Naples, Italy. February 14th, 2025

***Subject*: Responses to the Referee Comments on 'Strike-slip kinematics from crustal to outcrop-scale: the impact of the material properties on the analogue modelling'**

***To*: Solid Earth Referee**

Dear Referee,

First of all, my co-authors and I would like to sincerely thank you for the extremely useful comments and suggestions that helped us to significantly improve the paper.
All the critical comments have been addressed in the revised version of the manuscript, and the suggestions were fully taken into account, as explained in the following point-by-point reply.

We are confident that this revised version of the paper has significantly improved in line with the Reviewers' comments, and we sincerely hope it will fully satisfy the high standards required for publication in Solid Earth.

Sincerely,

Luigi Massaro
Research Fellow (RTD-a)
Dipartimento di Ingegneria Civile, Edile ed Ambientale - DICEA
Università degli Studi di Napoli Federico II
Piazzale Tecchio 80, Napoli 80125

Reviewer #2

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 analogue 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.

R: We do agree that such multi-scale comparison between models and natural examples would be useful. However, this is something similar to what we proposed in the paper Massaro et al., 2022 with one natural example in comparison to GRAM models. Providing this comparison at four different scales is not easy (in terms of finding the ideal photos of strike-slip shear zones) and a bit beyond the scopes of this study.

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.

R: Thank you for your suggestion. We acquired the vorticity images for all the experiments, and we can confirm that they are almost identical with respect to the shear strain. However, we will provide the images of the vorticity as supplementary material, and we added the method of calculation of the vorticity in the revised text (Chapter 5 "Experiment results"). Although we appreciated your suggestion about the vorticity as better suited to determine the strain along the strike-slip faults, we decided to show the shear strain (ε_{xy}) as it can give the information on the right-lateral or left-lateral movement along the faults, which is critical to define the P' shears.

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.

R: thank you for the suggestion, we added them to the revised text. Regarding the extrapolated cohesion, please see the point below.

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.)

R: The abstract has been reworked as suggested in the revised text.

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 dl? What is the reference coordinate system used, i.e. how is x- and y-direction oriented?

R: d_l was used to indicate the total displacement obtained from DIC analysis. However, in the revised text we changed it from d_l to d_t to make it clearer (throughout the text and in fig. 6 labels).
The information about the reference coordinate system has been added in section 5: "In our models, the x-axis and the y-axis correspond to the long and to the short side of the model, respectively.".

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.

R: this is a very interesting point, widely discussed in the literature.
We added a discussion on this in the revised text (Chapter 4.2), explaining that "Several studies demonstrated that under low normal stresses (250-400 Pa for Schellart (2000), 30 Pa for Mourgues and Cobbold (2003) the failure envelope is not linear but shows a convex-upward shape. Therefore, linearly extrapolated cohesion values at low normal stresses are overestimated (Schellart, 2000; Mourgues and Cobbold, 2003; Panien et al., 2006; Dooley and Schreurs, 2012). However, in this study the lowest normal load applied in the ring-shear tests is 2000 Pa and, therefore, the cohesion values are linearly extrapolated".

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.

R: thank you for your comments. The sand-clay graph in figure 3 has been corrected (the reactivation and stable-sliding values had been erroneously swapped) and, therefore, also figure 4 was corrected accordingly. The correct results were updated throughout the text and in the tables in the revised text.

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.

R: in the revised manuscript, we re-elaborated the DIC images with a fixed incremental step size of 0.42, or nearly 0.42 when not achievable. The latter case occurred a few times for the wet sand and GRAM experiments since they were manually displaced with the hydraulic winch and, therefore, have irregular displacement rates. Also, we added additional panels after 40 mm and will provided the videos of the full experiments as supplementary material.

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 sandclay and dry sand; the symmetric experiments). Is it possible that these are the square root values of (Dx2 + Dy2)? Please indicate. It might in fact be more intuitive if the values of Dx are shown in Figure 7 with displacement vectors (a selection of them).

