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Solid Earth
Discussions

Interactive comment on "The influence of detachment strength on the evolving deformational energy budget of physical accretionary prisms" *by* Jessica McBeck et al.

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Review of McBeck et al. "The influence of detachment strength. . ."

The paper describes sandbox experiments designed to shed light on the work done in deforming accretionary wedges. It continues a series of papers by the group in considering additional terms of the work budget. A secondary issue is the comparison between two endmember setups of the archetypical sandbox often referred to as "push" vs. "pull". It uses state of the art strain monitoring and force sensing techniques to derive at a more complete work budget formulation.

General comments:

I think this is a landmark paper for modern analysis of a classical experiment and a big step towards a complete work budget of sandbox experiments. The latter is of prime importance when arguing about the dynamic similarity with natural accretionary wedges. Also, giving the increasing resolution of experimental observations requires a re-assessment of the similarity in energetic terms. Only then, new interpretations and implications for nature are possible. I therefore think this paper is a timely and important contribution to be considered for publication after some minor issues are solved as suggested below.

One point that remains unclear to me is the role of Wint (internal deformation). You describe it as elastic strain energy, but is it recoverable? From my experience, I would argue that distributed plastic deformation (compaction) takes up quite a substantial amount (few percent) of the external work applied and should be considered a part of Wint? Could it be useful to add or split off another "damage" term describing the plastic internal work done? Also, the rigidity assumption (1 MPa) seems to me at the lower end (like for a low density, unconfined grain network rather than a well compacted, 5/6-sides confined sand pack) and increasing it could close the gap seen in the work budget. See my comments below on the respective section.

Since the paper has the potential to be an important contribution to the sandbox community, I would suggest adding a paragraph in the discussion where a comparison with natural wedges, and work done within them, is tried. I think it could be useful to do this to get an idea of how similar experiments are to nature in energetic terms and consequently what new inferences could be drawn from sandbox experiments for the prototype. Given the increasing amount of quantitative observations, the limits of interpretation should be well respected and I think this paper can help a lot in defining those limits.

A minor point is referencing the work of Malte Ritter. In the paper you cite Ritter et al. (2018) but it is not in the reference list, so I cannot judge which of his papers you mean. Since he published two papers in 2018 and one in 2016 (all Ritter et al.), that all fit neatly into this topic, I tried to sort out things below and make suggestions how to include his work here. I think his papers can serve to support your findings nicely.

Finally, I suspect you will publish data using GFZ Data Services in the framework of EPOS. Please contact GFZ data services soon enough to allow inclusion of the reference in this paper and register them as "assets" for this article. âĂČ Detailed comments:

Page 2 line 42: Ritter et al. (2018): you probably refer to Ritter et al. (2018a, see below). It needs to be added to the Reference list.

Page 4 Line 7 f: The "sandbox rheometry" models by Ritter et al. (2018b) give additional insights into the localization process in wedge experiments. They clearly observe an increase in total work done which is correlated with the onset of diffuse deformation prior to localization. This confirms your hypothesis "Prior to faulting distributed internal strain (Wint) may accommodate a larger percentage of the overall work budget than after thrust fault development,. . ." and may serve as reference here.

Page 5 Line 30 ff: What is the size of the glassbeads used?

Page 5 line 53 ff: There could be some more, basic information about the imaging setup used (SLR? How much MPx), treatment of distoration, calibration procedure, final resolution of incremental vectorfield/strain field (now in chapter 3.6), imaging frequency with respect to backwall movement (now on page 6 line 75f), software used for DIC. This is very useful information not only for evaluating the quality of your DIC analysis but will appreciated also by those people setting up new labs and interested in the way to do it.

Page 5 line 57: You refer here to the PHD thesis of Silvan Hoth for DIC. Actually, Adam et al. (2005) is the more proper reference for application of DIC/PIV to sandbox models.

Page 6 line 74: Why should there be a non-steady state backwall motion. Is this due to the motor, sticky parts of the compliancy of the force sensor-armed backwall? Could this be quantified? I ask because in the end you compare the force readings (at regular temporal intervals) with strain increments (not necessarily at the same regular temporal intervals).

