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Solid Earth
Discussions

# *Interactive comment on* "Seismic imaging of sandbox experiments – laboratory hardware setup and first reflection seismic sections" *by* C. M. Krawczyk et al.

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Review of MS submitted to Solid Earth Discussions Title : Seismic imaging of sandbox experiments – laboratory hardware setup and first reflection sections

Authors: Krawcyk, Buddensiek, Oncken, Kukowski

Krawcyk et al. present a unique new observational technique for imaging the internal structure of laboratory analog experiments (using granular materials).

The writing style and English language are generally good, though there are a few small errors here and there and occasional awkward sentences. I have tried to iden-



tify/correct these in the annotated pdf MS.

The authors develop and test a novel, small-scale seismic apparatus in a water tank with an acoustic source and multiple (eight) receivers in order to obtain images of the internal structure of granular and solid materials with different acoustic properties. First, I would like to congratulate the authors for having developed such a unique and potentially useful imaging tool. The concept is rather groundbreaking and offers promising possibilities for application to follow the evolution through time of a certain class of analog models (saturated granular materials in water).

The references are complete and up to date. They cover a handful of recent publications devoted to analog modeling in general and are particularly thorough with regard to the topic of different techniques available for imaging the surface, lateral and internal structure of sandbox experiments through time. A useful recent addition would be the following review paper: Graveleau, Malavieille & Dominguez, 2012, Experimental modeling of orogenic wedges a review, Tectonophysics, 538-540, p. 1-66, doi:10.1016/j.tecto.2012.01.027

There are however a few fundamental points that require discussion and/or improvement in the manuscript before it can be ready for publication.

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. In Figure 4 there appear to be at least two strong (and possibly up to 4 or more) phases associated with each major interface. It is the same in Fig. 6 (and is especially pronounced for the top of the plexi-glass). And this seismic response is well expressed for all interfaces in Fig. 8 (top of the model, internal layer and top plexiglass). I could not find any description of this ringing feature in the text. The authors need to describe this more clearly (and quantify it in terms of the frequency content and duration of the source wavelet and the true thickness of each interface).

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. 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).

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/4/C690/2013/sed-4-C690-2013-supplement.pdf

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Interactive comment on Solid Earth Discuss., 4, 1317, 2012.