TTIDE15 R1 Response to Reviewers

Thank you to both reviewers and the editor for their careful read and helpful suggestions for making the paper more clear and precise.

There is only one comment that gave us pause, and that was whether the model was in steady state due to the time series of the energy terms in Figure 5b. Reviewer 2 high be correct that there is some reflection from the boundaries that causes this oscillation in the energetics. Determining if this is the case explicitly would require re-running some diagnostics on the model runs, which we don't think the reviewer is actually asking for. The amount of energy is ~1/6 th of the forcing, and is likely quite diffuse, having bounced through at least one sponge, and likely at least two. It is unlikely that this extraneous term changes any of the qualitative aspects of the paper, and likely only marginally affects the quantitative aspects. We didn't want to look at an earlier tidal cycle to give the model time for high modes to fill out the control volumes. In future runs, we will keep an eye out for this behavior and check if it is due to imperfect sponges on the boundaries.

Reviewer #1: Review of "Reflection of linear internal tides from realistic topography: The Tasman continental slope" by Klymak et al.

In this study the authors consider the reflection of an obliquely incident internal tide by the eastern coastline of Tasmania. The behaviour is complicated by the presence of the Tasman Rise, a relatively shallow area off the southern part of the coastline, and the fact that the coast has a corner at its southern limit. Both of these features prove to be significant.

This is a study of a rather complex situation which, through a set of hydrodynamic model runs with varying topographic elements, does a nice job of teasing apart the various components of the response. In particular, the refraction and further focussing of the incident tide by the Tasman Rise, and the generation of a partially-trapped alongslope component at the southern corner are well described and isolated. So, rather than just confusing the reader, it builds up understanding of the response in a layered, incremental manner. As such, this study will make a very useful contribution to the literature as a clear and cautionary description of real-world complexity in internal tide interaction with coastlines/shelves. It is very much suitable for publication in the Journal of Physical Oceanography. My comments are relatively minor, and I would recommend publication with just minor revisions.

Minor comments:

Lines 23-24 (Abstract): The notion of 'removing energy from the internal tide and reradiating it further north' is perhaps somewhat imprecise over a slope, since modal decomposition doesn't yield dynamically independent baroclinic and barotropic modes there, so these are rather artificial constructs in a slope environment (in a sense, the coupling of barotropic and baroclinic components is the reason for internal tide generation at a slope!). The trapped component itself is certainly neither fully barotropic nor fully baroclinic. It certainly does a job of moving energy along the slope and ultimately radiating some of it in a different location, so the wording is not far off, but it's the sense of energy leaving the internal tide into some other box, being moved, then put back into the internal tide that I don't feel is quite right. Just something for the authors to think about!

Agreed. We were thinking about the deep-water "internal tide", so rephrased "Along-slope inhomogeneity is enhanced by a partially trapped super-inertial slope wave that propagates along the continental slope, locally removing energy from the deep-water internal tide and re-radiating it into the deep water further north."

Line 134, equ (2): Should there be a radial term here 1/sqrt(r) to give a consistent energy flux? As the wave spreads from the point source it is distributed around the circumference of a circle (i.e. over a distance proportional to r). Energy ~ amp^2. So, amp^2*r = const and amp ~ 1/sqrt(r). Rainville et al have this scaling. I realise that this is just a guess at an initial state for the model, so any inexactness will adjust through model dynamics, so it shouldn't affect results.

This was a typo in eqn 2. The code used for the amplitude was : `z = z0[ii]*sqrt(r0[ii]/r)*exp(1j*kr*(r))*exp(1j*phi0[ii])`, which has the 1/sqrt(r). Actually, some work went into making sure the boundary condition, the forcing in the sponge, and the initial conditions all agreed. In the run with no topography there is a bit of reflection from the far sponge, but the simulations are otherwise relatively clean with respect to the forcing.

