The manuscript (MS) discusses the influence of analogue model material properties on the resulting structures in a simple strike-slip experiment. Four different materials have been used: a sand-clay mixture, dry quartz sand, wet quartz sand and GRAM 2% (a mixture of quartz sand, hemihydrate powder and water). These four materials have more or less identical internal friction angles at peak strength (between 35° and 38°), but have notable differences in (extrapolated) cohesion. The mechanical properties of the materials have been determined using a ring-shear tester. In dynamically scaled experiments, cohesion and density ratios determine the length ratios. Hence, assuming a natural prototype with specific natural cohesion and density, one can determine the length scaling factors for each of the four materials used: 1 cm in the analoque model equals 927 m in nature for the sand-clay mixture, 637 m for dry sand, 114 m, for wet sand 22 m for GRAM 2%. Thus, by repeating the same analogue model experiment using different materials, one can potentially obtain insights in the type of structures that might form in nature at different scales.

Analogue models have been widely used to study different tectonic settings. However, few studies have investigated the impact of the material properties on the analogue modelling results. Hence, the study by Massaro et al. is a welcome contribution and of scientific significance. The approach and applied methods are overall scientifically valid. However, there are – in my opinion - some inconsistencies/errors in the presented data and analyses that need to be addressed (see below for further details). The overall presentation quality is good, although the legends in some of the figures is almost not readable, and I have suggsted to re-do several figures and re-arrange some of them to make them better readable.

Full review:

1. Does the paper address relevant scientific questions within the scope of Solid Earth
Yes.

2. Does the paper present novel concepts, ideas, tools, or data?

Other papers exist, in which different analogue materials have been investigated using a ring-shear tester. However, this paper then uses the ring-shear test results to select four materials with differences in density and extrapolated cohesion, that are subsequently used in one particular type of experiment: a simple strike-slip experiment. Using dynamically scaling ratios and assuming a specific natural prototype with given cohesion (26.3 Mpa) and density (2.37 gcm^{-3}), the differences in cohesion and density values of the analogue materials then determine the differences in length ratios, i.e. the ratio in length between model and natural prototype. Thus, the four models allow a multi-scale comparison of the kinematic and dynamic characteristics of an evolving strike-slip shear zone.

3. Yes, substantial conclusions are reached, e.g. that several quantified geometric and kinematic parameters show a positive relation with model resolution.

4. Yes, scientific methods and assumptions are valid and clearly outlined.

5. Yes, results are sufficient to support the interpretations and conclusions. It would be nice if the authors could add a natural example, showing structures from a natural strike-slip fault zone zooming in to different resolutions and comparing them with the model results.

6. Partly; there are some inconsistencies/errors that need to be corrected, but in my opinion they do not influence the overall conclusions. I suggest that the authors use vorticity instead of shear strain in their analysis (Fig. 8 and 10), as vorticity is better suited to determine the strain along the strike-slip faults as it is independent of the coordinate system used. But, as I see it, such a change would not invalidate the main interpretations and conclusions.

7. Yes, the authors give proper credit to related work and indicate their original contribution. There is one recent paper (published 16 December 2024) that the authors might want to include in their reference list and in their Table 1: Gonzalez-Munoz et al., 2024, Solid Earth 15, 1509-1523. And another one by Mourgues and Cobbold (2003) that they could use when discussing extrapolated cohesion from ring-shear tests.

8. Title reflects contents of paper.

9. No, I found the abstract too general. It should include more of what is mentioned in the conclusions, e.g. mention structures/parameters show a positive relationship with the model resolution (vertical relief, shear zone width, etc.)

10. Yes, the overall presentation is well structured and clear

11. Language is good, fluent and to the point.

12. Some of the formulae/symbols need to be better explained: what exactly is meant by d_l? What is the reference coordinate system used, i.e. how is x- and y-direction oriented?

13. I have indicated below the changes/clarifications that need to be made to figures and tables.

14. References appropriate

15. Not applicable.

Comments to text, figures, tables:

It has to be noted that the inferred cohesion values are extrapolated from the experimental ring-shear data (lowest normal load is 2000 Pa), and there is debate on whether a linear extrapolation at very low normal loads is justified – as assumed in this paper - (e.g. Mourgues & Cobbold, 2003, for a different view on dry granular materials). I believe this issue should be discussed in the text.

Figure 3 shows an inconsistency; the blue and red best-fit regression lines for the sand-clay mixture should be inverted. Figure 2 clearly shows that critical values at reactivation strength are systematically higher than at dynamic-stable strength for all applied normal loads for the sand-clay mixture.
This then has also consequences for Figure 4a: the coefficient of internal friction is 0.67 at reactivation peak strength and 0.61 at dynamic-stable strength. And consequences for Figure 4b, the calculation of the strain softening.

Figure 4a also shows the wrong peak strength friction coefficient values for the wet sand at peak strength and at reactivation peak strength. In Figure 3 these values are nearly the same at peak strength and at reactivation peak strength (both 0.69).

In view of these inconsistencies/errors I suggest that the authors carefully go through their figures, tables and text (section 4.2 "Cohesion and frictional properties") and correct accordingly. Table 4 for example also needs to be corrected for the values given for reactivation and dynamic friction angles/coefficients and for strain softening for the sand-clay mixture.