R: the experiment figures have been corrected and the legend text increased, as suggested.
The reference system used in the models has been added in Chapter 5.
In fig. 7 is shown the total displacement (D tot, and not Dx) from 0 to maximum value, therefore it does not discern between sinistral and dextral directions of displacement.

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

R: thank you for this suggestion, we did try in the first place to use the same colour legend for all panels, but since the values achieved at the different displacement and between the different materials are very different, what you have is a huge loss of information when the colour bar is much higher than what happens in a specific frame. Therefore, we decided to maximise the visualisation of the deformation in each frame by using individual colour bars. In this way, the colour bar is also giving a quick information on the maximum displacement, shear strain, and z-displacement achieved at that level of imposed displacement. However, we added more panels in the figures 7, 8 and 9.

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.

R: Thank you for your suggestion. As said in one of the points above, we will provide the images of the vorticity as supplementary material, and we added the method of calculation of the vorticity in the revised text (Chapter 5 “Experiment results”). Although we appreciated your suggestion about the vorticity as better suited to determine the strain along the strike-slip faults, we decided to show the shear strain (ε_{xy}) as it can give the information on the right-lateral or left-lateral movement along the faults.

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.

R: in the revised manuscript, fig. 11 has been completely reworked since the DIC images of figs. 7, 8, and 9 have been re-elaborated with the same incremental step size. Also, we included the data until 60 mm and reorganised the panels following your suggestions.

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 multiscale 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.

R: the 59 mm of displacement was a limit imposed by the GRAM experiment, so for comparison reasons we showed the models only up to that value. However, the other experiments were deformed for more than 59 mm and we think that may be useful to show at least in one figure the final model for each material. For the multiscale overview between the four models we have the figures 7, 8, 9, and 10 where all the panel are shown with the same values of imposed displacement.

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

R: the temperature and humidity of the laboratory where the mechanical tests and experiments were performed have been stated in the Chapter 3.3 as suggested.

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

R: We referred to the increase in the damage zone respect to the inner fault core. However, it was not clear, and we modified the sentence in the revised text.

Line 138: leave out "accurately

R: modified as suggested.

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

R: Corrected in the revised text.

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

R: we specify the dimensions of the models (100 x 60 x 10 cm) in the experiment set-up paragraph 3.2 (3.3 in the revised text) and in Table 3.

Line 281: add "imposed" between "maximum" and "displacement"

R: throughout the text we use "imposed displacement" to refer to the value of displacement externally applied to the rig (with the winch of the motors). In this case it is the maximum value calculated in that frame, therefore we added the term "incremental" to make it clearer.

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 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.

R: here we mention it as we are explaining the various strain properties analysed from DIC. However, it is not outstanding, and we noticed that is hard to see in the figures uploaded in the word document (we are confident that, if published, the high-resolution .tiff images will be much clearer), but in GRAM panels (since 6 mm of displacement) there are some faults with purple, blue, and light blue colour indicating sinistral shear strain.
However, we have re-elaborated the DIC images calculating a common incremental step size as suggested in a point above.

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.

R: corrected in the revised text.

Sito Web: www.dicea.unina.it | e-mail: dicea@unina.it | Pec: dip.ing-civ-ed-amb@pec.unina.it | C.F./P.IVA:00876220633

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

R: corrected as suggested.

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.

R: in the revised text, the experiments results were recalculated with similar incremental step size, therefore this normalization is no longer needed.

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

R: We meant the moment when the early R shear reach their maximum length. We modified it into "maximum length".

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?

R: the two low-cohesion materials (dry sand and sand-clay) show very low angles with respect to wet sand and GRAM. Therefore, the influence of the cohesive strength has to be critical in this.

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

R: Here we are referring to the natural system and to the fact, broadly discussed in the literature, that these systems have self-similar geometries across the scales. Therefore, one could say that there is no point in analysing them at different scales. We want to underline that the generic geometry may be self-similar across the scales, but some differences always occur (as you say about the angles of the early faults) and the dynamic aspects that lead to the formation of such geometries can be better understood at different scales.

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

R: corrected.