Changed equation and description to read:

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\begin{equation}
  p'(x,y,t)=\sum_{i=1}^{N}  \frac{a_i}{\sqrt{r_i}} \exp\left(j\left(|
k_t|r_i - \omega t\right)\right)
\end{equation}
where $a_i$ is the amplitude of the i-th source, $r_i=\sqrt{\left(x-
x_i\right)^2+\left(y-y_i\right)^2}$ is the distance to the source,
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A general comment about the superinertial semi-trapped wave component: The form of this response is not described in detail. In particular, it would be interesting to know where this mode lies in the continuum from (barotropic) topographic Rossby wave to (baroclinic) Kelvin wave. It is worth stating this, if even just by saying how the internal Rossby radius relates to the width of the slope (i.e what is the Burger number?), since such a response is not possible in all situations. If stratification is weak or the slope shallow, the trapped waves are towards the topographic Rossby waves limit and do not reach superiniertial frequencies, so the sort of alongslope response described is not possible.

Very minor comments:

Line 12: Maybe say confined horizontally, not in the more usual understanding of an internal tidal beam localised in the water column.

Changed to "confined laterally"

Line 21: Downstream in what sense?

Changed to "one focused along-beam"

Line 201: 'Catalyzed by a start-up transient'. Is this meaning the transient associated with initial model adjustment? This wording ('catalyzed') suggests that the transient is an important part of the dynamics leading to this response, but presumably it is not since the response continues? Wording it this way makes the slope wave sound like a model artefact.

Agreed, its not an artifact. Changed the sentence to read "This coupling is driven by the interaction of the internal tide and the shelf corner at $y=0$, and takes the form of a leaky super-inertial slope wave…" The along-slope inhomogeneity is necessary - an infinite regular slope doesn't see these waves.

Line 204: Do you mean across the slope (i.e. in x?)

Changed to: "The time series of the energy terms integrated in the volume bounded by $x=0$ to $80 \mathrm{km}$ and $y = 0$ to $400 km$ demonstrates that the bar"

Line 239-240: 'slight slope to the continental slope' isn't clear.

Changed: "The only difference between these two cases is the narrow shelf west of $x=0\ \mathrm{km}$ and the continental slope instead of a wall."

Line 337-338: This is the first mention of a mooring array. Perhaps introduce it a bit more, saying that this is a synthetic array to test analysis based on a point mooring in a known 3D context (I presume this is the intent!).

Changed: "The TTIDE field effort deployed a three-point mooring array to quantify the wave field offshore of the continental slope. Determining reflectivity from such a mooring array is significantly complicated by three-dimensionality and along slope variability."

Line 352-353: Why is there a low in the reflectivity at y=280 km? Is the slope subcritical here?

Changed: "The net reflectivity from these boxes ranges from 0.8 to a low of almost zero at $y=280\ \mathrm{km}$ where the slope is less steep"

Line 346: Not sure what is meant by 'lining up'

Changed: "A simple one-dimensional comparison of onslope and offslope fluxes does not yield useful results because the reflection from any given point on the slope radiates cylindrically, so it is necessary to integrate over volumes."

Fig 13: Say that this is a snapshot and that the pattern propagates, rather than a fixed pattern of the conversion.

Note that it is a tidal average, not a snapshot. Changed the caption to read: "Barotropic to baroclinic conversion averaged over the 18-th tidal period for different shelf widths from widest…"

Other very minor typos, spelling, grammar, sentences that require rewording etc: Lines 30, 47, 60, 68 (which model?), 77, 87-90, 139, 202, 255, 440-441.

Thanks for these, I got most of them.

Thank you again for your careful read and helpful comments.

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Reviewer #2: Review of manuscript :
JPO-D-16-0061
Reflection of linear internal tides from realistic topography: The Tasman continental slope
Jody M. Klymak, Samuel Kelly, Jennifer A. MacKinnon, Matthew H. Alford, Robert Pinkel and Jonathan D. Nash,

Reviewer name and affiliation (the reviewer does not wish to be anonymous)
Louis Gostiaux
CNRS Researcher
Laboratoire de Mécanique des Fluides et d'Acoustique
UMR CNRS 5509
École Centrale de Lyon - Université de Lyon

In the manuscript entitled "Reflection of linear internal tides from realistic topography: The Tasman continental slope", Klymak et al. propose a detailed study of the reflection processes of the mode 1 semi-diurnal internal tide on the Tasman continental slope. They made use of combined realistic numerical simulations with analytical description of the incoming tide. The energy budget is carefully done, separating the incident and reflected waves, and using the framework of Kang and Fringer 2012. The authors also realize idealized experiments, in which the shelf and ridges are replaced by simple geometric features, which enables to study separately the effects of diffraction and slope waves trapped on the shelf. The article is extremely pleasant to read, and I will only have minor comments below that may improve the understanding of a first time reader. Most of these comments concern section 2b, forcing description, which can be clarified, although there is no doubt that the forcing is correct.

