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Interactive comment on "Increasing transports of volume, heat, and salt towards the Arctic in the Faroe Current 1993–2013" *by* B. Hansen et al.

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We thank Referee 2 for very helpful comments, which we respond to below:

Major Comments:

Referee comment: To obtain the 20-year time series of volume/heat/salt, a vast number of methods are used. Although the methods are clear, the entire manuscript rely heavily on a technical report which seem to include all the important details. I suggest that these details (calculations, Figs, and tables) that are important to the paper to be included in a supplementary material. Continuously referring to the technical report does not cut it! Our response: After consultation with the editor, we have submitted a pdf file with supplementary material, including most of the content of the technical report, but modified to account for referee comments. In the revised manuscript, we now refer to this supplement instead of the technical report.

Referee comment: The authors are convinced that the decline in the subpolar gyre (SPG) strength is driving the observed 1993-2013 trends. The weakening of the SPG started after 1996 (e.g. Hatun et al. 2005, Fig. 2) , but figure 5 gives the impression that the trend was already underway, which I suspect is due to the 3 year running mean. Firstly, the annual means of Atlantic water temperature and salinity should be added to Fig. 5 (see comment 17). Secondly, I suggest adding a time series of the SPG index, either in the same or in a separate figure. Thirdly, a discussion on the decadal/natural variability of the SPG and its linkage to the Atlantic water in Faroe Current is strongly recommended; very little is said about this in the manuscript. Our response: We did not intend the cause of the TS variations to be a main focus in this paper since this was discussed in detail by Larsen et al. (2012) and we feel that a thorough discussion on this topic would increase the length of the manuscript unacceptably. We also believe that the referee has misread Fig. 2 in Hátún et al. (2005) since that figure, as well as our new Fig. 7 have the SPG weakening to start around 1994. But, we agree that the suggestions for improving Fig. 5 (now Fig. 7) are good and that we have done. We have also changed the reference to the SPG at the end of the abstract. Instead of "attributed mainly to the weakened subpolar gyre.", we now say: "which have been claimed mainly to be caused by the weakened subpolar gyre." in order to emphasize that this is not a result of this study.

Referee comment: I do not see a motivation for why, in equation 1, adding a constant Uk0 for each interval to make the anomalies absolute? What is the logic here? When using altimetry one would, from a dynamical point of view, add the mean dynamic topography (MDT) which already includes a set of measurements (geoid, MSS) and in-situ observations to obtain the absolute values. The authors should instead consider adding the MDT from AVISO and re-calculate the fluxes, or at least provide an analysis that the calculated absolute values are comparable to those of absolute dynamic

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height. If the relationship is poor, this also needs to be well motivated. Our response: The referee is correct that we ought to have justified our choice of methodology better. In the revised manuscript p. 5, l. 30 to p. 6, l. 5, we now say: "The constants Uk0 for each altimetry interval could be determined from the mean dynamic topography (MDT), available from AVISO, but this would have given a surface current that was broader and considerably weaker than indicated by our in situ observations, especially between A3 and A5, where most of the Atlantic water transport occurs (Supplement, Fig. 2.4.4). Instead, we use values for Uk0 that are determined from ADCP data and average geostrophic profiles that are derived from the CTD data as elaborated in Sect. 3.1.". Also, Figure 2.4.4 in what is now the supplement has been modified to illustrate this point.

Referee comment: In table 4, only flux estimates by Berx et al. (2013) are compared. How about the estimates by Rossby and Flagg 2012 that show only a difference of 0.3 Sv between the IFR and FSC. It seems that the authors compare with only Berx et al. (2013) to make the point that the Faroe Current is much more important. The authors know the literature well and, therefore, advised adding other relevant studies that have made an attempt to estimate the fluxes into the Nordic Seas to table 4. Of course, I see the subsection in the discussion about this, but also there the authors do not provide any numbers. Our response: We find the values reported by Rossby and Flagg (2012) difficult to compare with our values since they are of short duration, not contiguous, and exclude a fairly large (1.6 Sv) component, which is assumed to circulate around the Faroes. Their results have been updated by Childers, Flagg and Rossby with an extended data set and they actually have a larger difference between the IF-inflow and FSC-inflow than indicated by our Table 4 with 4.6±0.5 Sv across the IFR and only 1.5±0.2 Sv through the FSC (see summary by Childers et al., 2014). So, with their values, the Faroe Current would have been even more important. Our motivation for the studies included in Table 4 was mainly similarity in duration and methodology. In the revised manuscript p. 18, l. 25 and the caption for Table 4, this has been indicated. Also, we have added Childers value for the IF-inflow transport on p. 18, l. 7-8 in the

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Discussion.

