Comments of Reviewer 2 (Anonymous)
Replies of Authors
Changes in manuscript

GENERAL COMMENTS

This manuscript provides a review mainly of techniques used in the lab for studying earthquakes via analogue modelling. As someone who is not directly involved in that branch of research, I found it difficult to muster much interest during most of the paper. I am curious about the topic, but I find the review too inwardly focused and disorganized. Many laboratory studies are mentioned, but the review only lists the technical aspects of these studies, ignoring (except in section 6) the insight they (hopefully) provided. To me, the best reviews summarize that insight so that not only the people directly involved in this line of research but also researchers in ancillary fields learn something by reading it.

Perhaps the key issue I have with the paper is lack of focus. The title promises a review of experimental modeling "across timescales". I would have expected the paper to address what these time scales are and how the analogue models inform our knowledge of them. I am left wanting. Instead, Section 1 and 2 summarize many of the approaches available to analogue modelling, section 3 introduces scaling, section 4 summarizes rheologies, and section 5 monitoring techniques. All these sections are useful to learn how to build a model, but what do they tell us about the timescales of earthquakes and seismic cycles? They give the tools, but no the results. Section 6 is the only one where results are summarized. The abstract mentions a review of "cornerstones" of development, which actually does describe the content OK (except we don't know why each study mentioned is important, as much of the paper is a just a list of works) but if that's the motivation for the paper, the title is misleading.

Section 6 is the closest to what I was expecting in this review. Specific studies are mentioned. In a few cases, details of the setup are included, but, importantly, the results are mentioned.

We feel sorry for having called possibly wrong expectations. We indeed intended to provide a review on the modelling tools rather than on the modelling results. The latter is just beyond a single paper as the field of investigations, even when focused on experimental approaches, is very wide and includes various communities (seismology, geodesy, geology) because of the multiscale nature of the process. We here tried (1) to bridge those communities and (2) give an overview of existing approaches. This results in a paper structure which necessarily has a technical focus less than a thematic one. The specific scientific results are critically assessed in dedicated review papers by experts (Scholz, Marone, Rosakis, Lei etc) to which we generally refer in the text for further reading.

Following this idea the paper is organized as follows: Intro, Overview, Scaling, Materials, Monitoring, Applications, and Conclusions. Ch 6 (Applications) highlight common results from the different approaches. However, not at the depth requested by the reviewer.

The main effort of this paper was categorizing the approaches and their technical aspects and make them visible side-by-side. The section on scaling is actually the first of its kind, same as the discussion of analogue rock rheologies in the given context.

We took special care of these two main issues (timescale and results) during revision of the paper which led to changes at various locations in the text.

We address the timescales more explicitly now in the abstract, intro and conclusion:

Abstract:
"Earth deformation is a multiscale process ranging from seconds (earthquake rupture) to Millions of years (tectonic evolutions). Bridging short- and long-term deformation is addressed experimentally for more than a century."

Intro:
"Deformation of the earth involves timescales ranging from nanoseconds (processes at the rupture tip) to hundreds of Millions of years (Wilson cycle) and all spatial scales from atoms to the earth itself (e.g. Ben-Zion, 2008). Such a multiscale process pose major challenges to observation in nature as the instrumental and historical records are too short to capture a significant amount of the evolution. Simulation is a way to overcome such limitations. However, our knowledge of the physics of earthquakes and earth deformation in general is necessarily incomplete and does not allow to setup realistic numerical scenarios. Non-uniqueness of numerical solutions to typical problems of earth deformation at various time scales (e.g. inversion of rupture kinematics or mantle rheology from co- and postseismic observations, respectively) is another limitation of computer models. Experimental approaches have been traditionally used to address physical problems like earthquakes and seismic cycles and, more recently, bridging the gap between short-term instrumental/historical observations and long-term paleoseismological/geological observations."

Conclusions:
"We presented an overview of experimental approaches to model earthquakes,seismic cycles and seismotectonic deformation. The processes involved are multiscale posing the challenge to cross time scales from seconds (seismic deformation) to Millions of years (tectonic deformations) both in natural observation as in simulation and experiment. Since natural observations are intrinsically limited in resolution and period of observation, simulation by means of analogue and numerical modelling are key to understanding multiscale processes. An experimental approach to multiscale problems seems most natural because experiments happen in a time and space continuum in contrast to numerical models which need to be discretized.."

We added better descriptions of setups including two new Figures which also show results of spring slider and fault block models discussed in the text:

e.g. 6.1

"King (1991, 1994) who showed that large events tend to roughen the stress distribution while small events smooth. Moreover, he found that large events are dissimilar (i.e. not characteristic) and that rupture nucleation is not were peak slip accumulates. The frequency-size distributions found by King (1991, 1994) have been Gutenberg-Richter-like except for the system-sized events which recur approximately time-predictably."

