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Interactive comment on "On the modulation of the periodicity of the Faroe Bank Channel overflow instabilities" *by* E. Darelius et al.

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Comments from reviewer 2

We thank reviewer 2 for helpful comments and suggestions which we respond to below:

Comment: 1) General scientific comments The article combines observations - a set of one year-long moorings and SLA from AVISO - with results from a regional high-resolution model to describe the variability at the FBC sill and downstream of it. The observations reveal oscillations with variable strength and periodicity, which are consistent with changes in the outflow transport across the FBC sill. An increase in the outflow transport leads to downstream oscillations that are more intense and with a shorter period, and this is confirmed by the results from the numerical model and by

the baroclinic instability theory. This is clearly presented and it is also consistent with a previous study by Guo et al., 2014. The article also explores how changes in the barotropic background flow and upper layer currents can partially help to explain the changes in oscillation period; this is also consistent with previous studies.

However, a clear connection between the different parts of the paper, including the potential role of TRWs, is not provided.

Answer: We have made effort to improve the readability and better connect the different parts of the paper, e.g. by removing paragraphs that led the reader "astray" and by including opening statements in some of the sections telling the reader where the text is going and why. E.g. L 234-235: "The upper layer in the region is highly active and in the following section it will be shown that changes in the upper layer circulation coincide will changes in the outflow and in the oscillations."

Comment: It is not clear to me how important the numerical model results are in this study, since the main conclusions are mostly drawn from the set of moorings, the AVISO data and previous studies.

Answer: We find that the model complements the observations and that, although the background forcing and variability in outflow transport is not satisfactorily represented in the model, it is able to resolve the mechanisms linking transport variability to variability in eddy properties.

Comment: It would be useful to know how well the model represents the real world; I understand that the model was run for a different period than the observations, but some kind of model vs. observation comparison would be useful in identifying why the model fails to capture some of the processes discussed in the paper, including the inflow events discussed in the last paragraph of page 838.

Answer: We have facilitated the comparison with the real world by e.g. using the same scale in figures showing observed and modeled time series and we've included ob-

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served properties where such exist : the observed transport (from the modeled period) is included in Fig. 4f and Fig. 9c shows a comparison of modeled and observed SLA gradients.

Comment: The manuscript is overall well written. Once the different components and the general picture are clearly presented, it will provide valuable information to the community and, therefore, it will be suitable for publication.

Answer: Thank you!

2) Editorial comments

Comment: page 824, line 18: “for decreased plume thickness” I think it should be “for increased plume thickness”.

Answer: The phrase has been corrected and now reads: This is in agreement with results from linear instability analysis which suggests that the period increases while the growth rate decreases for increased plume thickness.

Comment: page 824, lines 22-24: This is an important statement. However, it is not clear in the text (see comment below about page 835, lines 7-8) how this was estimated. Answer: This is now clarified, see comment below and L278-280 in the text..

Comment: page 825, lines 3,9: “outflow” and “overflow”. What’s the difference between overflow and outflow? If there is no difference between them, I suggest using just one term throughout the paper for consistency.

Answer:While outflow can occur at any depth, an overflow is confined to the bottom. The word overflow is thus more precise and it is now used throughout the paper.

Comment: page 826, lines 6-8: This sentence is not clear, please rephrase it. Also, please clearly state that the goal of this study is to investigate “the modulation of the oscillations and . . .” (lines 9-11), and I suggest that you include a quick description of your findings at the end of this paragraph.

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Answer:We have clarified the sentence and the last paragraph (L50-57) now reads: Based on the first one year-long mooring record covering the vertical and horizontal extent of the plume (Ullgren et al., 2015), the variability in eddy kinetic energy and in the dominant periodicity associated with the eddies are described. The goal of the study is to investigate the observed modulation of the oscillations and its coupling to oceanic and atmospheric forcing. The mooring observations are combined with satellite observations of sea surface height anomalies and the analysis is extended to include results from a high-resolution regional model (Rasmussen et al., 2014). It is found that the observed variability in periodicity is directly linked to changes in volume transport across the sill, which in turn is strongly affected by the background flow."

