1 General Comments

In the following sections we respond to all comments raised by the reviewers. Reviewers' comments are reproduced in italic type followed by our response in roman type. We thank the reviewers for their constructive comments which helped to improve the manuscript substantially. Major changes were made in the "Methods" and "Errors and uncertainty" sections. During the revision process, we detected several errors in our methods. Because these are not covered in response to the reviewers' comments, in this section we briefly summarize these corrections and improvements.

1. There was an error in our energy flux calculations. As described in the original "Methods" section, the calculation of baroclinic fluxes relies on full-depth profiles of velocity and density. Our sampling covers a fraction of the water column and, as commonly done, we rely on fits to vertical modal shapes to recover full depth profiles. The baroclinic pressure anomaly (used in the baroclinic energy flux calculation) includes a cumulative depth integral term. In the original version, the pressure anomaly was calculated using Eq(4) of the original manuscript, from the (incomplete) cumulative depth integral of the measured vertical isopycnal displacement, ζ. (The syntax of Eq(4) was not clear and is now clarified; new Eq(6).) A modal fit was then made to the (erroneous) pressure anomaly profile. For profiles covering 50-70% of the water column, the error is substantial. In the revised version we instead calculate the measured buoyancy perturbation profile, and obtain full-depth profiles of the buoyancy perturbation using fits to the normal vertical modes. The baroclinic pressure anomaly is then calculated using the cumulative depth integral of the buoyancy perturbation profile; hence the pressure anomaly is correctly calculated over full depth.

2. Apart from the the error described above, the original calculations followed the methods outlined in Kunze et al. (2002) and Nash et al. (2005) that does not correct for the pressure perturbations induced by the surface tide. Relatively recent work (Kelly et al., 2010) shows that the decomposition of the baroclinic pressure perturbation calculated in this way can lead to errors of order 10 $\mathrm{W\,m^{-1}}$ in depth-integrated baroclinic energy fluxes when surface displacement (due to barotropic tides) is large. In our revised calculations of the baroclinic energy fluxes, we account for isopycnal heaving by movement of the free surface following Kelly et al. (2010). Details can be found in the revised manuscript.

The "Methods" section was slightly reorganized to accommodate these corrections. The initial steps of the baroclinic perturbation calculation and semidiurnal fits are applied to the velocity and buoyancy fields (not pressure). This description is followed by a section on vertical modes and modal fits.

Pressure perturbation calculation is now described in the "Energy and energy flux calculation" section. We further simplified and clarified the notation: for example, removed "reduced pressure" and used "pressure" throughout; removed "phase averaging" $\langle\rangle_\phi$ and used "time averaging" $\langle\rangle_t$ throughout; used z' in the integrand when integral limit was 0 to z, simplified units by removing kW, cph etc.

2 Reply to Reviewer 1

The topic of the manuscript is of great interest: quantifying sources of heat, which potentially affect the Arctic ice is of great importance. Materials presented in the manuscript are generally sufficient to justify conclusions made by the authors. Therefore I am in support for publishing the manuscript. There are, however, several issues resolving which may help the authors improve the manuscript:
We thank the reviewer for the constructive comments. We addressed the issues raised by the reviewer as described below.

\- *The authors need to provide solid evidence that their estimates of tidal parameters and their derivatives are robust. A statement like that on page 2257 that the authors expect unbiased estimates with error less than 50%... is just not enough. There are multiple sources of errors, which can potentially affect the authors conclusions. These sources should be explored, evaluated and quantified. For example, one of the biggest issues with the analysis is lack of long enough time series. How robust are tidal parameters derived from daily time series? What is impact of nonlinear interactions, which leak tidal energy from tidal frequencies up and down the spectra forming essentially diurnal and semidiurnal bands? How about near-inertial motions contaminating the tidal signal, which are barely mentioned in the text? It is essential that estimates derived from observational records are complemented by error bars. One can use, for example, bootstrap techniques in order to explore potential effects associated with short records and impacts of various external forces.*
Agreed. We now identified and quantified different sources of errors. We completely revised our Section 3.3: "Errors and Uncertainty" (now called: "Errors, uncertainty and caveats"). We also refer to the thorough study of Nash et al. (2005) which is a detailed technical study of uncertainty and bias estimates in baroclinic energy flux calculations for a variety of oceanographic sampling schemes. Using data-based Monte Carlo simulations, they assess

magnitude and parameter dependence of flux estimates made from temporally or vertically imperfect data. Many (but not all) of the issues raised by the reviewer are addressed in that study, with general conclusions.

Furthermore, we now estimate errors from identified sources such as harmonic analysis, imperfect vertical sampling, and projection onto flat-bottom normal modes. Please see the revised Section 3.3 for details of the calculations. Overall, we assign errors of 50 % to HKE and 100 % to APE and depth integrated baroclinic energy fluxes **F**. We provide errorbars wherever possible. We also discuss the possible contamination of the semidiurnal band by wind induced near-inertial waves. Data were collected in calm summer conditions (see Fer et al. (2010) for wind data during the cruise). Conditions were also calm for one month prior to the cruise. During the period 25 June - 2 August 2007, spanning the cruise period and 4 weeks prior to the cruise, average ERA-INTERIM winds in a 100-km radius region centered at our Station 2 was 5 $\mathrm{m\,s^{-1}}$, with no values exceeding 12 $\mathrm{m\,s^{-1}}$. Inertial motions in the upper mixed layer are typically excited by energetic and variable wind events. Of the inertial energy in the mixed layer, only a small fraction can propagate deeper into the water column as near-inertial waves (see for example Furuichi et al. (2008); Alford et al. (2012)). Given the lack of wind forcing, we do not expect significant contribution from wind-induced near-inertial waves. More detailed discussion and quantification are given in the manuscript.