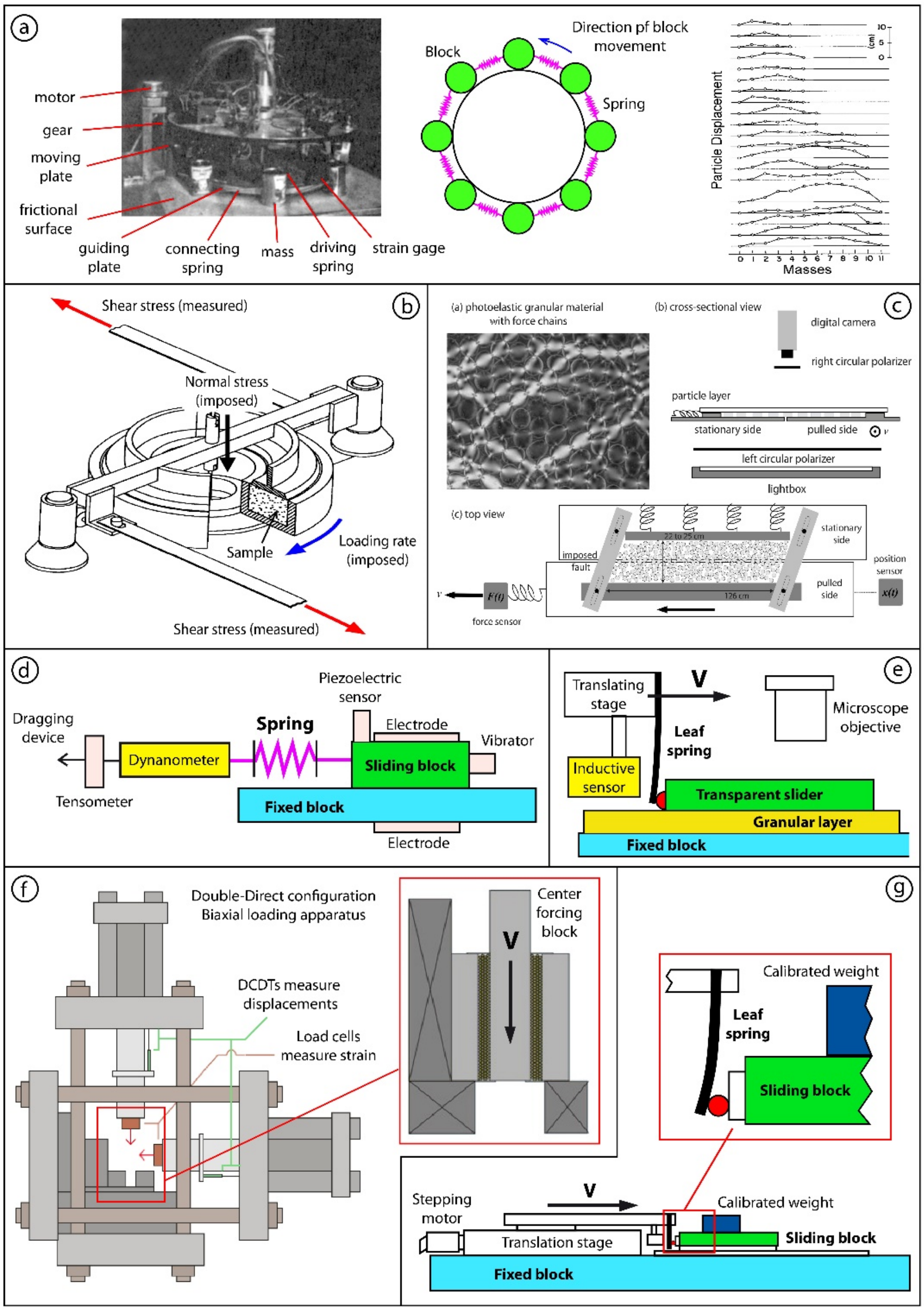


Figure 2: Examples of spring-slider model experimental setups and results: (a) Multiple spring-slider setup andexample of "rupture" by King (1991, 1994); (b) Schulze rings-shear tester used to characterize frictional properties of granular materials in this study (after Schulze, 1994); (c) simple shear setup and visualization of force-bridges in granular media by "see-trough" experiments of Daniels and Hayman (2008); (d) simple spring-slider setup used by Varamashvili et al. (2008); (e) Setup of "see through" experiments by Nasuno et al. (1998); (f) Double-direct shear configuration used to shear rods in various configurations by Knuth and Marone (2007); (g) simple spring-slider setup after Heslot et al. (1994).