As far as I can tell, the inconsistencies do not influence the length ratios, as it seems they are calculated using the correct (linearly extrapolated) cohesion values and densities.

The dynamic evolution of the structures in the four experiments, each with a different analogue model material, is illustrated with incremental displacement (horizontal and vertical) and shear strain data in Figures 7, 8 and 9. In my opinion it is unfortunate that the increments between the four experiments shown are not the same, i.e. in the sand-clay model the incremental step size is 0.21 mm (i.e. horizontal displacement of the base plate), in the dry sand model 0.42 mm, in the wet sand model 0.097 mm and in the GRAM 2% model 0.061 mm. I suggest to use (nearly) identical incremental step sizes in all four models and redo Figures 7, 8 and 9. Also, it would be nice to extend until 59 mm of imposed displacement, by showing panels in rows for 50 and 59 mm .

Furthermore, to me it is not clear exactly what exactly is plotted in Figure 7, and the color legends are so small, that they are partly unreadable. Also the reference system needs to be indicated in a figure somewhere. What is the x-direction (I presume parallel to the long sides of the model), what is the y-direction (I presume parallel to the short direction)? In the experiment, the upper half moves to the right, while the lower half moves to the left, but the colors indicate similar colour gradients from the centre upward or downward (for sand-clay and dry sand; the symmetric experiments). Is it possible that these are the square root values of ($D_x^2 + D_y^2$)? Please indicate. It might in fact be more intuitive if the values of D_x are shown in Figure 7 with displacement vectors (a selection of them).

In addition, I would use exactly the same color legends for all panels up to 10 mm of imposed displacement in Figures 7, 8 and 9 (this would also allow to have a single large color legend that is readable). Then it is also easier to compare the evolution up to 10 mm of imposed displacement.

I would then leave a bit of space (horizontally) with the lower two rows of panels showing 20 mm and 40 mm of imposed displacement. And I would add a third row of panels with 30 mm of imposed displacement. For the lower three rows, I would – if feasible – also use a single large color legend. Why do you not continue with the panels until 59 mm of imposed displacement in Fig. 7, 8, 9 and 10? You have all the information for all the four experiments

Fig. 8 shows the incremental shear strain. I suggest to use incremental vorticity instead. In contrast to shear strain, vorticity is not dependent on the orientation of the coordinate system, which is crucial when quantifying the deformation along faults that strike obliquely with respect to the coordinate system (e.g. Cooke et al., 2020), as is the case in the experiments shown in this manuscript. See also paper by Gonzalez-Munoz et al. (2024). Nefertheless, I believe that the overall patterns in Fig. 8 will remain largely similar, as the strike-slip faults in the model strike at low angles to the x-direction. However, it would be neat if vorticity were to be used.

Fig. 10 shows the total shear strain after 40 mm of imposed displacement. I would replace this figure with the total vorticity after 40 mm.

Fig. 11. Why do you show the first three panels only until 40 mm of imposed displacement. You have all the data until 60 mm.

I would reorganise this figure to make it more readable, e.g. make 6 panels in Fig. 11 with three panels in each row:
First row: incremental displacement, incremental vorticity and incremental vertical displacement (z-).
Second row. Total displacement, total vorticity and total vertical displacement
The remaining three panels of Fig. 11 (shear zone width, total number of structures, angles) could be placed below or even better in a separate Fig. 12.

Figure 6: I suggest to show structures only until 59 mm of displacement. Your analysis of the experiments only goes to max. 59 mm of displacement. This way, you would have a multi-scale overview incorporating four models at identical imposed displacement. And if you have a nice natural example with successive zoom-ins, you could make an extra Fig. 13 in the conclusions putting model and nature next to one another.

Further comments: Please indicate lab temperature and humidity, if known.

Text line comments:

Line 74: I would expect the permeability of the rock to increase towards the center of the fault system.

Line 138: leave out "accurately

Line 146: I am not sure whether papers in prep. are accepted.

Section 3.2. Please mention somewhere in the text the dimensions of the analogue model, including its thickness.

Line 281: add “imposed” between “maximum” and “displacement”

Line 284/285: why do you mention: “negative values indicating sinistral shear sense”. In all the panels of Fig. 8 I do not see any evidence for sinistral shear sense.

Line 292: they are “vertical shears” not horizontal ones. You can maybe specify that these “vertical shears” strike parallel to the boundaries of the moving base plates.

Line 306: add “incremental” in between “maximum” and “shear strain”, also in line 316.

Line 325: I don’t see a antithetic R’ shear (sinistral sense of shear) in the figure. I admit that the figure panel is very small, but I do see yellowish-reddish colors indicating dextral. So, same goes for the “highest sinistral shear” mentioned in line 326. Maybe you need to enlarge the panels.

Line 344: I am not sure if I understand how you can normalize to one incremental step size value in view of uncertainties using a manucal winch.

Line 359/360: What is meant with “peak extension”?

Line 371/372: Any idea why there is such a large difference in the angles of early R shears in GRAM2% with respect to the models with the other materials?

Line 383: truly “self-similar scale invariant geometries? The angles of early faults for GRAM2% and other materials are very different.

General: order of references in the MS: either chronologically or alphabetically; depending on journal requirements