Workshop Report

Workshop Title: How Physics-Based Earthquake Simulators Might Help Improve Earthquake Forecasts

Conveners: Ned Field and Nick Beeler; **Dates:** June 18, 2019

Location: Conference Room C, USGS Earthquake Science Center, Menlo Park, California

Introduction

This report summarizes the findings of the workshop, rather than giving a blow-by-blow account of each presentation (which readers can view themselves from the workshop web site: https://www.scec.org/workshops/2019/eqsims).

Modern Probabilistic Seismic Hazard Analysis relies on two main modeling components: 1) an earthquake-rupture forecast (ERF), which gives the likelihood of every possible fault-rupture event in a region and over a specified time span; and 2) a ground-motion model (GMM), which provides an estimate of shaking at a site for a given fault rupture. While these model components have traditionally been observationally-guided statistical representations, which are hampered by sparse observations at the higher magnitudes that dominate risk, considerable progress has been made recently with respect to utilizing more physics-based approaches.

With respect to ERFs, the class of relevant models are multi-cycle physics-based earthquake simulators, referred to hereafter as "physics-based simulators", which were the topic of a focused issue of SRL in 2012 (see Tullis (2012) for the preface). Rather than the traditional approach of inferring earthquake magnitudes from fault area or length using statistical scaling relationships, and associated frequencies of occurrence by matching fault slip-rate and/or paleoseismic recurrence intervals, these physics-based models apply tectonic loading to a fault system and utilize frictional properties on those faults to determine when and where earthquakes occur, with each earthquake transferring stress and thereby influencing future earthquake occurrences. The product is a synthetic catalog of earthquakes for a specified timeframe (thousands to millions of years, or whatever is needed for robust inferences).

Following the pioneering work of Rundle (1988) and Ward (2000), a number of such simulators have now been developed and published (e.g., Sachs et al, 2012; Ward, 2012; Richards-Dinger and Dieterich, 2012; Pollitz, 2012; Shaw, 2017; Schultz et al., 2017), and results look promising in terms of potential practical applicability (e.g., Tullis et al., 2012; Shaw et al., 2018). The RSQSim model of Richards-Dinger and Dieterich (2012) shows particular promise, which is why its further development has been a central focus of SCEC's Collaboratory for Interseismic Simulation and Modeling (CISM) project.

Of course, all models are wrong at some level due to necessary assumptions, approximations, and epistemic uncertainties with respect to model parameters. Practical application of these models has therefore remained controversial, as one can always see something better on the horizon, and this controversy seems to have impeded support of these models (rather than bolstering it).

Perhaps the most significant issue with respect to current physics-based simulators is that they ignore, or crudely approximate, the influence of propagating seismic waves (inertial/dynamic effects). They also generally ignore the 3D velocity structure, non-elastic effects at depth, off-fault yielding, and other things such as the influence of fluids. Single-cycle (single-event) dynamic rupture models (e.g., Harris et al., 2018) are better able to represent such effects, but computational limits currently prohibit such sophistication in the multi-cycle models needed for earthquake forecasting (note also that if and when we overcome these obstacles, the distinction between ERFs and GMMs can give way to a single unified model).

Getting more physics into our models has obvious scientific appeal, but the practical question is whether such models improve our seismic hazard and/or risk estimates, particularly with respect to inferred epistemic uncertainties. If these models are relatively expensive to maintain and operate, which they are, then another practical question is whether the value added is sufficient to warrant their use over less-costly or simpler alternatives. In other words, are current physics-based simulators useful, and if so, are they useful enough to be worth developing and maintaining?

The answer to this question depends on what you want to do with these models, or precisely what inferences you are trying to make. A model might not be reliable for one inference (e.g., the relative probability of various multi-fault ruptures) but perfectly fine with respect to another (e.g., the level of elastic-rebound predictability in the system; Reid, 1911).

Specific Inferences

One of the main goals of the workshop was therefore to enumerate the list of inferences we would presently like to make (as recently articulated by Field (2018)), which include:

General/Ultimate Inferences:

- Long-term rate of every possible earthquake, at some specified level of discretization, as utilized in National Seismic Hazard Maps (e.g., Petersen et al., 2015)
- Time-dependent earthquake probabilities (conditioned on known past events)

Specific/Indirect Inferences (constraints on traditional approaches):

- Multi-fault rupture plausibility
- Scaling relationships (mag-area, slip-length, etc.)
- Average slip along rupture, especially for multi-fault events (i.e., if the same event occurs 1000 times, what's the average slip distribution along strike? A multi-rainbow shape?)
- Magnitude Frequency Distribution of Faults (characteristic or Gutenberg-Richter?)
- Influence of Creep (rupture area reduction, slip-rate reduction, or some combination?)
- Elastic Rebound Predictability (what's the likelihood of a fault re-rupturing after a large event, and how does this evolve with time?; e.g., Field et al., 2015)
- Spatiotemporal Clustering (e.g., is ETAS an adequate approximate at large magnitudes; e.g., Field et al., 2017)?
- Other Time Dependencies, such as mode switching or super cycles (e.g., can this solve the Paleo Hiatus problem identified by David Jackson?)