Throughout the text, all conclusions should be placed in the context of uncertainties, which may come from limitations of the authors analyses. I would also recommend adding a section in Conclusions which may be called Limitations and confidence boundaries of the study where the authors would describe problems and limitations of their data, methods and conclusions.
Agreed. In the revised version we emphasize the limitations of our study, particularly in "Conclusions". We have a dedicated section on "Errors and uncertainty and caveats". We also include errors in the revised Table 2. A sub-section in the conclusions as the reviewer suggests is not added. We keep our Conclusions rather general and figures, when given, are based on regional volume integrals. Integrated dissipation is given as lower and upper bounds (covering a factor of two range). We tone down the estimate of average heat flux, and revise our main conclusion as "Although our regional calculations are crude, they underscore that the dissipation of baroclinic tidal energy can be a significant contributor to turbulent mixing and cooling of the Atlantic layer north of Svalbard.".

Comparison of modeling results and observations is not thorough enough;

after reading the manuscript it is difficult to evaluate the model performance based on presented comparisons with observations. This comparison should provide quantitative estimate of the simulation. Also, these two parts of the analysis should be glued together in a much better synergistic way; now they go almost separately. Based on these considerations, I recommend publication of the manuscript after major revision.

A thorough evaluation of model - observation comparison and model performance is not intended in this paper. The STORMTIDE model has been tested and evaluated elsewhere. We clarified this in Section 3.3, when discussing the limitation of the model results by amending:

"Previous evaluations against observations from satellite altimetry (Müller et al., 2012), tide gauges (Müller et al., 2014), and a detailed comparison of the generation of internal tides with observations, theory and inverse models (Müller, 2013) give confidence that the model simulated internal tides inherit some degree of realism: The internal wave generation and propagation of internal tides are reasonably well represented; near generation sites in the deep ocean, the magnitude of the surface signature of internal tides is reasonably well reproduced; the magnitudes and the regional characteristics of barotropic to baroclinic tidal conversion rates compare well with those in other models, and observational and theoretical studies."

Based on the previous evaluations, we argue that the magnitudes and the regional characteristics of barotropic to baroclinic tidal conversion rates can be used for the domain integrated analysis presented in our study. Given the present observational data set, conversion rates cannot be quantified or compared with the model. Furthermore the station coverage is poor in a highly variable region and the errorbars are large. The variability in the depth-integrated baroclinic energy fluxes inferred within 10-km of the observation stations is large (standard deviation is comparable to the mean value, see Table 4). A direct comparison of baroclinic energy fluxes and model-observation evaluation is not meaningful. Also note that we did not set-up a specific model for the Yermak Plateau region, but utilize tidal model data from earlier global runs, results of which are stored in a publicly available data bank. We only have 4 constituents for tidal velocities. This is because of limitations for the post-processing procedures from a global model, with 40 vertical levels and 2394x3600 grid points. Raw model output is not stored due to storage limitations, and thus we cannot redo the analysis.

We retain the structure whereby we present the observed variability at the stations, followed by regional energetics inferred from the model. Our approach is 1) to use (limited) observations to document the substantial tidal variability and levels of turbulence mixing, 2) use the tidal model results to infer integrated, regional energetics. This approach is further clarified in the end of the "Introduction".

Comments: 1. Intro: Writing is rough; this section is not well structured. Some sentences stand out from the text flow.
In the revised version we improved the Introduction (not detailed here). Improvement includes, in addition to including relevant literature and description related to trapped waves, restructuring and clarification.

2. Notations through the text should be unified (for example, now d and H are used for depth, notation for velocities is not consistent).
Done. For instance, we use H throughout, and in general we use a consistent notation.

3. P. 2252. How were rho with overbar and u bold with overbar obtained? On page 2253 there is definition of u_{bc}, if this is the same quantity, I suggest to define it where if first appears.
Variables marked by tilde (overbar the reviewer is referring to) are obtained using harmonic analysis. They are purely lunar semidiurnal sinusoidal oscillations (for the model data, other constituents are also obtained). u_{bc} is the total baroclinic velocity (before harmonic analysis). In the harmonic analysis we use the baroclinic perturbation, which is u_{bc} with time-average removed from each depth. In the revised version, for simplicity, we do not introduce u_{bc} anymore and use the baroclinic perturbation, u'. These are now defined properly and clarified in the text. In Section 3, before the detailed subsections, we inserted the following: "... The methods involved are summarized below. The analysis is based on the baroclinic perturbation fields (e.g., horizontal velocity and pressure) at tidal frequency which are obtained using harmonic analysis of the model and observational baroclinic perturbation fields. The baroclinic perturbations are indicated by a prime; purely sinusoidal fluctuations generated using the harmonic analysis results are indicated by a tilde over the corresponding variable...."

