**Ocean Science
Discussions**

Interactive comment on "Propagation and dissipation of internal tides in the Oslofjord" *by* A. Staalstrøm et al.

A. Staalstrøm et al.

ans@niva.no

article

AUTHOR REPLIES

by Andre Staalstrom, Eyvind Aas and Bengt Liljebladh

We would like to thank the referees for their constructive comments. Some of the comments are about the applied methods and the representativity of the data sets. Here we have now added more details in order to explain better our ways of reasoning.

Other suggestions are related to improvements of grammar and syntax, and we have followed all of these.

Below we have listed our replies to the different comments. After the General Comments the sequence of comments has been organized to follow the sequence of the manuscript. Only those comments that contain direct questions and suggestions requiring a reply from the authors have been quoted with full text in italics. Suggestions for grammar and syntax are listed with page and line numbers only.

GENERAL COMMENTS

Referee #1: This manuscript describes observations of tidally driven flow in the Oslofjord, largely focusing on 3 different locations, one near the main sill, one further inside the fjord and one outside the fjord. From these measurements, the authors attempt to deduce the phase speed, modal decomposition, and energy budget for the baroclinic tide, and finally estimate the mixing efficiency. The results are interesting and creative, but I have some questions about some of the assumptions and approximations, and therefore recommend revision before final publication.

Referee #2: This paper documents measurements meant to constrain the energy balance in Oslofjord, a well-studied fjord in Norway. The authors estimate the energy in internal motions, infer dissipation of those motions from drops in energy fluxes, and from that also infer the mixing rate in the fjord. I think this is a good start to this paper, but I have a large number of concerns, some of which may be presentation, and also of the conclusions drawn from the data. Hopefully clarifying these will improve the paper.

Referee #3: This manuscript describes internal tide observations in Oslofjord, estimates energy fluxes, and infers dissipation rates and mixing efficiencies. It is an

ambitious task given the dataset available but the authors have made a good attempt at calculating some robust values. However, there are some assumptions that need to be clarified and methods refined before the paper is suitable for final publication.

All three referees ask for a revision of the manuscript and clarifications of assumptions and methods. We have now rewritten parts of the text, added more information about assumptions and methods, and recalculated some of the values. More specific details are explained below.

Referee #2: I found quite a few of the analyses far too cursory to follow, while others went into too much detail.

We are not sure which analyses that contain too many details, but we have noticed that the referee is sceptical to the inclusion of Sections 3.2 and 3.3 in the manuscript. We have replied to these comments below.

Referee #2: To summarize, all the right sort of calculations are being attempted in this paper, and could make a nice contribution, but they have all been half completed, and not terribly believable as presented.

We do not agree that our calculations are only half completed, but we understand that in order to make the results more convincing more information about assumptions and methods may be needed. Hopefully we have been able to add the necessary information.

ABSTRACT

Referee #1: Line 4, line 11

Suggestions followed.

Referee #1: Line 5-6: This sentence (with specific values of amplitude ratios and error estimates) seems very specific for the abstract - I suggest instead focusing on the implications of these numbers for the physical conclusions.

This sentence as well as the whole abstract has been rewritten to be more general.

Referee #1: Line 13: "only a fraction" - a fraction of what? (I think of the total baroclinic energy lost in the basin) - clarify.

The text refers to a fraction of the flux in the denominator of the ratio defining the mixing efficiency R_f (Eq. 22). As stated above, the abstract has now been rewritten.

1 INTRODUCTION

Referee #1: The introduction reads too much like a list of references with little indication why these references are actually relevant to the current study. I suggest the authors rewrite it completely, focusing on the story of the physics they wish to outline, using the references as supporting evidence for the physical phenomena/understanding to date.
Referee #2: To start, the paper would benefit from an improved introduction. As noted, Oslofjord is well-studied, so what is not known, and what new will be learned from this study? The introduction is written as a literature review (explicitly so on Page 318, lines 1-9) and does not make a strong case to me for reading the rest of the paper.