The RSQSim Model

Due to the extensive developmental effort put into RSQSim in recent years, and the promise it now shows, about two hours of the workshop was dedicated to taking a deep dive on this model (presentations by Jim Dieterich and Bruce Shaw). RSQSim stands for **R**ate and **S**tate Earth**Q**uake **Sim**ulator (Richards-Dinger and Dieterich, 2012). It models a complex fault system using rectangular or triangular boundary elements with back slip. It avoids repeated incremental solutions of large system of equations by applying event-driven computations based on changes in fault sliding state, where each element may be in only one of three sliding states: 1) Locked (aging by log time of stationary contact); 2) Nucleating slip (analytic solutions of the rate-state equations for accelerating slip to nucleate earthquakes, and track the time- and slip-dependent breakdown process at the rupture front); and 3) Earthquake Slip (quasi-dynamic, in which slip speed is based on shear wave impedance). The model is thereby able to model millions of years of M≥4 earthquakes throughout a large complex fault system.

In addition to inferring hazard and risk implications, Jim Dieterich emphasized use of RSQSim as a scientific tool for investigating system-level processes of fault networks, such as the roles of quenched vs dynamical heterogeneities, fault system geometry, and physical parameters in controlling rupture occurrences. RSQSim can also model slow-slip events, fault creep, induced seismicity, and the interaction of these with normal tectonic events. Comparisons with fully dynamic, finite-element simulation for individual ruptures (using DYNA3D; http://swmath.org/software/17036) show good agreement, and rupture jumps between disconnected faults are in good agreement with more detailed rupture modeling. RSQSim also produces realistic Omori clustering as inferred from interevent waiting time distributions and space-time distributions. The rupture calculations appear to be free of numerical pathologies even though the representation of the rupture front break-down zone is quite crude.

One provocative result is that the California statewide magnitude frequency distribution from RSQSim exhibits a characteristic bulge at about M 7.25, raising the question of whether there is a problem with the model, or whether UCERF3's Gutenberg-Richter (with *b*=1) assumption (Field et al., 2014) might not be legitimate. Bruce Shaw showed how adding variable slip speed helps lesson this discrepancy, but it doesn't remove it completely. Lively discussions also revolved around whether the agreement between RSQSim and UCERF3 hazard estimates is "remarkable" (Shaw et al., 2018), or whether is a predictable manifestation of both models being constrained by the same fault slip rates (minimizing hazard changes).

Bruce Shaw also emphasize that we shouldn't ask what physics is missing, but rather what observations are not being matched. He also noted that physics-based simulators can help identify fault- and deformation-model problems, such as unphysical tears or slip-rate transitions.

Using one-million-year long simulated catalogs, Kevin Milner exemplified what RSQSim implies with respect to almost all the inferences outlined above (as well how making meaningful comparisons to UCERF3 is not as easy as one would think). For example, Figure 1 shows the slip versus rupture-length implications of RSQSim, and how it is inconsistent with one of the logic-tree branch options in UCERF3 (Hanks and Bakun, 2008). Is this telling us that this branch should be removed in future forecasts, or does it represent some implicit modeling bias of RSQSim? The relevant question is whether someone could create an alternative physics-based simulator that is consistent with the Hanks and Bakun (2008) scaling relationship.

Another example shown by Kevin Milner implies significant elastic-rebound predictability and general consistency with that applied in UCERF3 (which is not surprising because the UCERF3 algorithm was essentially inferred from RSQSim and three other physics-based simulators; Field, 2015). Again, the relevant and important question is whether anyone could build an equally credible physics-based simulator that does not exhibit elastic-rebound predictability.

Kevin also described how RSQSim results can also provide slip-time histories for full waveform modeling (e.g., in SCEC's broadband and Cybershake platforms), and that results are in good agreement with ground-motion-prediction equations. This could make an important contribution to the currently unresolved question of how to compute directivity effects for complex (e.g., multi-fault) ruptures (based on discussions with Yousef Bozorgnia).