4. Eq. 3 looks like a rather crude definition of vertical velocity. Is z (or, maybe, d) negative? How spatial derivative was taken from sparsely spaced observations? Is there any way to quantify errors associated with this estimate (I realize that it maybe tricky)?
This section and Eq.3 are for calculations using the STORMTIDE model data (note the section heading). The spatial derivative is not from sparsely spaced observations, but from the regular model grid. To clarify, and to avoid confusion we added "from the model data" in several places in Section 3.1. The definition Eq(3) follows from Kang and Fringer as cited. They call

this "barotropic vertical velocity", and it is a suitable definition. It is the vertical velocity due to heaving by flow over sloping topography. Given the topography grid and model barotropic velocity, there is no error associated with it. The convention is standard with z positive upward from the mean sea surface, i.e., bottom is $z = -d(x, y)$ where d is total depth (in the revised version we now use H for total depth throughout).

5. P. 2253. Flat bottom approximation is used for derivation of vertical modal structure and just mentioned in the text that it may lead to problems. Please quantify (if possible) effect of this approximation on your estimates.
We now quantify this in Section 3.3. Here, we inserted "The caveats associated with the flat-bottom assumption and the limited time and vertical span of observations are discussed in Section 3.3".
We improved Section 3.3 to include a discussion of the effects of flat bottom assumption, short time series and gaps in the vertical coverage. See also response to the first major comment.

6. P. 2253. Last sentence of the first paragraph. How suitable 15hr long record for harmonic analysis? Please quantify your estimates.
Discussed and quantified in Section 3.3

7. P. 2253, very last sentence. I did not understand this explanation.
This part is no longer in the revised Section 3.2.

8. P. 2254, very last sentence. Is this difference due to short length of the available records? Please provide explanation of the difference between the two tidal bands.
This sentence states the variance explained by the semidiurnal band velocity and vertical displacement. The tidal band is the same. Compared to velocity, a relatively less fraction of the vertical displacement is in the semidiurnal band. In this section we now also include the results for the diurnal band at stations with sufficiently long time series.

9. P. 2257, line 5. How this estimate of 10% error was obtained?
This result is Nash et al. (2005)'s work as cited. We now give more information on the methods of that study.

10. P. 2257, lines 14-15. Expectation of error less than 50% is good but not enough; please provide evidence.
Done; see Section 3.3 and also response to the reviewer's first major comment.

11. P. 2257, lines 15-20: How about S2 and O1 constituents for this area?
From the short time series of observations we cannot differentiate S2 from M2 (or from f), nor O1 from K1. At the station locations, STORMTIDE tidal current amplitudes at Station 2 (the most energetic station for all constituents) are (K1, O1, M2, S2) = (13, 6, 14, 5) cm/s, i.e. semidiurnal and diurnal bands are of comparable magnitude and typically K1 and M2 dominate over O1 and S2. We now inserted this information at the opening paragraph of Section 4. Furthermore the baroclinic S2 and O1 energy fluxes derived from STORMTIDE can be found in Table 4 together with K1 and M2.

12. P. 2258, lines 16-18. This may be also associated with errors in both model and observations.
Unfortunately we made an error in producing the tidal current time series from the STORMTIDE tidal ellipse data (hence in Fig 2b). The phases of the tidal constituents were not converted to degrees (from radians), as required by the t-predic function of the t-tide software. (Fig 2a, surface displacement is correct.) The agreement between the depth-average current and the STORMTIDE output is much improved. See also response to the reviewer's comment 22.

13. P. 2258, very last sentence. What conclusion can we derive from this sentence?
Here we emphasize that the diurnal band variability dominates. Enhanced diurnal currents over the YP are common (cited and discussed in the Discussion). In the discussion we also address topographic trapped waves observed both at the YP and elsewhere. Here we also inserted: "Consistently, depth-integrated baroclinic energy fluxes from STORMTIDE averaged within 10 km of the mooring location (Station 5) are dominated by K_1 (Section 5, Table 4)".

14. P/ 2259, lines 20-27: Does this difference suggest that the errors exceed 50% assumed by the authors?
These figures are no longer correct (due to our error in calculating baroclinic pressure anomaly, hence APE, see our general response point 1). The error estimate is also revised. Furthermore we no longer give results for mode 1 and sum of first 3 modes separately. We only give the sum of first 3 modes with errorbars (which are very large).

15. P. 2260, line 6: What is it expected?

These parts are removed during the revision.

16. P. 2260, line 16: Choice of error does not sound right.
This part is no longer in the revised version. However, in other parts we now use "the estimated error..." . Also, as described in response to various other comments, we now give appropriate error estimates.

17. P. 2260, line 13, Since this flat-bottom approximation is questionable, I would suggest to provide at least a hint to what degree we can trust it in this particular circumstances.
Addressed in Section 3.3.

18. P. 2260, lines 25-29: I do not follow this technique and I am not sure that I understood the method used to plot figure 4. Please elaborate.
We improved this part. In summary here we obtain lower and upper bounds on the full-depth integrated dissipation rate from our limited observed microstructure profiles. Deeper parts of the water column that are not covered by our instrument (or where the measurement noise level is reached) are filled with a low dissipation rate to obtain a lower bound. An upper bound is obtained by extending the deepest measurements to bottom and further increasing it by a factor of 10 near the bottom to account for turbulence and breaking of internal waves over the slope.

19. P. 2262, line 21. Are S2 and O1 comparable to M2 and K1 or just between themselves? Also, I would add observational estimates from Table 2 to Table 4 for direct comparison. Quantitative comparison is needed here.
We clarified this point. We avoid a thorough model/observation quantitative comparison. We explain our reasons both in the manuscript and also above, in reply to the third major comment of the reviewer.

