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Interactive comment on "Full waveform inversion of short-offset, band-limited seismic data inthe Alboran basin (SE Iberia)" *by* C. Gras et al.

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Review for: Full waveform inversion of short-offset, band-limited seismic data inthe Alboran basin (SE Iberia) Clàudia Gras, Daniel Dagnino, C. Estela Jiménez, Adrià Meléndez, Valentí Sallarès, and César R. Ranero

By Milena Marjanović

The authors present a study that combines different geophysical techniques to extract high-resolution information of the subsurface using band-limited data collected in deep water setting in the Alboran Sea. First, to overcome the limitation in the available source-receiver offset (streamer length 6 km), the authors use re-datuming technique and unveil the presence of the refraction signal, crucial in velocity modeling. Second,

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using the re-datumed data the authors perform travel-time tomography (TTT) using the information embedded in the refracted signal. In addition, using the original shot gathers the authors do TTT on reflection signal. The velocity model obtained using TTT they then use as a starting model to perform acoustic multi-scale Full-Waveform Inversion (FWI). This final FWI velocity model reveals geological structures that were not seen in TTT model or in seismic reflection images, e.g., the presence of $\sim$300 m thick higher velocity layer embedded in sediments that the authors attribute to evaporites deposited during Messinian crisis in the Mediterranean. The main contribution of the presented work is to show that by using adequate combination of geophysical techniques a satisfactory, high-resolution velocity model can be obtained even for the data that are collected in far from ideal conditions, here using limited streamer length in deep water setting and absence of low frequencies (<5 Hz) in data signal. In my view, this paper is tackling very important point for the entire Marine Seismic community, showing that many of the existing datasets should be revisited and analyzed with novel techniques to enhance our understanding of subsurface.

The paper is well written and the overall presentation of the content is well structured and clear. The figures are well presented and necessary to follow the text. However, I have several comments that would be good to address. I provide a detailed list above:

Page 2 Lines 20-21: The sentence is a bit redundant with respect to what you state on lines 10-11.

Line 25: part of the sentence ". . ., so there is typically no signal above noise below this frequency." This construction is a bit strange. Perhaps adding ". . .above noise level.."?

Line 29: There is one right bracket extra, please remove. In addition, it would be good to acknowledge the work of authors who did significant work on implementing redatuming technique and applying it to real data: Arnulf et al., 2011(GRL); 2013 (GJI); 2014 (JGR); Henig et al., 2014 (G-cubed) etc. In fact Qin and Singh (2017; 2018) use the code introduced by Arnulf et al. (2011).

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Line 22: It would be better to say: “to reveal refraction signal” instead of “to modify recordings”

Lines 26-28: These three sentences could be summarized into one.

Page 4 Line 25: It would be good that you list all of the processing steps that you apply to the data before re-datuming. You mention later in the text that you did some filtering, but it would be good make it clear what steps you applied prior to DC.

Question: Do you consider 3-D effect in downward continuation? You mention this as an important effect for FWI. Perhaps as you are doing only TTT using DC data this may not be relevant.

Page 5 It would be good to have a short paragraph that compares your re-datuming approach with respect to Arnulf et al. (2011).

Lines 19-21: This statement is not completely true. Arnulf et al. (2011) chose a flat surface to extrapolate the data; it is a choice of the authors not a limitation of the method (Qin and Singh use the same method to re-datum to follow surface close to seafloor).

Question: Do you DC the data exactly to seafloor surface? There are authors that argue that the extrapolations surface should be at least several tens of meters above seafloor surface (e.g., Harding et al., 2016 - G-cubed)? It would be good to comment this.

Question: You mention that you use variable water velocity. Have you quantified the effect of using constant vs. variable water velocity in downward continuation?

Lines 28-32: This is common to all DC techniques, it may not be really necessary to mention it after you provided detailed explanation of the technique.

Page 6 Lines 5-13: I agree that the amplitudes can be affected by DC and that one

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has to very careful what part of the data is suitable for FWI. Thus I think it would be good that you mention this more explicitly. This also support your decision to do FWI on non-DC data.

Page 7 While you provide detailed description of re-datuming, the description on tomography is limited. It is not clear how you combine refraction TTT on DC data and reflection TTT on non-DC data. Please, provide more information. It would be good to indicate the events reflection and refraction by introducing an additional figure or insert them somehow in the figures 4 and 6.

Page 9 Question: You mention that the number of iterations per frequency band is 10 and then you say that the stopping criteria is governed by the Arminjo rule. Does this mean that in all cases this criteria was reached within 10 iterations?

Section 4.2 This section is a mix of methodology and results and should be rewritten. The part including lines 20-30 belongs clearly to methodology section and can be used to address the comment I raised for the content presented on Page 7.

Question: In lines 23-24 you mention that you pick refraction arrivals for all of the offsets (up to 6 km). My experience with DC and your example shot gathers in Figure 6 show that your maximum offset for picking should be <4.5 km. This is a known problem in DC. How do you deal with this problem? What is the effect on TTT velocity, if any?

Page 11 Line 22: I would rather use “gently” that “softly’ in this context.

Page 15 Line 22: The elastic effect may not be significant in your case, as you have sediments on top of igneous basement. You may cite Warner et al., (EAGE abstract, I believe it is 2012).

Figures:

Figure 3 - The annotations are too small, as well as numbers indicating lat and long . Please, increase the font.

Figure 11 - Correct me if I am wrong, but it seems that you have completely reversed polarity in Initial/Resultant model with respect to Real data. It would be good to show residuals or at least provide a wiggle plot with observed and synthetically calculated data superimposed to understand if the problem is due to plotting or there is really an issue with data polarity.

Figure 16 - Is there a particular reason why you chose to do your interpretation on time seismic section?

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