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Interactive
Comment

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 1

We thank reviewer 1 for helpful comments and suggestions which we respond to below:

Comment: The manuscript presents results from one year long time series of mooring arrays located downstream of the Faroe Bank overflow. The measurements confirm previous observations of oscillations with periods between 3 and 6 days. Reasons for the change in period are explained for the data and a simulation with a high resolution regional model. Apart from confirming previous results, the main findings of the manuscript is the explanation of the changes in period as a result of changes in plume thickness and changes in background flow.

The manuscript is well written, but not easy to read and requires a high level of attention from the reader.

Answer: We have attempted to clarify and make the text easier to read, by for example removing the section about EKE on the FBC sill (p839, L5-26 and Fig. 13 in the old ms) and by including "opening sentences" in the beginning of sections to let the reader know where we are going; 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 with changes in the overflow and in the oscillations.".

Comment: Since several factors influencing the period are presented it remains unclear how important these individual factors are and whether they are connected or independently acting.

Answer: We have clarified the role of the different factors. L 360-365: "This suggests that the baroclinic instabilities are indeed 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 and thus indirectly to the oscillation period."

Comment: Particularly the discussion of the model results and the interpretation of the observations follow two different streams, which should be better linked.

Answer: The model results and the observations are now better linked.

Comment: I have a little bit a problem with the term "local barotropic forcing" as I understand the ocean as being forced by the atmosphere. My understanding is that this term is used in the observational community. Although it becomes clear over the course of the manuscript, I am afraid that many readers will not instantly understand what is meant. It would be good to first introduce the situation that leads to a local

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barotropic forcing and maybe add "by the background current" when it is first used.

Answer: We have reduced the use of the term “local barotropic forcing” and rather use “local gradients in SSH”. To clarify further, we now write: “The results presented in Sec. 3.4 suggest that the overflow transport variability on intra-seasonal time-scales is partly determined by the background flow, which has a large barotropic component. We therefore seek a relationship between the overflow transport and local SSH-gradients forcing a barotropic, background current“in the opening phrase of section 3.5 (L268-271).

Comment: Although the simulation with a numerical model illustrates nicely the complete circulation for which only sparse measurements exists and confirms some basic relations, it also shows substantial differences to the data. In particular, the modulation of the period, which is the main topic of the paper, hardly exists in the model or is at least substantially different. Since the simulated changes are mainly associated with the seasonal cycle, the correspondence of two curves with a similar seasonal cycle provides very little additional confidence. I suspect that the simulation either suffers from a missing spin-up or limitations due to the nesting approach - or both. For instance, a lack of resolution in the model that provides the boundary conditions together with the small domain of the model may not permit the development of meso-scale variability. Therefore, the fields seem to remain smooth and show not indication of any meso-scale variability or current meanderings. I recommend to extend the simulation, although as this might not be sufficient. This main caveat, the limitations of the model should be acknowledged and discussed. A comparison of the sea level variability between model and the AVISO product could illustrate the lack of variability in the model, and a direct comparison to the measurement from the array could demonstrate how well the model simulates the flow downstream of the sill.

Answer: To facilitate the comparison of observations and model we now use the same scale in Fig. 3-5 and the observed transport (from the modeled time) have been included in Fig. 4f. We now discuss the limitations of the model and its failure to capture

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the variability of the outflow in the text (L381-398) and we have included a direct comparison of modelled and observed SSH variability (Fig. 9c) as suggested. "The inflow in the upper layer on the Faroe Bank side is generally weaker in the numerical model, and strong inflow (weak overflow) events apparent in the sill moorings (e.g. in November-December 2008, not shown) are absent or only weakly represented in the model. The large fluctuations in transport observed at the sill (>1 Sv change over the course of a month) is thus not captured in the numerical model where the transport varies smoothly over the year. As a consequence, the oscillation period - shown in the observations to be strongly linked to the overflow transport - varies relatively smoothly in the model compared with the observations (compare Fig. 4d with Fig. 3d). Although less variable, the relation between transport and oscillation period is the same in the model and the observations (Fig. 6), suggesting that the mechanism linking them together is well represented in the model although the large scale background forcing determining the overflow transport is not. The variability in overflow transport and the transport itself is much too low in the model. 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."

Comment: Model results are shown from within the plume at 100 m above bottom while results from the observations come from 600 mab which should be well above the plume. The difference is not explained and it is unclear why 600 mab was chosen, later in the text an explanation exists why 600 mab may be suitable but it is not spelled out.