20. P. 2263, lines 3-4. This sentence reads like the authors question the importance of their conclusions derived from observations. It is honest but following this logic I have to ask them how valid are their observation-based conclusions presented in Abstract and Conclusions.
Because of the limitations by the ice edge, observations were only made on the southern part of the YP. These sites (observational stations) are not as energetic as the hot spots or significant conversion sites on the northern flanks. On the other hand, the dissipation rate profiles do cover several regimes (quiescent Station 1, typical Station 2 and turbulent Station 4). By extending the mid-depth dissipation rates to bottom and further including turbulent boundary layers we are confident that we produce synthetic profiles

of upper and lower bound dissipation rate representative of the YP. We clarified this in the text (end of Section 4). For the energy fluxes and conversion rates, however, we rely on the model results to infer region scale integrated energetics. Our abstract and conclusions are now consistent with this.

21. P. 2266, lines 9-10. Is there any way to quantify the effect of this contamination?
Addressed in Section 3.3.

22. Fig 2: Why do panel a use 8 constituents and panel b use only 4? For panel c, please show position of instruments. Panel a: What is impact of the motion of instruments on this record? Difference between model and observations in panel b should be evaluated.
During revision we found an error in Fig2b. The phases of the tidal constituents were not converted to degrees (from radians), as required by the t-predic function of the t-tide software. (Panel a is correct.) The agreement between the depth-average current and the STORMTIDE output is much improved. The evolution of the model tidal current also shows the expected increase in amplitude during spring tides (which was absent in the erroneous version).
Unfortunately, we only have 4 constituents for tidal velocities (hence only 4 constituents are used in panel b). This is because we are using archived model outputs from a global model downloaded from World Data Centre for Climate. Raw model output was not stored due to storage limitations. This is clarified in the revised version (both in the text and in the figure caption).
In new Fig2c, we show the position of the instruments. In total data from 19 instruments were used (Seaguard with CTD at the top, 23 m from surface, and 18 Microcats). All Microcats but two (at 125 and 163 m) were equipped with pressure sensors. Time series of depth of each instrument is inferred from the pressure record (the depth of the two Microcats without P-sensors were interpolated from adjacent sensors). The resulting time-depth structure of temperature and salinity is then gridded on to 20-m regular vertical spacing. This processing accounts for the mooring motion and we do not expect an impact on the analysis. This information is added to the end of Section 2.1

23. Figure 3. I guess u and v are baroclinic components, right? Please say so explicitly. What is the level of errors in these profiles?
Yes, we show the baroclinic velocity components. This is now clarified. (Note that we also show the barotropic estimates- vertical lines- but this was mentioned in the figure caption.) The errors are further quantified in the "'Errors and Uncertainty"' section.

24. Fig. 8: What does negative dissipation mean?
We thank the reviewer for pointing this out. We should not have negative dissipation rates, unless the steady-state depth-integrated balance (conversion - radiation = dissipation) is not valid, or there is an inconsistency in our calculations. We double checked our calculations and we did not find any errors. When averaged over the sufficiently long model output, the steady state approximation should be valid. In order to find an explanation for the reason for the negative dissipation, we plotted details of each term separately. It turns out that the negative dissipation rates (only for the M2 constituent) are associated with few grid points where weak conversion rates of order 10^{-3} W m^{-2} are co-located with very large positive radiation values (in total 10 grid points, all between the 750 and 2250 m isobaths with radiation values between 0.03 and 0.06 W m^{-2}). We did not investigate further the reason for these outliers but simply removed these points from the calculations. This information is included in the revised text.

25. Fig 9 caption: semidiurnal ENERGY flux?
Corrected.

3 Reply to Reviewer 2

We thank the reviewer for the constructive comments. We address the raised issues in the revised version as described below.

The paper describes the analysis of the tidal signal contained in measurements at 5 stations, each lasting about a day, and in short-term mooring. The analysis of a coarse model complements the data.
The Yermak Plateau lies north of the critical latitudes of both M2 and K1 - internal waves at the tidal frequencies cannot propagate freely. This is acknowledged in the paper, but never really discussed or taken into account. The barotropic tides are relatively large, and they do interact with topography. The baroclinic motions created are NOT internal tides, as internal waves at the tidal frequencies are commonly referred to. They are evanescent. I am really not sure that the accepted way to calculate energy flux $< pu >$ works when omega $< f$... The energy does not propagate away and the horizontal group speed is zero (or imaginary) - the wave is trapped by topography. What is the meaning of these quantities, then? $< pu >$ is not Ec_g. The authors do

refer to shelf waves on page 2261, but not much is discussed. The significance of the baroclinic energy fluxes really has to be better addressed, both for the model and for the observations.

We agree with the reviewer. In the previous version we did not emphasize or discuss the trapped nature of the internal tides at this latitude. Our motivation for the study, however, as described in the introduction, comes from the expected local mixing as a result of this trapped baroclinic response. We realize our discussion and the literature coverage related to this topic were not thorough. In the revised version we substantially improved the Introduction and the Discussion, now also introducing the trapped tides and the relevant literature. Some of the relevant revisions made are copied below from the revised manuscript.