Our references were included because they are relevant to physical processes of fjords, especially internal waves and vertical diffusion, but we see that the direct relevance to the present study may not be obvious in all cases. Accordingly we have followed the referees' suggestion and rewritten this section.

Referee #1: Also, the reader would be helped greatly by referring to a map of the region in the introduction, identifying locations such as the Drobak Sill, rather than

waiting until section 2.
We have added the reference.

Referee #2: I also think a clearer discussion of energy sources and sinks in the fjord would have improved this discussion, and much of the rest of the paper. I think all the info is in there, but it is scattered, and could use being made cohesive and clear. (i.e. energy comes in from the barotropic tide, which loses x% of energy to bottom friction, and y% to internal wave generation. Of the internal waves, a fraction radiates from the fjord, the rest breaks internally.) I think laying this out clearly would have helped one thing that irked me - not mentioning the barotropic loss until section 5.1. Wouldn't it make more sense to start with this number?
We agree that an introduction along these lines would be more interesting, and also that the barotropic energy flux and energy loss should be presented at the earliest convenience. Unfortunately we do not have all the mentioned numbers, but the section has been rewritten, and we have introduced the information that we have.

Referee #1: P316, line 22: "Without this reduction there would be no renewals of the deep water." - more explanation needed.
We thought it was obvious that a density reduction within the old deep water was necessary in order for new water being able to sink down and replace the older water. However, we have now added this explanation to the text. A figure, redrawn from Gade (1967), has been added to the end of this reply.

Referee #3: P317, line 3, line 8:
Suggestion followed.

2 AREA OF INVESTIGATION; FIELD MEASUREMENTS AND DATA BASES

2.1 THE OSLOFJORD

Referee #1: P319, line 2: indicate the inner and outer parts of the fjord on the map. Line 7: Indicate the Drobak jetty and westefrn inlet on map.
Our manuscript explains on page 319 what is meant by the inner and outer parts of the fjord, and the same text as well as the legend to Fig. 1b explains the locations of the eastern and western inlets and the subsurface Drøbak Jetty. Unfortunately the reduction in size from our submitted manuscript to the version presented by Ocean Science has made the figure almost unreadable. We hope that the size can be increased, but as for now we are reluctant to add more text and details to the figure. We have to discuss this with the technical editor.

2.2 RECORDINGS AND DATA BASES

Referee #2: I could not determine from any of the information where the many CTDs and temperature sensors were deployed.
We have now added a new table containing the number of sensors at the different stations.

Referee #3, P321, line 7, line 17, line 19:
Suggestions followed.

3 PROPAGATION OF INTERNAL TIDES

3.1 PHASE SPEEDS

Referee #1: P323, line 7: Please indicate (a) why you describe "displacement at 20m" - aren't you finding the displacement of a density surface? What density? How do you make your choice of density surface?

The referee is right; we are describing displacements of density surfaces located close to 20 m depth. This depth was chosen because it corresponds to the depth of the Drobak Sill (Fig. 1c), and also because it usually coincides with the transition layer (Fig. 3), where internal waves may be assumed to exist. The density surfaces chosen for Fig. 5a are those integer values of the density that are found closest to 20 m. We have now added this explanation to the text.

Referee #2: P323, line14: I don't think finding the strongest correlation between two stations at near the mode-1 phase speed at all implies there is no mode-2. Also, you need to be careful what depth you are considering - the mode-1 maximum in displacement is at a null in the mode-2 displacement for most stratifications. Given your stratification I'd expect to the mode-1 crossing is near 20 m, where you did this analysis. (BTW, it would help a lot if you plotted the mode shapes for the first 2 or 3 modes). I wondered why you didn't simply make a modal fit of the velocity perturbations, and estimate the energy density in each to argue the importance of mode-1 over mode-2. The circuitous method you used isn't very satisfying.

