

We thank all reviewers for their thorough review of our manuscript and the constructive comments and suggestions that were made. They have greatly contributed to improving the manuscript.

Anonymous Referee #1

Overview

This paper described a series of model sensitivity simulations of the outflow of dense water from the Storfjorden in Svalbard with focus on how tidal dispersion acts to modify the spreading of the plume. It is a very well written contribution, with some important conclusions. Considering the importance of deep water in the Arctic, and the lack of observations from the area, the present sensitivity approach is sound and of interest to the readers of Ocean Science. It also highlights the importance of including tides in regional ocean models – a process which has been neglected for too long. There are a few issues, however, which must be addressed before this paper can be published (see below). I therefore recommend a medium-major revision of the manuscript.

Major issues

There are two main problems which must be addressed. The first, and less serious, is that the model deviates quite significantly from the observation in Fig 3-4. Since the work is largely a sensitivity study, I can accept this (after all there are similarities), but this must be discussed better, and the sensitivity nature of the investigation mentioned and made cleared. It is also somewhat surprising that there is only a very weak surface mixed layer in the simulation shown. Was there no wind in this run? Please comment on this, and discuss it in the paper, e.g., could this affect the outflow dynamics of dense water?

The simulations shown in Figs 3 and 4 include wind forcing. The figure caption was amended.

We now explain the differences between observations and model in greater detail in section 3.3 (“Comparison with observations – plume”) of the revised ms.

To avoid confusion we removed Fig. 4 (cross-section E from Fer 2003) as it relates to a different overflow scenario and was taken at a different time of year.

The more serious issue is a complete lack of tidal validation of the model results. TPXO7.2 data is used as boundary forcing but does the model actually get the tides right around the plume? Since there is no tidal potential included over the domain it is quite possible the amplitudes are underestimated. This must be addressed, preferably by comparing modelled elevations at tidal frequencies to both the TPXO field and to tide gauge/pressure data. There is tides gauge data available from the area (there is data from Ny-Alesund and Bear island: see <http://ilikai.soest.hawaii.edu/uhsic/html/d0823A.html>), and including a comparison to those stations is crucial, along with a error analysis compared to TPXO. After all, showing that the model can reproduce tides is quite crucial since the paper deals with tidally driven dispersion.

We agree that correct representation of tides is crucial. We include a new section in the revised manuscript (sect. 3.2 “Comparison with observation – tides”) which compares tides in our model with observations taken in the region covered by our domain. The comparison of sea level elevation (using the data set suggested here, see new Fig. 2) shows that the model fits with observations with approximately the same degree of accuracy as TPXO7.2 (one of the best specialised tidal models) fits with observations.

Minor comments

The figures/figure captions need to be improved:

Fig 3.: Which wind scenario? The x-axis have different scales – please amend. I assume we sea potential temperature, salinity and potential density in the different rows?

The Figure shows the The HIGH scenario ($S_{sill}=35.8$) including tides and wind. The figure caption is amended and the horizontal scales are now adjusted to match. The different plots are labelled with θ , S , σ_θ for Temperature, Salinity and Density respectively.

Fig 4.: Which wind scenario?

Fig.4 has been removed.

Fig 5 and 6.: Which runs are shown?

They show the HIGH scenario ($S_{sill}=35.8$) with wind. Clarification is added to the captions of Figs 3 and 4 (Figs 4 and 5 in the revised ms).

Fig 7.: Mention that isobars have been superimposed, but from which run?

The bold lines represent isopycnals for 28.1 and 28.2 kg/m^3 . They are from the same run. Clarification was added to the figure caption.

Fig 10.: The colourbar related to the tracer concentration is missing

The colourbar is added.

p.14 l. 23 - which differences in k_{hor} are significant ? Please test if they are statistically different and shade differently in Fig 8c

k_{hor} is not a random variable but determined by a formula from the strength of the modelled currents. We therefore do not think that a test for statistical significance is applicable to individual runs. For the whole ensemble of runs, the sample size is very small – only 4 pairs of runs (or 8 pairs, if no-wind scenarios are also included).

p 15 l. 7 - Is the spring-neap cycle significant? M2-N2 modulation?

Yes, the spring-neap cycle can be seen in the maximum values of tidal κ_{hor} (Fig. 9, Fig. 8 in revised ms). The sentence is reworded because Fig.2 has been replaced in the revised ms.

The M2 and N2 are included in the model's tidal forcing, but the M2-N2 modulation effect is much smaller than the signal from the spring-neap cycle and was therefore not discussed in the paper.

p.15 l. 8 - 'does not significantly affect' - has this been statistically tested? If not, please rephrase.

