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Interactive comment on "High-resolution diapycnal mixing map of the Alboran Sea thermocline from seismic reflection images" *by* Jhon F. Mojica et al.

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Interactive comment on "High-resolution diapycnal mixing map of the Alboran Sea thermocline from seismic reflection images" by Jhon F. Mojica et al. Anonymous Referee 1 Received and published: 10 October 2017

We thank reviewer1 for the constructive comments and review that has surely helped to improve the manuscript. We have taken all comments and suggestions into account as indicated in our point-by-point answers below. To clarify our answers, we add a file (Review1-answer1.pdf) as a supplement, easily to read for you.

We agree that the relationship between observed oceanographic features and mixing distribution was unclear. Our main message is that there is not a clear correspondence between the location of IWs (> 100 m horizontal scale) and mixing hotspots, but rather between mixing hotspots and the location of large-amplitude features in the transitional domain (30-100 m horizontal scale). Based on this analysis as well as on previous results presented in Sallares et al (2016), we interpret that these large-amplitude features are the expression of shear instabilities (e.g. KH-type billows). This means that there is not a direct relationship between IWs and mixing. It tends to concentrate where IWs become unstable and instabilities develop, leading to turbulence. We clarify this message in the new version of the text (line 19-21, line 339-341, line 383-386).

We have estimated the k(x,z) map for internal waves and Batchelor regimes (figure rev.1-1). The lower mixing values produced by IWs as compared to the Batchelor regime are clear.

Specific comments: Lines 35-37: Please clarify here - I think that both your references refer to internal-wave phenomena

Thanks for the comment, references have been corrected (line 38).

Lines 50-53: I am not sure what you are saying here. We just want to describe the behavior of ε in a conservative flow. We have modified the text to make this point clear (line 51-52).

Lines 65: I think that lowered microstructure profiles are generally the most robust source of turbulent measurements. We agree, of course. Our point here was to note that these devices (VMP, microriders), provide measurements in just one dimension (either horizontal or vertical), but seismic data cover both dimensions at once. It is obviously with poorer resolution than microstructure profilers, but much better than that of conventional probe-based studies in the horizontal one (line 63-65)

Line 150: Do you have evidence for this? Could you compare acoustic reflection horizons to density horizons in the oceanographic data? We do not have direct evidence for this particular profile because we cannot invert density with this data and we do not have the appropriate complementary oceanographic data. However, in a previous study by our group with appropriate data, we showed that seismic reflectors do actually follow isopycnals (Biescas et al, 2014).

Line 165: If possible you should use integrated shear and or strain spectra to get estimates from CTD/ADCP data - perhaps you are limited by depth ranges? You are also missing some terms in for shear-to-strain ratio and inertial frequency e.g. see Waterman, S., K. L. Polzin, and A. C. Naveira-Garabato (2012), Internal waves and turbulence in the Antarctic Circumpolar Current, J. Phys. Oceanogr., 43, 259–282. You should at least quantify the omission of these terms and also explain how you decide on what you mean by 'uncertainty bounds' in several places in the text. You should also mention the errors associated with fine-structure estimates - particularly in regions away from the open ocean.

Assuming the energy dissipation in the thermocline (depth range <120m), we follow the Gregg89 model, where the observations agree with the predictions sufficiently well to suggest that the simplest way to obtain average dissipation rates over large space and time scales is through N$^2/(N_0^2) < (S_10^4)/(S_GM^4) > (Gregg, 1989). Thismodeliscommonlyappliedinthemid - latitudethermoclineasourobservations. Thatiswhyweusethissimplebutaccuratemodel. Onthe$
$R\omega$=âŇľV$_Z^2/((N^2\zeta_Z^2)) Themaintermomittedforusis (\zeta_z) therelativelocalchangeinbuoyancyfre$
$((N^2 - N_ref^2))/(N_ref^2) Forourdatathisvalueis\ 0.9. Thisvaluecanberelatedwiththelevelofstre$
$165). Sayingthattheresultsagree “withinuncertaintybounds'' wasanoverstatement, sowehave$
$24, 245 - 246, 376 - 379).$

Lines 195-200: This should be in methods Done (line 191-199)

Line 203: Spatial resolution - be careful what you mean by this as really each data point is an average over 1200m by 15 m box Yes, we agree. We must distinguish between

the theoretical resolution of the seismic data and that of the diapycnal mixing maps. For seismic data, the vertical resolution (i.e. the capability to discern between neighboring reflectors) is given by the Rayleigh criteria, whereas the horizontal resolution (i.e. the part of a reflector covered within half a wavelength of the seismic signal) corresponds to the first Fresnel zone. For our acquisition system, medium properties, and target depth, these are 2 m and 15 m respectively (it is explained in Sallares et al., 2016). However, this does not represent the resolution of the mixing map. In this case, we are calculating spectra and diapycnal mixing within windows of 1200x15 m, so this could be taken as the approximate resolution of the map. We have modified the text accordingly (line 197-199).

Lines 215: What scale are you computing shear over i.e. dZ? Also how to you quantify buoyancy frequencies, N? (Line 221) dz is 10 m. To calculate buoyancy frequency we use the expression below, where density is obtained from the XCTD data: N=$\sqrt{}$(-g/$_0(\delta(z))/\delta_z)$

Lines 319: Shear to strain ratios tell you about the frequency content of the internal wave field. You might well expect higher inertial content (i.e high shear to strain ratios) near the surface due to wind generation. As we describe below the level of stretching and squeezing of isopycnals by internal waves, is close to 1. Near the surface we would expect higher inertial content, but we consider the whole thermocline, where we can see a trending a robust regularity over the whole profile (figure rev1-1). This variation is consistent with the process already described in Sallares et al. (2016).

Lines 325: How do dissipation estimates compare for GM and Batchelor parts of the MCS spectra? This may tell you something about the role of IW in generating the turbulence/GM assumptions As it is shown in figure rev.1-1 and it is explained above, the general patterns in the diffusivity maps obtained with the GM (a) and Batchelor (b) parts of the spectra (including location of maximum and minimum values) are very similar. It appears to be a clear correspondence between the two diffusivity maps. However, the values obtained from the Batchelor part of the spectra are much higher than those obtained from the GM part. To us, this indicates the stronger influence of instabilities, rather than IWs, on diffusivity.

Line 334 and 351: Confusing regards what you are trying to report regards role of internal waves here - please clarify. As we explained above, our main point is that we do not see a direct relationship between IWs and mixing. Mixing appear to concentrate where IWs become unstable and instabilities develop, leading to turbulence. We have tried to clarify this message in the new version of the text (line 248-250, 334-336, 383-386).

Please also note the supplement to this comment:
https://www.ocean-sci-discuss.net/os-2017-72/os-2017-72-AC1-supplement.pdf

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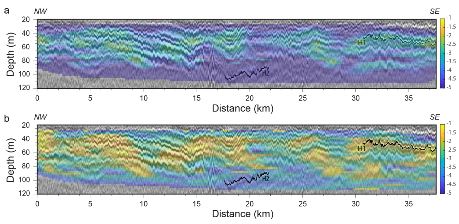

Figure rev.1-1. High-resolution $k_\rho(x, z)$ map overlapped with the HR-MCS image. Solid lines labelled H1 and H2, display acoustic reflectors located within relatively high- and low-dissipation areas (a) from internal waves Gregg89 model (b) and from Batchelor59 model.

Fig. 1.