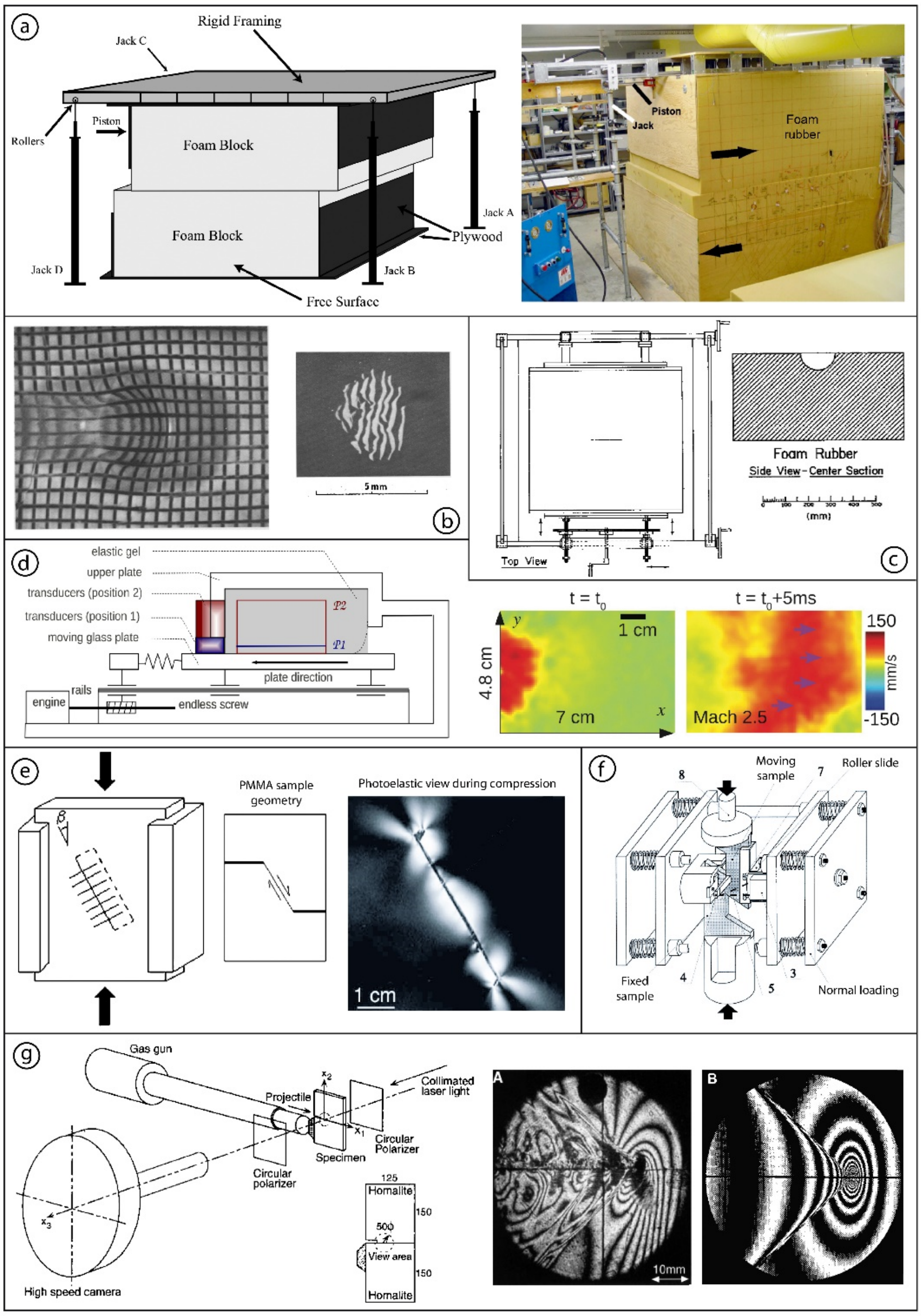


Figure 3: Examples of fault block model experimental setups and results: (a) Brune's foam block model as used in Brune et al. (1993); Anooshehpoor and Brune (2006) and Day et al. (2008); (b) "Schallamach wave" pattern as seen in sliding rubber block experiments by Schallamach (1971); (c) Setup after Archuleta and Brune (1975); (d) Setup and resulting rupture pattern from Latour et al. (2013b); (e) Setup and photoelastic pattern visualizing stress in the vicinity of a crack after De Joussineau et al. (2001); (f) Setup of Bouissou et al. (1998); (g) Setup and photoelastic fringe pattern associated with rupture in experiments of Rosakis et al. (1998) and Rosakis et al. (1999).

There is still vagueness and room for improvement, though. For example, Section 6.5 concludes the description of the Caniven et al. (2015) study with "The model results compare to numerical simulations of strike-slip fault earthquakes", leaving me wondering how they compared (well, I assume, but I can't be sure), to what specific aspects of these simulation the experiments can be compared,

In the case you are referring the comparability test are ongoing work and the sentence is a rather general one; we added some detail. We also added a more detailed description of the existing analogue–numerical comparisons:

"The model results are comparable to numerical simulations of strike-slip fault earthquakes in terms of seismic moment, slip gradients and postseismic response (e.g. Ben-Zion and Rice, 1997; Lapusta and Rice, 2003, Tullis et al., 2012a, b). "

"The analogue models of ***Rosenau*** *et al. (2009, 2010) have been cross-validated using finite element models (Pipping et al., 2016;).The numerical model of Pipping et al. (2016) replicated the laboratory results by means of the general deformation pattern of the wedge through seismic cycles, the recurrence behaviour (recurrence times and periodicity) as well as principal source parameters (slip distribution). Using numerical simulation, the frictional properties of the analogue model material were validated and augmented if not measured physically. Moreover the numerical model provided a highly-resolved image of the rupture dynamics beyond what was observed in the analogue model (see e.g. animation of analogue vs. numerical subduction earthquake cycles in Rosenau et al., 2016). Vice versa, the laboratory example served as an object for testing the general performance of the numerical modelling scheme and for the verification of the effectiveness and reliability of the algorithm for frictional contact modelling introduced there."*

"The analogue models of ***Corbi*** *et al. (2013) have been tested against numerical models using the finite element technique (van Dinther et al., (2013a, b). An extensive benchmark has been carried out analysing a set of parameters that are characteristic of both the interseismic and coseismic stages (i.e., recurrence time, coseismic displacement, rupture duration, rupture velocity, slip rate, rupture width and hypocentre location). A robust fit has been obtained for the majority of the investigated parameters of the reference model (van Dinther et al., 2013a). In particular, the mean of individual source parameters of the numerical model fall systematically within the standard deviation of the same parameter of the analogue models. The largest discrepancies between the two modelling techniques are observed for rupture width and hypocentre location and are attributed to difference in boundary conditions (i.e., the aseismic part of the megathrust doesn't create stress build up in the analogue models) and sampling rate, respectively."*

and, crucially, what new insight has been gathered from the experiment. I suppose it's not just a confirmation to earlier studies, but there is no way to tell, based solely on this manuscript. Similar vagueness pervades the paper (e.g. Page 6, line 25).