I do recommend the publication of the present article in JPO with minor corrections.

Abstract :
l15 incident/incident : maybe the denomination "incident response" is not the best choice.

Fixed

l25 it is very nice to predict the wave character from a resonance theory, but it is surprising to end the abstract on this point. Maybe you can quantify here this effect as it is done in the manuscript ll. 421-429

Thanks for the suggestion: “This wave is present even in a simplified straight-slope topography, its character can be predicted from linear resonance theory, and it represents up to 30\% of the local energy budget.”

1. Introduction
l.30 : Gentle topography... is thought to break. It is clear what you mean but I'm not sure the sentence is correct (to my grammar knowledge). The topography doesn't break.

Changed: "The internal tide response to gentle topography that is subcritical to the internal tide frequency is likely dominated by higher vertical modes,"

l.49 line-integrated -> depth-line-integrated

Changed.

ll 58-68 : I don't think that figure 1 helps understanding the text, which is already very clear (it takes some time however to understand the figure, with no axes, and for which the reader has to refer to the caption). In this very early stage of the article I would prefer concentrate on the text, that to my opinion does not need figure 1.

OK, I agree this was a bit of a weak figure. Hopefully the other reviewer didn't like it, but I think the text is clear without the illustration.

l.86 : you could already insist here on the fact that there is no barotropic forcing, neither local nor at the boundaries.

Changed: "and there is no barotropic forcing anywhere in the domain"

2. Model
l.98 : I appreciate that you acknowledge the Python scientific stack (in case you get criticized for this reference)

Their reliance on volunteer labour and project grants makes citing them appealing.

l.101 : with the low amplitudes used in the model, does the KL10 enhanced diffusivity have an effect here ?

Yes, there are overturns. At these resolutions one needs to be careful about how "real" they are versus overturns caused by discretization errors in the advection scheme. However an advantage of an overturn-based scheme (or a Ri# scheme) is that it allows the model to dissipate grid-scale noise.

l.108 : "therefore the most linear" : i did not understand the logical link with the previous sentence. However, line 153 it is said that the forcing is reduced on purpose (there, I see the justification of "as linear as possible"). Maybe leave the discussion on linearity to the forcing section.

Dropped this sentence, as its a bit confusing, and not really to the point. All I was trying to say was reducing the forcing to 0.01 mm/s velocity amplitude doesn't change the actual dissipation fraction in the model.

l. 124 : a plot of the stratification would be welcome, for instance attached to figure 2.

Good suggestion. Now panel c.

Figure 2 : the colorbar of the detailed bathymetry goes to -10000, whereas the whole study is performed at H<5000m. The two x and y lines giving the scale are not necessary, as the rest of the figures are plotted in kms.

Colorer fixed. The x-y lines are to show the alignment with the detailed topography, so we have left them in.

l.129 : justify the choice of two line sources and their location (probably topographic features of the Macquarie ridge ?)

Changed: “To simplify the generation problem we apply an analytical forcing to our model that is meant to represent a simplified version of the regional simulation pictured in \fref{fig:LocMap}a.”

l. 130-131 : "initial conditions set with this forcing" : not clear. If I understood correctly, the initial pressure/velocity field is set to the analytical forcing solution, and pressure and velocity conditions are further imposed at the southern and eastern boundaries for the rest of the simulation ?

Changed: “The initial conditions contain this wave field, and sponges in the southern and eastern boundaries are forced with it.”

l.132 : at a distance R from the line source -> maybe precise "upstream" from the line source. Also, you can emphasize that your calculation allows the line source to be of any shape, whereas it should be a portion of a circle if one wants to use Rainville et al 2010 formalism. R was noted r0 in Rainville, which is also the radius of curvature of the line source - could be mentioned here.