Comment: page 827, lines 9-11: Any specific reason for choosing a Morlet mother function? Are the results sensitive to this choice? Please state that in the text.

Answer: Yes, the Morlet mother functions give energy peaks that are narrower in spectral-space (albeit wider in time-space) and thus gave more information regarding the changes in period (that are observable by eye, and not "made up" by the wavelet). The calculations giving figure 6 could not have been carried out using the other mother functions. We've included a motivation for chosing Morlet in the text (L80-81) which now reads: "Also Paul and DOG mother function were tried, but since the energy peaks appear narrower in time-space and broader in spectral-space (Torrence and Compo, 1998) they were found less informative."

Comment: page 827, lines 21-23: This sentence is not clear, please improve it. Some explanation on the effect (if any) of the artificially elevated EKE values on the results would be useful.

Answer:The sentence has been improved and the effects of the elevated EKE are briefly discussed in the text (L91-98): "The EKE values are artificially elevated at the plume interface, since an instrument at a given level shifts from being within (typically recording high velocities) and above (recording low velocities) the plume as the plume

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thickness changes throughout the oscillation period. This will affect the absolute value of the calculated EKE and the presented values are therefore normalized (with the maximum value obtained at each mooring site). It is assumed that the effect on the temporal effect on the EKE is minor, as suggested by a comparison of time series calculated from S3 when including / excluding levels affected by the plume (not shown)."

Comment: page 828, lines 3-11: Some comments on the sensitivity of the results to the choice of T = 6 C would be good. For example, how would the results change (roughly) if T = 5 C?

Answer: In agreement with Ullgren et al, the 3C isotherm was chosen as the upper limit for the outflow at the C-array. Note that it is the kinematic transport from the sill (FB), which is insensitive to temperature thresholds, that is used in the comparison with periodicity and SSH. The text now reads: "If T=4C or T=6C is chosen the mean transport is increased with 10% and 27% respectively". (L109-110)

Comment: page 829, line 2: Please define DMI.

Answer: The Danish Meteorological Institute is now defined (L128)

Comment: page 829, line 19: Please state which Coast Guard.

Answer: The Faroese coast guard. (L144)

Comment: page 833, lines 4-5: "Fig. 3c" => "Fig. 5c"; "Fig. 3b" => "Fig. 5b".

Answer:The references have been corrected – thank you for pointing them out.

Comment: page 833, line 17: Please reference Figs. 7b and c at the end of the sentence.

Answer: A reference to the figures has been included.

Comment: page 833, line 22: "Faroe Shetland Channel" => "Faroe Shetland Channel (FSC)", since FSC is used throughout the text (e.g., page 834, line 27). Also, please

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include the location of the FSC in Fig. 1.

Answer: The text has been changed as suggested (L 316) and FSC is now indicated in Fig. 1

Comment: page 834, lines 15-20: Please reformulate this sentence. After reading it for a few times I could understand what you mean, but perhaps breaking it into two sentences would help the reader.

Answer:The sentence has been reformulated and shortened: “Inspection of Fig. 3, 5 and 8 suggests that low amplitude, long period or irregular oscillations occur when the mean upper layer current is sill-ward while energetic, relatively regular and shorter period oscillations occur when the mean upper layer current is transverse to the dense outflow.” (L257-259)

Comment: page 835, lines 7-8: Please explain how a correlation of 0.71 indicates that about 50% of the variance can be explained by the local barotropic force. It is not clear how this is done, so please include some explanation to help the reader. Answer: The squared correlation is a measure of how much of the variance in one parameter is explained by variability in the other parameter. R=0.71 gives rˆ2=0.5 and so about 50% of the variance is explained. The text has been changed to clarify this: ““The maximum correlation found is r=0.71, giving a squared correlation of rˆ2=0.50 which indicates that about 50% of the variance can be explained. . .” (L278-280).

Comment: page 835, lines 19-22: Where is the time series showing the thickness of the outflow across the sill? After a quick check, I could not find that in Hansen and Osterhus, 2007. A wavelet analysis of this time series could be extremely helpful.