Up to now the experiment is in the validation phase. That's why we cannot provide completely new insights at this stage for this specific case.

No changes in manuscript.

Finally, I found the paper difficult to read due to imperfections of language. It needs to be thoroughly edited. This may be a stylistic choice, but the authors seem to avoid commas at all cost and that makes many sentences long and confusing. On the other hand, they love "i.e." and "e.g." whereas I find it best to avoid abbreviations. I find many twisted sentences that, although possibly not incorrect from a grammatical standpoint, are certainly not the clearest way to present the information. In writing, as in modeling, simplicity leads to clarity.

We agree. As a german-italic-french consortium we are likely not the virtuosi regarding English and happy to receive all the improvements you gave us in the technical comments. We hope the current version has a better style.

We revised the manuscript respecting all technical comments.

C2
SPECIFIC COMMENTS
1) The abstract promises to discuss "limits, challenges and links to numerical models". I don't see that in the paper, expect for the occasional statement interspaced with general presentations. It's certainly not a focus of the paper. The stated focus on "scale models that are directly comparable to observational data on short and long timescales" is lost in the more general and occasionally very basic sections on modeling in general, rheology, and monitoring, which are imperfectly linked to observations and models.

See our reply in the beginning, the current structure is focused on technical aspects. These aspects form the basis (and therefore are linked) to the observations reported in chapter 6.

2) The introduction introduces the "issue" that the time constraint of the earthquake cycle is unknown. How then can it be argued that the analogue systems are properly scaled? Doing this requires that we know and understand the relation between the various timescales (e.g. nucleation stage, repeat time, postseismic duration). The paper doesn't make the point that these relations are well understood, quite on the contrary, whether in the lab or in nature.

Do you refer to the sentence:
*"This raises the issue of a **time constant** in the earthquake cycle that is far larger than the duration of most scientific observations."?*

What we meant is that we have only snap shots of stages of the cycle in nature. What we miss are long enough time series to understand the relation across time scales. It is exactly this what analogue models can provide: long time series. Each stage of the cycle may have its own (in places it is known but not necessarily stationary over several seismic cycles) time constant. This is of course an integral part of scaling by means of kinematic similarity.

3) The distinction between "fault block" models and "scale" models seems arbitrary to me: fault block models are scaled as well as the "scale" models, even though the scaling may not always be rigorous.

It is certainly the rigor in applying scaling to the models which let us differentiate between the two categories. Observations from fault block models can be extrapolated to nature while scale models can be truly upscaled.

Fault block models are labscale but not "scaled" in the sense of Hubbert's scaling theory, i.e. showing geometric, kinematic and dynamic similarity. Fault block models do rarely show geometric similarity (instead of representing a certain structural setting at a given geometric scale they represent an arbitrarily oriented and scaled fault interface), typically there is no defined length scale and the deformation is highly exaggerated. Fault block models show no kinematic similarity (which would mean loading rate and rupture speed etc. scale down consistently) and consequently cannot, by definition, be considered dynamically similar. Observations from those models cannot be directly scaled up to nature but only extrapolated qualitatively.

We included new examples of category 1 and 2 setups in this Section in order make the difference clearer. We also changed the name of the third category to "seismotectonic scale models" in order to emphasize the multiscale nature and more quantitative applicability of these models.

We changed the text in Sect. 2:
*"Here, we categorize analogue earthquake models into three groups with decreasing level of abstraction and applicability (Fig. 1, Table 1): (1) "Spring-slider models" in which elastic and frictional elements are physically discrete components of the setup (Sect. 2.1). **These models can only be applied conceptually to nature**. (2) "Fault block models" in which two elastic blocks, with similar or different elastic properties, are in frictional contacts (Sect. 2.2). **Observations from these models can be qualitatively extrapolated to nature**. (3) "Seismotectonic scale models" in which a distinct tectonic setting is realistically simulated at small scale and with boundary conditions mimicking as closely as possible the natural prototype (Sect. 2.3). **These models can be directly and often quantitatively upscaled to nature**."*

The schematics of Figure 1 imply that fault block models are in a strike-slip configuration with elastic blocks only whereas the scale models would be in a thrust configuration with both elastic and viscous layers. I don't see why one would be scaled and not the other.

The fault block models are usually not strike slip since this would imply a pressure gradient on the fault. Such a feature is not present in fault block models (which are loaded homogenously) but in scale models (indicated by the g-vector in Figure 1c)

See above, we added text and figure 2 and 3 showing examples of the two categories in order make the difference clearer.

Elastic moduli and friction properties are relevant in the all the models.

True, they are relevant but the elastic properties are not scaled in category 1 and 2 models according to scaling laws outlined in the paper (considering gravity and length scale).

Several of the scale models of Figure 2 do not have the kind of layering in Figure 1c and one is not in a thrust setting.

