

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

**B. Hansen et al.**

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We thank Referee 3 for very helpful comments, which we respond to below:

Referee comment: My overarching concern about this manuscript mirrors the other reviewers', namely, that the majority of the information about the method used here is in a separate document (Hansen et al., 2015) and it is unclear whether or not the content of that other document is peer-reviewed. Our response: We have followed the suggestions by the referees to submit a supplementary pdf document with the contents of the technical report, modified according to referee suggestions and now refer to that instead of the technical report.

Major Concerns:

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Referee comment: Section 2.2: Although in the technical report, details about the extrapolation of the ADCP data to the surface should be included in the manuscript because this extrapolation is a significant contribution to the overall error estimate (Hansen et al., 2015, page 48) and extrapolations, in general, must be treated very carefully. Furthermore, when reading the technical report, it does not describe the criteria for how “we extrapolated  $\alpha(z)$  by eye” (Hansen et al, 2015, page 9). I think that Figure 2 in this manuscript, which is not present in the technical report, is a useful starting point for explaining the extrapolation. To make for a consistent explanation, Figure 2.2.2 from the technical report should also be included in this manuscript. Our response: In the revised manuscript p. 4, l. 30 to p. 5, l. 3. we have added " From the observations, we find that the shape of the ADCP profiles at each site is very consistent so that the ratio between eastward velocities at two different depths is relatively constant in time (Fig. 2). Using observed and extrapolated values for this ratio (Fig. 2), we have extrapolated all the profiles from the long-term sites to the surface.". We have also, as suggested, added a simplified version of Figure 2.2.2 from the technical report, which is Figure 2 in the revised manuscript, and included more information on the extrapolation in the figure caption.

Referee comment: Section 2.3: I believe that Fig. 3 is one of the most insightful and pivotal figures in the method used here. However, I think the version in the technical report (Hansen et al., 2015, Fig. 2.4.4) is clearer because the extra black lines show precisely how the ADCP point measurements were interpolated onto the large-scale altimeter grid and a contribution of this interpolation to the overall error estimate. This interpolation should be outlined in the manuscript because it is, in my opinion, unclear, in the manuscript, precisely how the blue lines and black squares of Fig. 3 are used to construct the red line. Furthermore, at the end of Section 2.3, no mention of Fig. 3 is made even though Fig. 3 is, in my opinion, essentially the key result of Section 2.3. After explaining the interpolation process more fully and citing Fig. 3, I don't see the need to cite Table 2.4.4 in the technical report here as it contains essentially the same information. Our response: As indicated by this comment, we have found it

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difficult to keep an appropriate balance between the methods and results sections. In the revised manuscript, we keep the main treatment of this issue in sect. 3.1, but make a reference: " as elaborated in Sect. 3.1" to it at the end of Sect. 2.3. We have followed the advice to use the figure from the technical report, although modified, which in the revised manuscript is Fig. 5, and we have modified both Sect. 3.1 and the caption for the new Figure 5 to make the points raised by the referee clearer.

Referee comment: Section 2.4: I believe that the relative profile equation (Eq. 5.1.2, Hansen et al., 2015) should be included and defined in the manuscript. It won't add much length since it's a simple equation but it will be a huge help for readability. Furthermore, Section 3.1, line 12 says the relative profiles are "defined in Sect 2.4" but they are actually not defined since Eq. 2 shows the use of the relative profile but not the definition. Our response: In the revised manuscript p. 6, l. 14-17, we have added this equation (now Eq. (3)) and associated text. In Sect. 3.1, p. 7, l. 28, we have furthermore changed "defined in Sect 2.4" to "introduced in Sect 2.4"

Referee comment: Section 3.1, line 21: I am really confused by the reference to Table 2.4.1 (Hansen et al., 2015) here. At the end of Section 2.3, the authors state that  $U_{k\ddot{E}0}$  values come from Table 2.4.4 (Hansen et al., 2015). Please explain the difference between these two tables or confirm that it is only one and not the other that is used. If Table 2.4.1 in the technical report is cited, then some explanation must be provided in the manuscript for how the altimeter data is interpolated to the positions of the ADCP's because an interpolation is implied in Table 2.4.1 but not discussed in the technical report. Or, is the difference in the numbers at the ADCP stations due to the difference in averaging time span because NA and NE do not have exactly the same deployment times? Our response: In Table 2.4.1, individual ADCPs are compared with average velocities derived from altimetry for the altimetry interval including the ADCP site and the same period, whereas Table 2.4.4 synthesizes the information from all the long-term ADCPs in each interval between A3 and A5. This should no longer be confusing, since there is no longer any reference to Table 2.4.1 in the technical report in

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the revised manuscript,. With the new version of what is now Figure 5 and associated text modifications, this was no longer necessary.