We have changed Fig. 3 showing the density profiles. Only profiles at station S2 and S0 from one date are shown, but the first and second mode structures for these profiles are presented. The revised version of Fig.3 is attached. We have fitted the mode structure to the vertical displacements observed at station S2, and found that the displacement at 20 m depth can be explained entirely by a mode-1 wave. At 40m depth the mode-2 must be included in order to explain the variations. The correlation

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of the vertical displacements between station S2 and S5 at 20 m depth is used to estimate the phase speed, which is close to the result for mode-1. The potential energy density of the first mode is very similar to the original results. The text has now been rewritten to clarify this.

Referee #2: P324, line 23: "This indicates that the internal tide propagates as a first-mode progressive wave"; this seems a key argument, since you want to argue later that there are no mode-1 reflections, but you haven't done a good job of making it. A fit to Stigebrandt's model is pretty suspect without a lot more details of how you applied that model. Unfortunately none are given, just a sketchy description of the process. If this is important to your argument, you need to provide more detail. How does the forcing vary with time? How important is the stratification assumptions in getting the agreement? As you'll see below, I'm pretty dubious the wave is purely progressive, so I think the extra effort is warranted. You also use this model in Sect 3.2, where more details are revealed, so why not carefully describe the model?

Our key argument is that there is a practically linear relationship between the barotropic and baroclinic tides. The ratio between the amplitudes agrees fairly well with the value predicted by the simple two-layer model of Stigebrandt. Also the found internal phase speeds close to 20 m depth coincide with the results for a progressive mode-1 wave, in agreement with Stigebrandt's model. We agree that this does not prove that the internal wave is purely progressive and that there are no mode-1 reflections, only that our analysis supports the assumption that the dominant part of the wave is progressive. We have accordingly modified the text in the manuscript.

Referee #1: P324, line 2: Clarify which location these phase speeds are estimated for.

Two profiles from two different dates at station S0 have been used, providing similar results. This information is now added.

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Referee #1: P324, line 6:
Suggestion followed.

Referee #1: P324, line 10: "orbital currents" - please explain what you mean by this - do you mean "baroclinic currents"?
Referee #1: P324, eqn 4: Here is a definition of the "orbital current" - put this earlier, to define "orbital current" when the term is first used.
The referee's comment shows that the term "orbital current" is not good, and that the term "baroclinic current" should rather be used. We have also added that u' is a horizontal speed.

Referee #1: Also, what do you mean by "in the same direction as the internal wave" - do you mean in the same direction as the internal wave PROPAGATION? (i.e. the horizontal direction aligned with the propagation direction?)
The referee's interpretation is correct, and we have changed the text.

Referee #1: P325, line 15-16: "energy is transferred from the first mode to higher modes" - however, examination of the phase shift in figure 6 does not indicate multiple zero crossings that one might associate with higher modes.
The mode-1 vertical displacement at station S0 has a maximum at 50-60 m depth. We observe a zero-crossing of the baroclinic velocity in this depth, indicating a mode-1 wave. But the water depth at S0 is 200 m, and we do not know if there are more zero-crossings below 90 m. We have included a sentence about these considerations in the manuscript.

3.2 AMPLITUDES

Referee #2: I don't understand the merit in comparing local surface elevations to internal elevations. I guess you need to motivate it better, perhaps re-summarizing Stigebrandt's theory. Given that the surface tide is likely almost completely a standing wave, and you are arguing the internal tide is progressive, I don't see that the internal displacements and the surface will have any simple relationship.
The referee's doubts are correct, but we are not comparing local surface elevations to internal elevations, we are comparing the amplitudes of these elevations! The merit in comparing the amplitudes is that the linear relationship indicated by Fig. 8 demonstrates that we are able to predict with some accuracy the amplitude of the internal elevation from the amplitude of the surface elevation. This is what we expected and not a sensational result, but it is still a key result in our investigation because it confirms the relationship between barotropic and baroclinic tides, and it quantifies the ratio between the energies of the barotropic and baroclinic wave. We agree that a more detailed description of Stigebrandt's model may be useful, and accordingly we have now added some more results from this model to the section.

