Response to reviewer 2

May 7, 2018

The authors thank the reviewer for their careful reading of our discussion paper, and for their helpful and constructive comments regarding its content and improvement. The text of the review is reproduced below in black type; our comments are in blue; and changes to the original discussion paper are presented in italics.

Overview: In this manuscript the authors investigate the sources of freshwater transport in the Labrador Sea, the locations at which freshwater enters the central basin, the dynamical mechanisms responsible for this transport, and the controls on seasonal and decadal variability in the transport. Their tool is an unconstrained 1/12 degree multi-decadal integration of the NEMO coupled ocean/sea ice model, in combination with the offline Lagrangian particle advection tool ARIANE. The authors derive Lagrangian particle back-trajectories for waters in the upper 30m of the central Labrador basin over a 20-year period, and then compute statistics associated with the frequency at which particles cross into the basin and the salinities associated with the crossings.

The authors find that most of the particles originate from the shoreward and offshore branches of the East Greenland Current (EGC), in agreement with previous studies, and that the particle crossings occur predominantly in what they call the "Northeast" and "Southeast" sectors of the Labrador Sea. The waters entering from the inshore branch are fresher by 0.1 salinity units on average. The inflowing EGC inshore-sourced water exhibits substantial annual variability in both probability of particle crossings and, in the "Northeast" Labrador Sea, in its salinity. Based on this, the authors infer that inflow of relatively fresh EGC inshore-sourced water occurs in two peaks: one in September, and one around April.

The authors then contrast eddy kinetic energy (EKE, a proxy for eddy particle transport into the basin) and wind-driven Ekman transport as mechanisms underlying the diagnosed particle transport. Both EKE and Ekman transport exhibit seasonal cycles, though the Ekman seasonal cycle is much more pronounced in the "Southeast" section of the Labrador Sea, while EKE is more pronounced in the "Northeast" section. On interannual time scales, the probability of particles having entered the basin correlates significantly with the wind stress in both the Northeast and Southeast sections, but particularly strongly in the Northeast, where Ekman transport variations explain 50% of the variance in the particle crossing probability. Based on this, the authors infer that winds control interannual variations in freshwater inflow to the central Labrador basin.
We have based this statement on the quantitative result noted in Table 2. From this, we see that the Ekman transport variations explain more than 70 % of the variance in the particle crossing probability. In addition the paper concludes that wind controls interannual variations in freshwater inflow of the top 30 m to the central Labrador basin.

This manuscript addresses an important topic, the analysis is interesting and insightful, and in my opinion this work is worthy of publication in Ocean Sciences. However I have a long list of comments on the manuscript (see below), including some quite strong criticisms of the authors' methodology and the evidence supporting their central conclusions. My most major concerns relate to (i) the authors conclusion that freshwater enters the Labrador basin in two "pulses" each year, which does not seem to be supported by their calculations, and (ii) the authors' decision to focus their particle deployments and particle crossing analyses on the upper 30m of the water column, which inherently biases their results toward wind control of freshwater transport.

Therefore, major revisions of the manuscript, likely including substantial additional calculations, will be required to bring this up to a standard appropriate for publication. The manuscript itself is well structured but poorly written: as noted below, there were too many spelling errors, grammatical oddities, and instances of unclear phrasing to list in this review. The manuscript will therefore extensive proof-reading by a native English speaker during revisions.

Comments/questions:

At times I found it difficult to make my way through the manuscript due to the high density of grammatical and spelling errors, and awkward phrasings (in various cases so as to render the meaning unclear). I initially tried to catalogue these errors to pass them on to the authors, but quickly gave up due to the sheer number of them. During revisions the authors should pass the manuscript to a native English speaker for detailed corrections throughout, as I do not consider the current standard of writing to be suitable for publication. Additionally, in other places the writing is rather vague, and I have attempted to identify such instances in comments below.
We apologize for the errors in the manuscript. We note, however, that the other reviewer called the paper "well-written" and did not have the same comments regarding the language.

