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Ocean Science Discussions

Interactive comment on "Structure and forcing of the overflow at the Storfjorden sill and its connection to the Arctic coastal polynya in Storfjorden" *by* F. Geyer et al.

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Reply to Interactive comments on

"Structure and forcing of the overflow at the Storfjorden sill and its connection to the Arctic coastal polynya in Storfjorden"

We would like to thank the two reviewers for the time, effort, and interest taken to review our work. The comments they provided were encouraging and helpful, and we are glad that both referees were so positive to our work. We revised the manuscript following the suggestions of the reviewers. The original text from the different reviewers is reproduced below, followed by our response for each comment.

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Interactive Discussion

Discussion Paper

Bergen March 18. 2010

Florian Geyer, Ilker Fer and Lars H. Smedsrud

Anonymous Referee #1 Received and published: 2 March 2010

General comments The paper provides a clear and concise discussion around the behavior of the overflow of brine-enriched shelf water (BSW) across the sill from Storfjorden, Svalbard. The discussion is based on a one year primitive equation model run, including a dynamic and thermodynamic sea ice module, and comparisons with direct current measurements of the overflow. The current measurements are published by two of the authors in a previous paper, and the present paper closely follows and tests the methods used in the previous paper. I think the paper adds to the understanding of how wind can influence the days to weeks variations in the overflow. Moreover it strengthens the reliability of the simplifying assumptions commonly made in previous estimations of the BSW overflow from Storfjorden.

Specific comments I have only one small comment. Station 1 is mentioned in line 29, page 28, in connection with Figure 11, but not shown in the figure. The sentence can be excluded, and position 1 removed from Figure 2.

Changed according to the reviewer's suggestion. Former stations 2 and 3 are now stations 1 and 2. Figure 11b is revised accordingly.

Technical corrections Line 18 on page 28. I believe it should be 11b instead of 11c.

Changed as suggested

Anonymous Referee #2 Received and published: 4 March 2010 This is an interesting and well written paper. It describes a simulation of dense water overflows at Storfjorden sill in 1999-2000, and how these flows relate to the Storfjorden polynya. The key result of the paper is that, while the polynya is the main source of the brine enriched dense water that fills the deep Storfjorden basin, the overflows themselves are wind driven rather than of thermohaline origin. Indeed, the authors claim that easterly winds cause

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surface Ekman flows into the Stofjorden basin that are compensated by an outflow of dense water at depth. This result certainly merits publication in Ocean Modelling. However, I have a few problems with the model setup, the duration of the integration and the conclusions drawn from the simulation. The paper could be greatly improved if the authors took in consideration these concerns. The comments below are organised by page and line in the paper. Those that I consider particularly important and specially worth addressing are indicated with an asterisk.

Page 18, lines 1-2. High salinity shelf waters formed on Arctic shelves are believed to be the main source, perhaps the sole source, of halocline and deep waters in the Arctic, but there is insufficient for stating this in such a matter-of-fact manner.

We agree with the reviewer. This opening sentence of the abstract requires justification and is not suitable for the abstract. This entry is not crucial, and we simply removed it from the revised version.

Page 19, lines 21-23. State clearly that both the total dense water production and the Storfjorden dense water formation are model estimates.

While the estimates of Winsor and Björk (2000) are based on a polynya model, Storfjorden contribution we refer to is inferred from observations of the overflow transport (Schauer, 1995; Geyer et al. 2009). We clarified these points by revising as follows: "Using a time-dependent polynya model, Winsor and Björk (2000) estimate a total dense water production of 0.7-1.2 Sv (1 Sv = 106 m3 s-1) in the Arctic coastal polynyas. Volume transport of the Storfjorden overflow inferred from observations (Schauer, 1995; Geyer et al. 2009) amounts to 3 to 6% of the total dense water production, suggesting that the contribution of the Storfjorden polynya is significant. The reader is. . .".

* Page 21. lines 21-28. There is no spin-up of the high-resolution Storfjorden region model. The basin is likely to be still responding to the initial conditions by the end of the first, and only, year of simulation. Why to carry out an initial spin up of a few years

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up to August 1999 before the beginning of the simulation. How does this “cold start” of the model affect the results?

The ROMS model system was spun up gradually, and as such there is no "cold start" of the runs. Further details can be found in Budgell (2005). The high resolution run was initiated by the fields from the intermediate-scale model, and thus provides better initial fields than any climatology. We agree that this essential information lacked in the original manuscript, and we include the following more detailed description in the revised version:

“The ROMS model is used in a three-stage one-way nesting configuration (Budgell, 2005). A basin-scale model for the North Atlantic and Arctic Ocean was initiated in 1948, and forced daily by surface fluxes obtained from the US National Centers for Environmental Prediction (NCEP)/US National Center for Atmospheric Research (NCAR) re-analysis (Kalnay et al., 1996). The surface forcing was corrected for the high-resolution model ice concentration as described in Budgell (2005). Fields of wind stress, sensible and latent heat fluxes, solar and long-wave radiation, and precipitation were used for forcing the model.

The intermediate-scale model (average grid size of 9.3 km) covered the Barents and Kara Seas and was run for 1990-2002, initiated with 1990 fields from the large-scale fields. This intermediate model provided the initial and boundary conditions for the 2-km horizontal resolution Storfjorden model run. The model was run for the duration of one year, starting with ice-free conditions in August 1999. The nesting steps are performed using an open boundary flow relaxation scheme (Engedahl, 1995) for both ocean and ice variables. Tides are not included in the present simulation.

