Solid Earth Discuss.,
https://doi.org/10.5194/se-2018-45-RC2, 2018

Solid Earth
Discussions

Interactive comment on "To what degree the geometry and kinematics of accretionary wedges in analogue experiments is dependent on material properties" *by* Ziran Jiang et al.

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Received and published: 6 September 2018

Dear Authors,

I have received two independent reviews (one uploaded in the system, the other attached below) of your submitted ms. Despite the research topic is relevant since it is thought to improve the state of knowledge regarding the physical properties that control the evolution and mechanical behaviour of accretionary wedges at convergent plate margins, both reviewers indicate that much work is needed to render the manuscript suitable for publication. I concur with this evaluation.

In addition to comments made by the reviewers and by Dr. Roseneau (see SC1 and SC2), I would note these major points that should merit consideration when preparing a revised version of the manuscript: (i) The scope of the manuscript and the geological problem is not fully described/introduced. A vast literature exists regarding the structural, mechanic and dynamic controls on the accretionary wedge evolution at convergent plate margins. The idea to develop new achievements regarding the state-of-the-art dealing with the mechanics of accretionary wedges is thus relevant and challenging at the same time. Therefore, the scope of the study and its relevance should be made more explicit; (ii) Existing literature on the analogue and numerical modelling of thrust and fold belts and accretionary wedges is not adequately quoted. Fundamental papers on the mechanics of the thrust and fold belts (introducing the "critical taper" theory), such as Davis et al. (1983, JGR), Dahlen et al. (1984, JGR), Dahlen (1990, Ann. Rev. Earth Planet Sci) are not quoted; (iii) As it stands, the manuscript appears as poorly organised, often mixing data presentation with inferences and presenting data in the discussion section. Consequently, the presentation of the experimental results appears to be rather chaotic and non-systematic; (iv) Results should not be compared only to nature, rather (and above all) to existing analogue and numerical modelling studies [see e.g. Morgan (2015, JGR), Gray et al. (2014, J Struct Geol), Ruh et al (2012, Tectonics), Simpson (2011, Tectonophysics), Bose et al. (2009, J Struct Geol), Gutscher et al (1998 , JGR)] and critically discussed. I would emphasise that the comparison with the natural cases appears, at least for the Zagros case, too simplistic, since ductile rheologies should be also taken into account. As general remark, your experiments suggests that an in-depth strength drop affects model evolution and style of deformation. This should be compared, for example, with the existing literature models dealing with brittle-viscous rheologies rather than simply to natural cases. Furthemore, a more deep analysis of the implications of your results in terms of the critical taper theory could increase the significance and the potential impact of the presented results. Potentially relevant (but not fully made explicit) are also the outputs of this study in terms of the structural styles of the accretionary wedges in nature. In other terms,

broader impact of the presented research results is largely (and critically) depending on how the outcomes of the sensitivity analysis of main physical parameters investigated in this study can be extrapolated to improve the knowledge of the main physical properties that control orogenic wedge evolution at convergent plate margins.

Finally, despite I am not a native English speaker, I found the text rather rough in some parts. Therefore a revision of the English text is necessary to improve readability of the manuscript.

I have uploaded an annotated version of the submitted pdf file where the above points are further detailed.

Based on the above, since the required amount of revision is a major one and a general re-focusing and re-organisation of the manuscript is necessary, my decision is to reject the manuscript at this stage.

Sincerely, Federico Rossetti

REVIEWER#2 (formal review)

Dear editor,

In their manuscript entitled "What degree the geometry and kinematics of accretionary wedges in analogue experiments is dependent on material properties", Jiang et al., report on a study consisting of a set of analogue experiments on accretionary wedge formation, for which more or less the same set-ups but different materials have been used. Further, the thickness of the basal detachment was varied. Experiments were conducted in the "push mode".

The experiments of this study are very similar to each other and parameters have been tested the variation of which will not result in large but potentially significant differences. Therefore, physical properties of the analogue materials used (sand with two different grain size spectra, micro glass beads) need to be known with high accuracy, experimental set-ups need to be extensively described, and observations need to be

thoroughly discussed.