It is true that the linear wave at critical latitude has zero group velocity. But once trapped, these waves behave like topographic Rossby waves and may propagate along the slope with the corresponding group velocity (but decay rapidly across slope). We see no problem in using $< pu >$ for calculation of the baroclinic energy fluxes. We can also refer to several other papers that calculate the subinertial baroclinic energy flux using this method, both from models (Tanaka et al., 2010) and observations (Johnston and Rudnick, 2014). Furthermore, particularly for the observations which use the (near-inertial) semidiurnal band, freely propagating vorticity-trapped M2 waves are possible with a reasonable negative background relative vorticity. Finally, the baroclinic energy fluxes for both M2 and K1 shown in Fig 6 are consistent with an alongslope energy propagation for a limited distance from their generation sites. The vectors are typically aligned along the isobaths, and the magnitude of the fluxes decays rapidly across the slope, consistent with topographically trapped waves. These waves are forced (by tides) and damped (by friction). Similar interpretations of the figures and the results are also included in the rest of the revised version, where relevant. We discuss these points in the Discussion. Some of the revisions made to the Discussion are copied below from the revised manuscript.

Some revisions made in the Introduction include: "... The propagation of linear internal tides is possible, however, only equatorward of the critical latitude at which the Coriolis parameter, f, equals the tidal wave frequency, ω. The Coriolis parameter changes with the latitude, Ψ, through $f = 2\Omega \sin \Psi$, where $\Omega = 7.292 \times 10^{-5}$ s^{-1} is the Earth's angular rotation. The critical latitudes for the principal lunar semidiurnal tide M_2 and the diurnal tides K_1 and O_1 are near $74^\circ 30'$ and 30°, respectively. Poleward of the critical latitude the wave equation changes form from hyperbolic to elliptic, and no progressive linear solution is allowed (Vlasenko et al., 2005).

The solution is evanescent, exists locally where the barotropic to baroclinic energy conversion occurs, and decays exponentially in the vertical and in the cross slope direction. Baroclinic disturbances in response to tidal flow over topography above the critical latitude are thus topographically trapped near the generation site (a continental slope, a ridge, or a seamount). The energy propagation of topographically trapped waves is possible along the slope, around the topographic feature with negligible radiation away in the cross slope direction. This is analogous to the sub-inertial baroclinic trapped waves propagating around isolated seamounts (Brink, 1989). Trapped tides dissipate their energy locally, or elsewhere along the topography, leading to substantial vertical mixing (Padman and Dillon, 1991; Tanaka et al., 2010; Johnston and Rudnick, 2014).

.... A recent numerical study in the Barents Sea show internal M_2 tides with bottom-enhanced energy density and dissipation rates, trapped in the vicinity of their generation sites (Kagan and Sofina, 2014). The bottom-trapped tides in the Arctic Ocean are of particular importance because the most of the Arctic basin is located north of critical latitude of the most energetic tidal constituent M_2. Estimates from global numerical models of the conversion rates of barotropic to baroclinic tidal energy in the Arctic region are roughly $5\,\mathrm{GW}$ ($1\,\mathrm{GW} \equiv 10^9\,\mathrm{W}$) (Simmons et al., 2004; Müller, 2013). Using modal energy density of trapped tides and an ad hoc decay timescale, Falahat and Nycander (2014) computed about $1.1\,\mathrm{GW}$ of topography-trapped internal tides (sum of the K_1, O_1, and M_2 constituents, where about 70% is M_2). ... At the latitudes of the YP, except from the principal solar semidiurnal tide (S_2, with critical latitude near 85°), all diurnal frequencies and the principal M_2 semidiurnal frequency are sub-inertial; locally generated internal tides are trapped. The critical latitude also serves as a "turning latitude" for low mode internal tides generated elsewhere below the critical latitude, and propagating poleward. At the YP, M_2 frequency is only slightly sub-inertial ($f \approx 1.025\omega_{M2}$), and an anticyclonic relative vorticity can shift the effective Coriolis frequency, f_e, relaxing the trapping and the turning latitude. For $f_e < \omega_{M2}$, progressive solutions are allowed; so called near-inertial vortex-trapped internal waves (Kunze and Toole, 1997). As we show in the following, the YP is a site of substantial barotropic to baroclinic energy conversion. It is thus expected that the baroclinic energy extracted from the surface tide cannot propagate away and likely dissipates locally, contributing to turbulent mixing where the internal tide is forced. This is supported by observations in the YP region that show localized energetic turbulence and mixing (Padman and Dillon, 1991). Earlier investigations ..."

Some revisions made in the Discussion include (practically the first half of the Discussion): ".... Is it realistic to have baroclinic energy fluxes and radiation for tides above their corresponding critical latitudes? Analogous to barotropic continental shelf waves, variable bottom topography of ridges, seamounts and plateaus in homogeneous water can support trapped waves (Rhines, 1969; Huthnance, 1974). If, for example, diurnal tidal frequency is close to the natural frequency of one of such free wave modes, then the topographically trapped free wave can be resonantly excited by the oscillation of the diurnal tide. Using arbitrary stratification, Brink (1989) showed that sub-inertial baroclinic trapped waves are also supported at isolated seamounts. Wang and Mooers (1976) show that in a continuously stratified ocean with sloping bottom, topographic Rossby waves are the only form of sub-inertial wave motion (for a negligible coastal wall), and reduce to barotropic shelf waves and to bottom-trapped waves in the limits of small and large stratification, respectively. The energy of the topographically trapped waves propagates along the slope, around the topographic feature with a decay scale of Rossby radius of deformation, and with negligible radiation in the cross slope direction. Sub-inertial internal wave energy and energy fluxes have been observed and modelled elsewhere (Allen and Thomson, 1993; Tanaka et al., 2010; Johnston and Rudnick, 2014; Robertson, 2001; Kunze and Toole, 1997). The internal Rossby radius, c_1/f, for the first mode eigenspeed obtained from the modal analysis of our observational data, varies between 3 and 5 km. The bottom slope between the 1000 and 2000 m isobaths along the northwestern slope, representative of the generation sites, is 0.06 (±0.03). The cross-slope distance covered between the 1000 and 2000 m isobaths is thus 17 (±8) km. If the trapped wave generated between these isobaths decays with the Rossby radius, a decay to 5% of the background value occurs between 9 to 15 km. Hence the substantial inferred radiation near the isobaths where conversion occurs is plausible.