The fine-scale, 2-km resolution, domain is shown in Fig. 1. The analysis in this study concentrates on the area shown in Fig. 2. The model domain is obtained by a rotated polar stereographic map projection. The bathymetry is interpolated from the 2’ global dataset of the US National Geophysical Data Center (2001 version;

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http://www.ngdc.noaa.gov/mgg/fliers/06mgg01.html). The land mask is modified manually to fit the global self-consistent, hierarchical, high-resolution shoreline database (GSHHS) coastline (Wessel and Smith, 1996). The bathymetry is smoothed by a Shapiro filter, in order to minimize pressure gradient errors associated with abrupt topography changes. The smoothing was repeated until the slope parameter of Beckman and Haidvogel (1993) was less than 0.25. In total 30 vertical levels are used, with a finer resolution near the surface and the bottom."

Page 22, line 8. How many passes of the Shapiro filter do you apply?

The Shapiro filter was applied until the r factor (originally called the slope-parameter by Beckmann and Haidvogel (1993) was less than 0.25. This is now clarified in the text (see the last lines in response to the previous comment).

Page 22, line 10. Sensitive heat?

Changed to sensible heat. The ordering of the paragraph changed. This part now includes more information about the model forcing as specified in reply to the comment on page 21, line 21-28.

Page 23. line 9-11. Explain why concentration may not be a good variable for identifying the polynya area.

We inserted the following to clarify this point: "We identify the polynya as grids with a mean ice thickness less than 0.3 m, which is the transition between young and first-year ice (WMO, 1970), consistent with Smedsrud et al. (2006). Ice concentration alone is not a good criterion for identifying the polynya in a high-resolution model, because open-water areas in the model quickly freeze over with thin solid ice. In nature, this ice forms as free-floating small frazil crystals that are packed by the wind toward the leeward side of the polynya."

Page 23, line 19. Please, provide a brief explanation of why tidal forcing of the polynia is negligible based on Ersdal (2009).

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Ersdal modifies the polynya model of Skogseth et al. (2005) to include tidal forcing. She concludes that wind forcing of the polynya is dominant as ice production increases by only 2.7% on average for 1997-2002, when tidal forcing is included. This information is now added in the text. "...negligible (Ersdal, 2009). Using a modified version of the polynya model of Skogseth et al. (2005a) to include tidal forcing, Ersdal (2009) reports only 2.7% increase in ice production when averaged between 1997 and 2002."

Page 24, lines 8-9. What was the vertical range of the ADCP?

We clarified this point by inserting "When sampled at 4-m vertical bins, the ADCP had a typical range of about 100 m which varied seasonally depending on the scatterers in the water column."

Page 24, line 19. Provide a figure comparing your modelled cross-sill component of the velocity with the observations of Geyer et al. (2009). Is the simulated overflow larger than observed because the model is undergoing a transient from its initial state?

The model run and the observations are for different years. This makes the model-observation time-series comparison difficult. We produced a figure as suggested, however, deemed it not very instructive and do not include it in the revised version. The modeled overflow is not larger than observed values, but within the range of the observations of Geyer et al. (2009), with a mean seasonal overflow of 0.07 Sv observed e.g. in 2006.

* Page 27, lines 20-22. There is quite a bit of explaining to be done here. How is it that Ekman inflow and bottom outflow seem to be causally linked from January to July but not at other times? Is the winter agreement just a fluke? You would need to integrate the model for at least another year to confirm, or otherwise, any strong relation between both flows.

This part is a discussion of the surface Ekman flow balance of the fjord and its possible shortcomings for predicting the overflow. The Ekman flow balance was suggested to

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have some skill in modulating the deep outflow in Geyer et al.(2009). Here, in light of the model data, we suggest that caution is needed in interpreting the Ekman inflow and the bottom outflow response. It is also important to keep in mind that for any dense overflow response to surface Ekman forcing, dense water has to be present in the fjord, which is not the case outside the overflow season. The last part of the paragraph was modified to make this explicit:

"For instance, in early January significant FE has no discernible overflow flux, as very limited dense water is present in the fjord this early in the freezing season. We therefore conclude that while the net surface Ekman flux into the fjord can give a good indication of the overflow volume flux and its variability, caution is needed in interpreting the Ekman inflow and the bottom outflow response, as they are not directly equivalent. This conclusion is also supported by the qualitatively different response of fjord circulation and dense overflow to wind forcing by, respectively, east-southeasterly and east-northeasterly winds (Fig. 8, 9)."

Pages 30-31. Section 4.3. I find this discussion a bit confusing, as it is not clear which transports are model estimates and which ones are from observations. Please, clarify.

In section 4.3, we apply the methods used by Geyer et al. (2009) on the model data. Thus all transports in this section are model estimates, and the differences between the different model estimates give an indication of the systematic errors involved in the estimates of Geyer et al. (2009). For clarification, we now state at several places in this section that the model data is used for the analysis, and the following sentence was added at the beginning of the section: "In this section, the regional ocean model results are used to test assumptions commonly made when interpreting field data of the Storfjorden overflow. We use the model data only, and calculate the overflow volume transport using various assumptions including identical methods applied to observations from a single mooring."

Figure 2. All contours are unlabelled. Please, include labels.

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Changed according to the reviewer's suggestion

Figure 5. Ditto.

Changed as suggested, also for figure 6.

Figure 7a. Ditto.

Changed as suggested

Figures 8a and 9a. Ditto.

Changed as suggested

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/7/C67/2010/osd-7-C67-2010-supplement.pdf

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Interactive comment on Ocean Sci. Discuss., 7, 17, 2010.