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Solid Earth
Discussions

Interactive comment on "Analogue earthquakes and seismic cycles: Experimental modelling across timescales" *by* Matthias Rosenau et al.

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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 re-

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Discussion paper

view 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. 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, 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).

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.

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.

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.

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

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.

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... Later on (page 5, line 13), you mention you want to focus on studies using analogue rock materials instead of rock samples. Why? And where are the results? You mention "a large body of work" twice in that paragraph, but only list them. What did they see? What is the key point of these papers if the context of the present review? 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). Also at the end of section 2.2: what did all these studies using blocks of different materials see?

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

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.

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.

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. 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). The definition of Sm in Table 2 is different from Eq. 2, and that table includes a Stokes number whose importance was not discussed in the text. 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”. 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? Why did you switch from v/l in Eq. 3 to d\epsilon/dt in equation 5? 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.

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

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?

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?

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

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.

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)

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

TECHNICAL COMMENTS This list is not exhaustive, as I do not have the time nor the qualifications necessary to pick up every grammatical or stylistic issue with this paper.

Awkward expression, twisted sentences, etc.

_ Page 1, line 8: “joined the forefront of the research” is just weird. . .

_ Page 1, line 13: “We here review the cornerstones” (“here” should be after “review”; cornerstone implies that the study will change the direction of science but that case is never made explicit, except for Reid (1911)”.

_ Page 1, lines 29-30: It’s trivial that “seismogenic” faulting should be a mechanism for earthquakes. That’s what the name means!

_ Page 2, line 3: “we focus here tectonic earthquake modelling” or “we focus here on the modelling of tectonic earthquakes”.

_ Page 2, line 8: “which affect notably their relevance”. Isn’t that redundant with what is above?

_ Page 2, Line 13: “which is”. There’s no clear subject in agreement with the singular “is”.

_ Page 2, paragraph starting with “New technological advances”. The topic of the paragraph changes from “new advances” to “the issue of a time constant”. This kind of change must be avoided to produce clear writing.

_ Page 3, line 1: “we present here”.

_ Page 3, line 9-10: “Here, we categorize. . .”.

_ Page 4, Line 30: “to dilute”. To circumvent?

_ Page 5, line 32: “allow to simulate”. They make it possible to simulate (?) they allow the simulation (?)

_ Page 6, line 10, 11: “Laboratory scale, not “labscale”. Also at page 7, line 10.

_ Page 7, line 4: one of the remarkable points OF THIS STUDY is the

_ Page 7, line 10: the ending of the sentence “especially if scale models are considered” is trivial: the whole sentence is about scaling, which can’t be done if you are not considering a scale model!

_ Page 7, line 18: replace “then” by “in which case”. Also in line 20.

_ Page 7, line 19: replace “Alternatively” by “, or” (to go with “Either” in line 17, or remove that “Either”. Also, be more explicitly by what you mean with “(MHz)” (that’s a unit of measurement, but you include it as a modifier for “techniques”. It’s not a technique).

_ Page 8, line 9, replace “and the Ramberg Number . . .” by “. Second, the Ramberg Number. . .”.

_ Page 9, line 22, at the end write “does not play a role” (play no role is too colloquial)

_ Page 9 line 26: "Here belongs" is very awkward (and should be plural anyway as several things are listed next).

_ Page 10, line 18: in English, no "live" in "everyday experience".

_ Page 18, line 29: I don't understand "In contrast regional tectonics surfaces"

_ Figure 4 caption: I suggest rewriting the beginning "Scaling of parameter values from laboratory (model) to nature (prototype). (a)..."

_ The caption of Figure 12 mentioned "creep" and "transient slip" but that nomenclature is different from what is in the figure itself.

Improper terms

_ Page 1, Line 12: "culmination"

_ Page 1, Line 20: "perceptible shaking". Not every earthquake leads to shaking that can be felt, which is the meaning of perceptible.

_ Page 4, line 31: "They allow investigating..." (use present tense as this hasn't changed).