Sentence is reworded as to read:

“In Fig. 8b the vertical viscosity ν_{ver} within the bottom layer is largely the same in the non-tidal and tidal case, suggesting a weak effect of tides on ν_{ver} .”

Anonymous Referee #2

This is a very nicely written modelling paper providing new insight on the distribution of dense water exported from Storfjorden, investigating the influence of tidal dispersion of the Storfjorden overflow plume. I enjoyed reviewing it. The paper is skilfully structured, the text is concise and figures are clear, descriptive and illustrative of the results. The methodology is presented in sufficient detail. The authors demonstrate good knowledge of the literature and put their results into context using several other previous studies. This will be a valuable contribution to Ocean Science. I recommend publication subject to moderate revisions suggested below.

The main message of the paper builds on the fact that the horizontal diffusivity coefficient according to Smagorinsky scheme is correct and representative of the physics in the vicinity of the headland south of Spitsbergen. This is probably true, but needs a brief discussion on the associated caveats etc. to convince the readers (like myself) mainly dealing with observations.

We clarify the underlying physics and applications of the Smagorinsky scheme in section 2.2 (“Model numerics”) and refer to the relevant literature for a more complete evaluation of the scheme and a test of its efficiency.

Tracer concentration, tides or no tides, shows SFOW on the shallow shelves west of Spitsbergen. I am not aware of any observations supporting this. It appears that the bottom layers of the entire shelf in the southern half of west Spitsbergen is SFOW. This deserves a discussion. Probably this means $TRC_{1+2+3} < 0.05$ or 0.1 is rather “noise” than SFOW?

All branches of the plume are diluted to same extent. Low tracer concentrations are not noise, but do imply that observed temperature and salinity would correspond fairly closely with ambient shelf values and are not characteristic of the plume “core”.

We have amended the revised ms in Section 4.2 (“Two layer structure and bifurcation”) to clarify this.

Tidal excursion distance is important in quantifying whether tide-induced shear dispersion is important in a system. This is not discussed. What is the tidal excursion around the headland? Your model horizontal resolution is 3 km; does this adequately resolve the tidal excursion?

The tidal excursion distance for the M2 tide is 5.4km and for the K1 tide it is 7.4km for the surface currents. They are now given in section 3.2 (“Comparison with observations – tides”).

In our model, tide-induced shear dispersion is parameterised by the lateral diffusivity rather than being represented by explicit resolution of tidal excursion. This is now clarified in section 2.2 (“Model numerics”). The model’s horizontal grid therefore does not need to resolve the tidal excursion distance.

According to the “shear dispersion” section of Geyer & Signell, longitudinal dispersion coefficient, K , in open channel flow with log velocity profile scales as Uh (U is the flow away from the bottom boundary layer and h is water depth). For the overflow plume, an analogy can apply; K scales with the volume flux per unit width of the plume. This is of course variable across the plume section (small at the edges of the plume, large at the core etc.). Is this important for the dynamics? How does plume Uh compare with tidal Uh ?

If K due to tides scales as Uh (where U tidal flow and h depth), Uh is the tidal volume flux. Yes, the tidal velocities increase on shallows, but we cannot claim so for Uh .

This argument only applies for the shoaling. But even for uniform depth, tidal currents are stronger around headlands.

Geyer & Signell (1992) assume a logarithmic velocity profile for the estimation of the shear dispersion coefficient K . The Smagorinsky scheme in our model is less idealised and uses the actual velocity field (without assuming a log profile) to calculate horizontal dispersion. The actual velocity used by the Smagorinsky

scheme is a combination of tidal, plume and all other velocities at a specific time and place.

Fig 2: I don't see the point with this figure. Tidal elevation will vary significantly within the domain and this time series near the southern boundary is probably not representative for the site of interest (the hatched box or section in Fig 7). It would be more interesting to show a map of contours of tidal excursion over a spring-neap cycle and another map of tidal velocity maximum over a spring-neap cycle.

We agree. This figure is replaced by a new figure which validates the modelled tides against observation (see section 3.2). The tidal excursion is not the relevant process for plume propagation.

P702, li16-24: A thicker plume does not mean increased mass flux or volume transport. Please clarify.

We agree. This is now clarified in Section 4.3 of the revised ms and reworded as "Increased plume thickness (as shown by tracer concentration) is closely matched by a greater cross-sectional area between sea bed and the isopycnals (overlaid in Fig. 5) to indicate greater potential for downslope transport."

