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Dear Marc,
many thanks for your positive review. It is very helpful to clarify the manuscript along the points that you have raised. Explanations are given below.
With best regards from all authors, Lotte.

Reply to *Interactive Comment* by M.-A. Gutscher

..…………..

➢ Many thanks for the compliments !

A useful recent addition would be the following review paper: Graveleau, Malavieille & Dominguez, 2012, Experimental modeling of orogenic wedges a review, Tectonophysics, 538-540, 1-66, doi:10.1016/j.tecto.2012.01.027.

➢ We included the reference in the introduction and accordingly in the list.

The quality of the seismic images and the duration of the source wavelet: Throughout the MS the seismic images shown appear to be of fairly good quality, but when one looks more closely, there seems to be significant "ringing" in the source. ………..

➢ We included an additional paragraph about the ringing in Chapter 4. We also refer to Buddensiek et al. (2009), where a more complete discussion of these issues was already presented.

The authors (section 5, bottom of page 13) discuss the effect of the material (sand vs glass beads) on the ability to image a "shear zone". The problem is the only experiment where a shear zone in sand is shown (and not really described) is for Fig. 4d (where there is a tiny, far too tiny inset) and where the shear zone appears to be horizontal. Unfortunately, it leaves little to no visible trace in the seismic section. For the final experiment shown (Figs. 7 and 8) there is an inclined shear zone, described as having a dip of 30°, extending from the lower left corner to the surface (according to Fig. 7). The problem is that in the seismic section this shear zone is only visible at the surface (and directly below, along a vertical path in the 4 to 5 phases of ringing). There is no inclined structure discernible extending to the lower left corner. This is a crucial point, because the authors claim the shear zone is visible (though it is not apparent to me) and this is also held up as one of the most important potential applications of the new technique – to image internal structure and especially faults, in a non intrusive way and thus to be able to continue to observe the future evolution of a given analog experiment. In the annotated MS I also raise the following point. An artificially induced shear zone (created by pulling a wire through a granular layer cake) is almost certainly not the same as a tectonically induced shear zone (with the associated processes of grain compaction throughout and a very localized shear bad where grain dilatation occurs). The latter is a true fault, the former a disturbed zone. But getting back to the more crucial question at hand (the ability to image internal structure - a shear zone within the granular layer cake), I would offer several specific suggestions: 1 - If the authors wish to compare the effect of the host material on the ability to image a shear zone, then the two layer-cakes investigated must have exactly the same geometry (and the same size and dip of the shear zone) 2 - all of the seismic images shown throughout the MS should be displayed at something close to 1:1 (no vertical exaggeration). As it is, although the layer-cake is typically 40cm long and about 5 cm thick, the images are square (which represents a roughly 8:1 vertical exaggeration). 3 - the authors must demonstrate that their shear zone is visible beneath the surface. As it stands I have seen no sign of the subsurface expression of the inclined shear zone in Fig. 8.

- In now Figure 9, we included arrows and labeling to indicate the shear zone positions. Showing seismic data as exaggerated time sections is very common, especially if one wants to allow for better vertical separation of distinct events. Thus, we still prefer these images, because no further quantitative analysis is intended here. For clarity, we mentioned the VE in the figure caption.
- We agree that our point cannot be that this is very suitable to image shear zones as it stands. Instead, we rephrased the paragraph, and picked up the difference of artificial shear zone (=disturbed zone) and tectonic shear zone (=grain compaction and dilatation) as mentioned above.

4 - Since the ultimate goal of this imaging technique is to observe various stages in the evolution of a sandbox experiment, the authors should try to show (if possible) and discuss (at the very least) how this could occur. And this raises the whole question of sub-aqueous (saturated) sandbox experiments, a topic that is very little studied until now. 5 - The possible use of anisotropic materials for sub-aqueous experiments: If sub-aqueous analog experiments are to be conducted, then it seems appropriate (even necessary) to discuss and/or test alternating layers of isotropic and anisotropic materials, on the one hand in order to obtain variations in acoustic properties (without always having to resort to the use of materials with different densities) and especially in order to better reproduce the layering which occurs in natural examples (sedimentation processes) and the variations in pore fluid pressure also known to occur in natural examples of thrust wedges and believed to be largely responsible for the variations in internal and basal friction which govern the mechanics and evolution of thrust wedges. But this seems to be a vast topic and probably well beyond the scope of the present manuscript (which is focused on the observational technique).

- Yes, this is indeed a vast topic and not the purpose of this manuscript. Here, we want to show the general feasibility of our idea and approach, and of course further developments are necessary. A note on this is included in the outlook.
- We are happy to see how the manuscript inspires discussion now that the first step in the lab is made.

Please also note the supplement to this comment.

- We made all language corrections marked in the annotated manuscript.