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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 #2

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This manuscript presents seismic reflection data from the Alboran Sea and outlines a method for producing a map of diapycnal diffusivity across one profile. The major conclusion is that the profile shows patchy turbulence on the scale of a few kilometers horizontally and 10-15 meters vertically. Further, the authors observe greater mixing in areas of internal wave instability. Results are compared to estimates of turbulence made from XCTD and ADCP data as well as background reference models. Additional analyses of filtered slope spectra examine the relationship between diapycnal mixing and the assigned internal wave and transitional subranges.

The introduction and background are clearly written and well presented. However,

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the manuscript takes on the substantial challenge of developing a new method and presenting scientific conclusions at once, and in a relatively short format. As a result, I think many issues addressing the methods, presented data, clarity of conclusions, uncertainties, and reach of results are insufficient.

Major Concerns: Data handling and methods are insufficiently explained. The authors need to be clearer about the stated resolution. It is not accurate to apply a 1200x15 m grid to a 30x3 m grid and claim improved resolution. Many of the 30x3 m cells will not have tracks in them. In fact, at CMP spacing of 7.5 m, you can only have 4 or 5 traces represented (depending how you treat them) and few realistic and meaningful spectra can be taken at that scale. The authors need to explain what happens when tracks are longer than the 1200 m box. They state the tracks are 1.5-21 km long, so no tracks would fit inside the 1200 m grid length and position vertically is also unaddressed. Each track would be included in hundreds of 30x3 m grid cells which seriously undermine any claims at resolving patchy turbulence at their stated resolution.

The authors need to show more of the data that support their methods and conclusions, particularly the reflector tracks and many more spectra. To establish this as both a methods paper and support their science conclusion, these data must be shown and clear. First, show the 68 reflector tracks and discuss their distribution, including why application of a k value obtained on the large scale can be applied to the small scale and how to handle regions lacking tracked reflectors. Second, slope spectra rely on the aggregate data of many tracks to have statistically characteristic behaviors (Klymak and Moum, 2007 part II). All of what we are shown are single-track spectra. Third, more justification of the horizontal wavenumber bounds for the sub-regimes (IW, instability/transition, and turbulent) are needed to illustrate these are accurate bounds for sub-regimes all over the 2D line.

The authors need to be clearer about the role of basic oceanographic features and the expression of turbulent structures. Lines 245-246 state “there is no clear visual correspondence between the k anomalies and the most obvious of the imaged oceano-

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graphic features such as IWs" which seems to be against the main thrust of the paper that sub-mesoscale features can be examined through their turbulent expressions, particularly lines 19-21 in the abstract as well as a few points in section 4.

The manuscript thesis is about ocean mixing, as such, the turbulent subrange should be examined for tracks H1 and H2. Perhaps there are corresponding traits between the IW subrange and turbulent subrange, or the transitional subrange and turbulent subrange that may clarify the expression of turbulence by mesoscale and sub-mesoscale oceanographic features.

Uncertainties are problematic and under addressed. In the abstract (line 23) and conclusion (line 372) results are stated to be within uncertainty bounds but nowhere in the data do the authors show or discuss any uncertainty assessments. In section 3.2 uncertainty is briefly mentioned, but again, is simply stated that values are within uncertainty bounds. If the conclusions are supported within an uncertainty range, please show and elaborate.

The major conclusions for the filtered spectra analysis are under supported. Lines 378-381 deliver major conclusions about the relationship between IWs and overturning as well as shear instabilities and mixing hotspots. From the data presented, it appears these conclusions are drawn from the analysis of 2 tracked seismic reflections, H1 and H2. This overstates what is observed in the data, particularly when those two tracks were chosen as end-member individuals chosen for their position in "anomalously high (H1) and low (H2) mixing patches" (lines 264-265).

General Comments: The manuscript thesis states that authors produce a map of diapycnal mixing that show patchy nature. However, they often refer to an average k as a benchmark to compare to conventional methods. The authors need to discuss why they would average the entire map when the main thrust of the paper is that it is heterogeneous. Additionally, the authors need to justify why just 1 XCTD and 1 ADCP data set would accurately reflect the average for their produced mixing map. For example, even if the XCTD was collected concurrently, what would the implication be if it was dropped at 8 km where k is high, or at 22.5 km where k is low?

Please explain how signal-to-noise is calculated. A signal-to-noise ratio of higher than 8 with 6-fold data is surprising.

Much of section 3 (first paragraph) should be expanded and put into section 2

Please explain why tracks H1 and H2 were analyzed with 1 km windows when the turbulent maps were analyzed with 1.2 km windows. The difference in these windows change the wavenumber range of the IW spectra and might aid some of the confusion around the handling of internal waves in the manuscript. Further, why limit the analysis to $\sim$1 km? If they exist, larger IWs should carry even more energy and may be important.

At many points the authors state there are "no clear correlation" or similar language between filtered spectra (e.g. lines 287, 329-331). Were correlations and statistics taken for each of the 68 tracked reflectors to support, or not, a relationship between the filtered spectra? If so, this should be a major point of the paper and have supporting figures.

In figure 6, please show the actual fit lines for these data. This would allow for a brief discussion in the text of how you calculate spectral energy levels beyond a reference to Sallares et al. 2016.

Specific comments: Line 85: Citation should be Holbrook et al., 2003. Line 175: Authors need to state and explain their choice for b the scale factor Lines 329-330: Suggest rewording. The authors infer IW-induced mixing is not efficient enough to keep the overturning in this dataset, I do not think the data shown makes it clear, particularly on a global scale. Line 340: Suggest rewording. A smooth seafloor likely suggests a lesser role in the generation of hotspot mixing, if it is disregarded entirely, please explain.

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