Changed: “The forcing is similar to that suggested by \citet{rainvilleetal10}, except here our line source is digitized as a number of discrete point sources along the line, and their response in the domain summed.” We really just wanted to cite the lineage to Rainville et al, not go into detail of the differences in the calculation.

l. 134 : why "mode 1" ? In equation5 you introduce other modes, you can be more general here. Nothing specific of mode 1 in eq2, except wave vector k_t

Agreed. But notationally its simpler to lay out what we did, rather than also introducing k as a function of mode.

eq2 : introduce p'_i(x,y,t) that is used in eqs 6 and 7
eq2 : is a_i a function of (x,y) ? If not, where is the 1/r decrease due to cylindrical spreading, present in Rainville equation 3 ?

Fixed. Equation now reads: p'(x,y,t)=\sum_{i=1}^{N} p_i(x,y,t) = \sum_{i=1}^{N} \frac{a_i}{\sqrt{r_i}} \exp\left(j\left(lk_tlr_i - \omega t\right)\right)

eq2 : why k_t, what does the t stand for ? Why not introducing k_m for mode m, and c_m=NH/(2m\pi) the eigenspeed of mode m ?

Changed k_t to \mathrm{k} since k is really a vector.

l140.: H is the water-depth Which value is used here ? Fixed I guess for the wavefield computation

Doesn't really matter here. H is the local water depth. Its value doesn't come into the calculations here, just the normalization convention we are using (there are a couple different ways to normalize the eigenfunctions).

l153 : Why is this better than letting the internal tide propagate through the domain ? How long are the "startup transients" in numbers of M2 period ? Isn't this transient for the incident internal tide not dominating figure 6b ?

It takes two tidal periods for an incoming tide to even hit the bathymetry, so it saves two tidal periods of spinup. The transients are indicated by figure 6b, where energy is pretty much steady state by 12 -13 tidal periods. So, it probably doesn't matter too much either way. Intuitively, it makes sense to me to get the mode-1 energy filled out as quickly as possible, even at the expense of some start-up transients are mostly higher mode.

Fig 3 : "analytical forcing" : it is the energy flux which is plotted. This is confusing, if the forcing is only applied at the southern and eastern boundaries. In fig 3a I see only, if I understand, the initial condition for the energy flux. Also, isobath are contoured, but the real topography is not used in the wavefield computation ? If the contour lines are only her for information, please precise it.

Changed: Energy flux representation of the analytical response to the forcing used to drive the models used in this paper. Two mode-1 internal wave sources are located to the south east (blue lines). The model domain is rotated 12 degrees from geographic so the shelf break approximately lies along $x=0$. The outer model domain is the rectangle with the thick green line. The inner green rectangle denotes 1-km lateral resolution. Outside this rectangle resolution increases 3.5\% per cell to a maximum of 5-km in the second largest rectangle, and outside this rectangle grid size increases to a maximum of 10 km. The sponge layer is indicated by the largest thin-green rectangle. The 250, and 3000-m isobaths are contoured, though the bathymetry is not used in the analytical response. Arrows show the direction of the energy flux, and are scaled by its strength. b) Energy flux of analytical response of energy reflecting from a wall at $x=0$, north of $y=0$, in a flat-ocean domain.

l158 : 0 to 400km, but you extend your study to 600km fig 12.

Sure, but that is not our "main focus"…

figure 3 : is there a sponge layer south ? The text says north and west. Why are there 3 green small rectangles ? The domain is the thick line I guess.

See above

l.159 : if there is no sponge layer on the eastern side, what is the boundary condition, can the reflected beam reflect and re-enter the domain ?

Changed: “The sponge regions on the southern and eastern boundaries are forced with this forcing. The northern and western boundaries are sponges where the velocity is slowly …”

l.172 : the southward energy flux may come from dissipation along the beam propagation, resulting in a default in reflected southward flux ?

There is no dissipation in the idealized response, this is all analytical. N/S fluxes are the result of interference between the counter-rotating ellipses of the internal tide velocities.