Answer: We are uncertain about what is meant here since the lines referred to by the reviewer do not mention a time series of thickness of the outflow. Figure 22 in Hansen and Østerhus shows a time series of monthly mean interface height between 1995 and 2005. We have produced a time-series of interface height at the sill during the

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2012 deployment and looked at wavelet analysis of the time series – it did not provide additional information. The time series of (kinetic) thickness from FB for the period of investigation is now included in Fig. 5c and mentioned in the text:" The variability in (modeled and observed) transport is to some extent reflected in the plume thickness both at the sill and downslope in the plume region (Fig. 5c and Fig. 4e); low transport values are typically associated with a thinner than normal plume, notably during T1 and T4." L(225-227)

Comment: page 836, lines 26-29: In the model, the oscillations (downstream of the sill) follow the outflow transport and the velocity of the upper layer (at the sill). However, there is not a connection between the outflow transport and the plume thickness at the sill. Is that right? if so, it needs to be presented in the a clearer way (perhaps some additional plots would help).

Answer: The plume thickness at the sill was not shown in the earlier version of the manuscript – it is now included in Fig. 4e and shows a seasonal signal similar to that observed in transport. We now state that clearly: "The variability in (modeled and observed) transport is to some extent reflected in the plume thickness both at the sill and downslope in the plume region (Fig.5 c and Fig. 4e); low transport values are typically associated with a thinner than normal plume. . ." (L225-227)

Comment: page 838, first paragraph: Are these evidences that TRWs also play a role in the observed variability? If so, please make this clear. How about the numerical mode, does it also show the presence of these waves? I don't understand what is meant by "to changes in the intrinsic period", please clarify. Answer: Yes, these are evidences that the baroclinic instabilities are manifested as TRWs and the same relationship (between beta and the period of oscillation) is found in the model. The text has been changed to clarify this and to include the model results: "Consistent with theory, the observational data show a tendency for low values of beta _v during time periods with longer oscillations, e.g. in November and May. The same relationship is found in the model data (not shown). This suggests that the baroclinic instabilities are indeed

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manifested as TRWs and that a part of the variability in the observed oscillation period is caused by changes in the properties (wave length, intrinsic period) of the generated eddies. Advective effects caused by a variable background current may contribute to the observed variability in periodicity. The two processes are not independent, since the background current is shown to be directly linked to the overflow transport 365 and thus indirectly to the oscillation period." L(358-365)

Comment: page 839, lines 1-4: Why does the model fail to capture inflow events? Please comment more on that.

Answer: The missing inflow events are caused by the nesting approach. This is now commented on in the discussion: "The lack of variability in the model is likely related to the nesting approach. Large scale features influencing the overflow that are missing in the coarser large-scale model providing the boundary conditions will be missing also in the regional, high resolution run. A comparison of SSH-variability obtained from satellite altimetry and model output shows that the variability in SSH gradients is relatively well represented in the model, although extreme values are typically 20-30% lower. The link between SSH-gradients and overflow transport that is prominent in the observations, is missing in the model." (L392-398)

Comment: page 840, lines 11-15: Have you explored the possibility of having TRWs generated by the outflow passing through the sill? Marques et al., (2014) GRL have shown that such waves can be generated by the outflow of Antarctic dense water through troughs. Depending on the ambient characteristics (i.e., ambient stratification, shelf slope, alongslope background flow), the energy of these waves may propagate in the opposite direction (or even be trapped) as the phase velocity/outflow. This could explain why the signal is not observed downstream. It is possible that neither the Nordic WOCE dataset nor the numerical model (HYCOM) captured these waves.

Answer: We are not sure we understand the reviewers comment fully – we do state in the ms that the baroclinic instabilities are manifested as TRWs in the upper layer and

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provide some evidence (the change of beta_v with period) that they are so both in the model and in the observations. These would be TRWs generated by the outflow, indeed similar to the results by Marques et al. We now cite their article in the paper. L343-345: "Marques et al. (2014) have shown that TRWs can be generated by dense overflows and their model study showed that the TRW properties varied in time depending on e.g. the background current and the density of the overflow."

Comment: page 841, lines 2-4: Please reformulate this sentence. If possible, I also recommend including a final comment on the broader implications of your findings. For example, how can future observational programs or climate models benefit from these findings?