This reviewed version of this manuscript was sent to a professional editor for revision and we are positive that grammatical and spelling errors are no longer present.

p1, L10-12; p10, L6-7; p13, L4-5: I am not convinced that the authors' evidence supports this conclusion. I was initially confused by the authors' wording in the abstract, where they claim that they diagnose two peaks of freshwater transport into the LS; I wondered why they distinguished the first peak as being associated with "a large number of shelf water particles". After reading the manuscript, it became clear that the converse statement is more relevant: the second peak in the salinity anomaly (in the particles from the inner EGC entering via the "Northeast" section of the LS) is not associated with a large number of shelf water particles, at least not compared to the first. Given that the actual freshwater flux may be expected to be related to the product of the salinity anomaly with the number of particles, is this second peak even worthy of note?
It is true that the second peak is not associated with a particularly large number of crossings (compared to the first peak) but we do believe that it is still worth noting. Freshwater anomalies can occur because a large amount (large number of particles) of freshwater enters the region, or because a smaller amount of really fresh water enters the reason (e.g. during the second peak). Also not that during the first peak a large amount of salty water enters the basin in the Southeast. This could have the effect of balancing the high number of crossings of freshwater in the northeast. Hence the second peak might even be stronger in terms of how the freshwater impacts the basin, since in the fall the water entering in the southwest is much fresher.
Perhaps the authors could produce some quantitative estimates of the freshwater flux associated with this "peak" to support their conclusion, but my reading of their current results is that there is really only one peak in the freshwater transport into the LS, occurring around April.
We have estimated a freshwater flux from the number of particles that cross into the basin and their salinity (not shown). Unfortunately, the calculation is limited by the model's resolution. One issue is that more than one particle could cross within a Eulerian grid cell but the model would not distinguish this and would instead count the crossing twice. After further consideration, we did not feel that the calculation warranted publication.
Instead we use the probability of fresh/salty water entering the basin. Doing so did not change, but instead confirmed, the correlative findings (between particle crossing probabilities and potential forcing terms) which was also found when initially working with the an estimate of the freshwater flux.

p1, L16-21: This discussion should be accompanied by supporting citations.
Apologies for the omission. References have been added

p1, L19: "the salty basin" - does this simply refer to the central Labrador Sea? In general I found the authors' "basin" terminology to be ambiguous. They should clarify how they and previous authors distinguish basin from shelf, and ensure that nomenclature is consistent with previous studies.
Yes, "the salty basin" does refer to the central Labrador Sea. We define our definition of the basin on p.10, l.293 *"We refer to the Labrador Sea basin as the region that is offshore of the 2500 m isobaths"*. However, we see that it would be useful to the reader to mention this definition sooner, and have added the following in the introduction:

p.1 l.30 *Offshore of the boundary currents, in the salty basin*, […]

p2, L11: There appears to be a missing citation here (replaced instead with a "?").
This has been fixed.

p2, L23-24: Do the authors' findings not contradict this? By my reading, the authors diagnose a much stronger Spring pulse of freshwater than in Fall. In the Discussion (p13, L7-8) the authors explicitly state that the opposite is true, and that their findings are consistent with Schmidt and Send 2007. I think a more candid discussion of differences between the authors' findings and previous results is required, as currently this is difficult to reconcile.
Our findings indeed support Schmidt and Send's findings. We find a spring pulse, by itself it is stronger than the fall peak, but considering the large number of particles with high salinity that enter the basin at the same time in the model, the overall effect of freshening on the basin is small according to our metrics. The fall peak seems weaker at first glance, but considering that there is relatively fresh water entering in the southeast also, the peak becomes much more significant. We have added an additional comment to clarify this in the "*Seasonality of crossings*" section.

p3, L8; p4, L33; p5, L20; p9, L12; p13, L13 (and more; I gave up listing them): At various points the authors make vague statements such as "substantial buoyancy is lost", "the model well represents", or "a strong WGC". Without some quantitative measure, descriptions like "substantial", "well" and "strong" become simply subjective judgements on the part of the authors.
We have edited the manuscript with an eye on such statements and have reworded them on many occasions.

p4, L5-6: Please check the value given for the bi-Laplacian viscosity. If this value were used, the time scale for viscous mixing at the grid scale (4km) would be on the order of 10,000 years!
Apologies for this typo, it should have read 3×10^{11} m^4/s . This has been corrected.

p4, L9: Is "integrated" the correct word here. If I understand correctly, DRAKKAR is a reference surface forcing dataset with components drawn from various existing datasets, rather than a model that is integrated forward in time.
We can see how "integrated" could be interpreted incorrectly in this context. We have changed the sentence to:
"It is used for the period 1958 – 2012".

p4, L24: Please state the data source used for the river runoff.

