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Interactive comment on "A stable Faroe Bank Channel overflow 1995–2015" *by* Bogi Hansen et al.

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General response The comments by the three referees have been very constructive and positive. We have tried to address them all as detailed in our responses to individual comments below and as carried out in the changes made in the revised manuscript. In our responses, we refer to page and line numbers in the revised manuscript, where all but the smallest text corrections (except comma deletions or similar) are red.

In addition to the changes suggested by the referees, we have made some minor changes to a few phrases for better readability. In addition, we have added a new paragraph to the beginning of Sect. 4.4, which relates our results to the global energy budget (p. 15, l. 25-32 in revised manuscript). This is an obvious and important implication of our results that we should have mentioned in the original manuscript.

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Discussion paper

We have also added brief references to this to the abstract (p. 1, l. 16, 20, 26 in revised manuscript) and a reference (p. 19, l. 22-24 in revised manuscript).

Anonymous Referee #1

Comment 1.1: First, in Section 3.3/Figures 6 and 7, the salinity is discussed as a function of potential temperature. I understand that the observations are mainly for temperature, but a far more obvious way to look at this would be to plot potential density (which mainly depends on salinity in this region of the T/S space) against potential temperature. In general, throughout the manuscript with a very few exceptions, the authors attempt to relate temperature changes to density changes, which just does not make much sense given the nonlinearities in the equation of state. When isotherms and isopycnals are equated (as on p. 13) without any proper justification, the wrong conclusion could be drawn.

Response: The referee is correct that for the cold and deep part of the overflow (but not the upper part), density is determined more by salinity than temperature and some of our text seemed to neglect that. In the revised manuscript, this should now be removed. It is not clear to us what the referee suggests by the words: "to plot potential density (which mainly depends on salinity in this region of the T/S space) against potential temperature" since salinity is changing with time. In any case, it appears that our reason for presenting Figures 6 and 7 has not been clear in the original manuscript. We have rewritten the text introducing these figures (p. 8, l. 30 – p. 9, l. 5 in revised manuscript). Hopefully, this addresses the concern raised by the referee.

Comment 1.2: Second, the factor of 5-10 on p. 9 confused me. With a 5-10% contribution to the mixture I would expect that changes in the temperature/salinity would be reduced by a factor of 10-20. How is the factor of 5-10 derived?

Response: The Referee is absolutely correct. We have corrected this embarrassing error. (p. 10, l. 5 and p. 13, l. 13 in revised manuscript).

Comment 1.3: Textual: - There are quite a few commas in the text where there should be none. - p. 4, l. 1: profiles - p. 4, l. 8: an evaluation cannot draw a conclusion, but it can lead to a conclusion - p. 10, l. 8: close to the bottom

Response: We have corrected these

Anonymous Referee #2

Comment 2.1: Regarding the structure of the paper: a large part (almost half) of the discussion is dedicated to overflow modification. This is an important topic, and this discussion section (4.3) gives a good overview of literature on the topic. However, the proportion of the discussion section that is dedicated to overflow modification is surprising considering that this topic is not even alluded to in for example the paper abstract or title. My suggestions would be to a) update the abstract to mention the discussion on overflow modification b) consider shortening this section of the discussion.

Response: Following the recommendation of the referee, we have shortened the discussion on overflow modification by removing reference to the new CTD observations (see response to comment 2.2). In addition, we added a sentence on overflow modification to the abstract (p. 1, l. 19-21 in revised manuscript).

Comment 2.2: As mentioned in section 4.3, water mass transformation occurs mainly downstream of the FBC, that is, downstream of the long-term measurements that are the main focus of this manuscript. Some new observations from the downstream region are presented in section 4.3, namely, eight CTD stations occupied 20-21 May 2016. In view of the known high level of short-term variability (oscillations) in this part of the overflow, briefly mentioned in the manuscript on p. 12, L26-29, how does one confidently interpret 8 profiles taken during 2 days? Are the observations in this snapshot representative? Do these measurements add significantly to the substantial body of work done in this region over decades, which includes repeated CTD sections, moorings, etc.? Personally, I am not convinced that the new downstream CTD profiles add enough new information to motivate their inclusion in the manuscript. I recommend focusing on the truly impressive data sets: the time series from the moorings at the sill, and from the repeated standard CTD sections.

Response: We have followed this advice and deleted Sect. 3.4 and Fig. 9 and also almost all reference to these observations in Sect. 4.3; only retaining a brief reference to “unpublished data” in lines 22-25 on page 14 of the revised manuscript.