Reality Checks

The sentiment of the RSQSim developers and others is that the physics in the model is right enough to provide useful earthquake-forecasting constraints, whereas the earthquake physics community is more skeptical. Where is RSQSim as forecasting tool, and with respect to exactly which inferences? This question led to a series of presentations that identified a variety of potentially important effects that are missing from the model. In other words, what physics is missing, can we add it, and will it matter for hazard and risk? The afternoon presentations and discussions were on known effects that arise in the brittle portion of the crust (Nadia Lapusta, Junle Jiang), in the transition zone from brittle to ductile (Eric Dunham), and those in the lower crust (Fred Pollitz).

Dynamic earthquake simulations have been possible since the early 1970's, so much is known about the physics of shear zones in the brittle crust from simulations using lab, field, observational and theoretical results. As detailed by Nadia, there is much potentially important physics missing from RSQsim: propagating seismic waves, rupture dynamics, 3D velocity structure, off-fault yielding, fluid effects, dilatancy, localization/delocalization, shear heating/dynamic weakening, and slip-dependent fracture energy/breakdown distance. In the larger context of numerical simulations there was a lively discussion of whether or not the complexity in RSQsim is in part a numerical artifact from using 'over-sized' cells - this is an important issue and the key participants (Dieterich, Shaw, Lapusta and others) will be encouraged to collaborate and quickly resolve this concern. Junle summarized progress within the SEAS SCEC initiative. SEAS are whole cycle, ideally fully elastodynamic models that are an intermediate development between the fault system scale simulators and single event simulations discussed by Nadia. In addition Junle presented creative solutions developed within SEAS to objectively compare models with different numerical resolution limits and the resolution limitations that arise dynamically - e.g. those on the length of the cohesive zone behind the rupture tip. The group discussion evolved to the bottom line question raised by Tom Jordan: how close are we from undertaking rudimentary fully dynamic system scale brittle simulations that could be used to assess the importance of dynamic effects in rupture forecasts? The high performance computing experts in the room (Dunham, Kroll, Withers) were not certain but estimated on the order of years rather than decades

Simulations of rupture interactions within the brittle-ductile transition region, summarized by Eric, are not nearly as evolved as those of the brittle crust alone, amounting to fewer than five studies to date, all in 2D anti-plane strain. Nonetheless it is now known that the presence of a deeper rheology exerts zeroth order influence on rupture: being responsible for stopping rupture at depth, the (time-dependent) loading rate of the seismogenic zone. Key physics to include in addition to the transition itself are shear heating and pore pressure as both influence the position of the BDT/locking depth. Eric also raised concerns about adequacy of the backslip loading assumption used in RSQsim - this is another issue that should be quickly resolved by a co-ordinated collaboration amongst workshop participants.

Fred presented published results from his earthquake simulator with a viscoelastic layer. The computations are quasi-static deformation accounting for viscoelasticity of Earth's ductile lower crust and mantle, via precomputation of interaction coefficients that represent time dependent stress transfer among the model fault patches (Pollitz, 2017). Prominent effects are large relaxations, afterslip in the near-fault region, characteristic large magnitude earthquakes, and interactions between the viscosity and earthquake properties (b-values, coefficient of variation of recurring ruptures). Again the studies to date are limited to Pollitz (2012; 2017); notably the analytic solutions developed allow viscoelasticity to be added to the more standard brittle simulators (RSQsim, Allcal). In the subsequent open discussions the RSQSim developers acknowledged the need and a desire to incorporate creep in the brittle portion of the crust and to add viscoelastic deformation to RSQSim.

The Path Forward

While the reality-check talks identified a number of other effects that might be important, there was not enough time to systematically consider each in the context of the inferences listed above. The value of continued and aggressive physics-based simulator development does not seem to be in question, at least not among the attendees. Careful consideration does need to be given to the robustness of each inference, and a future UCERF4 effort would seem to provide the perfect opportunity to do so in formal, relevant, and opportune way. At the same time, we should attempt to construct focused, smaller-scale experiments in order to cross validate RSQSim (and other similar models) against those that include dynamic waves and/or some of the other missing attributes noted above; this process is illustrated in Figure 2, which was presented by Tom Jordan in his "Building and Summarizing Consensus" wrap-up discussion.

The bigger issue is how we support these efforts, which require broad collaborations between earthquake and computer scientists, access to significant high-performance computing resources, and efforts to curate and make the models available to outside communities. In recent years the RSQSim development has been support by the Collaboratory for Interseismic Simulation and Modeling (CISM), which is a SCEC special-project funded by the Keck foundation. This funding will soon run out, and follow-up resources have not been identified. A common criticism is that we need more, alternative multi-cycle simulators (e.g., to represent epistemic uncertainties), but the reality is we are struggling to maintain even a single model.