Answer: The presented time series from mooring S1 are from 80 mab and "within the

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plume” while the wavelet analysis (Fig3d, 5a and 11) were made using values from above the plume. The reason for choosing the upper layer velocities for the wavelet is to (a) avoid frictional effects within the interfacial/bottom boundary layer and (b) avoid spurious effects caused by the instrument moving in and out of the plume as the plume thickness changes. This is now stated in section 2 (L84-86) and model data from 600 mab are used for wavelet calculations also in the model (fig. 4d).

Details Comment: Abstract l 2 North Atlantic l

Answer:Thank you for noting the typo, the text has been corrected.

Comment: L 16 Although the relation is probably technically correct it is hard to understand. Maybe that can be described differently, e.g. that the increase of from xx to yy Sv changes the period from ss to tt days.

Answer: Thank you for your suggestion, we’ve changed the text as suggested. L12-15: “The observed variability in oscillation period is directly linked to changes in the volume transport across the sill: the oscillation period increases from approximately three days to about six days when the transport decreases from 2.4 to 1.9 Sv “.

Comment: l 18 The period increases for decreased plume thickness!

Answer: Thank you for pointing this typo out! The text has been corrected (L15-17.)

Comment: l 19-24. It is not clear how these details fit together and what message one should take home. There are many different explanations offered at once, some seem to contradict others (l 21-22). Maybe due to abstract length limitation too many details were put into too few sentences. In particular what does "but the changes .. intrinsic period of the instability is modulated" mean here.

Answer: The abstract (and the rest of the text) has been clarified and some of the details have been removed as suggested. L15-18: “This is in agreement with results from a linear baroclinic instability analysis which suggests that the period increases while the growth rate decreases for decreased plume thickness. Advective effects,

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caused by the variable background current, further modulate the observed periodicity on up to 1 day time scale."

Comment: P 826 l 8 what is modulated?, it seems the modulation

Answer:The section has been changed following comments from reviewer 2 and the phrase has been altered. L 50-54: "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."

Comment: l 23 rotation from the northern direction?

Answer: Yes. We've clarified as suggested: "The angle of rotation is 34 and 31 (clockwise, from north) for the C- and S-array, respectively" (L67-68).

Comment:P 827 l 16 "spectral density" of the velocities!

Answer: Yes. We've corrected the text: "...is the spectral density of the velocities in the along / across slope direction, respectively" (L89-90).

Comment: l 19-21 Why are only values above the plume used for S3?

Answer:All moorings are now treated in the same way and the phrase has been removed.

Comment: l 21-22 How are those instruments treated, are they taken out or is it not possible to detect which are affected?

Answer: It is difficult to determine what levels are affected and no data have been removed. Assuming that the artifact influence the absolute value and not its evolution in time, we present normalized time series in Fig. 3e. Time series of EKE from S3 calculated with and without the plume levels included vary in detail but not in large-scale features. This is now stated clearly in the text: "This will affect the absolute

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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 evolution of 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)." (L94-98).

Comment: P 828 l 5-7 How well does the array cover the whole outflow? Is it possible to add a statement?

Answer: The array covers the vertical and horizontal extent of the plume relatively well since the flow is confined to the channel and the isotherms below the upper instrument on the mooring most of the time. We've information about the coverage in the text (L110-112): "The mooring array covers the extent of the plume relatively well, since the overflow is confined to the channel in the horizontal and the 3âŮęC isotherm is within the vertical extent of the moorings 100 / 99.8 / 73% of the time at C1 / C2 and C3."

Comment: P 829 l 20-21 Not clear where SSH and barotropic velocities come from. It seems to be a different source than the DMI model.

Answer: SSH and barotropic velocities are from the hindcast – this is now more clearly stated in the text: "Initial fields and boundary conditions (barotropic and baroclinic velocities, salinity, temperature and sea surface height) are provided by a hindcast archive from the Danish Meterological Institute covering the Arctic and the North Atlantic Ocean with a resolution of 10 km (Ribergaard, 2012)" (L45-147)

Comment: l21-24 Please provide a reference or source for the DMI hindcast or further details on this simulation. In what year and with what initialconditions was the model started?