Comment 2.3: Speaking about impressive long-term measurements: Is this the first time the whole 20 year time series is presented except in a technical report (Hansen et al., 2015a)? In that case it is a substantial extension of the data set (a doubling of the 10-year time series from e.g. the important HØ2007 paper), and perhaps that should be stated outright in order to make the contribution of this paper clear. If not, other recent manuscript that use all or most of the 2-decade time series ought to be referenced and pointed out.

Response: This is now done in the revised manuscript (p. 3, l. 9).

Comment 2.4: The early years of the FBC overflow time series gave quite a different impression, namely a reduction in the strength of the overflow (Hansen et al., 2004; cited in this manuscript, but only to describe a simple water mass mixing scheme). Even though the earlier conclusion of decreasing overflow [since 1950] (Hansen et al., 2001; not cited in this manuscript) has already been refuted in e.g. Olsen et al., 2008, these earlier papers and conclusions (as well as papers refuting them) - by these authors and others - are part of the history of the FBC overflow time series, and form an important backdrop to the discussion about the overflow stability. This should be included in the introduction and discussion sections.

Response: We have added a paragraph on this to the introduction (p. 3, l. 5-8 in the revised manuscript) and added the reference (p. 18, l. 18-19 in revised manuscript).

Comment 2.5: Minor comments P2, L 24: study the long-term variations

Response: The word “long-term” was inserted (p. 2, l. 26 in the revised manuscript).

Anonymous Referee #3 Comment 3.1: A few points (including the discussions of mixing and the brief presentation of new cruise results in Fig.9) are somewhat tangential to the main thrust of the paper, but all of these are interesting and relate in some way to the scientific discussion.

Response: We agree that Fig. 9 and some of the associated text was rather tangential and have followed the advice of Referee #2 to delete it, keeping only a brief reference (See our response to Comment 2.2).

Comment 3.2: My principal criticisms as a scientific reviewer center on the need for additional analysis of the "kinetic overflow" approach (described in HO2007) to better define random and bias errors, as well as the sensitivity to particular parameter choices. With this manuscript's renewed focus on long-term changes (and a longer companion hydrographic dataset) there is an opportunity to develop increased confidence in the data quality and interpretation. HO2007 developed the "kinematic overflow" (KO) approach required by the lack of simultaneous velocity and CTD measurements and established that (a) the velocitydefined interface does co-vary with a temperature-defined interface (isotherm height), although the relationship is not extremely tight, and (b) velocity at adjacent mooring locations is highly correlated, so that a single mooring could be used to represent the flow through the entire channel. However, both of these relationships introduce some error into the final transport (with an unknown level of reduction due to averaging when computing standard errors). When investigating long-term trends, the KO calculation is most vulnerable to trends in these possible bias errors, so it is important to construct timeseries of (a) the difference between the annually-averaged velocity interface and a particular isotherm height (e.g., 7 degrees), and (b) cross-stream gradients in hydrographic temperature, density, or isotherm slope.

Response: Referee #3 raises some very relevant issues and we have tried to address them by including a new table (Table 3) and additional text at the end of Sect. 4.1 (p. 11, l. 8 – p. 12, l. 6 in the revised manuscript) and also two tables (Tables S1 and S2) and a figure (Fig. S9) in the supplementary document. We feel that this has strengthened our conclusions and have replaced “kinematic overflow” with the more general “overflow volume transport” in the heading of Sect. 4.1 and elsewhere (p. 10, l. 26, p. 11, l. 1, p.12, l. 8 in the revised manuscript)

Comment 3.3: The principal temperature timeseries presented is from the near-bottom measurement at the ADCP location, but more relevant for the AMOC would be the average properties (T, S, and density) of the overflow layer. Apparently no attempt has been made to compute these (using, for example, annual averages from the hydrographic sections), although the relationship between bottom temperature and interface height has been presented in HO2007. From the T-S changes presented in Fig.7, it is clear that the overflow layer has undergone changes, but how do these impact the layer average (using interface definitions based on density, temperature, depth, or average velocity profile)?

Response: Again, the referee has made a very useful recommendation that we have addressed and which we feel strengthens the manuscript substantially. To address this issue, we have added Table 4 and associated text into Sect. 4.2 (p. 13, l. 3-8 in the revised manuscript).

Comment 3.4: One particular hole in the KO analysis is the missing transport above the selected interface (the height where the velocity drops to 50% of the maximum). HO2007 pointed out that outflowing water above this level could include contributions from both dense overflow and entrained Atlantic Water from above, claiming that the overflow water in the layer is likely compensated by Atlantic Water below the interface. For volume budgets, all outflowing fluid needs to be included, while from the watermass perspective, a density-anomaly-weighted transport might be more appropriate. Differences among these choices could have a large impact on the detection of small trends in temporal variability in the presence of a large annual cycle and monthly wind-forced variability. Therefore, it is important to investigate the long-term trends in all of these neglected components.

