Research Interests **Bruce E. Shaw**

Earthquakes remain one of the great unsolved problems in Earth Science. While seismology has made much use of earthquakes, for example using focal mechanisms as a key step in establishing plate tectonics, and using them as sources to gain the best images we have of the Earth's interior, a fundamental gap remains at the very core of this problem; we do not understand the most basic features of earthquake themselves. We can measure many aspects of them, and thus a rich phenomenology is known, but the most fundamental questions about *why* these behaviors occur remain unanswered.

From a basic science point of view, earthquakes are an extremely intriguing system, involving some of the central issues at the forefront of research in the sciences: spatial-temporal complexity, power-laws over a huge range of scales, self-organization, and localization. At the same time, very practical issues drive us to seek a theoretical understanding of earthquakes. The destruction they wreck on human society means we must build to deal with the hazards, yet the most destructive earthquakes happen so rarely that we have insufficient data to do this properly from observations alone. The long repeat times of large earthquakes means it will take hundreds of years to observe even one cycle. Hence, we must use theories to be able to extrapolate from sparse incomplete data sets. Even if the question of *when* earthquakes will occur remains elusive, progress in understanding *what* happens when earthquakes finally do occur will help in the design of better buildings, ultimately saving lives and property.

To crack the earthquake problem requires the ability to work across a wide range of disciplines. We are, most centrally, missing the equations of motion for earthquakes, equations which produce the rich observed phenomenology. Finding these equations will require using the tools of applied mathematics and physics, drawing from the knowledge of materials science, rock mechanics, seismology, geology, geodesy, and other disciplines. The ability to integrate theory and observations is critical to this work, for there are a vast array of a-priori theories, which must be winnowed and guided towards better theories by the observational constraints.

My goal is to find these equations of motion for earthquakes. Two features of the observations encourage us in our search for a unifying theoretical description. First, apart from differences in focal mechanisms, events from around the world look remarkably the same; so similar that we cannot tell from the source motions one place from another. Thus, commonalities in earthquakes are transcending a remarkable range of variations in rocks. The second feature which encourages the search is the wide range of different kinds of behaviors observed. These behaviors, all arising from the same origin, the same motions on the fault, make the theories ultimately testable.

While finding a realistic set of equations of motions is a daunting task, continuing results from a relatively new dynamical approach I and others have been pursuing have been encouraging. We have found a remarkably rich complexity arising from a dynamical instability in the rupture process. Deterministically chaotic slip sequences can emerge in elastodynamic fault models when the frictional forces on the fault decrease with slip or slip rate. This complexity is observed even on completely uniform systems, through the self-organization of repeated ruptures. This has, in recent work with Jim Rice, been confirmed to occur in the continuum limit.

Quantifying the rich behaviors in the models, and comparing them with observations has been a central aspect of my work. The distribution of sizes of events, the occurrence of small events preceding large events, the relative locations of events, and the scaling of slip with rupture length are just some of the behaviors emerging from the models I have examined. My most recent work focuses on the radiated energy from motions on the fault. This is an aspect of the behavior which can only be studied, in a self-consistent manner, with fully dynamic models. The radiated energy is a particularly important aspect of the behavior, because it is what is recorded in seismograms, and what causes the damage from shaking. Results from the modeling indicate we may be able to use the scaling of far field radiated energy with moment and source length to constrain possible source physics [Shaw, 1998]. With the models I have now also measured the spectral content of the radiated energy for a catalogue of events spanning a wide range of sizes, a first for deterministic models. Interestingly, the magnitude dependence of the spectra show expressions of slip pulses in the source motions [Shaw, submitted].

For the future, we need to both make the models of an individual fault more realistic, and also link the faults together to examine fault systems. Having shown a rich dynamic complexity on the geometrically simplest fault, the next important level to explore is more geometrically complex faults. I have formulated a new approach to the problem of nonplanar elastodynamic faults, which I am in the midst of implementing. With this advance, I will be able to study the interaction of stress heterogeneities with geometrical heterogeneities. There are a number of applications of this next level model, with a number of observable consequences expected, in terms of the distribution of sizes of events, compressional wave relative to shear wave radiated energies, and focal mechanism diversity. Looking to the future we may also need to go beyond treating faults as infinitely thin boundary conditions, and deal with the guts of some of the microphysics going on in what is really a boundary layer.

We need to study not only individual faults, but multiple interacting faults in fault systems. I have already looked at analogue laboratory clay models [Spyropoulos, et al, 1999] and numerical models [Spyropoulos, et al, submitted] of the evolution of fault systems, growing under quasistatic conditions. These models which grow faults are important since not only the earthquakes, but the faults themselves, are products of a self-organizing process of accommodating brittle deformation. The next step is to look at such systems in the dynamic regime. Many interesting possible applications will then arise, including one with very big implications for seismic hazard, the issue of cascading ruptures on multiple segments.

The long term goal is to produce models along the lines of climate models, with forecasts concerning the seismic potential of different geographical areas over timescales dictated by the understood physical mechanisms. We would also develop models which give realistic pictures of typical scenario events, much like atmospheric models which give realistic storms. We would ideally like to have the equivalent of weather models as well, for short term deterministic predictions. Here, it is unclear what the limits of predictability may be in this highly nonlinear and probably strongly chaotic earthquake case; understanding the physics of the system will make more clear whether this may be possible.

The earthquake community has recently begun to work towards a modeling effort analogous to what the climate community has been doing, with large scale modular integration of the underlying physical processes. Not knowing yet exactly what equations to use, the individual investigator will play a central role in developing the best descriptions of the relevant physical processes. This has been a central thrust of my research. As a community, we are just now entering into the stage of combining these modeling efforts into a broader integrated community effort. I am actively involved in the growing US effort, through the multidisciplinary Southern California Earthquake Center, now as Columbia University's representative, and am collaborating with the exciting international effort of the APEC Cooperation for Earthquake Simulation, involving combined efforts of the Japanese, Chinese, Australian, and US programs. The Japanese are building a 10 Terra-flop computer to be dedicated to the climate and earthquake problems.

There are tremendous payoffs for success in the earthquake modeling effort, but nevertheless great usefulness even in partial progress. We do not have to understand the whole system to contribute useful knowledge to society. In analogy with El Nino, where advances in understanding the physics of one part of the system led to useful forecasts for some areas over some timescales, advances in understanding aspects of the earthquake system could have great benefits. We are in the midst of a transformative era in our understanding of earthquake physics, and this will have big payoffs to society.

While earthquakes are the central core of my research, I also maintain a broader interest in nonlinear dynamics in the Earth Sciences, and am currently collaborating on a project involving landscape evolution, and on a project looking at impacts of climate change on extreme events from a dynamical systems perspective. Some of the results from a recent paper on the climate work [Khatiwala, Shaw, and Cane, 2001] may be very significant. In looking at the response of simple nonlinear systems to very simple forcing (constant forcing on the Lorenz equations, in this case) we have found great sensitivity of extreme persistent events to small forcing. Even while the means change only by a small amount, the frequency of occurrence of extreme events changes by huge factors; this is a result of changes in the stability of different regimes of the attractor. If this effect carries over to the real climate system, a question we are continuing to pursue, it has huge implications for society, in terms of potential changes in the frequencies of floods, droughts, and other climate events with global change.

At the most general level, I am attracted to problems which have interesting spatial-temporal structure, where simple dynamic models may have something to contribute, where results from the models can be tested with observations, and where understanding the problem better would have practical benefits to society.