Referee #1: P326, line 14, line 24-25: Similarly,... This correction needs to be made numerous places in the text.
The corrections have been made.

Referee #1: P327, eqn 7: I have trouble seeing how eqn 7 follows from eqn 6.
Equation (7) does not follow from Equation (6) without any other information, but our manuscript says on page 327 that the amplitude ratio of Equation (6), based on Stigebrandt's two-layer model, produces the expression of Equation (7). We have added a brief description of Stigebrandt's solution to clarify this.

Referee #1: How does a ratio of amplitudes depend on the phase of the internal wave relative to the sill?
It is the phase speed of the internal wave and not the phase that enters Equation (7). The baroclinic current speed will be proportional to this phase speed c_i and to the amplitude a? of the internal wave, and in Stigebrandt's model a certain baroclinic current speed is required to cancel the barotropic current at the vertical wall representing the sill. The barotropic current speed is of course proportional to the amplitude of the barotropic wave, and thus the two amplitudes become linked together. Our manuscript says on page 327, line 12, that "Stigebrandt (1976) solved the linearized shallow-water equations with no rotation for the two-layer case, while applying a local boundary condition at the sill that cancelled the barotropic current in the lower layer."

Referee #1: Also, what do you mean by Y, the surface area of the fjord inside the sill - do you mean the entire surface area of all the basins?
Yes, and we have now added "entire" before "fjord".

Referee #1: How can the amplitude of the internal wave just near the sill depend on the total surface area of the basins,
Again this is a model result. Equation (7) contains both the surface area Y of the entire inner fjord and the cross-sectional area A* of the channel close to the sill. The net volume transported into the inner fjord will depend on A* and a?, and it has to be equal to the product of Y and the surface elevation.

Referee #1: especially when you show that the internal waves are essentially dissipated before they reach basin 5?
The internal waves are (in Stigebrandt's model) needed at the sill in order to cancel the barotropic current below sill depth. Farther away into the inner fjord the problem of horizontal speeds into a vertical border is solved by the reflection of the barotropic wave at the border, and the internal waves are not needed. The dissipation of the

internal waves is a part of Stigebrandt's model.

3.3 TIDAL FREQUENCIES

Referee #2: not sure why you included this section. I guess its nice to see the harmonics decay at S5 relative to S2, but...
Fig. 9 shows that the harmonic constituents are significantly weaker at S0 and S5 than at S2, and in the section it is pointed out that no signs of an M2 internal wave are found in the innermost basin. We think these results are important because they illustrate how the baroclinic tidal energy varies within the fjord. Fig. 9 also presents another interesting detail. Sea level recordings from the inner Oslofjord exhibit at times, and usually during spring tides, some peculiar humps in the graph. These are caused by the harmonic overtides, as mentioned in Section 2.1. Similar phenomena can be observed in current recordings. We think it is useful to show that the harmonic overtides M4 and M6 are distinctively present in the internal tides as well.

4 ENERGY DENSITY AND ENERGY TRANSPORT IN INTERNAL TIDES

4.1 CALCULATION OF ENERGY DENSITY

Referee #3: P329, Equation 11: This should be Ek not Ep I think
The error has been corrected.

Referee #1: P330, line 1-3: What is the averaging period for the energy density

calculations?

The period of the sliding mean values presented in Fig. 10 is set to 25 hours to cancel out the tidal oscillations. This information is now added to the figure legend.

Referee #2: P330, line 2: I'm also not clear on the physics of measuring things at S5. It's not in the main channel. Is that not possibly a problem?

There is no clear main channel at this east-west cross section in the fjord. The western part is the widest, but during inflow the currents are usually strongest in the eastern part. During outflow the currents in the two parts may be more equal. When we should moor a restricted numbers of rigs, a choice had to be made between the western and eastern parts, and the eastern one was chosen. This certainly represents a problem, and especially for the estimated energy transport and dissipation, as discussed at several places in the manuscript.

Referee #3: P330, Lines 8-10: These PE/KE ratios seem very low. The analysis should be double checked.