Answer: The regional simulation of the Faroese islands has spun up for 1 year (1999) and then a 10 year hindcast was simulated. The DMI archive used for boundary conditions were started by repeating the year 1999 5 times. After this the hindcast span from

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2000 to 2009, thus the simulation has been running for approximately 14 years which should be sufficient. A reference for the hind cast archive and information about the initialization, spin up and forcing period is now given in the text, which reads: Initial fields and boundary conditions (barotropic and baroclinic velocities, salinity, temperature and sea surface height) are provided by a hindcast archive from the Danish Meterological Institute covering the Arctic and the North Atlantic Ocean with a resolution of 10 km (Ribergaard, 2012). The hindcast was spun up by repeating the 1999 forcing five times. The regional model was then run for the period 1999-2009." (L145-149)

Comment: P 830 Since the region is extensively observed, it would be nice to show some comparison between the model and the observations for instance as a section along the array. Although the model has been used and validated by Rasmussen et al (2014) it is not clear how well the model represents in particular the overflow and the meso-scale variability that is observed with altimeter data. From the discussion I trust that it does a decent job in simulating the overflow, but there seems to be lack of meso-scale variability in the model. A direct comparison with the observations would provide more confidence how well the model simulate the processes that affect the changes in the oscillation period.

Answer: We have included a comparison of observed and modeled SSH-gradients (Fig. 7c) and we've also included the observed transport from the modelled period (Fig. 4f). The model does a poor job in representing the variability in outflow transport (and the modelled outflow transport is also too low). We believe this is due to the nesting approach and the lack of large scale forcing on the outflow. While the variability in outflow transport is too small, the response in the periodicity to changes in transport variability agrees nicely with the observations, suggesting that the processes linking transport variability to eddy periodicity is correctly represented in the model. The issue is now further discussed in the discussion (L381-396). "The inflow in the upper layer on the Faroe Bank side is generally weaker in the numerical model, and strong inflow (weak overflow) events apparent in the sill moorings (e.g. in November-December

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2008, not shown) are absent or only weakly represented in the model. The large fluctuations in transport observed at the sill (>1 Sv change over the course of a month) is thus not captured in the numerical model where the transport varies smoothly over the year. As a consequence, 385 the oscillation period - shown in the observations to be strongly linked to the overflow transport - varies relatively smoothly in the model compared with the observations (compare Fig. 4d with Fig. 3d). Although less variable, the relation between transport and oscillation period is the same in the model and the observations (Fig. 6), suggesting that the mechanism linking them together is well represented in the model although the large scale background forcing determining the overflow transport is not. The variability in overflow transport and the transport itself is much too low in the model. 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."

Comment: P 831 l 9ff Isn't 600mab well above the plume and why is the 100mab of the model compared with the 600mab of the observations? Later it is said that Reszka et al.(2002) suggest that the baroclinic instability will lead to Rossby waves in the upper layer. If this is the reason for choosing 600mb it should be explained first, and it still remains unclear why the model results are based on 100mab values.

Answer: Thank you for pointing this discrepancy out. The presented time series from mooring S1 are from 80 mab and "within the plume" while the wavelet analysis (Fig3d, 5a and 11) were made using values from above the plume. The reason for choosing the upper layer velocities for the wavelet is that the oscillations are clearer there, since there is more variability or noise within the plume and since an instrument at a given level here is likely to move in and out of the plume as the plume thickness change. To be comparable, the wavelet in Figure 4d (showing modelled time series) is now

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based on data from 600 mab, as in the case of the observations (Fig 3d, 5a and 11) and we've added the following text to the paragraph describing the wavelet analysis in section 2, L83-86. "The oscillations are seen throughout the water column (Darelius et al., 2013) and the wavelet analysis is carried out using data from levels well above the plume to avoid frictional effects within the interfacial/bottom boundary layer and spurious effects caused by the instrument moving in and out of the plume as the plume thickness changes".

Comment: l 15-20: For how long was the model run - only this year mid 2008 -2009? Shouldn't there be a spin-up to reach the equilibrium EKE of the model. For instance, mesoscale variability in the upper layers may interact with the lower layers and explain the modulation, while the model fields are smooth and the period may remain constant because of this.

Answer: The model was run for the period 1999-2009. This is now stated in the manuscript L(148-149) "The hindcast was spun up by repeating the 1999 forcing five times. The regional model was then run for the period 1999-2009." Indeed mesoscale variability in the upper layer influence the periodicity indirectly - since it influences the outflow transport – and directly through advective effects. The lack of variability in the model is now discussed (L 392-400) and we've included a figure comparing observed and modelled SSH-gradients (Fig. 9c). We also state – as suggested by the reviewer – that the lack of variability in the model is the reason for the relatively constant oscillation period (L385-387).

Comment: P 833 l 3 "height" above the bottom?

Answer:Yes, the text has been changed accordingly to "here defined as the height above bottom of the 6C isotherm".