Figure 1c is a synthetic example including the sum of features of those models shown in fig 4 in a simplified matter in order to illustrate the main differences between cat 2 and 3 models (e.g. layering, gravity, dimensionless numbers etc.).

I agree that there is likely a difference in the rigor of scaling between the models built recently in the authors' labs and earlier efforts, but I don't see them as forming an entirely new category of models. If the classification is based on the complexity of the loading system (rigid blocks, elastic blocks, visco-elasto-plastic blocks) then the name of the proposed categories is misleading.

No, it is not the loading system which makes the difference. We think the applicability of scaling laws to these new models makes a distinction which warrants a new category. See reasoning above why we think only cat 3 are scale models.

We try to clarify the justification of the new category in the text and two new figure (see above).

4) Section 2 is essentially a list of works. It shows that people do different things but doesn't explain why these different approaches were adopted (why this material vs. that material), what problem or question new developments are trying to address, and what we learned as models grew in sophistication. It is as if an architect was describing a monument by listing stones that were used without telling us why there is such a variety of material and what the final building looks like.

A main goal of this paper was to give an overview of existing experiments not a critical and comprehensive assessment of results in all three categories. For this we put much effort in listing and categorization. This might not be very attractive for a reader but necessary and useful. A review of approaches and results would need a review paper on each of the categories.

Section 2 gives an overview of approaches. Section 3-5 are basic sections, the bricks if you like, (scaling, materials, monitoring) needed to illustrate the general approach. Section 6 then gives the applications and describes key observations gained from the individual approaches, the building if you like.

We reworked section 2 adding new examples and figures. Results are reported in section 6.

5) I'm amazed that there is summary of what controls frictional sliding more recent than Brace (1972) (page 5, line 9). As much as I like that paper and respect its historical value, it might be good to mention some of the developments from the last 45 years. . .

The chapter follows a chronological order starting indeed 45 years ago. The reference you are referring to was meant to summarize only those early experiments. As said in the next sentence we do not aim to review all rock mechanics experiments since.

Later on (page 5, line 13), you mention you want to focus on studies using analogue rock materials instead of rock samples. Why?

Because the manuscript deals with analogue models, not with rock mechanics (apart from the notion that stick-slip in rock mechanics experiments are viewed as an analogue (analogue mechanism, not model) for earthquakes).

And where are the results?
Results from the different setups with respect to key thematic areas are reported in section 6.

You mention "a large body of work" twice in that paragraph, but only list them. What did they see?
One "large body" refers to the rock mechanics experiments that we exclude from our review (in the same sentence).
The observations from the other "large body" can be found in section 6 (probably not every result but the general findings).

What is the key point of these papers if the context of the present review?

They serve as examples for spring slider setup in a wider sense (where, as you say below, elasticity is stored in the loading device, but not in the sample)

We describe now the ring shear tester setup in more detail which is a member of this experiment family (see reply to comment 11, new text and figure below).

By the way,
I don't see why this discussion belongs in the section on spring-slider models, as the loading machine acts as a deformable loading block (fault-block model).

No, the key feature of spring-slider setup is the separation of spring and slider. This causes the rupture area to be constant and slip to be homogenously distributed causing slip events to be system-sized and characteristic. This is what happens in deformation rigs where the slider is the sample and the spring are the compliant parts of the loading machine. Block models allow heterogeneous slip to occur, which is the key difference.

Also at the end of section 2.2: what did all these studies using blocks of different materials see?

Chapter 6 reports key observations, especially in the context of rupture dynamics block models are the main source of observations.

6) The issue that a rigid slider distribute stress uniformly (Page 5, line 23-25) is exactly why people developed models with a network of springs (King 1994, Heslot et al., 1994, which were mentioned earlier in the section).

Heslot et al. (1994) is a simple spring-slider.

We added in order to clarify:
"Multiple spring-slider systems (e.g. Burridge-Knopoff, 1967; King, 1991, 1994) aimed at overcoming this limitation and succeeded in generating more complex slip and recurrence pattern (see Sect. 6.1). "

and

*"Fault block models, **beside multiple spring-slider models**, have been developed to circumvent the strong assumption of uniform loading and release inherent simple spring-slider models. (…) This setup also allows small (partial) and large (complete) scale failures to occur **in a less segmented fashion compared to multiple spring sliders** bearing the potential to generate more realistic frequency size distributions.*

What did they see? What did they learn about earthquakes from these models?

Chapter 6.1 reports key observations from spring slider experiments regarding statistics of EQs, especially in the context of eq statistics spring-slider models are the main source of observations. We added in Sect. 6.1:

"King (1991, 1994) who showed that large events tend to roughen the stress distribution while small events smooth. Moreover, he found that large events are dissimilar (i.e. not characteristic) and that rupture nucleation is not were peak slip accumulates. The frequency-size distributions found by King (1991, 1994) have been Gutenberg-Richter-like except for the system-sized events which recur approximately time-predictably."

7) Page 6, line 6-17: why is a rigid plate appropriate to model the slab? Slabs are also

elastic. Even though the wedge above the slab is generally softer than the slab due to its elevated temperature, thermal conduction implies that there is no actual temperature jump across the subduction interface. Therefore, the footwall is as deformable as the head wall at least over some length scale.