We are thankful for this suggestion. During the check an error in the procedure was found, and the PE/KE ratio is now about 0.4 instead of 0.004 at station S0.

4.2 ESTIMATED ENERGY FLUXES

Referee #1: The authors calculate energy fluxes at a single location using a variety of methods, which helps to establish the relative robustness of the results. However, all methods rely on extrapolating from a single point estimate to an integral flux through the fjord cross section by multiplying by the cross sectional area. Although the authors do not give many details about the area used in this calculation, I assume it is the full cross-sectional area of the fjord at this location? This assumes that the flux per

unit area at one location can be extrapolated across the whole fjord, yet there is no evidence that this is the case. For example, the internal tide could have more of a beam-like character in the horizontal, and not widen as the fjord widens. Or as an internal wave beam enters a shallower region the depth integrated flux could remain constant, and then the flux per unit area would be higher in a shallower region than in a deeper region. Can the authors provide some reasoning to back their assumption that the flux at one profile can be extrapolated, and provide some estimate of the possible range of the cross-sectionally averaged flux estimate, given these uncertainties?

We have included a comparison of the mode-1 internal Rossby radius (6-10km) with the width of the eastern channel at stations S2 (500m) and S5 (900m) in the text, and argue that the amplitude of the vertical displacement will not change significantly across the channel. We have estimated the uncertainty of the energy fluxes caused by having only one station in each section to be ± 15%. This is due to the uncertainty of the depth chosen for the calculations of the energy density. A shallow station will overestimate and a deep station will underestimate the energy flux, since the energy density usually is lower deeper down in the water column. This new information is included in chapter 4.2.

Referee #1: P331, line 6-7, P332, line 24:

Suggestions followed.

Referee #1: P332: Give details on the A, cross-sectional area, used in these calculations.

The areas of the applied cross-sections are now presented in a new Table 3, and lines marking the locations of the sections have been added to Fig. 12. The revised version of Fig. 12 is attached.

Referee #2: P332, Eq. 16: My biggest problem with this is the $F = c_g E$ method of calculating energy fluxes. This is well-known to have huge problems: a) if there is any

energy in the inlet not associated with the wave moving at c_g, then this number will be too high. b) if there is any reflections, even of the mode-1 wave, this number will be too high. To fix a) you should bandpass near the tidal frequency and fit mode-1 to your data. To fix b), you should check that $Ep = Ek$, which you did, except Ep was not equal to Ek, it was larger. And at S5 it was smaller. That's a pretty classic sign of a partially standing wave, isn't it? What's more, F calculated from mode-fits to u' and p' gets a far lower number than $c_g E$. Its hard to say without you doing frequency and mode-filtering, but my guess would be your wave is not entirely progressive, and that the $u'p'$ estimates of energy flux are closer to being the correct ones. Besides, you say in Sec 5.1 that there is 250 kW of barotropic energy available, so how could the S2 energy flux be anything near 480 kW?

This comment has been very helpful, and we have recalculated the energy flux using the energy density method. We have split the energy density into parts for the different modes and only used the part for mode-1, but the effect on the resulting energy flux is not great (1-2% reduction). In the original calculations linear interpolation of the energies was applied. If we instead use linear interpolations of the vertical displacements, the calculated potential energy density becomes about 19 % smaller. Still the energy density method produces results that are higher than the numbers obtained by the other method. We agree that the energy density method probably overestimates the energy flux, but we have kept the revised calculations in the manuscript because we think it is a point that the different methods give different results.

Referee #3: P333, line 13: "300 kW" the energy flux must have been horizontally integrated to get this value. What are the integration limits? I assume it is across fjord, but what evidence is there that the mooring location is representative of the whole across fjord section?

We have decided to remove this estimate from the manuscript, since the focus of the paper is on the baroclinic energy loss inside the Drobak Sill.

