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Why We Need a New Paradigm of Earthquake Occurrence

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ABSTRACT

Like all theories in any branch of physics, theories of the seismic source should be testable (i.e., they should be formulated so that they can be objectively compared to observations and rejected if they disagree). Unfortunately, many widely held theories of the seismic source, such as the elastic rebound paradigm and characteristic earthquake model, and theories for applying them to make probabilistic statements about future seismicity, such as probabilistic seismic hazard analysis (PSHA), either disagree with data or are formulated in inherently or effectively untestable ways. Researchers should recognize that this field is in a state of crisis and search for a new paradigm.

Discussion of economic policy is monopolized by people who learned nothing after being wrong.

Paul Krugman, 2015

10.1. INTRODUCTION

Seismologists can perhaps learn from other branches of physics. At one time, it was widely believed that light traversed a hypothetical physical medium called the "ether." *Michelson and Morley* [1887] set out to measure the velocity of the Earth with respect to the ether, but instead found that the velocity of light was constant. This discrepancy was resolved by Einstein, who discarded the notion of the ether altogether, and instead proposed the special theory of relativity. The point of this episode is that an intuitively appealing concept, such as the existence of the ether, must be ruthlessly discarded when it is shown to conflict with observed data. In this paper we suggest that the time has come to discard some intuitively appealing concepts regarding the seismic source.

In spite of the Tbytes of data recorded daily by tens of thousands of seismographic and geodetic observatories, there is no satisfactory model of the earthquake source process. We just have semiqualitative models dominated by the elastic rebound paradigm. This paradigm identifies stick-slip on preexisting fault planes as the predominant mechanism for generating large earthquakes, while failing to provide any mechanism for producing the vastly larger number of smaller earthquakes [*Bak*, 1996; *Corral*, 2004]. If this paradigm was correct, it would support the characteristic earthquake (hereafter abbreviated as CE) model, which would provide enough regularity to make it possible to produce reliable hazard maps and might also facilitate reliable predictions of individual large events. On the other hand, the surprise inevitably induced by the occurrence of "unexpected" destructive earthquakes (Figure 10.1) reminds us how wrong this picture is [*Geller*, 2011; *Stein et al.*, 2012; *Kagan and Jackson*, 2013; *Mulargia*, 2013]; also see critical comment by *Frankel* [2013] and reply by *Stein et al.* [2013]. Yet the CE model

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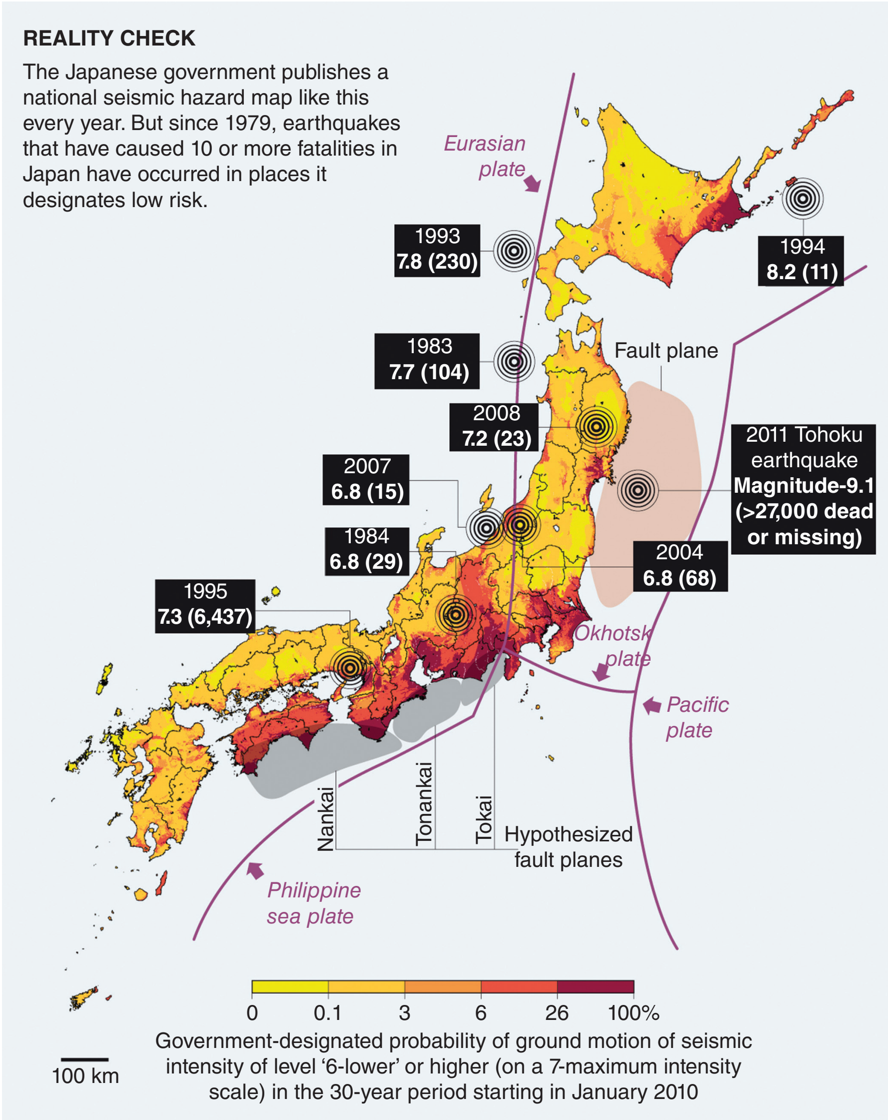