_ Page 6. Line 14: "several", not "few" scale models (few implies that there should be more, and it's quite negative. I think you want to impress upon the reader that models actually exist, so "several" is more appropriate).

_ Page 7, line 21: delete "such" (dyadic has not been explained before)

_ Page 8, line 14: replace the ending with "typical of tectonic applications"

_ Page 9, line 15: Replace "coherent" with "consistent", missing word "with THE stress scale"

_ Page 10 line 23: "inherit" is wrong (there's no passing of the characteristic from one level to the next).

_ Page 22, line 8: "regularly" implies a recurring phenomenon, following a pattern, especially if it occurs at constant intervals. I don't think we know that is indeed what happens for "system-size" events, especially in nature, due to our very limited dataset.

_ Page 24, line 9: that first sentence is just odd. Maybe "Rupture dynamics, which includes the study of earthquake nucleating, the transition to dynamic rupture, and its arrest, has by far the broadest range of applications of the phenomena that can be studied by analogue experiments".

_ Page 28, line 27: therefore, not thereby

_ Page 28 line 32, also page 29, line 7: does "2d" stand for "2D" (two-dimensional)? If so, use a capital (as for 3D). Also, I don't understand why having a rigid conveyer plate makes the model 2D. Does that change the width of the model? _ Page 29, line 7: not sure if you mean "few percent", which means "not a lot of percent", or "a few percent".

_ Figure 4: The blink of an eye (not an eyeblink) is a duration, not length.

Typos

_ Page 1, line 28: replace "flouring" with "flourishing".

_ Page 1, line 28; Page 4 line 3: No apostrophe in "1960's" (it's not a possessive)

_ Page 4, line 4: "to reproduce stick slip instabilities".

_ Page 4, line 5: "designed TO BE as stiff. . . but compliant enough. . ."

_ Page 7, line 7: "Scaling laboratory scale observations to nature"

_ Page 7, line 11: no capital S in "This section".

_ Page 7, line 18: no capital in "millions".

_ Page 8, line 13: missing parenthesis before "Table 2".

_ For consistency, please decide whether to use "micron", "micrometer", or "micrometre". All three are used at different part of the text.

_ Page 8, line 25: scaleS (it's used as a verb here).

_ In general: please format the exponents in the powers of ten correctly as superscripts (e.g. missed in Page 9, lines 2-3). Use consistently italics for symbols (e.g. missed in Eqs. 7, 8) and subscripts when needed (missed in Eqs. 9, 10)

_ Page 9, line 30: fix spaces and hyphens "... derived straightforward from the scaling rules..."

_ Page 19, line 8: no space in "inline"

_ Page 19, line 27: add parentheses around 1991.

_ Page 19, line 28: delete "and" at the beginning of the line?

_ Page 20, line 27: Don't start sentence by "i.e.".

_ Page 22, line 9: Delete "they".

_ Page 22, line 13: "It is defined AS the standard deviation..."

_ Page 23, line 28: "THE deformable...".

_ Page 24, line 4-5 "... magnitude decreaseS with depth ..."

_ Page 25,line 19: no s in "increase".

_ Page 26, line 5: "... in THE viscoelastic models..."

_ Page 27, line13: no capital but two n in millennia.

_ Page 27, line15: no capital in Fig. 6b, c

_ Page 28, line 29: missing h in earthquakes

_ Page 29, line 31: "into which is embedded ..."

_ Page 29, line 17: no capital S in “In summary”.

_ Page 30, line 1: present tense “include” (the materials have not finished including these rheologies).

_ Page 30, line 21: “the material presented here”.

_ Acknowledgment: please specify the kind of data. It’s too vague as it is.

_ Figure 8: label “Cacao”, not “Kakao”.

_ Figure 9 is referred to in the text (section 5.3) after Fig. 10 (Section 4.3.1).

_ Caption of Figure 10: no capital A in Maxwell.

_ Caption of Figure 13: “upwards and downwards in the seismogenic zone”.

_ Whether references are in italics or not in table 1 is inconsistent.

_ Table 5: “gauge”, not “gage”

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