*Response to Revisions*

**Dear Editor van Dinther, Dr. Bauville and Dr. Rosenau,**

**Thank you for these careful revisions. We have made several changes following your constructive comments. The most significant of these changes includes a new supplementary figure (Fig. S5) that shows how frictional work changes when we assume varying ratios of principal stresses (in which the tectonic normal compression exceeds the lithostatic), and how internal work changes when we use different elastic moduli to convert strains to stresses.**

**We respond to individual comments point-by-point below with bolded text. We number our responses for clarity, continuing from the numbering scheme of the revisions of Dr. Bauville.**

**Sincerely,**
**Jessica McBeck**

***Comments from Referee:***

**M. Rosenau (Referee)**

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Received and published: 15 October 2018

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.

**16) Indeed, $W_{int}$, as calculated, would be recoverable. $W_{int}$ is calculated as the volume integral of the strain energy density (SED) in the region outside the faults in this analysis, and in previous work (e.g., *Cooke and Madden*, 2014). Some portion of the stored strain within the sandpack between the faults may be recoverable upon loading and unloading; such recoverability accounts for the drop in external force upon development of new thrust faults. The off-fault strain measured in the experiments captures the elastic, as well as inelastic and plastic, strain. However, by using elastic constitutive properties to estimate the stresses from the strains, the estimated $W_{int}$ reflects the elastic strain energy density. We have now specified this point in the text (lines 228-230).**

**17) We have now added an additional figure showing how the estimates of $W_{int}$ vary with differing assumed effective elastic moduli (Fig. S5).**

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.

**18) We have added discussion of these important points, including potential differences between the experiments and crustal prisms, and the limits of potential interpretations (new Section 5.2).**

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.

**19) We have now added the appropriate reference, and included additional references to this work.**

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.

**20) We now include the doi to the GFZ data repository for this work.**

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.

**See #19 above.**

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.

**21) This is a good point, and we have added it to the paper (line 112-114).**

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

**22) We have now added this important detail to the paper (line 135).**

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.

**23) We have now added these important steps to the Methods section (lines 171-176).**

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.

**24) We have replaced this reference with the more appropriate one.**

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).

**25) The non-steady backwall motion seems to arise from the screw motor, and not from the sticky parts of backwall-force system. At the slow motor speed used here, we could observe by eye the slowing and speeding up of the motor. So, these differing rates influence the force measurements to the same degree as the strain measurements (observed through DIC). We now include these details in the text (lines 191-194).**

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.

**26) See #16-17 above, and the new supplemental figure (Fig. S5). In addition, we have included further discussion of potential errors in the effective stiffness estimate in the supplementary information. The low apparent stiffness of the sandpack reflects the ease of the sand grains to rearrange rather than the stiffness of the sand particles. For this reason, the relatively low value of stiffness does not surprise us. In the supplemental information, we now discuss how using a shorter compaction length (rather than the full length of the sandpack) impacts the calculation of internal work. Using a length that matches the extent of the high contraction region near the backwall (20 cm) reduces the effective stiffness estimate, and consequently reduces the estimate of internal work. This reduction does not change our general conclusion that internal work comprises a small portion of the energy budget.**

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.

**27) We feel that this new approach, although technical in character, deserves to be mentioned in the main text because of its innovation. Many researchers may find this to be a beneficial approach to assess to robustness of their DIC results.**

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.

**28) We have now added further description of the general evolution of the experiments (line 282-286), including schematics in Fig. 3.**

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).

**29) We have now provided the correct reference.**

Page 10 line 22f and 26f: There seems to be redundancy here!? Page 11 line 31: Fig. 4C not 5C

**30) We have made these corrections.**

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

**31) We cannot robustly estimate the change in shear stress along the sliding faults, so we cannot provide confident estimates. In Section 5.1, we discuss how the contribution of $W_{seis}$ and $W_{grow}$ could contribute to the work budget deficit.**

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.

**32) We have now improved the layout and coloring of this figure, including schematic images.**

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.

**33) We agree that this figure can be confusing at first. However, using two transparent bands increases the complexity because these bands overlap on the plots, producing apparently 3 bars. In addition, we would like to plot the pairs of experiments together in order to better compare the differences and similarities between experiments with glass and sand detachments.**

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)”.

**See #20 above.**

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 distribu- tion 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.

Ritter, M.C., M. Rosenau, O. Oncken (2018a): Growing Faults in the Lab: Insights into the Scale Dependence of the Fault Zone Evolution Process, Tectonics, doi: 10.1002/2017TC004787

Ritter, M.C., T. Santimano, M. Rosenau, K. Leever, O. Oncken (2018b): Sand- box rheometry: Co-evolution of stress and strain in Riedel– and Critical Wedge– experiments, Tectonophysics, Volume 722, 2 January 2018, Pages 400-409, ISSN 0040-1951, doi: 10.1016/j.tecto.2017.11.018.

——————end of review————————

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2018-93, 2018.