire manuscript to see if the meaning of these

sentences retains the intended meaning.

Figure A1: Please label clearly which plots correspond to positive/negative phases, or at least explain/refer to labels in the caption ("AO+sd" is not very intuitive on its own). I would also suggest adding the number of years that go into the mean (e.g. "n=3") somewhere on each subpanel (not necessary but would aid interpretation).

Reply: Thanks for the suggestion, we will explain the meaning of the label (e.g. "AO+sd") in the caption of Figure A1 and add the number of years into the mean calculation in the sub-panels of Figure A1.