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Interactive comment on "Wind-driven transport of fresh shelf water into the Labrador Sea Basin" *by* Lena M. Schulze and Eleanor Frajka-Williams

Anonymous Referee #2

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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

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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.

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 cal-

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culations, 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.

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? 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.

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p1, L16-21: This discussion should be accompanied by supporting citations.

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.

p2, L11: There appears to be a missing citation here (replaced instead with a “?”).

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.

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.

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!

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.

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

p4, L26: In addition to bottom friction, pressure forces also exchange momentum between the ocean and the solid earth.

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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?

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.

p5, L11-12: Is this statement based on model experiments, or is it simply a speculation?

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.

p5, L26: “in many studies” is not a suitable substitute for citations

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

p6, L14-15: This statement should be supported by evidence if the authors plan to retain it in the manuscript.

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

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occurs during backward time-integration.

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.

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".

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?

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.

p8, L24-25: I found the authors' geographical descriptions confusing because "south-

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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.

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".

p11, L18-19: The authors describe the correlation as "significant", but do not define the criterion for statistical significance.

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.

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

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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.

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?

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).

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.

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?

Fig. 4: Please provide a scale for the probabilities associated with the sizes of the

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circles.

Fig. 6: A legend would improve the clarity of this figure.

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?

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.

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

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.

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.

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.

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