Reference has been added.

p4, L26: In addition to bottom friction, pressure forces also exchange momentum between the ocean and the solid earth.
Thank you for pointing that out.

p5, L2-4: The authors appear to have omitted item 3) from their list of 4 changes to the NEMO model. Also, what changes were made to the (presumably sea floor) topography?
The typo of the list numbers has been corrected.
We changed number 2) in the list to "2) steeper topography along the Greenland Coast" to highlight the changes we were referring to.

p5, L9-11: I disagree with this statement. The correct location and magnitude of the ML depths shows that NEMO accurately represents the ML depths. It is a point in favor of NEMO accurately representing the LS state and circulation in general, but is hardly a clear-cut demonstration of the model fidelity.
We agree that the initial discussion overstated the model fidelity based on the measure of ML depths. We have softened the statement to:

> ***p.7 l.207:*** *In the NEMO N06 model, the deepest winter mixed layers in the Labrador Sea basin are located in the western basin, consistent with observations (Pickart et al., 2002; V_age et al., 2008; Schulze et al., 2016), (Figure 1). The model tends to over210 estimate the mixed layers in the Labrador Sea basin (Courtois et al., 2017), but the agreement of the mixed layer depths and location indicates that the boundary current, and advection of freshwater and heat into the basin, are represented well.*
> *Without this representation the basin strati_cation would be weaker and mixing would be stronger. This in turn would result in mixed layers in the wrong location that are much deeper than in the observations. The relationship between fresh shelf water and mixed layers in the basin can be seen in a previous model study (McGeehan and Maslowski, 2011).*

p5, L11-12: Is this statement based on model experiments, or is it simply a speculation?
It is based on theory and a comparison with the previous version of the NEMO model (not shown) that did not have realistic mixed layer depths.

p5, L19: The model and ARGO salinity distributions look qualitatively different to me: there are many ARGO profiles measuring relatively low salinity in the middle of the LS basin, and the shape of the high-salinity region looks to be quite different. Perhaps this is simply due to my subjective interpretation of Fig. 1. To remove the ambiguity here, the authors could provide quantitative metrics of the similarity between the modeled and Argo-derived salinities. Perhaps some of the apparent disagreement stems from the seasonal cycle in the measurements? The authors hint at this on L24. but do not show any data on the model vs. Argo differences in the seasonal cycle.
It is true that there are some differences in the ARGO and model data. However, there are also similarities, such as the general distribution of salty and freshwater in the basin and the magnitude and amplitude of the seasonal cycle. While we do not show the seasonal cycle, it is described:

> **P. 8, L 236:** *"Seasonal cycles of the basin-averaged salinities in NEMO and from Argo data are in phase with peak salinities in February - March and the freshest water in September. Modeled salinities are overestimated by 0.1 between November - June. "*

p5, L26: “in many studies” is not a suitable substitute for citations
We have edited the manuscript with an eye on such statements and have reworded them on many occasions.

p6, L9: Where is “outside” the 2500m isobath? Toward greater depths or toward shallower depths?
“outside” has been changed to “inshore”

p6, L14-15: This statement should be supported by evidence if the authors plan to retain it in the manuscript.
We have referenced Figure 7 to support this statement. Figure 7 shows the seasonal composites of the EKE.

p6, L29-30: At various points the authors’ descriptions of the particles becomes confused by the fact that they are calculating back-trajectories, so e.g. it is difficult to tell what “the last time” a particle crosses the LS boundary actually means. In this example the ambiguity is between the first chronological crossing and the first crossing that occurs during backward time-integration.
We agree that this can be confusing, but have made sure that the entire manuscript is consistent in how the direction of the trajectories are described. We have also changed the paragraph referred to here to:

