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Ocean Science Discussions

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Interactive Comment

Interactive comment on "Descent and mixing of the overflow plume from Storfjord in Svalbard: an idealized numerical model study" *by* I. Fer and B. Ådlandsvik

I. Fer and B. Ådlandsvik

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Reply to Reviewers:

We thank both reviewers for thorough reading of the manuscript and their constructive comments. We benefited from and addressed all comments in the revised version. A detailed response to the comments of the reviewers is given below. Reviewers' comments are reproduced starting with CO. Reply is below each comment, starting with RE.

Reply to Reviewer 1:

CO. P860-01: Can you consider if it would be constructive adding the boundaries for

Jan-March and April-June described in the text. It may be that Fig 1 then becomes too complex.

RE. Yes, we agree that this would help the reader. We added all three boundaries used for constructing the ambient profile on Fig. 1a. We did not exclude the third region (July-September) as addition of it did not make Fig.1 too complex.

CO. P866 line 27-28: Is this the difference in model stratification a consequence of how the hydrography was constructed * i.e. related to the profiles in Figure 2??

RE. Yes. We clarified this by adding the part marked between *** below. "... the model stratification in the ambient, *** as a consequence of the implemented hydrography (Fig. 2),*** is different from the summer conditions which will also affect the diffusivity estimates ...."

CO. P867 line 24. I note that sometimes you use TR=5% and sometimes TR=0.05. Can you check consistency throughout.

RE. We now used the threshold TR = 0.05 consistently throughout. Two instances of 5% (p867, li24 and p868 li8) were changed.

CO. P868 lines 9-15. I feel that there is a lot of detail in Fig 10 that you cover very quickly in these sentences. There is some interesting hydrographical discussion to have here, please consider expanding this section.

RE. We expanded this section as given below. We also revised Fig 10 in response to a later comment below. When doing so, we noticed that the original Fig 10 was after the analysis of the model results when we did not employ tracers. The plume thickness was then inferred from salinity / temperature and velocity conditions. In the original submitted work we analyzed the data after the run with tracers that allow more reliable detection of the plume. All figures but Fig 10 were produced with the results following the tracer analysis. We apologize for this error. We reproduced the figure (Fig 10) and hence you will notice some changes (for t = 150 day) from the original version. Revised

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text is as follows:

"The core T-S properties at the bottom layer at t = 0 and t = 150 day are shown in Fig. 10. Those at t = 0 correspond to the ambient, as specified by the initial profiles in Fig. 2, at the appropriate depth of the core along the plume path. The ambient properties at the bottom layer along the plume path encounter Atlantic Water (AW) core and Norwegian Sea Deep Water (NSDW) at about 100 km and 400 km, respectively, downstream from the sill. At day 150, the core T-S properties of the plume are modified as a result of entrainment and mixing. The BSW at the sill is a mixture of the dense source water, the prescribed basin water (to the sill level) and the ambient properties above the sill level. This mixing takes place as the source waters propagate from the discharge locations towards the sill. From the sill towards the shelf-break (about 220 km), the overflow plume properties trace a mixing line between the diluted source water at the sill and AW. Farther downstream the mixing takes places in a triangle formed by the plume properties at the shelf-break, AW and NSDW (Fig. 10). Because the cold plume entrains relatively warm AW, it appears as heat source for the ambient after about x = 300 km where it is mainly driven by its salinity excess."

CO. P869 line 1: i runs from 0 to 2. But it runs from 1 to 3 in the caption in Figure 11.

RE. Corrected. Now i runs from 0 to 2 at all places. We also replaced QTR2 with QTR1 on Fig 11a.

CO. P871 line 1-4: I think that you should try and rationalise these differences in plume thickness little more, particularly against the observations of Quadfasel et al. This is a large discrepancy. Variability in the overflow or limitations of the model??

RE. Both the variability in the overflow and the limitations of the model are likely responsible for the discrepancy. Lacking a systematic sensitivity study, it is difficult to answer. Also note that far downstream where the plume/ambient interface is very diluted (especially in the model) the detection of the plume using a TR=0.05 condition will lead to thicker plumes than what eye would pick from T/S profiles. The latter is done when interpreting the observations. Here we added: "...(1995). Both the variability in the overflow and the limitations of the model are likely responsible for the discrepancy between the observations and the model results. Lacking a systematic sensitivity study, we cannot evaluate the role of the variability in the overflow. In the Fram Strait, the plume/ambient interface is highly diluted and the plume thickness is likely overestimated using TR = 0.05 threshold. Albeit very small, the background mixing level for scalars prescribed in the turbulence closure scheme (Sect. 2) can be significant in diluting the plume. On the other hand, lacking tracer information, plume thickness inferred from observations might be biased low as low source water concentrations will not be immediately obvious in the hydrography. The decreasing vertical resolution .....";

CO. Fig 2: Consider putting in two horizontal lines to indicate the depths ranges used in the formulation of the ambient hydrography.

RE. In the revised Fig 2. we marked these depths.

CO. Fig 3. It is not so easy to relate the location of Figure 3 to the regional map of Figure 1. Is there any way you can help the reader here?

RE. We added two rectangles on Fig 1b to mark the location of the Fig 3 frames.