P703 li 14-19 & Fig7a. It would be nice to show a section similar to Fig6 but for the cross-section analyzed, shown in Fig 7a. I am curious whether the two-branched structure you sketch in Fig 10 appears in this average section. It will also clarify the "main" cascade and the remainder.

We agree and add 2 more panels to Fig. 7 (Fig. 6 in the revised ms) showing a cross-section of tracer concentration along the same transect where the transports were calculated. The deeper slope branch and the shallower shelf branches are now visible. Discussion of the new figures is added to section 3.4 in the revised ms.

P703 li 20-25 & Fig7b. (Looking at the no wind cases) It appears like the relative difference between no tide and tide is amplified for moderate and high salinity overflow compared to the weak overflow. Is this because the weak overflow propagates in shallower water?

Yes, that would also be our interpretation of the results in Fig 7b (Fig 6b in revised ms).

P706, li7: You haven't really showed plume thickness and volume transport in the early stage of the overflow (in Storfjordrenna), except perhaps the thickness in the cross-section of fig 6. Overall, there are several occurrences of claims that some properties have been shown (but not done so). Please tone down such comments.

The introductory paragraph to section 4.1 is revised to address this comment.

P706, li 18-21: you have not confirmed that tides augment the downslope transport. Lower concentration left in the fjord area does not confirm this. Given that sill depth is about 120 m, your results showing elevated concentrations above 100 m on the Spitsbergen shelf suggest, on the contrary, augmented upslope transport.

We agree this statement was not clear and it is removed in the revised ms.

P706, Discussion around DEk. I guess this depends on how you define the plume thickness. In Fig6 a-b the core of the plume (warm colors) leaning on the slope are of similar thickness, supporting this scaling. Ekman dynamics cannot be expected to account for what's going on in the diluted waters more than 100 m above the bottom.

We agree. The origin of increased plume thickness is now clarified in the discussion section 4.1.

P708, li 9: This two branch structure is not presented from the model data (before Fig 10). Perhaps the authors are referring to Fig5b? The structure is not so clear. Perhaps the suggested cross section figure above would help.

We agree. We have added new panels (c) and (d) to Fig. 7 (Fig. 6 in revised ms) following the earlier suggestion. The accompanying text in section 3.4 now clarifies this point.

Minor comments:

P692, li 22-23: the latitudes are in degrees & minutes (not seconds)

Corrected as advised.

P694, li5: tidally-induced shear dispersion: the reader would benefit from a brief description here in the introduction (although you describe how it works later in discussion).

We agree. The model does not resolve but parameterises shear dispersion. Relevant parts of the text are modified accordingly (section 1 “Introduction and section 2.2 “Model numerics”).

Eq1: what is D_{hor} ? (not clear from the text). If it is horizontal diffusion coefficient, what is κ_{hor} in fig 9a?

D_{hor} is the horizontal diffusion term in the model. This is now stated explicitly.

P698, li8: I suggest to remove Skogseth et al. 2009 reference which is an event based supercooling observation of high salinity, not representative of the context here.

Amended as advised.

P700, li22: ambient conditions are not described (except that we know they come from a global simulation)

In the revised ms we now clarify (in section 2.3) that the initial conditions and the boundary forcing for the ambient conditions were taken from the time period from August 1985 to August 1986 of the global NEMO 1/12° model run.

P701, li28: somewhere around here please indicate your threshold of tracer concentration for detection plume thickness. Is it 0.01 as you mention above eq 2?

Yes, a note on the threshold value ($C_{TRC} = 0.01$) is added.

References:

Pawlowicz, R., Beardsley, B., and Lentz, S.: Classical tidal harmonic analysis including error estimates in MATLAB using T TIDE, Computers & Geosciences, 28, 929–937, doi:10.1016/S0098-3004(02)00013-4, 2002.

Anonymous Referee #3

This paper describes simulations of the overflow of dense water from Storfjorden which have been designed to examine the impact of tidal effects. The simulations, performed with and without tidal forcing, are idealized enough to isolate the tidal effects, yet realistic enough to be relevant to the real ocean. The numerical experiments are well designed, the analysis is clear and focused, and the results are original. I therefore recommend publication, following some minor revision largely for clarification.