Comment: P 834 l 5-6: I don't understand the term "barotropically forced current". I can understand that the plume is forced by the background flow that has a large barotropic component. Eddies and meanders in the North Atlantic Current have a typical extent

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of a 500 m or more. The baroclinic mode one will not show that much variation over the top 500m either. If these eddies and meanders encounter regions with less than 1000m water depth, they appear more barotropic, but this does not means that they are barotropically forced and there is no indication on the forcing mechanisms presented in the manuscript. I suggest to describe this as a current with a large barotropic component at this location.

Answer: As suggested, we now write that the observed current has a large barotropic component: “Above the plume, the velocity profiles show little variability in the vertical, suggesting (together with the relatively large sea surface height (SSH) gradient derived from AVISO) that the upper layer current has a large barotropic component.” (L254-256)

Comment:l 23-24: I would describe this as an interaction between the upper layer variability due to eddies and meanders of the NA current rather than a barotropic forcing. When the eddies and meanders encounter the plume there is a barotropic component which will interact with plume, but one cannot argue that the eddies and meanders are barotropically forced without further information.

Answer: We agree with the reviewer, and we have changed the text accordingly: “The results presented in Sec. 3.4 suggest that the outflow transport variability on intra-seasonal time-scales to some extent is determined by the background flow, which has a large barotropic component.” (L268-269)

Comment: P836 l 1-4 Maybe you could say that the set up of Reszka et al is a baroclinicly unstable situation. Although the sentence before suggests this, it provides only weak evidence and the reader may still wonder if other mechanisms are more relevant and discussed by Reszka et al.

Answer: This is a good point, thank you. The word “baroclinic” has been inserted in the beginning of the phrase to clarify and the text now reads: “Baroclinic stability analysis of a parabolic shaped plume on a sloping bottom. . .” (L302)

Comment: P835 l 4 Not clear how the results in Fig.10 were obtained. Did you run numerical simulations similar to Reszka et al or solved the eigenvalue problem, details about this should be provided. The results depend on other parameters such as N2. Nothing is said here what has been assumed. If you consulted their Table 1 still further information is needed.

Answer: We used the code developed by Reszka et al (2002) and parameters relevant for the FBC region, as listed in Guo et al, 2014. This is now more clearly stated and the parameter values are explicitly given in the text. L302-306: "Baroclinic stability analysis of a parabolic shaped plume on a sloping bottom using the code developed by Reszka et al. (2002) and parameters (depth =900 m, density anomaly 0.41 kg m3, buoyancy frequency N2 = $8.12\times10-7$ s$-$2 and slope s=0.01) relevant for the FBC overflow (Guo et al., 2014) yields that the period and wavelength of the most unstable wave decrease and the growth rate increase with increasing plume thickness (Fig. 10a).

Comment: l 9-10 One should keep in mind that in the model the link is a common seasonal cycle which alone does not provide much indication for a link since a dependence on the seasonal cycle exist almost every in the global ocean.

Answer: While this is true, the relationship between oscillation period and transport in the model is the same as in the observations suggesting that the processes responsible for the link are represented in the model.

Comment: l 21: I would call this interaction with the upper layer or background circulation.

Answer: The text has been changed as suggested and now reads: "One possible reason is that while the transport variability in the observations and in the realistic model to a large extent is linked to changes in the background circulation,. . ." (L320-321)

Comment: l 23 Figure 7 shows the velocity at 200m and not the barotropic velocity.

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Answer: Correct,thank you for pointing this out. The upper layer is relatively homogenous and the shear is most likely of barotropic origin. We have changed the text to clarify this: "In addition, the observations and the realistic model show a velocity shear across the channel in the sill region within the upper layer (Fig.7) that is absent in the idealized model. This shear is likely to be of barotropic origin since the upper layer is relatively homogenous across the channel." L(322-325)

Comment: P 839 l 5-6 What could be the reason for this increase in BC?

Answer: High values of BC mean that this is a region where potential energy is transferred to kinetic energy, i.e. where isopycnals are flattened and baroclinic eddies grow. Why this occurs here (and not further upstream/downstream) is likely due to a combination of topography and outflow properties. We have included a short description of the baroclinic conversion rate where it first appears in the text. L169-170: ". . .the associated baroclinic conversion (BC) rate, which measures the rate of energy conversion from mean potential energy to eddy potential energy."

Comment: Figure 5: axis values are missing in (a). Say that (a) is the same as 3d that the reader does not have to wonder. I would also keep the same (better) colorscale.

Answer: We now state in the legend that (a) is the same as 3d and the color scale are the same. Values on the y-axis are now included.

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