Analogue models are simplifications of nature. While it is true that the compliancy contrast across the plate interface is small (< 1 order of magnitude) resulting deformation pattern are asymmetric with much smaller displacements in the footwall. This is very similar to what is shown by the Rosenau models where the lower plate is actually loaded by flexible device. However, we did not discuss this detail here. It was certainly a rather strong boundary condition in the first experiments of Rosenau et al. and Corbi et al. which has been overcome only recently by the models involving a rubber belt or foam slab (Rosenau 3D, Dominguez).

We clarified it:
"A similar setup has been used by Rosenau et al. (2009) but with a granular, elastoplastic wedge on top of a less compliant conveyer plate or rubber belt (Fig. 4b)."

8) I don't understand the analogy from adaptive time scaling in experiments and adaptive time stepping as a numerical method. The numerical strategy involves changing time step so that the solution becomes more stable or accurate. The solution itself is modified. The physical time scales, lengths scales, and other scales of the modelled system are not modified. The adaptive time scaling does the opposite: the solution is unchanged, but different scales are used when extrapolating different phenomena to natural conditions. This analogy baffles me.

Possibly the analogy is wrong.
We deleted it at the two instances where it was mentioned.

9) The section on scaling is important and starts to address the issue of multiple time scales. Once again, unfortunately, it mostly states what is done without presenting many results. In addition, there is quite a bit of confusion there as some quantities are either incompletely defined, or substitutions occurs without justification. I feel the authors could do a better job linking the non-dimensional numbers and the scaling relations.

We have thoroughly reworked that section and tried to better define parameters and justify substitutions and relations.

To start with, in Equation 2, please define the measure or component of stress that are you using. Is it an invariant, a shear stress, or a normal stress? The words in the equation say "pressure force", which is weird, as pressure is a force (over unit area) and isotropic, whereas I suspect that shear stress is used here. Neither rho*g or sigma/l have units of force. (same issue in equation 3).

We now use cohesion as used in sandbox modelling. But it can be substituted by any stress or strength measure with the unit Pa. We clarified this in the text (see reply to Reviewer 1).

The definition of Sm in Table 2 is different from Eq. 2,

Fixed!

and that table includes a Stokes number whose importance was not discussed in the text.

Stokes number is referred to in 3.2 in a section on limits of scaling.

I am confused, in line 15-16, how Sm and Ra can dictate, among other things, length scaling, when the statement is "for a given length scale".

Fixed!

Reads now:

"To achieve similitude these numbers have to be the same in the model as in the prototype. For a given length scale (usually suitably chosen for handling the model in a lab), Sm and Ra dictate the stress scaling in the brittle and viscous regimes, respectively."

Note also that no brittle scaling has been defined. What if the model is not viscous? Can there not be a number equivalent to Ra but using, for example, inertial forces? I see Ra as a special case of Sm when the stress is controlled by viscous processes (\sigma=\eta*v/l). Why are they treated as different numbers?

Brittle scaling uses Sm with cohesion (or frictional strength) in the denominator. Inertial forces play a role only in the dynamic regime. Ca can be used to scale the brittle dynamic regime when substituting K for cohesion or frictional strength.

Why did you switch from v/l in Eq. 3 to d\epsilon/dt in equation 5?

v/l in equation (3) is a shear rate as in the original definition of Ra. deps/dt is strain rate which we consider more general and practical than shear rate. Since both have the same dimension, they are interchangeable.

Page 9, line 18: why restrict the scaling to "typically"? How other than with Re would you scale dynamical effects? The final paragraph of Section 3.2 belongs earlier, as that scaling is used in the analogy of moments at the top of page 10.

The message is that dynamic viscous effects cannot be scaled in analogue models. However, because of the viscoelastic nature of the earth the coseismic stage is mainly elastic, coseismic viscous deformation might be a neglectable. This is what this paragraph is dedicated to.

We delete "typically".

10) Section 4 on rheology is written as a level that doesn't help with the topic of the review. It is also not really "historical" as it doesn't describe how ideas and approaches have changed over time, just a portion of current understanding. It would be appropriate for a textbook, but defining all the possible rheologies seems a waste of space.

We see this section as a basic contribution to the topic summarizing a minimum set of rheologies. We don't think it's a waste of space.

In addition, these definitions are not rigorous. For example, Hooke was referring in 1676

to "The power of any springy body is in the same proportion with the extension.", which gives F=kx. It is not equivalent to Eq. 12, which is a differential form that allows for residual strength or strain. The diagram of Fig. 5 shows non-linearity and possibility residual strength, and while this is more realistic than F=kx, it is also not Hooke's law.

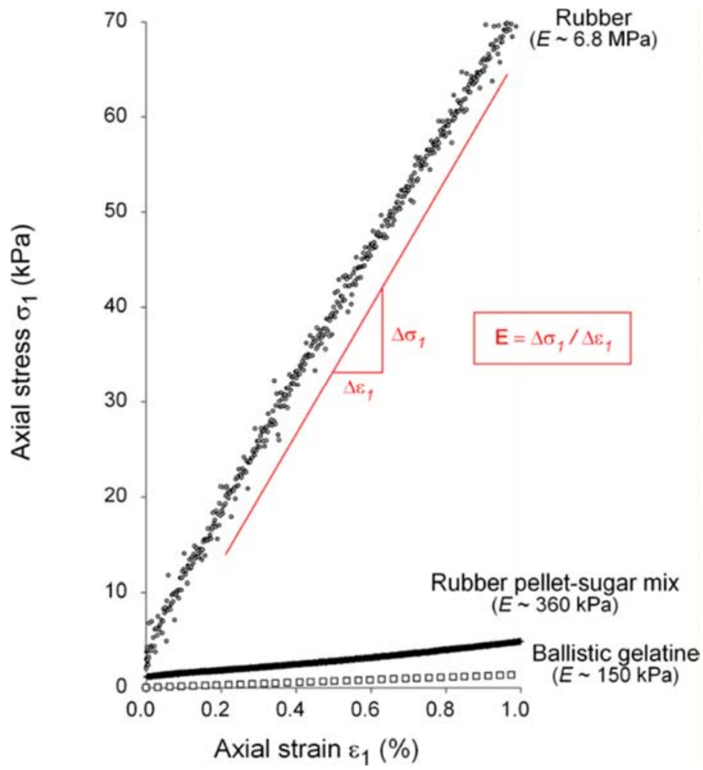
We changed text and the figure accordingly