5 DISSIPATION OF INTERNAL TIDE ENERGY AND VERTICAL DIFFUSIVITY

We have realized that in some parts of the text the term "dissipation" should be substituted by "turbulence production" or "baroclinic energy loss", since not all of the baroclinic energy flux that disappears in an area dissipates. Some of the loss is used to increase the mean potential energy.

5.1 ESTIMATES OF DISSIPATION

Referee #1: This is the weakest part of the paper, in part because of the extrapolation made earlier for the flux calculation, and also due to additional poorly justified assumptions. For example, in equation 18 you propose that the baroclinic energy flux at S3 is equal to twice that at S5 (where you have measurements). This does not seem to be to be a good approximation - firstly, dissipation between S3 and S5 is ignored, and secondly you have not made it clear up to this point that F3 included averaging over only the part of the fjord width to the east of the island - is that the case? Any dissipation estimate would really be between the locations of your measurements (i.e. S2 and S5) not between S2 and S3 as you claim. Hence you cannot state that 40-70% of the energy flux is dissipated within 7km, but rather over 10km (the distance to S5). I suggest confining your budgeting to the regions bounded by the locations where you actually have measurements.

We have made it clearer in the manuscript that the focus is on the eastern part of the inlet, and we have now confined the energy budgeting to the region bounded by stations S2 and S5, as suggested by the referee. Thus the assumption that the

dissipation is zero between S3 and S5 is no longer needed. For clarity this region is now marked with a gray colour in Fig. 12. On the other hand we still have to make an estimate of how much baroclinic energy is transported from the eastern to the western channel after station S3. We have assumed that this energy flux is of the same order of magnitude as the energy flux measured at station S5 based on the cross-sectional areas on either side of the Aspond Island. To test the effect of this assumption on the estimated baroclinic energy loss we have applied a value equal to the flux at S5 $\pm$ 50%.

Referee #3: P334, line 6:
Suggestion followed.

Referee #3: Page 334, line 20: "4000 kW" Where does this value come from?
The estimate is based on the mean difference between low and high water and the surface area of the fjord, and this information is now moved forward to the introduction.

Referee #1: P334, line 23: As far as I can tell (the labels on figure 1 are too small to read, even when the page is blown up to full screen), Aspond Island is between basin 2 and 3, so the energy INTO basin 3, not out of basin 3, goes around this island.
The correct text should read: "The energy flux out of Basin 1 has to go on either side of the Aspond Island (Fig. 1b)." The Aspond Island is marked on Fig. 12.

5.2 VERTICAL DIFFUSIVITY

Referee #2: This suffers from a complete lack of detail into the mixing calculation. The integral of $d\rho/dt$ has to be horribly noisy, and I'm not sure I'd believe it anyway because you aren't constraining the advective fluxes into the fjord. You are trying to

detect changes caused by mixing rates of $10-3\ m2/s$. This is a tiny number. I think it'd be wonderful if you could believably integrate Eq 21, but you've shown absolutely no detail on what is a complicated calculation, so I don't have any confidence you have done this correctly.
Referee #1: To be incorporated into the energy budget, these diffusivity estimates need to be made over the whole basin. At what location are the density profiles used for these estimates? If they are made for a single location for each basin, how can you justify using them in the volume average used in equation 22? Some parts of the basin, e.g. near topography, might have much greater mixing, and very different stratification to other locations.
Eq. (21) is in our opinion a very simple and reliable budget method originally used in the Oslofjord by Gade (1967, 1970). Since these papers are not easily accessible, we agree that more details should have been offered. Eq. (22) contains two major sources of uncertainty: the time derivative $d\rho/dt$ for the whole integrated volume and the vertical gradient $d\rho/dz$ at Y. The instantaneous diffusivity in the open ocean is highly variable, but below sill depths in a fjord basin the temporary fluctuations are smaller. Still the average value of $d\rho/dt$ will be sensitive to the choice of time interval. On shorter time scales advection and vertical convection within a basin may even lead to a positive value of $d\rho/dt$, thus making estimates of the mean diffusivity impossible. The change of potential energy between two instants, however, can be calculated rather accurately. To assess the uncertainty resulting from the choice of time interval, results from Gade (1967, 1970), based on data from 1963-1965, are now compared to our results using data from 2003 and 2009. The resulting estimates of uncertainty are included in Section 5.2. It is our experience that below the sill depths of a basin there are practically no horizontal differences of ρ most of the time, implying that it suffices with one station in the central part of the basin to estimate the time-averaged value of $d\rho/dz$.
In order to avoid the possible problems caused by horizontal advection, we have now restricted the calculations of Kz to the depth interval 90 to 125 m for all basins, and we