The bottom-trapped tides in the Arctic Ocean are of particular importance because the most of the Arctic basin is located north of critical latitude of the most energetic tidal constituent M_2 (Falahat and Nycander, 2014). Using global numerical models, baroclinic tidal energy (including poleward of the critical latitudes) are reported by Niwa and Hibiya (2011) for the major diurnal constituents and by Müller (2013) for the major diurnal and semidiurnal constituents. In an idealized study, Falahat and Nycander (2014) examine the bottom-trapped internal tides for the M_2, K_1, and O_1 tidal constituents over the global ocean. To infer the energy conversion rates from the barotropic tides to bottom-trapped internal tides, they calculate the energy density for linear inviscid waves and assume that the trapped wave energy, for all vertical modes, decays over a 3-day time scale. This ad hoc time scale is representative of Fieberling Seamount which is a strongly forced and damped system with large dissipation and eddy diffusivity (Kunze and Toole, 1997). In the Arctic Ocean, Falahat and Nycander (2014) infer about 1.1 GW of topography-trapped internal tides (sum of the M_2, K_1, and O_1 constituents, where about 70% is due to M_2). The diurnal component, of about 0.3 GW, is one order of magnitude less than the diurnal internal tides in the Arctic Ocean reported in Müller (2013), hence should be considered as a lower bound.

At high latitudes, diurnal tides with frequencies close to the half of the inertial frequency are a likely source to force resonant trapped waves (Huthnance, 1974). In the Rockall Bank, the diurnal tidal frequencies are close to the resonance frequency of a natural trapped mode progressing clockwise around the Bank, leading to strongly excited diurnal currents consistent with observations (Huthnance, 1974). A similar excitation of diurnal currents was also shown around the Bear Island near the M_2 critical latitude (Huthnance, 1975). Yermak Plateau is an area known to have resonantly enhanced diurnal tides, particularly over the northern flanks (Hunkins, 1986; Chapman, 1989; Padman et al., 1992). Using a barotropic model with an idealized axisymmetric submarine plateau both Hunkins (1986), and Chapman (1989) showed near-resonant diurnal trapped topographic waves propagating around the YP. While Hunkins (1986) looked at the free waves in a frictionless ocean, Chapman (1989) included friction and forcing by rectilinear K_1 tidal currents (i.e., forced and damped trapped waves). The topography of the YP, however, is not axisymmetric (Fig. 1). Padman et al. (1992) showed that dispersion relations derived separately on the northwestern and eastern flanks of the Plateau suggest free diurnal waves on the northwest slope (near the CEAREX-O site) but approximately zero group velocity on the eastern slope. This is inconsistent with the axisymmetric model and with a resonant interaction mechanism related to the path length of a free wave that encircles the entire plateau. They proposed an alternative generation where diurnal energy is due to topographic diurnal waves on the eastern part where the group velocity is near zero, allowing maximum amplification. Although these studies address the barotropic diurnal currents in neutral stratification (for simplicity), they are applicable to baroclinic solutions (Brink, 1989). For example, Tanaka et al. (2010) uses the baroclinic coastal trapped wave solutions to explain the sub-inertial diurnal baroclinic tidal energy propagating around the Kuril Islands. On the continental slope of the Laptev Sea, Pnyushkov and Polyakov (2012) report that the baroclinic solutions of the topographically trapped waves show no significant change of the cross-slope structure of tidal current and sea level amplitudes compared with the barotropic experiment.

Sub-inertial internal wave energy and energy fluxes have been observed and modelled elsewhere. Over the Juan de Fuca Ridge, trapped (laterally and vertically) baroclinic subinertial motions were reported (Allen and Thomson, 1993). In a numerical study, Tanaka et al. (2010) show that most of the internal wave energy subtracted from the diurnal barotropic tide is dissipated within the Kuril straits. K_1 tidal frequency is sub-inertial in this area and the tidal energy is fed into topographically trapped waves which propagate along slope around each island with negligible radiation away from the straits. Energy subtracted from the K_1 barotropic tide is approximately 30 GW; most of the energy dissipates locally, only 0.6 GW radiating out from the analyzed domain. The local conversion and dissipation balance is similar to that we find near the YP. Johnston and Rudnick (2014) observe topographically trapped diurnal internal waves along the California continental slope and over the Santa Rosa Cortes Ridge in the Southern California Bight. The diurnal (sub-inertial) internal tides are more energetic than the semidiurnal internal tides and are associated with elevated diffusivities near topography. Using current measurements in the upper 200 m in about 2700 m deep water on the continental slope of the Laptev Sea, Pnyushkov and Polyakov (2012) infer baroclinic tide in the upper 50 m, twice as energetic as the barotropic tidal currents. Numerical solutions of trapped waves over the continental shelf and slope suggest resonance and enhancement of semidiurnal energy consistent with the observations. Poleward of the critical latitudes near-inertial internal waves have also been observed. For a review of the near-inertial internal tides in the Weddell Sea in Antarctica see Robertson (2001). Over Fieberling Seamount near 32oN Kunze and Toole (1997) report vortex-trapped near-inertial diurnal internal waves. The dissipation rates are strong enough to dissipate the K_1 motions within 3 days, implying a strongly forced and damped environment. At the YP, M_2 is the strongest semidiurnal component . D'Asaro and Morison (1992) report that tides here are an attractive source for enhanced near-inertial band energy where sub-inertial M_2 tide generated on the seamount is trapped to the seamount by the barotropic vorticity field.