> **p.9 l.295:** *While the particles were released in the basin and tracked backwards, we will refer to there trajectories forward in time (e.g. particles enter the basin and end up at their release point). A particle is considered to have entered the basin if it crossed the 2500 m isobath from shallow into deeper water within the top 30 m of the water column. If a particle crosses the isobath multiple times, only the last crossing before reaching its release point is considered.*

p7, L2-3: This is an important methodological point that requires more explanation, and in fact I am concerned that this choice biases the author’s results toward wind control of particle crossings. The authors only deploy particles within the top 30m, (approximately within the Ekman layer) and only count particles as having “crossed” into the LS central basin if they do so within the top 30m. On p6, L22 the authors claim that “most freshwater is contained in the upper 30m”. First, how much is “most”? Second, storage depth does not necessarily equate to transport depth - it is quite plausible that freshwater could enter over a greater range of depths, but only accumulate in the upper 30m.
If the authors had deployed their particles over a greater depth range then they could defend their focus on the upper 30m, as they could compare freshwater inflow in the upper 30m against that occurring deeper than 30m. I consider this to be quite a serious caveat: this choice could potentially explain the apparent dominance of Ekman transport over eddies in controlling the diagnosed interannual variability in freshwater transport into the central LS, and the discrepancy between the relative magnitudes of authors’ diagnosed “pulses” of freshwater inflow and those reported in previous studies.
This is a good point. It is true that the method might be slightly bias towards Ekman transport, mainly because particles are only released in the Ekman layer. Because of this, we have addressed this issue in the discussion where we show that the surface 30 m make up 60% of the total freshwater flux over the top 100 m and that eddy fluxes become more important only when extending the calculation to 200 m.

Releasing particles over the entire water column would be crucial if attempting to close the freshwater budget of the Labrador Sea basin. This would be very interesting and it is true that eddies might be the dominant means of advecting freshwater to the basin. However, from ARGO floats and repeat hydrography sections by Yashyaev et al. we do not expect the deeper water to be fresh. Typically, below about 100 m the warm and very salty Irminger water dominates. Hence when trying to describe pathways of freshwater into the basin, we have opted to consider the surface layer, since the deeper water of the boundary current has been shown to be salty. This is a choice made throughout, and it does differ from other choices made to investigate the freshwater transport (models) or freshwater content (observations – e.g. Straneo 2006, Häkkinen 1999). While we agree that this choice highlights the freshwater transport by Ekman transport, this is also a meaningful way to distinguish between the layers in the Labrador Sea. Ekman transport is surface intensified, and while we so not attempt to determine the thickness of the Ekman depth, we expect that the top 30 m will capture the variability of the signal. Eddies would be likely to transport both the surface freshwater and the subsurface warm/salty water (Hatun et al 2007) which could actually decrease their role in freshwater transport into the Labrador Sea.

p7, L11-12: I am confused by this statement: don't the authors define "entering the basin" to mean that particles have crossed the 2500m isobath? Perhaps this relates to my earlier comment about the authors' vagueness in referring to "the basin".
The basin in this case is defined as the region offshore the 2500 m isobaths. This definition does exist earlier in the manuscript, where we define the particles that are considered to have crossed the 2500 m isobaths.
Hence the manuscript states that *"Of the remaining 323,084 trajectories that are not categorized as crossings according to the above criteria [...]"* In this category fall particles that enter the basin from the south, hence the North Atlantic but have never been in shallower water.

p7, L19-23: The criteria listed here are not mutually exclusive: do any particles satisfy multiple criteria? If so, is the determination of their origin performed following the logic indicated in these sentences?
Yes, the criteria where chosen such that no particle satisfies multiple criteria.

p7, L30-31: Difficult to parse because "end of their lifetime" actually refers to the chronological starting position of the particles - see earlier comment on the clarity of the authors' description of the particle trajectories.
When referring to "end of their lifetime" in the manuscript, we always refer to the end of their one year runtime. This is consistent with Sections 2.2 and 2.3.

p8, L24-25: I found the authors' geographical descriptions confusing because "south-east" actually refers to the eastern side of the LS region in which particles are deployed, while "northeast" actually refers to the northern tip of this region. I suspect other readers might similarly be misled by this terminology, and recommend changing to something more intuitive.
We believe that the naming of our sections is consistent with their location. The southeast refers to the southern part of the eastern side of the Labrador Sea basin, the northeast refers to the northern part of the eastern side of the Labrador Sea. While we could have named the northeast the 'north' it would have been more difficult to describe the more gradually sloping north to the northwest region in the Labrador Sea. While it would have been simpler if the central axis of the Labrador Sea were meridional, we did consider this choice extensively, and opted for the one used in the manuscript as the most generic.

