EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-1014-RC1, 2022

Comment on egusphere-2022-1014

Pablo Granado (Referee)

Referee comment on "Analogue modelling of the inversion of multiple extensional basins in foreland fold-and-thrust belts" by Nicolás Molnar and Susanne Buiter, EGUsphere, https://doi.org/10.5194/egusphere-2022-1014-RC1, 2022

Dear Nicolás, dear Susanne,

it has been a pleasure to review your ms entitled "Analogue modelling of the inversion of multiple extensional basins in foreland fold-and-thrust belts". The set up of the modeling and the rationale are sound and interesting. It has great imporance for several projects in the foreland of the Alps for instance, where radioactive material may be store in those kind of grabens in a nearby future.

There are few comments on the edited pdf version of the ms that you will find, and that I encourage to address. In general the ms is well written, but I have ecountered several lines that I may have not been able to properly understand.

I will start with the illustrations. I think the ms will benefit from including a rheological profile attached to the Mohr circles. I failed to understand if the syn-rift in fill is made up exclusively of microbeads, or not.

I also think the ms will benefit from showing the images of the sandbox models, without the differential and cummulative strains on top. These can go aside. It is hard to judge the results of the modelling without the models them selves available. At the end, we, as modellers, would like to see the models, the results.

I also miss the results of the extensional phases, along with their differential and cummulative strains overlapped. This is very important to have the pre-shortening configuration (with its inherited fabric) properly characterised, and to be able to judge the role of inheritace. Same as for figures 8 and 9. These are very nice-looking colored cartoons, but not the real results of the model.

There are two comments that I am concern with:

Line 119. "Both sand and microbeads obey the Mohr-Coulomb criterion, where a time-independent rheological behaviour can be assumed" . I understand you mean a non-strain rate dependent rheology, as stated at the footnote of Table 1. However, this statement has been questioned long ago by two works, those of Lhornamm et al. 2003 and Adam et al. 2005 that propose behaviours that depart from the typically adscribed to coulomb materials. Can you comment on those, and question your basin inversion results based on their works?

Line 200. "With ongoing shortening, the thicker sand pile will require an increase in σ1 to maintain the same shortening
rate (Fig. 3e)". Do you mean differential stress (Sigma 1 minus Sigma 3), rather than shortening rate?

Comparison with natural case studies.

Schmid et al. 2008 propose the subduction of the Adriatic lithosphere beneath the European one (change in polarity across the Eastern Alps). I do not personally agree with their interpretation since is conjetural and based on the imaging of geophsycal anomalies that do not fit with overall paleogeographical, surface, and shallow subsurface data and models; maybe you could find other reference that suits better to your modelling. Inverted and transported half-grabens are better outcropping in the French Western Alps. I have provided additional references that could suit better to your work.

Other than this, I think the ms is good for publication after addressing some minor revisions and considerations in the annotated pdf file

Best regards

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Please also note the supplement to this comment:
https://egusphere.copernicus.org/preprints/2022/egusphere-2022-1014/egusphere-2022-

1014-RC1-supplement.pdf