CO. Fig 10. I'm not clear what the gray data points are. At t=0, when there isn't a plume, I am interpreting these as the T-S property at the appropriate depth of the core along the plume path as specified by the initial profiles in Figure 2. Can you clarify this aspect of the figure and caption. Would it also help to add the white bullets with the same distance along the path as you do for t=150d??

RE. The reviewer's interpretation is correct. We clarified this in the text and also in the figure caption. Also note that we reproduced Fig.10 (see reply to p868 comment above). On p868 we added: "........ in Fig. 10. Those at t = 0 correspond to the ambient, as specified by the initial profiles in Fig. 2, at the appropriate depth of the core along the plume path."

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CO. Fig 12 (c): The line indicating G is very thin and faint.

RE. In the revised Fig.12, this line is darker and thicker.

CO.Textual corrections: P857, line 16: *an inclined boundary HAVE been * P865, line 27. Can "Farther above the bottom" be replaced with "At shallower depths"?? P870 line 1: I don't think "diagnosed" is the correct word here. Probably "calculated" or even "used".

RE. All three textual corrections are incorporated in the revised version. For the last correction, we use "used".

—- END OF REPLY TO REVIEWER 1 ——-

Reply to Reviewer J. Huthnance:

CO. The paper would be improved by some discussion (at least; if not trials) of sensitivities to factors not included or taken as fixed: especially open boundary conditions (which could drive tides or barotropic flow along the slope west of Svalbard), density of winter-cooled water originating the plume. This could reinforce interpretation of the comparison with observations.

RE. We agree with the reviewer. While a realistically forced simulation of the Storfjorden overflow is in progress, in this study we deliberately presented a single run with idealized forcing- a step between the previously published 1.5 layer model of Jungclaus et al. and on going realistic simulation. Although we have performed several runs during the preparation of the paper (not shown), we did not perform a systematic sensitivity study. We chose not to report different runs as the paper is already very lengthy as a result of detailed comparison with observations. Following J. Huthnance's suggestion we add the following to discussion (section 7.1) [and also parts of it to section 3 together with other forcing caveats in response to other comments below]:

"The descent and mixing of the overflow plume depend on several factors not taken into account in this study. Wind forcing is generally important in the region. Numerical

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experiments not reported here show that wind from south into Storfjorden significantly enhances the overflow rate. The density difference between the plume and the ambient water is of main importance. In the present study, this depends on the prescribed properties of the plume water. In a more realistic setting, the density structure of the ambient water will depend on the inflow of Atlantic and Arctic water masses across the open boundaries. The model results accordingly suffer from the lack of the mean oceanic circulation and the effect of winds and tides."

CO. Section 3.2, page 861 line 5 "joining"; the depth ranges to form the composite temperature and salinity profiles: it would help to outline the basis / method for the "joining". Line 7: "5-scan running mean"; what is a "scan"? The kink at 500 m is surprising.

RE. The seasonal hydrography used from the World Ocean Atlas Data is at standard depths. The constructed ambient profile (section 3.2.) is again at standard depths. We did not use a special method to join different segments. We clarified this point to avoid any confusion. We employed a 5 data point running mean (clarified). The ambient profile at standard depths was then implemented in the model layers by linear interpolation (clarified). On Fig. 2, the kink at 500 m is simply due to the change of vertical scale at 500 dbar (100 dbar intervals down to 500 dbar and 500 dbar intervals below). The change in vertical scale is stated in the figure caption and we emphasized it by a solid (instead of dashed) grid-line at 500 dbar. We chose to change the vertical scale to show the ambient at the shelf more clearly.

CO. 3-comments gathered here: Section 3, page 861 or 862. Somewhere here should be some statement of open boundary conditions other than the temperature and salinity profiles. Is there sensitivity thereto, especially any transport along the slope? C.f. pages 864, 865.

Section 5.1, Page 864, line 21 "absent in the model forcing"; better to state this sort of thing a priori, c.f. comment on Section 3.

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Section 5.2, page 865, lines 12-14 "none of which were incorporated in the simulation", and last line "owing to the idealised forcing". Again, better to state this sort of thing a priori, c.f. comment on Section 3.

RE. In response to these three suggestions we added the following to section 3:

"In a more realistic setting, the density structure of the ambient water will depend on the inflow of Atlantic and Arctic water masses across the open boundaries which could drive tides and the barotropic flow along the slope west of Spitsbergen. Owing to the idealized forcing employed in this study, the West Spitsbergen Current and the mean circulation in Storfjordrenna, and the effect of tides and wind are absent. Furthermore, the ambient circulation at the sill is not captured and the density of the winter-cooled water originating the plume is prescribed."

CO. I would suggest reordering the numbering and treatment so that hydrographic Section 2 is in figure 4 and discussed before Section 3 that would be in figure 5, i.e. work "downstream" [the timings of the observations do not relate to each other so should not influence the choice].

RE. Done. We swapped figures 4 and 5 and revised captions, text and cross-references throughout.

CO. Section 5.3, page 866, line 18: better ".. decreases through time ..".

RE. Corrected as suggested.

—— END OF REPLY TO J. HUTHNANCE ——

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