3. Realistic model
l. 180 : expecte (typo)
l. 197 : "partially" balanced (if not this is inconsistent with fig 6b). I think it is important to note here that the PC flux conv is only partly associated with the slope waves.

Changed to “mostly balanced”.

l.207-208 : interpretation of the oscillations ? Westward propagating reflection from the eastern boundary of the domain ? It this is the case, t=9T is then the most realistic time to look at the simulation ?

Thats certainly possible. On the other hand the high modes take a long time to equilibrate, so looking too early doesn’t give the energy a chance to fill out. This energy bump is relatively small, (1/6 of the initial forcing) and is not likely to change any of the results in the paper. We have added “The oscillations were not explicitly examined, but likely result from imperfect sponges.” to the discussion of this time series.

Fig 6ac : at which time ?
Fig 6 : refer to the same color (cyan or blue) in 6a and 6b
Fixed.

Fig 6ab : dE/dt is basically the sum of the three other curves, with signed conventions. Could you plot -dissipation, and in the legend put dE/dt in last position to make it clear that this is the sum of the three other curves.

I did this a few ways. I prefer to think of dissipation balancing flux convergence, and then the other terms add up to the difference. Therefore I like to plot the big terms with the same sign, otherwise its less obvious that they balance.

Eq 8 : Define "Convergence" as -Nabla.H.Fbc. You can also write Convergence+Conversion +Dissipation, with Dissipation a negative value. It may make the reading easier.

I think that dissipation is almost always defined as a positive quantity when it is a sink from the kinetic energy equation.

4. Simplified geometries
l. 249 : "total" complex amplitude

Fixed

eq12-15 : use x and y labels instead of u and v for clarity, with \vec{F}=Fx.\vec{x}+Fy.\vec{y}

Done. Not sure when I got in the habit of using "u" and "v"

l.284 : the title "Tasman rise only" is confusing, as the shelf reflection is also discussed here.

Thanks, fixed to "Tasman Rise and Simplified Shelf"

5. Discussion
l. 335 : good to mention it is arbitrary, but it is necessary to chose a control volume

l.348 : do the 80km boxes have the same x extent as the large volume control ? Please precise it.

Fixed. Yes, same x-extent…

l.358-365 : this paragraph could be detailed here. Why not in fig 12b also using 80km boxes for the comparison with Kelly model ? And restrict the comparison to the "valid" part of the simulation (initially said to stop at y=400km) ?

We could smooth the Kelly response, but thats pretty easy to do by eye…

The modal decomposition is not clear : what does "all the modes" mean in fig 12 caption ? I understand the thick black line (with gray superimposed ?) as the mode 1 reflection coefficient, but what are the other gray lines : the portion of the mode 1 incident wave transferred into mode 2 during the reflection ? If so, do we expect a reflection coefficient value of 1 when all the modes are introduced (almost reached for the upper gray line) ? Also, does the Kelly method take into account the presence of the ridge ? More discussion is welcome here.

Changed to: `"The black line is the mode-1 reflectivity from a linear model \citep{kellyetal13b}, the next four grey lines are the cumulative sum of modes 2 to 5, and the last grey line the sum of all the reflected energy. These do not sum up to one because the linear model has some ``viscosity'' that removes some high-mode energy."`

We do expect the totals to sum to one.

Not sure what ridge you mean. The Tasman Rise? If so, then no, the calculation doesn't include the Tasman Rise. If the Rise were much longer in the north out direction (i.e. we had a ridge/slope bathymetry) that might have worked, but the diffraction from the Rise makes that impractical.

l. 407 : is it possible to derive an analytical formula for the O(1) value of the along slope wavelength ?

Not that I know of. You need to determine the resonant modes…

Figure 13 : mention the slope with (20-40-70km) Why four cases here ?

Fixed (in caption). Four cases was arbitrary.

Figure 16 : why do "ray tracing like" features appear, is it due to the resolution method ?

Right - the linear model has “steps”.

Figs 8 and 11 : at which time is the Real case considered ?

20th tidal cycle; fixed in the captions.