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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.

Anonymous Referee #1

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

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Interactive Discussion
Discussion Paper

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

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.

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 exist why 600 mab may be suitable but it is not spelled out.

Details

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Abstract l 2 North Atlantic 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. l 18 The period increases for decreased plume thickness! 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.

P 826 l 8 what is modulated?, it seems the modulation l 23 rotation from the northern direction? P 827 l 16 "spectral density" of the velocities! l 19-21 Why are only values above the plume used for S3? l 21-22 How are those instruments treated, are they taken out or is i not possible to detect which are affected? P 828 l 5-7 How well does the array cover the whole outflow? Is it possible to add a statement?

P 829 l 20-21 Not clear where SSH and barotropic velocities come form. It seems to be a different source than the DMI model. l21-24 Please provide a reference or source for the DMI hindcast or further details on this simulation. In what year and with what initial conditions was the model started? 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 descent 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.

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

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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.

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, meso-scale 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.

P 833 l 3 "height" above the bottom?

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 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.

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 then a barotropic forcing. When the eddies and meanders encounter the plume there is a barotropic component which will interact with plum, but one can not argue that the eddies and meanders are barotropically forced without further information.

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.

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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.

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.

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

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

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

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.

Interactive comment on Ocean Sci. Discuss., 12, 823, 2015.