Answer: The sentence has been changed to: "The energetic, short period oscillations are more efficient than the long period oscillations in spreading the dense water on the slope, both to shallower and greater depth." (L431-432) As suggested we have included a final comment on the findings "While our understanding of the influence of eddy generation on plume mixing and entrainment is far from complete, the findings provide a link between outflow transport and eddy properties that can be invoked in a parametrization of outflow mixing. As a first step, future observational programs ought to be designed in order to test if the relationship is transferable to other outflow regions." (433-436)

Comment: page 848, Fig.1: This figure needs to be improved. I think you should combine the upper and lower insets into one (i.e., show the study region and model domain in the same plot). It is hard to see which area is covered by the numerical model; maybe by showing a few isobaths in the inset this would be clearer. On the main plot: a schematic showing the main path of the outflow would help the reader.

Answer: Figure 1 has been modified as suggested; the insets are combined, the 500 m isobaths is shown and arrows show the path of the FBC outflow.

Comment: page 849, Figure 2: If possible, please make these panels slightly larger

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and include the location of at least a few mooring that are shown in Fig. 1 in one of the panels.

Answer: The panels are enlarged and the locations of mooring C1-3 and M are included.

Comment: page 852, Figure 5: In the top panel the x and y values are not shown, please fix that. Also, please make the panels slightly larger if possible;

Answer: The size of the panels have been increased and the missing y-values are now shown. X-values shown in panel (e) are valid for all panels.

Comment: line 4: “and at the sill from FB at the sill”, this is confusing, please rephrase it;

Answer: The phrase has been corrected and now reads: Transport estimates based on data from array C (blue) and FB (red).

Comment: panel c: is that averaged velocity? If so, please specify in the text.

Answer: The data are low pass filtered – this is now stated in the text which reads: “Observed outflow velocity (low pass filtered, 30 days) at FB in June 2012 - May 2013, ”

Comment: page 854, Figure 7: If possible, please include the location of at least a few mooring that are shown in Fig. 1 in one of the panels. Is panel d important? I do not recall any reference to it on the text. If this panel is not important, I recommend removing it from Fig. 7. Otherwise, please change some of the colors (the red-ish and black-ish) since it is hard to differentiate them.

Answer: The mooring locations are now indicated with red circles and panel (d) had been removed.

Comment: page 855, Figure 8: Again, please use a different color for either T1 or T5.

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Answer: T5 is now shown in black and the mean current in (b) in grey.

Comment: page 857, Figure 10: It is hard to differentiate the blue and green plots in a and b. Please change the colors or make the lines thicker. What are the different curves in c? please explain that in the caption.

Answer: The colors have been changed and the lines are now thicker. The black and red curves in (c) show the period observed at a stationary mooring (for a given intrinsic period) as a function of background velocity. The legend now read "Period observed at a stationary mooring as a function of background velocity for an oscillation with an intrinsic period and wavelength of 3.33 (4.5) days and 75 (80) km [red (black) line]. The corresponding period-wavelength pairs are marked with circles in the same color in (a-b)."

Comment: page 861, Figure 14: "EKE" => "EKE in the 2-6 days band".

Answer: The text has been changed as suggested and now reads: "Time series of EKE in the 2-6 days band from wind observations in Torshavn during 2012-13".

Comment: Typos page 824, line 6: "is" => "are" page 824/826, line 13/10: "high resolution" => "high-resolution" page 825, line 3: "_" => "=" page 826/827, line 24/17: please add a comma before "respectively" page 827, line 12: please remove the comma after "highest" page 829, line 11: "z level" => "z-level" page 830, line 4: "is" => "are" (I think) page 830, line 19: "heatflux" => "heat flux" page 836, lines 15,19: Please remove the commas before "highly" and "that" C408 page 836, line 2: "linke" => "linked" page 842, line 24: "deep water" => "Deep Water" page 848, line 2: "black, labeled" => "black-labeled"

Answer: The typos above have been corrected. Thank you for pointing them out.

Comment: page 843, line 24: "Faeroe" => "Faroe"

Answer: I assume the reviewer refers to p. 843 L9 and L13 since there is no Faeroe in line 24. These lines have not been changed, since this is the spelling used by

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