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Comment

## ***Interactive comment on “Biased thermohaline exchanges with the arctic across the Iceland-Faroe Ridge in ocean climate models” by S. M. Olsen et al.***

### **Anonymous Referee #1**

Received and published: 15 August 2015

The paper presents a critique of the ability of climate models to represent the detail the exchanges across the IFR accurately, by focussing on a particular problem event, the weakening of the inflow during most of 2003. This weakening appears in the observations but is absent in the climate models. The authors show using a combination of climate model, theoretical model and observations why this discrepancy exists and, that whilst it does not affect mass balance, it may have implications for the net heat and salt transport in climate models. Thus the paper is ultimately directed toward ocean climate modellers and, in highlighting a problem that I suspect many of them are aware of, calls for more effort in the region to improve their predictions.

For me the more interesting result is that the reduced inflow appears not directly at-

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tributable to local wind forcing but is the result of a large scale baroclinic response of the ocean to the anomalies in the wind field across the Nordic Basin, shown in Fig. 5. I'm sure I'm not the first to wonder whether the excitation of low frequency baroclinic modes in the Nordic Basin contributes to the variability of transports across the GSR. In which case a more direct link between atmosphere and the depth of the 27.8 isopycnal might be found in the large scale distribution of sea level atmospheric pressure rather than the wind stress. But anyway, that's not what this paper is about.

Where I think that the paper is weak is in the discussion of the impact of the resulting misrepresentation of the ventilation rates on climate model sensitivity. What's missing is a comparison of the error due to the failings in quantifying IFR exchange with other things that affect ventilation, such as uncertainties in atmospheric heat transport and the hydrological cycle for example. Climate modellers still have many problems to overcome and I'm sure would appreciate knowing an assessment of the importance of an accurate representation of IFR exchange.

Overall, though, I find no reason to doubt the results of this analysis, particularly as the authors are acknowledged experts in the oceanography of the IFR. At first the connection between inflow and overflow appears to be built on a number of rather tenuous steps, but Fig. 5d showed me that those steps were justified. I have no major objection to its publication once the small number of minor corrections listed below are addressed.

Page Line 1472 6 'Hereby' is definitely the wrong word, but the right word depends on what is meant 10 change to 'have shown to be ...' 1473 26 change to 'has shown to be' or something similar 1478 19 change to 'not allow resolution of the' 1480 1 '26 cm' is inconsistent with the scale shown in Fig 4c. 1484 16 change to 'termed the reconstructed' 1486 5 I think you mean 'when all the inflow branches are included'. What you've written is not clear. 1487 24 'small' is less ambiguous than 'low' here 1502 Fig. 2 The pink band tends to obliterate the grey lines when the pdf is printed out. Is there a better colour arrangement you could use? 1502 Fig. 2 Remove '(black)'

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in 4th line of caption; 5th line: 'with the observed'. 1503 Fig. 3 y axis scale – as commented above 1504 Fig. 5 The arrow heads are very hard to read, and it's quite important to be able to read them. The figures blow up well on my iPad, but then the arrows appear rather crude. I know it's difficult to resolve this issue satisfactorily for all media but I thought I'd point it out in case you were looking for a little encouragement to improve the figures.

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Interactive comment on Ocean Sci. Discuss., 12, 1471, 2015.

**OSD**

12, C547–C549, 2015

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