.

"In linear elastic solids, as in springs, elastic strain is generally linearly related to the applied stress in the same direction (Fig. 5):

$$E = \frac{\Delta\sigma 1}{\Delta\epsilon 1}, \qquad (12)$$

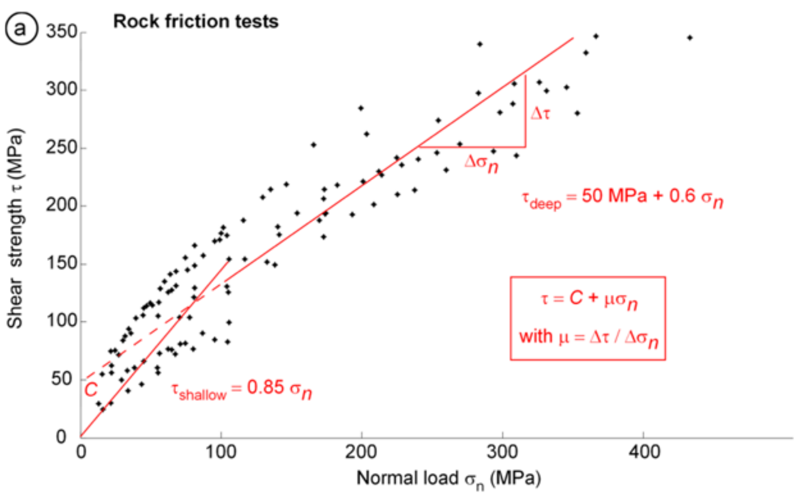
which is a differential version of "Hookes's Law"."



Also, Byerlee's law (Fig. 7a) is not a generic linear relation but refers to specific sets of parameters (those next to the line fits, but not in the label for \mu = \Delta\sigma_n / \Delta\tau) To save time and space, I will not give details of typos, unclear statements for this section (suffices it to say it needs as much editing work as sections 2 and 3) as I think it first needs to be reworked to focus on what is truly needed to understand the time scales of seismic phenomena in the lab.

We changed the text and figure accordingly.

"While the Mohr-Coulomb criterion originally describes frictional faulting of an intact rock, the same graphical method can be applied to describe frictional sliding on pre-existing faults and retrieve the respective set of parameters ("Byerlee's Law"). Accordingly, fault rocks at very shallow crustal levels (σn < 100 MPa) rocks have virtually no cohesion and a relatively high friction coefficient of 0.85, while at deeper levels rock appears cohesive (C ~ 50 MPa) and has a friction coefficient of ca. 0.6 (Byerlee, 1978)."



The section on slip models, for example, is entirely irrelevant.

*If you mean the section on **crack models and dislocations** (4.1.1.):*
We think those models are important as they serve as benchmarks for the analogue models (and vice versa).

*If you mean the section **recurrence models** (slip vs. time predictable) (6.1):*
Time and slip predictable models are very basic models to be included here.

*If you mean the section on **rupture models** (pulse vs. crack): same, basics, to be included we think.*

I do need to point out that unlike what is written at the top of P.17, I find that Burger's body is considered to be more relevant then either the Maxwell or Kelvin-Voigt models in recent studies

We do not disagree:
P. 17 top sentence says that "Maxwell is more relevant than Kelvin Voigt", below we say "A more elaborate viscoelastic rheology is the Burgers model …"
We especially agree that Burgers model is nowadays more often used to model postseismic deformation. But how do we know whether a complex model is better than a simple one? The problem of inverting postseismic deformation is non-unique and there are examples were simple models fit equally well. Applying "Occam's razor" we should favor simpler models (with less parameters) as long as they are qualitatively good enough.

We added in conclusion:
"New materials remain to be explored. Especially non-linear rheologies both in brittle and viscoselastic regimes will contribute to more realistic analogue models in future. A rigorous material characterization is prerequisite. For example, implementation of Burgers rheology in analogue models including postseismic mantle relaxation appears as a necessary step in near future."

and that the presence of multiple time scales of postseismic relaxation was seen in many studies long before Wang et al. (2012), e.g., Savage and Svarc (JGR, 1997), Nishimura et al. (Tectonophysics, 2000), Kenner et al.
(JGR 2000) and others.

Certainly, but Wang is a useful Review though. Since we do not want to discuss transient rheologies here, and only make the point that such concepts exist we refer to his review. We would like to minimize references here.