1. Comparison with the Antarctic scenario (Anslope).

Much of the discussion is devoted to examining the difference between the results seen here (where the inclusion of tides leads to less overflow water reaching the deep ocean) and the results seen in the Antarctic Ross Sea (Anslope) region (where the tides enhance the off-shelf transport). The authors suggest that this discrepancy is caused by the presence of the headland topography in the Svalbad region, leading to tidally-generated lateral dispersion, whereas in the Antarctic, topographic variability on the shelf was less important. I wonder if this is really the main cause of the differences seen in these two cases? Firstly, in the Antarctic case, unlike the present case, without tides it is very difficult for dense water to move off the shelf (probably again because of the lack of topographic variation). Here, the Storfjordrenna provides a mechanism for steering some of the dense water off the shelf, even in the absence of tides. So the difference in the behavior without tides in the two regions will influence how tides impact the overflow.

Thank you for pointing this out. We have significantly rewritten the discussion section 4.1 to address this issue.

Secondly, while in both this study region and in the Antarctic, the tides lead to greater mixing, the impact of that mixing on the fate of the dense water will depend in part on the ambient stratification, and ambient currents. What is the density of overflow water after passing around the headland region of increased mixing, with and without tides, relative to the ambient water? (i.e. What would the section of figure 6 look like, if taken further around the peninsula?) Can the difference with the Antarctic also be understood in terms of the

consequences of the tidal mixing for the density structure relative to the ambient stratification?

Yes this is our interpretation as well. Further downstream from the location of Fig. 6 (Fig.5 in the revised ms) the density of the plume is reduced in tidal runs. We attribute this to tidal effects on the shelf (mainly lateral diffusion and subsequent widening of the plume causing a more diffuse plume under tidal conditions). This difference to the Antarctic setting is now clarified in discussion section 4.1.

2. Equation 1 not clear

Make it clear that D is operating on tracer T , that T is any of heat, salt, passive tracer. Also, the $\text{div.}p$ notation is confusing here, since p has not been defined. The sentence "The horizontal diffusion is represented by two factors" is not quite correct – the horizontal diffusion depends on those two factors, rather than is represented by them.

The "p" was a typesetting mistake. This has been corrected and the other suggestions have also been implemented in the revised ms.

3. Section 2.4, Atmospheric and tidal forcing.

Can you clarify - does the atmospheric forcing correspond to the "normal year", or a particular time period?

The model is run for a particular time period from August 1985 to August 1986 to coincide with the deep cascading event described in Quadfasel (1988). This is now clarified in sections 2.3 ("Boundary and initial conditions") and 2.4 ("Atmospheric and tidal forcing").

4. κ_{hor} differences in figure 8.

Since the main differences in the plume behavior with and without tides are seen in the lighter, upper part of the plume, I suggest it would be more relevant to compare κ_{hor} in that part of the plume, which may not coincide with the bottom layer. Or perhaps you might average κ_{hor} over the whole plume thickness (as defined by the tracer), in addition to showing the bottom values.

We agree. Fig. 8 (Fig. 7 in the revised ms) is extended as suggested. It shows the horizontal diffusivity coefficients for the upper layer (51-100mab) in addition to the plots for the bottom layer (0-50mab).

5. Section 4.1 The "AnSlope" hypothesis.

Rather than referring to the "AnSlope" hypothesis, I would prefer to see this hypothesis spelled out in physical terms. Perhaps "tidal-augmentation of downslope dense water transport"?

We agree. The section 4.1 has been rewritten to take account of this suggestion.

6. Discussion in section 4.1, p707 in my copy

I found some of this discussion a bit confusing. Can you quantify how much shear dispersion applies to your scenario (i.e. through the lateral spreading of the tracer)? The discussion of unresolved processes did not seem very relevant to me - in your simulations, it is the velocity gradients, rather than transient eddies or turbulence, which are generating the higher κ_{hor} values.

We agree. The discussion (and section 2.2 "Model numerics") now clarifies that despite resolving velocity gradients we do not resolve the excursion of the tracer to explicitly simulate shear dispersion, which is instead parameterised by the Smagorinsky scheme.

7. Interaction with coastal current, last paragraph of section 4.1

In the absence of tides, isn't part of the reason for the dense plume not being mixed into the coastal current due to the difference in density?

We agree and clarification is now given in the discussion (last paragraph of section 4.1).

The plume dives under the current mostly because of its greater density, rather than because it is "narrower and thinner".

We agree and have corrected the relevant sentence.

