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Ocean Science Discussions

Interactive comment on "Mapping turbidity currents using seismic oceanography" *by* E. A. Vsemirnova and R. W. Hobbs

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Revision to m/s "Mapping turbidity currents using seismic oceanography" by Vsemirnova and Hobbs.

The authors present a multichannel seismic (MCS) image of the Faroe-Shetland channel that show prominent reflectors within the water layer, one of them near the seabed. They demonstrate that these reflectors do not correspond to thermohaline contrasts and claim that they are instead the seismic signature of turbidity currents, based mainly on the correspondence with colocated (but not coetaneus) optical backscatter data. Based on the observed seismic reflectivity, they estimate a sediment concentration of ∼45 mg/l at the time of the survey. To my knowledge, this is the first direct evidence that oceanic structures other than thermohaline can be detected, imaged and

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-possibly- characterized with MCS data.

My overall opinion of the work is rather good and I am persuaded that their main conclusion (e.g. MCS data can image turbidity currents) is correct. However, I have a number of comments –listed below- to be addressed by authors before acceptance, especially those related to the quantification of the sediment loading. My recommendation is moderate revision.

List of comments:

Title > I am not native English speaker, but I would not say that we “use” seismic oceanography. I would rather change the title to something like “Mapping turbidity currents with seismic oceanography data”

P. 1804, L. 13: (here and in other parts of the text) I would not say you are actually “inverting” the sediment concentration but ”estimating” or "calculating" it.

P. 1806, L. 15-22: A high-res bathymetry figure showing the network of mounds would help to illustrate the text.

P. 1807, L.4: were acquired > was acquired

P. 1807, L. 19-21: “the lack of reflectivity (. . .) the layer is well mixed” > I’d say this assumption is too strong. You are lacking high frequencies in your system, the layer between the reflector and the seafloor could be more complex than it appears to be in your profile.

P. 1807-1808, section 3.2: Seismic data were acquired in 1994, whereas oceanographic data and measures were taken in 1999 > Please indicate dates/season of the year for the two surveys. I wonder if there could be some kind of seasonality in the formation of turbidity currents? And are there climatological constraints? Additional justifications are required to compare data that are not coetaneous.

P 1808, L. 25: Any empirical data to justify validity and possible uncertianties asso-

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ciated to the linear interpolation made in equation (1)? References on this would be welcome.

P. 1809, L. 8: course sediments > coarse sediments

P. 1809, L. 10: "it is unlikely that we have a single particle size" > Don't you have the sediment traps and water samples to answer this question and fix the particle size?

P. 1811, L. 14: "(. . .) to give a value of 45+-25 mg/l" > The only source of error considered appears to be that in R. But there are other assumptions made that can also have their own uncertainties associated (e.g. density, velocity of suspended sediment, boundary roughness, or the linear interpolation of properties made in Eqs 1 and 4). An effort to quantify these errors and to demonstrate that they are negligible would be welcome.

Also, I find necessary to contrast your calculated value for sediment loading with values measured with sediment traps and water samples (not only estimations based on optical backscatter). Is this value realistic?

P. 1815, caption Fig 1(b): "the inset in rectangle (. . ..) a strong seabed current" > well, this is what you try to demonstrate. I do not think this should go in the caption.

Interactive comment on Ocean Sci. Discuss., 8, 1803, 2011.