Referee comment: I believe that the error estimates (Hansen et al., 2015) are a critical aspect of this work because it provides the context for interpreting the variability of the time series. The estimates in the technical report are sufficient and I believe they should be included in this manuscript. Our response: In the revised manuscript, Sect. 3.6 has been completely revised based on this recommendation.

Furthermore, I think the error estimate in transport of  $\sim 0.5$  Sv is similar to the 0.4 Sv RMS difference quoted in Section 3.3.1. This consistency should be noted in the manuscript as a quasi-independent double check on the error estimate. Our response: In the revised manuscript, p. 12, l. 21-24, we have modified the text to say: "For monthly (4-week) averages, the correlation coefficient between these two estimates is 0.94 with an average difference of 0.03 Sv and a rms (root-mean-square) difference of 0.3 Sv (Supplement, Table 5.3.2). This is 6% of the average volume transport and well below its estimated uncertainty (Sect. 3.6)."

Referee comment: Please explicitly interpret the significance of the trends in fluxes relative to the uncertainty estimates. The stated uncertainties nearly encompass the entire variability of each time series (e.g.  $\pm 0.5$  Sv is the vertical range of Fig. 6b). I interpret the relatively large uncertainties ( $\sim 13\%$  for volume,  $\sim 12\%$  for heat, and  $\sim 15\%$  for salt, Hansen et al., 2015, page 48) to mean that there is an upward trend in the time series but the uncertainty in each data point is of the same order as the trend so the increasing trends are perhaps just barely visible. Our response: In the original version of Sect. 3.6, we tried to emphasize that most of the error sources for the average transport values are in the form of biases, which should not affect temporal trends. In the revised manuscript p. 16, l. 28-31, we have tried to make this argument more clear: "Most of the uncertainty sources, quoted above, may be seen as biases. Thus, they affect the average transport values, but should not affect the temporal changes in transport appreciably. Any remaining errors should be included in the statistical

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uncertainties cited for the overall trends of transports (Sect 3.3 to Sect. 3.5)."

Minor Comments:

Referee comment: Line 4: "intense" not "intensive". Our response: Has been corrected in the revised manuscript p. 1, l. 13.

Referee comment: Table 4: needs citations in table caption or in table rows. Our response: In the revised manuscript, we have added reference (and period) to the table (with track changes disabled, but coloured red) and modified the table caption.

Referee comment: Figure 1 caption: Please define both Atlantic inflows. Only one branch, over IFR, is listed and "FSC" in the bottom right of panel a is not defined or mentioned. Also, mentioning both inflows is necessary to be consistent with Table 4. Our response: In the revised manuscript p. 30, l. 6-7, we have added the sentence: "The other main inflow branch (the FSC-inflow) is also shown".

Referee comment: Section 3.2, line 16: This list of contributions is nice but a phrase or sentence is needed after the list to indicate which ones are being used. Our response: In the revised manuscript p. 9, l. 1-2, we have added the sentence: "In the next three sections, we try to estimate these".

Referee comment: Section 3.2.2, line 11 – j is used as a station index here while k is used as an ADCP position index and also an altimeter grid box index. It would help the reader if these indices were explicitly defined together at the same time, perhaps in the context of Fig. 2, and a separate index was used for each position type. Our response: We believe that we use only two indices in the manuscript: k for altimetry points and j for station index, but we agree that this could be more clearly stated. In the revised manuscript p. 4, l. 5, we have inserted " Properties at these stations are labeled by the index j (j = 1 to 14)". And, on p. 5, l. 15, " We use the index k (k = 1 to 8) to identify these points".

Referee comment: Section 3.3.2, line 23 "choices" instead of "choises". Our response:

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In the revised manuscript p. 13, l. 10, this has been corrected.

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

**OSD**

12, C640–C645, 2015

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