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Interactive comment on "Mapping turbulent diffusivity associated with oceanic internal lee waves offshore Costa Rica" *by* W. F. J. Fortin et al.

Anonymous Referee #2

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Review of 'Mapping turbulent diffusivity associated with oceanic internal lee waves offshore Costa Rica' by W. Fortin and coauthors

General comments:

I have read the manuscript and found the topic and data very interesting. However I see serious problems in the method applied here. I also think that the interpretation of the lee waves in general is not always proper.

The authors have done a decent job assembling, analyzing and putting into context the seismic observations offshore Costa Rica. The data indeed shows that is possible to image oceanic lee waves with high resolution. It also shows regions upward and downstream of the lee-wave-creating sea floor ridges where the seismic images indicate a possibly different regime with more variability at higher horizontal wave numbers.

As main novel aspect of the paper the authors use a coarse resolution map of oceanic diffusivity derived from tracked reflectors in the seismic image together with a higher resolution map based on a fourier transform of the seismic data to derive a high resolution map of oceanic diffusivity.

It is this step which I have serious doubts about. In their analysis the authors follow a publication by Holbrook et al. (2013) where the approach was first described. As far as I understand this approach assumes a (locally linear ?) relationship between the spectral levels of certain subranges of the horizontal wave number spectra of slopes of isopycnals/reflectors and of the horizontal wave number spectra of seismic amplitudes (or more simply fourier transforms of the seismic data). Holbrook et al. (2013) show some oceanic and modeled examples to prove that such a relationship exists. I have been thinking quite a while on how such a relationship can exist at all, as at first thought the slope of the reflector and the amplitude of the reflector do not appear to have anything in common. I do now think that for small vertical reflector displacements such a relationship might indeed exist. Though it is probably not linear. In my thought experiments (I leave it to you to create some simple idealized analysis of these to prove or disprove me) I also found that in real world data this relationship could very often break down.

The outcome of the thought experiments is that there should only be in a very simple case a relationship between the two spectra. That is in the case of relatively small vertical excursions (<1/2 seismic wave length). In the case documented here (see Figure 7, center area) this is obviously violated.

The synthetic fields which both this manuscript and Holbrook et al, (2013) use to document the reliability of the approach do only take the internal wave field into account. Any other processes and variability present in the ocean or any problems with the seismic acquisition that will have an effect on the reflectors and their amplitudes are not

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considered. I am thus reluctant to take these as a proof for the concept.

I am thus not happy with the method to enhance the resolution of the diffusivity maps.

Regarding the lee waves I am not sure whether the ideas of the authors are properly thought through. A constant flow over a ridge will create a particular wave like pattern of velocities and vertical excursions of isopycnals. This is what the authors state to occur here. In Nikurashin and Ferrari (2010a) they find that this particular regime (page 1068, lower left) should in fact NOT lead to much mixing. Some areas with elevated shears will of course create more mixing than otherwise, but it is the time dependent flow (typically through tides) that creates variations in the lee wave pattern and thereby much of the mixing (see also Klymak, Legg and Pinkel, 2010). You do not give any information on the time dependency of the currents, so the ideas about the mixing areas using the above references are somewhat stretched.

So in summary, I think the authors need to give a more theoretical proof that the reflector-slope and amplitude spectra are reliably related. I also think the authors need to better take into accound under which particular conditions the lee-wave references apply. My suggestions thus is to drop the enhanced resolution part and concentrate on the coarse resolution diffusivity map. Together with some more information on the local current regime (tides, other time variations) it could be better put into the proper lee wave context.

So here are some thought experiments (I am sure you can easily construct many more realistic problem cases):

- Case 1 We have a single reflector which is simply horizontal. This reflector (as also in all subsequent cases) has in the vertical been folded with a gaussian with the width of 1/2 seismic peak wave length i (15m in the case of the data here, I think). - The average slope of the tracked reflector for a particular horizontal wave length x will be zero. - The fourier transform/coefficient for that same horizontal wave length x will also be zero.

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Zero energy in both spectra. The relationship holds.

- Case 2 We have a single reflector which is sinusoidal at horizontal wave length x and has a vertical excursion much smaller than the 30m vertical seismic wave length. - The average slope of the tracked reflector for the horizontal wave length x will be some small value. - The fourier transform for the horizontal wave length x will also indicate a non-zero value.

This is where I think there actually is a proper relationship between the two spectra. With increasing amplitude of the sinus you will get larger slopes AND larger deviations in the amplitude. Though the relationship is probably non-linear.

- Case 3 We have parallel positive reflectors which are sinusoidal at horizontal wave length x, in the vertical one seismic wave length apart (30m) and have a vertical excursion on the order of +/-30m (or the seismic wave length). - The average slope of the tracked reflector for the horizontal wave length x will now be quite significant. - The simple horizontal fourier transform will now encounter a serious problem as it 'sees' amplitudes from two different (!) reflector crests. I actually think the fourier transform will 'see' energy at higher horizontal wave numbers than actually exist in the data.

The relationship is broken.

- Case 4 We have parallel positive and negative reflectors both sinusoidal at at horizontal wave length x and half a seismic wave length (15m) apart in the vertical. The vertical excursion is about half a seismic wave length (15m). This is a simple warm interleaving layer. - The average slopes of the tracked reflectors for the given horizontal wave length will again be significant. - The simple horizontal fourier transform will in this case find energy at the same horizontal wave length. But it will be elevated as a horizontal slice now cuts through positive and negative reflectors.

The relationship might still be there, but is a different one than in case 2. Should you have case 2 and 4 in one region analyzed together, this is a problem.

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- Case 5 Like case 4 but the vertical excursions differ between the two. This would e.g. be an interleaving water mass that gets thinner.

Not sure what this will do. But for sure disturb the relationship.

- Case 6 Like case 4 but the positive and negative reflectors get so close that they cancel each other over only part (!) of the analyzed region.

Not sure what this will do. But again for sure disturb the relationship. Likely again more energy at higher wave numbers. In the best case this will be taken care of by the S/N ratio criterion of Holbrook et al. (2013), but it still might create problems in borderline cases.

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