Response: In this manuscript we have not attempted to treat the FBC-overflow in relation to volume budgets and we believe that question to require considerably more extensive observations and analysis. The referee is, however, justified in questioning whether variations in the layer above our defined interface could influence our conclusions. In the revised manuscript we have tried to answer this by considering the effect of two alternative definitions of kinematic overflow (p.5, l. 17-27 and p. 11, l. 5-7 in revised manuscript + supplement Fig. S2). Although average transport values may be affected by this, we find that the long-term variations and trend are not.

Comment 3.5: One issue that has not been mentioned in any of the papers or tech reports by the Torshavn/Bergen group is an intermittent contamination and low-velocity bias in 75KHz ADCP data, apparently caused by side-lobe bottom reflections, that has recently been discovered by the Hamburg group maintaining the Denmark Strait transport moorings. See the Quadfasel, Jochumsen, et al, presentation at the Feb 2015 NACLIM meeting linked here: http://naclim.zmaw.de/fileadmin/user_upload/naclim/Archive/Meetings/Annual_meeting_2015/PPT/S1.2-1_Detlef_Q_Overview.pdf. Although the issue seems to be most pressing in the Denmark Strait locations, there is a suggestions that the same issue could at least occasionally influence the FBC moorings. Has any attempt been made to quantify and/or eliminate this? The DS issues seem to vary with instrument version (especially internal processing algorithms), mounting hardware configuration, and local bottom properties. The possibility is important enough that should be addressed (if not in this publication, then another upcoming one) even if the FBC dataset does not require the kind of major corrections applied to the DSO measurements.

Response: We are aware of this problem, but do not believe that it affects our results. To our knowledge, this problem arises mainly for some RDI Long Ranger ADCPs. We have mainly used RDI Broadband ADCPs and do not see similar symptoms. The only exceptions are a Long Ranger deployment at FB from September 2012 to May 2013 and a Long Ranger deployment at FG from May 2008 to May 2009. The first of these deployments did indeed show similar behavior to that seen by the Hamburg group in the Denmark Strait, as documented in a technical report: http://www.hav.fo/PDF/Ritgerdir/2014/TecRep1401.pdf. Fortunately, there was a Broadband ADCP at the same site, which originally was assumed to be lost, but was later recovered. Thus, we have not used the data from the Long Ranger at FB. In the other case, the Long Ranger at FG was in a trawl-proof frame, which keeps instrument tilt very small and may also block side-lobes. Whether that or different firmware is the explanation, this system has not shown these symptoms neither during this deployment nor during other deployments in overflow regions (e.g., Olsen et al., 2016).

Comment 3.6: p.5,l.6. What is meant by the statement that a barotropic current could introduce a bias in the transport? The moored ADCP measures absolute velocity and is not vulnerable to a level-of-no-motion assumption. Is this referring to the fact that the interface (arbitrarily defined as the level at which the current speed is 50% of the max) will be shifted by a barotropic current? (It will, but so will the true transport.) Or is it related to the possibility of barotropic recirculation making the mooring location less representative of the average transport through the channel. This is indeed a possibility, but can't be diagnosed from measurements at the mooring alone.

Response: This paragraph was not well phrased and has been deleted from the revised manuscript. The problem is now treated in a different way in the last two paragraphs of Sect. 2.3 (p. 5, l. 17-27 in revised manuscript and Fig. S2 in revised supplement).

Comment 3.7: Since the approach presented here is clearly documented and has been explored from a number of angles, I don't feel that a large amount of revision should be required for publication of the current work. However, the lack of sensitivity analysis on the KO formula and the remaining un-pursued lines of investigation into possible KO biases described above (including water above the velocity-defined interface, velocitytemperature interface differences, and cross-stream gradients) make this unique longterm dataset weaker than it could otherwise be. I'd encourage the authors to follow up these issues. For example, my calculation "by eye" from Fig.8 of HO2007 suggests that the missing transport above the interface could be 10-15% of the total, and this layer could easily have long-term variability distinct from the lower layer. Certainly, a better estimate than mine can be made from the data.

Response: We agree that the original manuscript was lacking in this regard and thank the referee for the detailed comments. We hope that the revised manuscript (see responses to comments above) has clarified these issues.

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/os-2016-56/os-2016-56-AC1-supplement.pdf

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