Can one identify a more important activity with respect to influencing the quality of earthquake forecasts 10 years from now? Even if RSQSim is deemed premature for all inferences at this time, doesn't this argue for even more aggressive development going forward (to fix the inadequacies)? The USGS has had a very minimal role in developing multi-cycle simulators in recent years, yet these models seem critical to not only satisfying the USGS mission, but also with respect to maintaining leadership with respect to state-of-the-art forecast developments. It would therefore seem important to move beyond funding these as "special" projects, and more toward funding them as critical, central activities. Given personnel transitions and vulnerabilities, identifying individuals to take on associated leadership roles is equally urgent.

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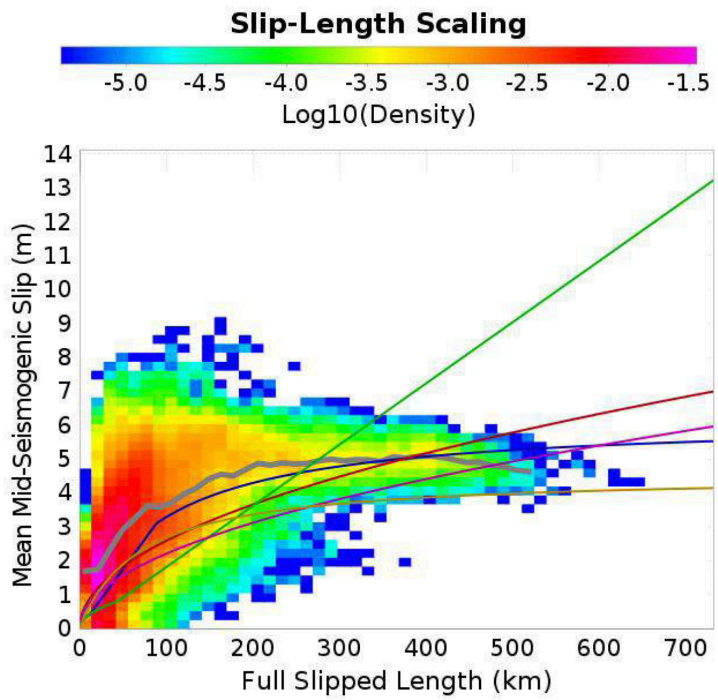

Figure 1. Comparison of RSQSim slip-length implications (colored squares show the distribution and the thick gray line is the mean) with the models used in UCERF3 (Field et al., 2014), implying that the Hanks and Bakun (2008) relationship (green line) is an outlier for lengths greater than 300km.

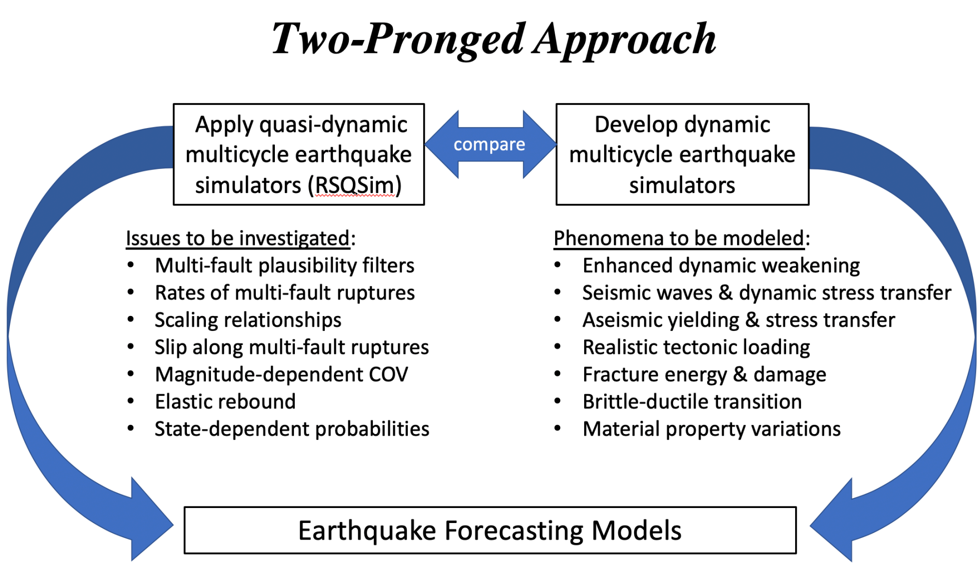

Figure 2. Image from Tom Jordan's presentation illustrating a two-pronged approach for future developments.