At the YP region, the trapped diurnal tides are likely generated by resonant forcing of diurnal tides through the processes described in Chapman (1989) and Padman et al. (1992). The trapped semidiurnal tides are possibly generated locally. Internal waves are generated over critical slopes where the ratio $\gamma = \beta/\alpha$ is unity. Here β is the topographic slope and $\alpha = (\omega^2 - f^2)^{1/2}(N^2 - \omega^2)^{-1/2}$ is the characteristic along which linear internal waves with frequency ω propagate (i.e., the horizontal slope of the internal wave ray). Although the critical condition ($\gamma = 1$) is optimal, internal wave generation at supercritical slopes ($\gamma > 1$) is also common. The critical condition also leads to enhanced shear and turbulence (Eriksen, 1985). As the turning latitude where $\omega = f$ is approached, however, the non-linear terms become increasingly important and the parameter γ becomes a crude indicator of the potential for internal wave generation (Vlasenko et al., 2003). Poleward of the turning latitude negative background vorticities can effectively reduce the inertial frequency and thereby potentially allow for linear sub-inertial internal waves (Kunze and Toole, 1997). .... An entirely different generation mechanism is possible when non-linearity is strong, leading to unsteady lee waves at relatively horizontal length scales (see e.g., Vlasenko et al., 2003). The STORMTIDE results, however, do not include the effect of such non-linear internal wave generation.

In that spirit, results from the paper are generally compared with lower latitudes - Hawaii, Luzon, etc. In these location, internal tides are free waves and can propagate way. One of the major results of the paper, according to the abstract, is (almost???) all the barotropic-baroclinic conversion is dissipated locally. Of course! I find it surprising (suspicious?) that calculations show any energy flux at all! I dont understand that.
Yes, admittedly our comparison with other studies were mostly from lower latitude sites where energetic, freely propagating internal tides were observed. In the revised version we remove such references and refer to several recent studies that quantify trapped internal tide energetics. The relevant revisions can be found in the response to the first comment. As described above, baroclinic energy flux is possible. We agree that there is no surprise in that subinertial barotropic-baroclinic energy conversion dissipates locally. But to our knowledge noone has yet quantified the rate of conversion of surface tide energy to baroclinic energy, the spatial distribution of the baroclinic energy fluxes and the energy dissipation rates in the Yermak Plateau region. The regional importance of local dissipation of this energy was also not reported previously.

p2249 L10: Please rephrase, this sentence is either wrong (the Arctic doesnt have tidal velocities of 5-10 cm/s everywhere), or misleading. Why would the results not be relevant for a place where tidal velocities are 4 cm/s? What are the results? That energy is trapped and has to dissipate locally? That would be true for any velocities.
We simply removed this sentence.

p2250 L25: model implicitly resolves... What does that mean? Mesoscale and internal waves are on very different time and spatial scales, it seems strange to lump them together.
The sentence has been modified as follows: "The STORMTIDE model is formulated on a global tripolar grid with an average horizontal resolution

of about 10 km, which becomes as small as 5 km in high latitudes. Thus, the meso-scale ocean circulation is implicitly resolved. There are 40 vertical z-levels and the time step of the numerical scheme is 600 s, sufficient to simulate the low-mode internal tides."

Section 3.1 - This has to include some mention of the fact that the dispersion relation doesnt hold here... omega < f, so horizontal wavenumber is imaginary (evanescent). What does that mean for the energy flux? What is the divergence of that quantity? Im confused.
We now include these points and discuss accordingly. See also response to the first comment. At this methods section we inserted:
"where $\tilde{p}$ is the perturbation pressure associated with tidal motions and $\tilde{\vec{u}}$ is the baroclinic tidal current vector, both obtained using harmonic analysis of the model output. At the latitudes of the YP, $\omega_{M2} < f$ and the long wave solution is evanescent (the horizontal wavenumber is imaginary and the group speed, c_g, is zero). The common approach of relating $\tilde{p}\tilde{\vec{u}}$ to the baroclinic energy flux, Ec_g where E is the energy density, should be interpreted with caution (see Section 3.3 for discussion). We further derive..."