Anonymous Referee #4

This manuscript describes the numerical simulations of Storfjorden overflow with and without tidal forcing. The authors described that tidal simulations induce horizontal mixing in the gravity current especially around the Sorkapp headland. Although the manuscript is interesting, I do not recommend the manuscript for publication as of now. It needs significant changes and additional simulations. My concerns are the following;

1) This is a sigma layer model with 3km resolution and they use 50 sigma layers. In terrain following models generally a horizontal smoothing is applied to make sure that Haney number is less than 0.4. The authors performed this smoothing but there is also additional criteria Beckmann & Haidvogel which requires that the ratio between horizontal and vertical resolution shouldn't be high. That's the reason why most of the sigma grid models use only 25-30 sigma layers in their simulation. The authors should describe what the effect of this high vertical grid spacing in spurious mixing is. They should run an additional experiment with 30 sigma layers and compare the results.

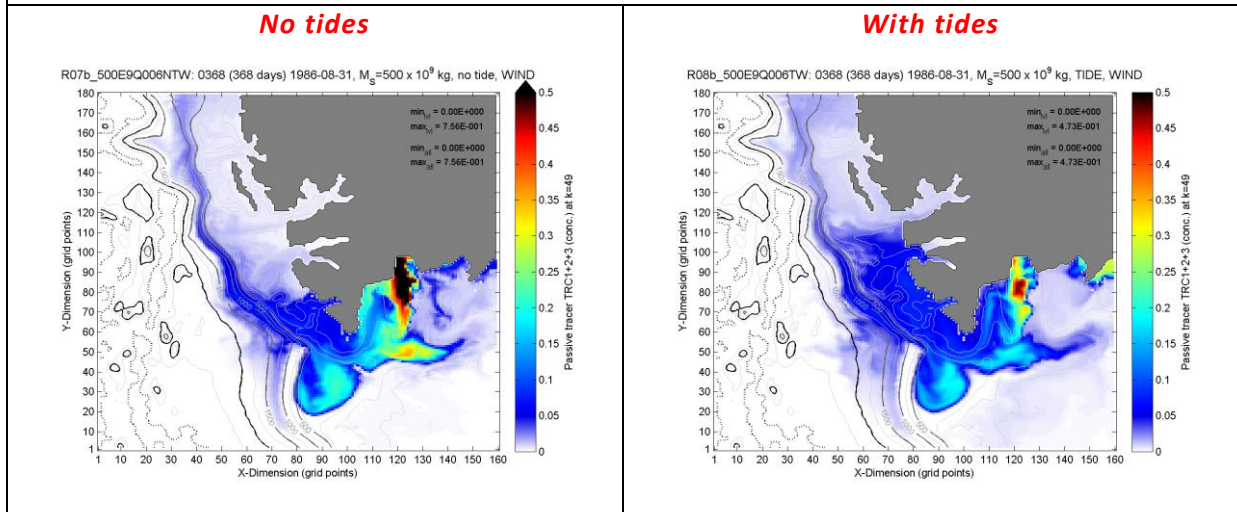
The increase in model resolution is a natural trend driven by the growing availability of high performance computing resources. The models of the 60s had 4 vertical layers; current operational models have between 30 and 60. There is nothing wrong to have a good resolution as long as the model is stable, which is obviously the case here.

Nevertheless we were curious about the reviewer's suggestion and repeated 2 runs (MEDIUM scenario, with wind, with & without tides) in a model configuration using 30 s-levels instead of the original 50. The new runs with 30 levels specify "only" 10 levels over the first 120m metres above the bottom instead of 16 as in the original runs. Below we show plots similar to Fig.5 (new Fig.6 in revised ms). The colour shading shows the tracer concentration in the bottom level (k=49 in original runs, and k=29 in new runs).