Page 7 line 7ff: It is not entirely clear to me how the distributed strain (diffuse compaction) is related to elastic strain (which should be too small to be observed) given the rigidity of sandpacks? In Figure S2 you use quasi-linear segment of the strain hardening curve – is this really the elastic loading path? The values around 1 MPa appear, as you describe in the Appendix, too low. From every day life experience 1 MPa is what foam has as a Young modulus. What would happen if you consider 100 MPa in your calculation, do the numbers become unrealistic, or may this fill the gap in the work budget described in Ch. 5.1? In general it is not quite clear to me how you calculate Wint (you refer to Cooke and Madden but it would be good to recall it here with a formula and/or figure). When I understand correctly you use the curves such as in fig S2 to derive the stress/strain relationship (elastic modulus) and calculate strain based on the assumption that backwall push is transmitted all over to the opposite side of the experiment (i.e. ratio of backwall displacement and experiment length). If so, I would argue that actual strain is underestimated as experiments with force sensors on both sides (Maillot et al.) showed that force is transmitted only at later stage of such an experiment, when the decollement is closer to the opposite side.

Page 8 line 41ff: The kinematic compatibility assessment is highly appreciated but it may be better placed into the appendix because of the technical character. Probably it is OK if you put the conclusion ("Assessment of the accuracy and precision of the method results in a resolution of incremental vector field of about XX px / YY mm") in chapter 3.2.

Page 8 line 55 ff: To better understand the general model evolution I suggest to prepend a short qualitative description of the evolution of the experiments (sequence of faulting, how many thrust in total,. . .) before starting the detailed description. See also comment on Fig3 a below.

Page 9 line 70: Ritter et al. (2016) /not (2018) is the actual reference for the weakening behavior of sand and glassbeads faults (also consistent btw with lower absolute stress drops in glassbeads compared to sand).

Page 10 line 22f and 26f: There seems to be redundancy here!?

Page 11 line 31: Fig. 4C not 5C

Page 12 line 74: Why is Wgrowth not considered here, is it impossible to constrain from the setup?

Figure 3

A: - text: “glass bead detachment”; - it’s a bit busy with the combination of 4 setups. Either a different colorcode and bigger, or two plots? - I don’t quite understand: Are the peaks labelled fore- and backthrust secondary peaks following the “first thrust pair” peak. In other words: Do those fore-and backthrusts represent the first thrust pair? From the text it appears that these are new thrusts forming after the first pair? To clarify in gnereal, I suggest to add a sequence of images showing each stage of new thrust formation covering the full experimental run (the 1000 seconds), probably in appendix? Or a movie of each experiment? And a short qualitative description of the evolution of the experiments in chapter 4.

B-E: - The light vs dark blue is difficult to see. I suggest to use more different colours.

Figure 4:

- Also quite busy the figures and the different markers and their assignment to experiments and view are not easy to capture.z - Maybe use two transparent bands instead 4 lines to indicate the phase of localization. - Maybe display experiment in individual panels.

Page 14 line 34f: I suspect you will publish your data with GFZ Data Services in the framework of EPOS. I so, the sentence should be “Data. . .. Have been uploaded to the GFZ Repository published open access in McBeck et al. (2018)”.

References:

Adam, J., J.L. Urai, B. Wieneke, O. Oncken, K. Pfeiffer, N. Kukowski, J. Lohrmann, S. Hoth, W. van der Zee, J. Schmatz, (2005), Shear localisation and strain distribution during tectonic faulting - New insights from granular-flow experiments and high-resolution optical image correlation techniques, Journal of Structural Geology, 27(2), 183-301,doi:10.1016/j.jsg.2004.08.008.

Ritter, M., K. Leever, M. Rosenau, and O. Oncken (2016): Scaling the Sand Box - Mechanical (Dis-) Similarities of Granular Materials and Brittle Rock, J. Geophys. Res - Solid Earth, doi.: 10.1002/2016JB012915.

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