p9, L24-31 (but also at various other points in the manuscript): The authors mischaracterize the probabilities that the calculate as e.g. the "probability of particles ... to enter the basin" (note that here the grammatical oddities are the authors'). The authors calculate the probability of particles having originated from a given region, given that their back-trajectories crossed the LS perimeter. This is different from the probability of waters originating in, e.g., the EGC inshore region crossing into the central LS - to calculate this the authors would need to compute forward trajectories for particles initialized throughout the EGC inshore region. Strictly speaking, the probability that the authors' particles enter the basin is 100% because their trajectories all end in the central LS. The authors should rewrite all sections of the manuscript that discuss these probabilities to accurately characterize the results. E.g. on p10, L1-2, "inshore water is about twice as likely as offshore water to enter" might be more accurately written as "entering water is twice as likely to have originated from inshore as to have originated from offshore".
We have revisited the manuscript keeping this comment in mind. The reviewer is correct that the probability of particles entering the basin is 100% because all particles were released in the basin. While it is true that all particles end up in the basin, here the probabilities refer to the percentage of those particles that did so crossing through a certain region or in a certain time period. This is described in Section 2.5.

p11, L18-19: The authors describe the correlation as "significant", but do not define the criterion for statistical significance.
The reference for the method with which the correlation was calculated is given in:

> ***P.17 l.498:*** *The timeseries for EKE and Ekman transport are correlated with the probability anomaly using the Pearson method (Thompson and Emery, 2014).*

p13, L30-32: Here the authors explicitly decline to address the mechanism via which EGC offshore water is transported into the basin. I do not think this is acceptable in a manuscript that explicitly aims to quantify the relative roles of different mechanisms of freshwater transport into the LS. This point should be addressed in detail in a revised manuscript.
Unfortunately, addressing all mechanisms of freshwater transport into the Labrador Sea is beyond the scoop of this paper. Here, we have focused on diagnosing the regions of freshwater transport into the basin, and used the additional eddy and Ekman analysis to add insight to those central results. Eddies are the canonical view, while wind-driven transport became the unexpected major player in the study following the interannual analysis, after we discovered that eddy and Ekman transport could not be distinguished based on the seasonal cycles alone. However, we do agree that this is an intriguing question and that it would be great to study not only the impact of wind, but also the impact of other mechanisms on the freshwater transport. As the reviewer addressed earlier, for this particles should be released throughout the entire water column to avoid a bias towards wind driven exchange between the shelves and basin.

p14, L4-5: This calculation is likely to be sensitive to the choice of the reference salinity, and may be producing a misleading estimate of the Ekman freshwater flux. The authors calculate the mean and eddy components of the freshwater flux across the "northeast" and "southeast" sections of the LS boundary - a useful complement to the Lagrangian analysis that serves as the focus of the paper. That is they integrate the boundary-normal components of <u><S-Sref> and <u'(S-Sref)'> along the boundary, where angle frackers < > denotes a time average. Now, the eddy component is insensitive to Sref because <u'>=<S'>=0 by definition, so <u'(S-Sref)'> = <u'S'> + <u'Sref> = <u'S'> - <u'>Sref = <u'S'>. However, the mean component is <u><S-Sref> =

<u><S> - <u><Sref>. If the boundary integral of the boundary-normal component of <u> is non-zero (which seems very probable given the short lengths of the “northeast” and “southeast” boundary segments, and the prevailing northwesterly winds), then changing Sref will change the computed freshwater flux. Given that the choice of Sref is arbitrary, this renders the authors’ estimate of the Ekman freshwater flux arbitrary. A solution is to integrate both the eddy and mean components over the full ocean depth, and to perform the integral along a contour of the time-mean depth-integrated streamfunction - this guarantees that the along-contour integral of <u> is zero, and therefore removes the arbitrariness introduced by Sref.
We agree that the calculation is sensitive to the choice of reference salinity. However, the choice of reference salinity is not arbitrary but was instead defined as the average salinity in the surface layer of the basin. In this way, it is used to determine whether the particular transport of water has a net freshening or a net salinifying effect. Unfortunately, this was not clear in the writing, for which we apologize. We have added a sentence at the beginning of Section 4.