Several parameters like grain size and aspect ratio, bulk density, friction coefficients and cohesion have been measured e.g. from SEM photos or with the ring-shear tester at GFZ, Potsdam. Whereas Table 1 provides a good overview about the results of the measurements, the table lacks the presentation of errors. For the frictional parameters, these are included in the figure showing measurement results and regression lines, but lacking at all otherwise. Although the authors stress that besides the impact different materials with different properties have on the evolution of experimental wedges they are interested in the "human factor", they do not provide any information on how often they repeated their measurements. Similarly, they do not report on temperature, humidity, and other properties they mention in their introduction. Cohesion has been found to be a parameter difficult to estimate. However, the authors do not report on repeated measurements and data processing. It is very confusing that they present a certain range of each parameter when describing their materials while it is not clear if they talk about the variability among various materials.

The experimental set-up is described, but a figure is lacking. What kind of lubrication was used? There were two experiments with only sand – but what was the material the bottom of the experimental box was made of? There will be always a "detachment" as with pushing the backwall you force the material to detach. Measuring angles from cross-sections is not very accurate – what was the error here? Can you really measure angles as accurate as 0.1°? Which points did you use to measure the taper? This is important as the authors intend to identify the impact of rather small differences of material properties on wedge growth. Have the experiments been repeated?

When describing the results of the experiments in terms of how the wedges looked like after 40 cm of shortening, the authors already interpret differences they find – this is misplaced as it should be a theme of the discussion. When describing your results, please stay neutral! You say that your wedges grew self-similarly – however, you do not show any sequence of images. And keep in mind, that a wedge needs to consists

of at least a few thrust slices before if behaves according to the critical taper concept and that some parts of a wedge are not in a critical state (e.g. Lohrmann et al., 2003). Thus, your arguments would be much stronger if you had applied more shortening. You very often say that the wedge reached a critical slope – how do you know without having performed a critical taper analysis?

A main difference between your experiments "without a detachment" as you call it, and the experiments which have a thin basal layer made of glass beads is the in the latter experiment the basal detachment has some topography and will evolved in the weaker material. Thus, you need to be very cautious when comparing your experiments.

The role of the basal detachment has been addressed many times before, e.g. Gutscher et al., 1996; Contardo et al., 2011, ... Thus, thorough discussion is possible and relevant studies need to be cited.

Analysis of the experiments presented here seems to be restricted on purely geometrical parameters. Therefore, you cannot claim anything about kinematic evolution – to do so you need to monitor fault activity. Is there any difference between the temporal evolution of fault activity and fault reactivation between the experiments of this study?

A sufficient size of the sample is crucial when performing statistical analyses – is this the case here? I doubt this.

Basically, studies focusing on very thoroughly analyzing experimental wedges and elucidating the impact of relatively small parameter variations and relatively little varying set-ups etc. are very important as they provide insight into the relative importance of potential control parameters, the robustness of experimental set-ups, etc. Such studies offer data to better understand the similarities, differences, and variability between natural wedges. Further, such studies are valuable as they also confirm the feasibility of an experimental set-up. Thus, the present study would be of importance, however, the present version of the manuscript is not at all ready for publication. Several additional analyses should be performed as indicated above. Further, experiments need to be

described in more detail. Errors should be quoted with much more cautiousness. The same, cautiousness is a pre-requisite when stating e.g. that a wedge is in a critical state. To do so without a proper CT-analysis is a no go.

How to proceed? I strongly recommend to reject this manuscript in its present version and ask the authors to use the comments and reviews they get to improve their study. This will in any case mean they should perform more analyses, and may also mean they need to repeat some experiments and apply more shortening. However, this effort should be worthwhile as then they could come up with a proper manuscript. As it was quite difficult to read the manuscript due to the poor language, I also highly recommend proofreading by a native speaker.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2018-45/se-2018-45-RC2-supplement.pdf

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