Minor comments 5. It is highly recommended to simplify the sentences, which are quite lengthy at times. Our response: Probably correct. In our revision, we have tried.

Referee comment: Section 2.3: Although satellite altimetry is central, the authors do not give any details at all about the altimetric dataset used. This needs to be done, and the relevant papers should be cited. Our response: In the revised manuscript p. 5, l. 11-13, this has been done.

Referee comment: Section 2.3, Page 1018, line 20: clarify which variable you are performing EOF analysis on. Our response: In the revised manuscript p. 5, l. 16-17, the text now says: "An Empirical Orthogonal Function (EOF) analysis on the SLA values from these 8 points revealed ... ".

Referee comment: Section 3.2.1, line 20-24: Why using two different definitions to get the AW time series? Please be consistent, either use the core of Atlantic water or the average between 100-150 for both temperature and salinity. Our response: In the revised manuscript, we now use the 100-150m average at N03 for salinity as well as for temperature. We have recalculated the regression coefficients for the old Eq. (7) (new Eq. (8)) and recalculated salt transport although the changes were barely discernible. Table 3 and the new Figs. 7 and 10 have been updated as well as other relevant information in the manuscript and the Supplement.

Referee comment: What confidence test is used throughout the paper? This needs to be mentioned! Our response: In Sect 3.6 of the revised manuscript p. 16, l. 31 to p. 17, l. 2, we have added: " The statistical uncertainties of the trends are the 95% confidence limits for the slope of the regression line when annually averaged transport values are regressed on the year using the t-distribution." and we refer to Sect. 3.6 the first time the trend analysis is performed on p. 14, l. 4..

Referee comment: Section 3.3.2, line 14: change “was below 10 % of the average” to

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“was 10 % below the average" Our response: The referee seems to have misunderstood what we wanted to say. In the revised manuscript p. 13, l. 29, we have attempted to clarify this: "was below one tenth of the average".

Referee comment: Section 3.4, line 25-28: The authors need to lighten up the reader why it is important to know the outflow temperature? Our response: In the revised manuscript p. 14, l. 7-10, this has been clarified: "The heat delivered by any inflow branch to the Arctic Mediterranean (Nordic Seas and Arctic Ocean) equals the heat lost by the water before it exits again. Thus, the heat transport is proportional to the temperature of the inflowing water minus the temperature of the water when it returns back to the Atlantic Ocean".

Referee comment: Section 4.1, Page 1031, line 8: Recommend to provide the estimates from the papers cited in line 6 (Rossby and Flagg, 2012 and Childers et al. 2014). Our response: In the revised manuscript p. 18, l. 7-9, we have added the value reported by Childers et al. (2014), which updates the Rossby and Flagg (2012) results: " Their values were updated by Childers et al. (2014) who reported an average inflow of 4.6$\pm$0.5 Sv across the IFR. Their value is higher than ours, although the uncertainty intervals overlap, but differences in definitions and timing make detailed comparisons difficult."

Referee comment: Section 4.1, Page 1031, line 9-10: What are the volume/heat/salt fluxes from these models? Our response: In the revised manuscript p. 18, l. 11-13, we have added the value reported by Sandø et al. (2012): "and Sandø et al., (2012) found an average inflow of 4.7$\pm$1.2 Sv using a high resolution model.". The volume transport reported by Olsen et al. (2015) is not directly comparable since this and presumably most other low-resolution models have difficulties in disentangling Atlantic inflow and overflow across the IFR.

Referee comment: Section 4.2, Page 1032, line 27: Suggest to add the paper by Skagseth and Mork, 2012: Heat Content in the Norwegian Sea, 1995-201 (ICES journal

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of Marine Science). Which discusses the increase in relative ocean heat content due to advection of warmer Atlantic water. Our response: In the revised manuscript p. 19, l. 25, this has been done and the reference added.

Referee comment: Section 4.2, Page 1033, line 8: No need for the AMOC abbreviation here. Our response: In the revised manuscript p. 20, l. 4, this has been deleted

Referee comment: Fig. 4: Suggest to add a realistic bottom topography, and change the color bar. Instead of a continuous color scale, assign one color to each contour. Our response: We agree that this would make the figure look nicer, but the stepwise appearance of the bottom topography seems to be a feature of the contouring software. We might perhaps circumvent that, but this would imply extrapolation beyond the coverage of our CTD data, which we hesitate to do.

Referee comment: Fig. 5: To be consistent with Figs 6-9, thin lines of the annually averaged Atlantic water temperature and salinity should be added. Our response: In the revised manuscript, this has been done

Referee comment: Fig. 5: Recommend rephrasing the second sentence in the caption. Our response: It is not quite clear to us, what the referee wishes us to do.

Interactive comment on Ocean Sci. Discuss., 12, 1013, 2015.