In Section 3.3 we included:
"At the latitudes of the YP diurnal and lunar semidiurnal internal waves are evanescent with zero group velocity, and our calculations (for both the observations and the model data) using the common approach of $E\vec{c_g} = \tilde{p}\tilde{\vec{u}}$ should be interpreted with caution. The water column is not a solid boundary, and forced internal waves in the water column will leak their energy from the generation site to within a limited vertical and lateral extent. Trapped sub-inertial baroclinic motions, however, are also possible and may propagate along the topographic contours (several examples are cited in Section 6). Our results thus represent the baroclinic fluxes in a given frequency band, induced by pressure and velocity perturbations associated with tidal response over topography, with possible leaking internal waves and trapped waves. "

p2259 L5: But the internal tide can propagate away in these places! The Yermak Plateau is different! p2260 L6: There is no radiation! Baroclinic motions at the tidal frequencies would be trapped waves, not free waves.
Agreed. We removed these references to the Hawaiian Ridge and the Mendocino Escarpment at this point. For comparison of our observed semidiurnal baroclinic current amplitudes with other relevant (trapped waves) literature we inserted: " ...The amplitudes are comparable to those from a numerical modelling study of the Barents Sea that show less than 5 $\mathrm{cm\,s^{-1}}$ semidiurnal baroclinic velocity at intermediate depths (Kagan and Sofina, 2014). For comparison, trapped baroclinic diurnal current amplitudes off the Southern

California Bight (Johnston and Rudnick, 2014) are typically 2-3 times more energetic than the semidiurnal current amplitudes at our YP stations. . ...”

p2260 L25: Dont you expect epsilon to grow as you approach the bottom? So, ε_{max} is an under estimate, when you dont reach the bottom (most cases). It is not an upper-bound.
Agreed. We revised our method to include a 100-m thick turbulent bottom boundary layer where dissipation rate increases by a factor of 10 toward the seabed. See Section 4 for details. Figure 4 has changed. Depth-integrated max dissipation rates of Table 3 have changed. The domain integrated dissipation upper bound increased to 1100 MW (from 1030MW).

p2261 L8: a typical poleward flux. Typical of what? Barotropic tides are large (planetary) waves that can propagate in any direction - it depends on the basin shape. Also, the semidiurnal fluxes look just as divergent as the diurnal ones...
Agreed. We simply removed this sentence.

p2261 L16: Shelf waves that propagate freely? Dont they need a shelf? Again, the values that are used to compare the results are all south of critical latitudes.
This part is completely revised (see response to comment 1).

p2262 L12: negative conversion... indicates interactions between locally and remotely generated baroclinic tides: But these waves cannot have come from somewhere else - they cant propagate! How do you explain negative conversion rates?
The wave can propagate but only along the topographical structures, in our case the plateau. Thus, the explanation is still possible. Negative conversion rates are seen in all models, and it is beyond the scope of our study to cover this.

p2263 L23: What do you do about places where the conversion rate is negative? Why is dissipation always positive, yet C has both signs?
Conversion has both signs as explained in response to the previous comment (and in the manuscript). Negative conversion rates are typical of all numerical studies of baroclinic tide energetics. It is not an artifact of our model or analysis methods. We emphasized this point in the revised version. Dissipation is simply conversion less the radiation (see Section 3.1). In dissipation calculations we use all data points of conversion rate and radiation (divergence of F), without doing anything specific. Apparently change of sign in

C is typically accounted for opposite change of sign for radiation.

p2266 L6-7: You mean the semidiurnal HKE + APE, right?
Right. Clarified.

p2266 L10-20: I find this paragraph confusing - what is the point here?
Removed.

p2267 L12: within the range inferred from observations... You are being generous. There are only 5 points in a very variable field. The extrapolation to the bottom is sketchy, and the 10% fraction being hotspots seems very arbitrary. From these observations, you could really get any answer you want...
It is true that we have a very limited data set in a very variable field. We tone down our discussion. 10% fraction being "hotspots" is not arbitrary, and the calculation is detailed in the p2263 lines 23-26 of the original version. After the reviewer's comment we also improved the integration to bottom of the measured dissipation rate. We are confident that produced minimum and maximum bounds on the dissipation rate which should be representative of the YP.

p2268 L2: So, if you said that it happened over a slighty thicker layer (300m), you would get half the heat flux, right? Hmmm.
The relation is not linear but of course the resulting heat flux will be different. The temperature gradient and the stratification averaged over a thicker layer will be different. 300 m is not slightly thicker than 150 m (it is twice as thick). We justify our choice of the layer between 100 and 250 m (hence 150 m thick). This is where the domain-averaged annual mean profile linearly increases down to the core of the AW at 250 m (below that, the temperature decreases).

p2269 L1: I note that you dont need a critical slope to generate internal tides, or trapped waves.
True. And we mention this also in the first lines of the discussion of the original version. Reading "....Although the critical condition ($\gamma = 1$) is optimal, internal wave generation at supercritical slopes ($\gamma > 1$) is also common....". The trapped diurnal tide generation is likely through resonant response at the YP to diurnal tidal forcing. In the discussion, we clarify these candidate mechanisms: "... At the YP region, the trapped diurnal tides are likely generated by resonant forcing of diurnal tides through the processes described in Chapman (1989) and Padman et al. (1992). The trapped semidiurnal tides are possibly generated locally. Internal waves are generated over crit-

ical slopes .....Although the critical condition ($\gamma = 1$) is optimal, internal wave generation at supercritical slopes ($\gamma > 1$) is also common......"

We now also include in discussion ".... An entirely different generation mechanism is possible when non-linearity is strong, leading to unsteady lee waves at relatively horizontal length scales (see e.g., Vlasenko et al., 2003). The STORMTIDE results, however, do not include the effect of such non-linear internal wave generation."

In the Conclusions we repeat the candidate mechanisms of trapped tide generation: "At the YP region, the trapped diurnal tides are likely generated by resonant forcing of diurnal tides through the processes described in Chapman (1989) and Padman et al. (1992). The trapped semidiurnal tides are possibly generated locally over near critical slopes. When a plausible negative background relative vorticity is allowed, we find the bottom topography in these regions critical to the semidiurnal frequency...."

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