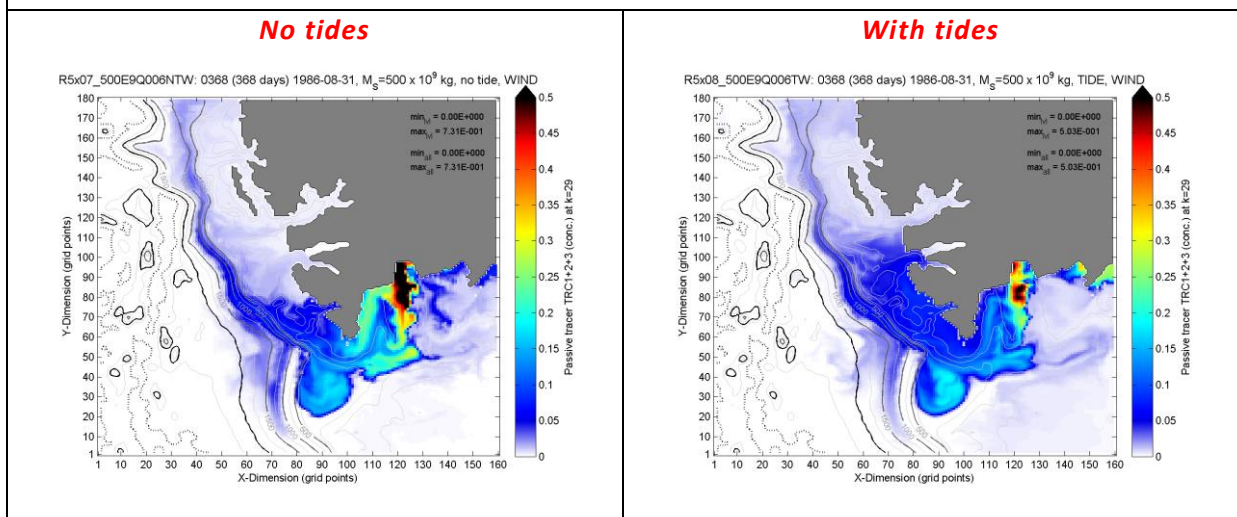
The plots are identical in all of the first order characteristics that are discussed in the paper (e.g. greater concentration of plume tracer on the shelf in the tidal case).

We therefore prove that our original decision of using 50 vertical levels did not result in the instability of the model.

50 s-levels (original runs as presented in the paper)



30 s-levels (new runs)



The notion about '50 layers are too high a resolution' is inspired by concern of violation of the 'hydrostatic consistency' in non-Z-level coordinates when so called 'Haney parameter' $R > 1$.

The non-Z-level vertical grids are prone to errors in calculation of the pressure gradient force (see e.g. Rousseau and Pham, 1971). The error is partly caused by violating the condition for 'hydrostatic consistency', which for a sigma-grid is often written in the form

$$|R| < 1; \quad R = \frac{\sigma \cdot \delta_x H}{H \cdot \delta \sigma}$$

where σ is the sigma coordinate of a numeric cell, H is its depth below sea surface, $\delta_x H$ is the change in depth of horizontally adjacent grid cells, and $\delta \sigma$ is the vertical grid size in sigma coordinates. On a sloping topography this condition is severely restrictive. Mellor et al (1994) have shown that when using fine vertical resolution, a numerical scheme can be hydrostatically consistent even when $R > 1$ (and of course > 0.4) as the 'inconsistency error' is proportional to

$$Err \sim (\delta \sigma)^2 (1 - R^2)$$

and the error can decrease with increasing vertical resolution. This estimate is specific to the advective scheme used and the best test is to try and compare with observations or (in an idealised setting) with analytical solution.

Ezer and Mellor (2004) compared sigma- and z-level grid simulations in a highly idealised dense water flow down a sloping bottom. Mellor et al (1994) demonstrated, in an idealised setting, that the model is stable and the spurious currents decrease with time even when $R=13$, if the vertical resolution is sufficiently fine. The modelling of a near- bottom density current over a very steep (39°) slope using the s-coordinate grid (a derivative of sigma) was stable and in a good agreement with laboratory experiments at $R>100$ subject to having 4-10 s-levels within the thin bottom plume (Wobus et al., 2011). The 'safe' value of R also depends on the actual topography and the numerical scheme (Shchepetkin and McWilliams, 2008).

Here we use the same (complex) vertical grid as in Wobus et al. (2013), which proved to be stable. The effect of hydrostatic consistency is discussed in detail in the recent paper by Shapiro et al. (2013).

References:

Rousseau, D. and Pham, H. L.: Premiers resultats d'un modele de prevision numerique a courte echeance sur l'Europe, La Meteorologie, 20, 1–12, 1971.

Mellor, G. L, Oey, L.-Y., and Ezer, T.: Sigma coordinates pressure gradient errors and the seamount problem, J. Atm. Oc. Tech., 15, 1122–1131, 1994.

Ezer, T. and Mellor, G. L.: A generalized coordinate ocean model and comparison of the bottom boundary layer dynamics in terrain following and z level grids, Ocean Modell., 6, 379–403, 2004.

Shchepetkin, A. F. and McWilliams, J. C.: Computational kernel algorithms for fine-scale, multiprocess, longtime oceanic simulations, edited by: Ciarlet, P. G., Elsevier, Handbook of Numerical Analysis, 14, 2008, 121–183, doi:10.1016/S1570-8659(08)01202-0, 2008.