> ***p.13 l.415:*** *To quantify if water is fresh or salty we will refer to a reference salinity of 34.95 - the average salinity of the top 30 m of the basin between 1990 -- 2009.*

p14, L6: The authors equate the mean freshwater transport with the Ekman transport, but the mean flow need not be entirely Ekman - are the authors sure that other contributions to the cross-boundary mean flow are small?
Actually, here we do not conclude that the mean freshwater transport is equal to Ekman transport. We find here that the mean freshwater flux due to eddy fluxes is a magnitude smaller than the mean freshwater flux due to Ekman transport, which is the only comparison we made as other mechanisms are beyond the scope of the investigation.

p14, L9-10: I think this sentence is a reasonable take-home message from the study, in contrast to the abstract, which I suspect rather over-states the strength of the authors’ conclusions (see other comments above on the methodology).
The abstract has been changed to better represent our conclusion.

Fig. 2: How did the authors select this particular pattern of particle deployment? I am struggling to discern the rationale behind the particular pattern shown here.
The red dots in Figure 2 show the particle release locations. The locations were chosen to be a regular grid covering the entire central basin while remaining away from the mean boundary currents.

Fig. 4: I initially thought that the authors had chosen to rename “Greenland” as “Salt”, before realizing their intent. Perhaps they could move this label to the left of the figure?
Label has been removed

Fig. 4: Please provide a scale for the probabilities associated with the sizes of the circles.
A scale has been added

Fig. 6: A legend would improve the clarity of this figure.
A legend is already part of the figure (panel c), but has been made larger for clarity

Fig. 8: The authors use EKE as a proxy for the freshwater transport by eddies in their consideration of seasonal and interannual variability. However, EKE alone does not dictate the eddy transport - a better proxy would be something like the square root of EKE multiplied by the

salinity difference across the LS boundary. How much seasonal/interannual variability is there in this gradient?
That is a great suggestion. For now we decided to use EKE as a proxy for potential eddy activity. While it does not dictate eddy transport, it does show variability which in turn is a good indicator for shedding of eddies. As with the freshwater calculation initially used (see response to comment on p1, 10-12), we anticipate that a true freshwater calculation based on the offline Lagrangian trajectories would be difficult to defend.

Fig. 10: This figure does not distinguish between waters originating from the EGC inshore and EGC offshore regions. Given that it appears to be the EGC inshore waters that are primarily responsible for the freshwater transport, it would be prudent to make this distinction, particularly given the potential impact on the correlation between winds/EKE and particle crossings.
It is true that this Figure only distinguishes between the water originating from the southeast, and northeast and not between water from inshore or offshore EGC. After much debating, we decided to not add the offshore and onshore water to the figure since it makes the figure really busy and hard to understand. However, Table 2 shows the correlations between the EKE and Ekman transport and the inshore and offshore components in the southeast and northeast.

Fig. 10: Why does the Ekman transport estimate only go back as far as 1992?
This has been fixed.

Fig. 10: The authors should highlight the differing axis ranges between the panels, as this might mislead readers - in fact I would argue that the axis ranges should be identical for this reason.
We have highlighted the different axis ranges in the caption. In the end, we opted for distinct ranges as otherwise it would be difficult to see any variability in the left panel (smaller range), if a reader were interested in the southeast region in particular.

Fig. 10: How strong are the computed correlations if annual, rather than three-month, averages are used? Much of the correlation might simply be due to the strong seasonal cycles present in the time series.
The correlations are still strong when considering only the annual average since, as stated in **p.16 l.496:** *"To consider variations beyond the seasonal cycle, the mean seasonal cycle for 1990 – 2009 is removed and the resulting anomalies are shown in Figure 10 […]".*

Fig. 10: Plotting the probability anomaly over time may actually produce misleading results, because this only measures the number of particle crossings relative to the numbers of crossings in other sections of the LS perimeter. That is, a probability anomaly could arise due to more/fewer particles crossing the northeast section, or it could arise due to fewer/more particles crossing elsewhere. I would recommend switching to a measure of the absolute number of particles crossing to remove this ambiguity
We have considered this and analyzed the figure using both the absolute number as well as the probability anomalies of crossings. The results remain the same and we decided to show probabilities rather than absolute numbers, since this is a measure used throughout the entire paper.