still obtain the differences between the different basins that were the purpose of this exercise. We have also compared our results for Kz with the values found by Gade (1967, 1970).

Referee #1: P336, line 5: Please give details of Y(z), the "hypsographic curve for the basin". How is this different from the topographic depth?
The hypsographic curve Y(z) does not represent depths but the integral Y of the horizontal areas of the basin at the depth z. Consequently Y(0) will be the surface area of the entire inner fjord, islands excluded. Since we are using the absolute values of Y(z), we have now just as well termed it "the horizontal area of the basin at depth z".

Referee #3: P337, line 8, line 9, line 11:
Suggestions followed.

Referee #3: P337, line 14: What local processes?
The local process is internal waves losing energy locally. The new sentence reads: "We think that this is because the mixing is a result of internal waves both losing energy locally as well as further into the fjord."

Referee #1: In summary, I find the energy budget calculations, involving flux estimates, dissipation estimates (through divergence of the flux), and mixing efficiency (through diffusivity), to contain several unjustified assumptions. I strongly encourage the authors to be more careful in extrapolating point measurements to basinwide estimates, and to at least give some measure of the uncertainty in doing so, and hence the resultant uncertainty in the dissipation and mixing efficiency estimates.
We refer to the replies presented above in this section. In summary we have tried to estimate the uncertainties when we extrapolating point measurements, and the results of the estimates of baroclinic energy loss is revised.
In our revised text we do not estimate the baroclinic energy loss between S2 and S3

(Basin 1), but between S2 and S5, and we no longer estimate the diffusivity in each basin up to 20 m depth. We still include a mixing efficiency based on the work against buoyancy summed up over the fjord inside the Drobak Sill below 20 m depth. We have included the estimated uncertainties of the energy fluxes and diffusivities.

6 SUMMARY AND FINAL REMARKS

Referee #3: P338, line 6, line 9, P339, line 2, line 4:
Suggestions followed.

Referee #3: P339, Lines 9-10: It should be possible to diagnose standing waves from the observations. See Martini et al. (2007, GRL).
We are thankful for this useful reference. In this paper the energy density method (E*cg) is combined with the perturbation pressure method (p'u') to calculate the group velocity. However, we have interpreted the difference between the two methods as a measure of the inaccuracy of the energy flux estimates, so we do not think we can use this method.

TABLES

As mentioned above, we have included two new tables.

FIGURES

Referee #1: Figure 1: This figure is far too small. I couldn't read much of the text, even when I enlarged the page to full screen.
We agree. As explained earlier in our replies, the figure in its original size can easily be read. We have to discuss this with the technical editor, or make a new Fig. 1b. In order to solve part of the problem we have considered calling Fig.1a-b a new Fig.1, and Fig. 1c a new Fig. 2. The new version of Fig. 1a-b and Fig. 1c is attached.

Referee #1: Also, please mark all the locations mentioned in the text, e.g. the inner and outer fjord, the Aspond island, the jetty etc.
As remarked earlier, the text and figure legend explain the locations, and we are not sure if the addition of more text will improve the figure. However, we will consider what the best solution is.

Referee #1: Figure 7, Figure 8:
Suggestions followed.

Referee #1: Figure 9: Can you show the 99% significance level, or some other way of showing which peaks are significant?
The harmonic analysis provided by the Matlab program t-tide, distinguishes between significant (95%) and not significant values of the peaks in the frequency spectrum. The relevant information is now included in the figure legend.