Wobus, F., G.I. Shapiro, M.A.M. Maqueda, J.M. Huthnance: Numerical simulations of dense water cascading on a steep slope, Journal of Marine Research 69, 391–415, doi:10.1357/002224011798765268, 2011.

Wobus, F. , G.I. Shapiro, J.M. Huthnance, M.A.M. Maqueda (2013) The piercing of the Atlantic Layer by an Arctic shelf water cascade in an idealised study inspired by the Storfjorden overflow in Svalbard, Ocean Modelling, in press, doi:10.1016/j.ocemod.2013.03.003

Shapiro, G., Luneva, M., Pickering, J., and Storkey, D.: The effect of various vertical discretization schemes and horizontal diffusion parameterization on the

performance of a 3-D ocean model: the Black Sea case study, Ocean Sci., 9, 377-390, doi:10.5194/os-9-377-2013, 2013.

2) In fact, the authors should mention spurious mixing in a different subsection. In the overflow cases, numerical diapycnal diffusion is very important and it should be handled properly. They should describe if they can measure the numerical mixing or not. They use k -epsilon vertical mixing parameterization followed by Warner et al. However they should cite Ilicak et al. (2008) paper which describes performance of difference two-equation turbulence closures in Red Sea overflow case.

References; Ilicak, M., T.M. Özgökmen, H. Peters, H.Z. Baumert and M. Iskandarani, 2008: Performance of two-equation turbulence closures in three-dimensional simulations of the Red Sea overflow. *Ocean Modelling*, 24, 122-139.

The suggested reference is added to the text in section 2.2 (“Model numerics”).

3) In the model description, they say “South of the sill the flow forms eddies and fills the depressions in the Storfjordrenna”. The model has 3km horizontal resolution, what is the Rossby radius of deformation in that latitude? How can they resolve these mesoscale or submesoscale eddies? I cannot see any evidence from the figures, there might be some filaments but not eddies. They should describe eddies in details if they resolve them.

The Rossby radius in the area of interest, the north-eastern Nordic Seas and Fram Strait is ca. 10-15 km, as estimated from moorings and drifters (Andresson et al., 2011; Beszczynska-Möller et al., 2011), and decreases to ~5 km in the fjords of Svalbard (Skarðhamar & Svendsen, 2010). Chelton et al. (2011) asserted that scale of the observed eddies is typically 2-3 larger than the Rossby radius. Therefore, with the 3-km horizontal resolution, obviously not all eddies are resolved in the model, but most of them are. Hence, the model is actually eddy-resolving/eddy-permitting, as in fact all the models available for the area. There are computational and physical (hydrostatic assumption) limitations to have a fully eddy-resolving model (< 1 km resolution). The configuration presents state of the art in the field.

References:

Andersson, M., K. A. Orvik, J. H. LaCasce, I. Koszalka, and C. Mauritzen (2011), Variability of the Norwegian Atlantic Current and associated eddy field from surface drifters, J. Geophys. Res., 116, C08032, doi:10.1029/2011JC007078

Beszczynska-Möller, A., R.A. Woodgate, C. Lee, H. Melling, and M. Karcher. 2011. A synthesis of exchanges through the main oceanic gateways to the Arctic Ocean. Oceanography 24(3):82–99, doi:10.5670/oceanog.2011.59

Chelton, Dudley B., Michael G. Schlax, and Roger M. Samelson (2011), Global observations of nonlinear mesoscale eddies. Progress in Oceanography 91(2), 167-216, doi:10.1016/j.pocean.2011.01.002

Skarðhamar, J., and H. Svendsen. "Short-term hydrographic variability in a stratified Arctic fjord." Geological Society, London, Special Publications 344.1 (2010): 51-60

4) In Figure 2, they should also plot the southern boundary model ssh, so we can compare the sea levels between the tidal forcing and model results.

We agree. Figure 2 has been replaced in the revised manuscript. The new section 3.2 evaluates the accuracy of the tidal forcing in comparison with observations.

5) In Figs. 3 and 4, the discrepancy between model results and observations are very significant. The authors seem very happy with the results but I would strongly recommend them to adjust their wording.

We agree and we now clarify the reasons for the discrepancies and discuss them in more detail in the revised ms. Fig. 4 was removed to avoid confusion.

6) In Fig. 5, showing the last sigma layer tracer concentration might be misleading. The last sigma layer is very thin. They should plot the vertical averaged or sum of total passive tracer concentration in the gravity current.