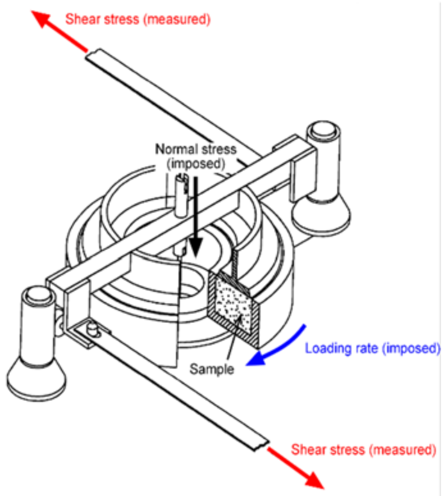
We now write "(Wang et al. 2012, and references therein)"

11) The Schultze ring shear apparatus plays a prominent role in the collection of the mechanical data presented. Yet its description is minimal (page 15, line 21-22). Please describe in more detail what that apparatus is and how it works. Maybe include a schematic of this apparatus?

Good point!

We describe the setup now in chapter 2.1 and show it in Figure 2 as an example of spring-slider setup.

"Several studies which focus in frictional behaviour of granular rock analogue materials (e.g. sand, glassbeads) at low loads (kPa) used a Schulze ring-shear tester (Schulze, 1994, Ritter et., 2016; Klinkmüller et al., 2016; Panien et al., 2006; Lohrmann et al. 2003) which serves here as an example of spring-slider device used to generate analogue earthquakes (Fig. A1). The ring-shear tester consists of a 4 cm high annular shear-cell made of stainless steel holding approximately 0.1 and 1 liter of the sample material. A ring-shaped-lid is placed onto the filled cell. The lid is subjected to a normal force in order to control normal load on the sample. While the cell is rotated, the lid is prevented from rotation by two tie rods connected to a crossbeam. The force necessary to shear the material is measured continuously. To ensure shearing inside the material and prevent slip between the lid and the granular material, the lid has 20 vanes protruding 4 mm into the material.The loading system is compliant enough (~1.3 kN/mm) to generate sticks-slip in a variety of materials at loads below 20 kPa. Results of this setup are presented on several occasions in this paper."



12) Section 5 (monitoring techniques) reads like a long list of approaches. As before, I'd like the authors to maybe compare more explicitly what can be learned from using these techniques. Looking back at all these works, what would you recommend using to answer different questions?

The idea was to give a short overview of what exists and what observables, at which resolution and coverage, can be retrieved. The categorization according to the coverage (local, regional, global) and the drawing of parallels to seismology and geodesy was meant to get an easy overview about the capabilities of the different techniques. Apart from the text there is Table listing these approaches making it easy to compare. Moreover, most techniques currently in use have dedicated review papers (e.g. Lei and Ma, Adam, Rosakis…) to which we refer. We think this is a convenient way to guide the user finding a suitable monitoring technique.

13) I found section 6 to be much better written than the rest of the paper and more useful, in that it details not just what was done, but also what was learned from these experiments. It finally explains something about seismic phenomena and reveals the usefulness of (a few of) the experiments mentioned earlier. There is room from improvement, though. For example, b-values are mentioned page 21 line 30, before the concept was introduced in page 22 line 7.

On its first occurrence the term "b-value" is listed along with other parameters which are defined in the following two sentences. We consider this close enough.

In page 23, stick-slip is discussed line 18 but defined line 25. My other comments on this section are minor.

We now define stick- slip in the introduction:
"With the rise of the plate tectonic theory in the 1960s accompanied by thriving of seismology and experimental rock mechanics, stick-slip instabilities (the cyclic slow accumulation and sudden release of stress along frictional interfaces) along pre-existing discontinuities, i.e. tectonic faults, has become the most prominent earthquake mechanism"

14) At the end of section 6.4, we are presented again with a technical aspect (how Brune and Anooshepoor excited their models) without being told what they learned in that study.

Those models are somehow apart from the modelling approach represented by the rest of the paper. That is why we only give this short note to this specific approach.

15) Check the references. Several are missing elements. A few are using all-caps for the journal. Sometimes, the first name appears first (A. Alshibi)

Fixed!

16) What exactly are the "Nature example" shown in Figure 14? Neither the caption nor the text give us this information.

Added in Figure and reference list:

"Schmalzle, G., T. Dixon, R. Malservisi, and R. Govers (2006), Strain accumulation across the Carrizo segment of the San Andreas Fault, California: Impact of laterally varying crustal properties, J. Geophys. Res., 111, B05403, doi:10.1029/2005JB003843.

Cakir, Z., A. M. Akoglu, S. Belabbes, S. Ergintav, and M. Meghraoui (2005), Creeping along the Ismetpasa section of the North Anatolian fault (Western Turkey): Rate and extent from InSAR, Earth Planet. Sci. Lett.,238,225–234, doi:10.1016/j.epsl.2005.06.044.

Peltzer, G., F. Crampé, and G. King (1999), Evidence of nonlinear elasticity of the crust from Mw7.6 Manyi (Tibet) earthquake, Science, 286, 272–276, doi:10.1126/science.286.5438.272.

Fialko, Y. (2004), Evidence of fluid-filled upper crust from observations of postseismic deformation due to the 1992 Mw7.3 Landers earthquake, J. Geophys. Res., 109, B08401, doi:10.1029/2004JB002985"

All other technical comments:

We agree.

We changed the manuscript accordingly.