As the tracer tracks the spread of the densest waters, the highest concentration can safely be expected in the last sigma-layer. The fact that this layer is thin is irrelevant as this is a numerical, not a real, layer.

The vertically averaged concentration of the height of the water column could be misleading as it will depend on the water depth.

7) The authors only showed the difference of horizontal spreading between tides and no-tides simulations. However, the most important thing in overflows is the diapycnic mixing. They should describe and show that how much amount of vertical mixing is changed between simulations.

We agree that diapycnal mixing is an important factor in dense water plume propagation. Fig 9b (now Fig 8b in the revised ms) shows the vertical viscosity coefficients in tidal and non-tidal scenarios and reveals little difference between the tidal and non-tidal case. The vertical diffusivity shows the same behaviour due to a (near) constant Prandtl number. This is now clarified in section 3.5.

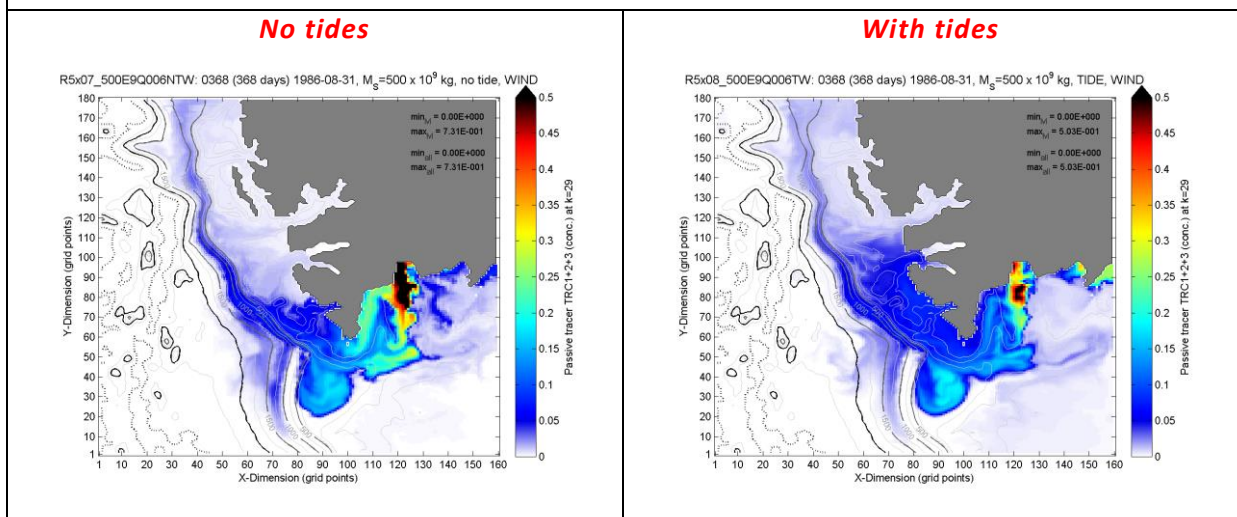
8) I read their previous paper where they implemented no-slip boundary conditions. That flow was much smaller scale than the current study. In a 3km horizontal resolution model, using a no-slip boundary condition, just because you have higher vertical resolution, is insane. The authors claim that they resolve bottom boundary layer! They should perform an additional experiment with drag coefficient parameterizations and compare the results. Otherwise this manuscript should not be accepted.

We do not agree that the use of a no-slip condition is inadequate. In any real flow the velocity tends to zero at the bottom, and bottom drag is produced by the viscous stress associated (mostly) with the associated Ekman layer. The Ekman transport which depends on the difference between stress at the bottom (exactly) and stress above the Ekman layer is represented if the Ekman layer is resolved. Using a no-slip bottom boundary condition, as we now do, implies the explicit representation of the bottom Ekman layer. The model's s_h -coordinate system resolves 16 layers adjacent to the topography and near the bottom the layers are 1m – 7.5m thick (e.g. 1m in 100m depth, 4m in 200m depth, 6m in 300m depth,

7.5m in 2500m depth). In the depth range of interest (i.e. on the shelf between 0 and 300m depth) the Ekman depth is typically 20–30m so that the bottom Ekman layer is resolved.

As suggested by the reviewer, we repeated tidal/non-tidal simulation of the 30-level configuration with quadratic bottom drag instead of a no-slip bottom boundary condition. Below are plots showing the final state of the model at the end of August (compare with Fig. 4 in the paper). The difference is minor.

30 s-levels (no-slip condition)



30 s-levels (quadratic drag)