Figure 10.1 Comparison of Japanese government hazard map to the locations of earthquakes since 1979 (the year in which it became the official position of the Japanese government that the Tokai region was at high risk of an imminent large earthquake) that caused 10 or more fatalities [*Geller*, 2011].

and other variants based on classical continuum mechanics remain the prevalent framework for modeling earthquakes and earthquake hazards.

The phenomenological laws describing the statistics of earthquake occurrence (i.e., general trends over a large number of events) are well known. These include the law of *Gutenberg and Richter* [1956] (hereafter abbreviated as GR) for the relationship between magnitude and frequency of occurrence, and the Omori law, for the time evolution of the rate of aftershock occurrence [*Utsu et al.*, 1995; *Rundle et al.*, 1995; *Kagan*, 1999; *Scholz*, 2002; *Turcotte et al.*, 2009]. Both prescribe a power-law behavior, which suggests scale-invariance in size and time. Note, however, that above some corner magnitude (typically on the order of $M_W \approx 8$ for shallow events), the scale invariance breaks down, as the power-law behavior is tapered by a gamma function for large magnitudes [*Kagan*, 1999].

From the point of view of recent developments in nonlinear physics, earthquakes are an example of self-organized criticality [*Bak et al.*, 1987; *Bak and Tang*, 1989]. According to this view, supported by the fact that tiny perturbations such as the dynamic stress induced by earthquakes at distances of more than 100 km [*Felzer and Brodsky*, 2006] and the injection of fluids in the crust at modest pressures [*Mulargia and Bizzarri*, 2014] are capable of inducing earthquakes, the Earth's crust is governed by strongly nonlinear processes and is always on the verge of instability [*Turcotte*, 1997; *Hergarten*, 2002].

In contrast to this nonlinear picture, the classical deterministic model based on continuum mechanics starts from the CE model: a large earthquake is assumed to be the result of the elastic strain released when some limit is exceeded. Under this view, earthquakes are assumed to possess both a characteristic energy and time scale [*Scholz*, 2002; *Turcotte et al*., 2009].

The elastic rebound paradigm [*Gilbert*, 1884; *Reid*, 1911], which is based on geological intuition, predates the formulation of plate tectonics. This paradigm requires earthquakes to be stick-slip instabilities ruled by the physical laws of rock friction on preexisting planes [*Marone*, 1998; *Scholz*, 2002; *Dieterich*, 2009; *Tullis*, 2009]. Modeling efforts based on this paradigm assume both that frictional behavior at low pressures and velocities can be extrapolated to the higher pressure and velocity regime in actual faulting *in situ*, and that the simple geometries and small sizes that can be handled in laboratory experiments are representative of conditions on actual faults in the Earth. The continuum approach is commonly applied to CE models through the rate- and state-dependent friction (RSF) law, even though experiments suggest that the RSF law no longer holds at high sliding velocities due to various mechanochemical reactions that are induced by the frictional heat [*Tsutsumi and Shimamoto*, 1997; *Goldsby and Tullis*, 2002; *Di Toro et al*., 2004; *Hirose and Shimamoto*, 2005; *Mizoguchi et al.*, 2006].

10.2. MODELS OF EARTHQUAKES

Earthquake models can be classified into two categories [cf. *Mulargia and Geller*, 2003; *Bizzarri*, 2011]. The first uses as-detailed-as-possible representations of the source and attempts to reproduce the mechanical aspects through the constitutive laws of linear elasticity; the introduction of body force equivalents to dislocations simulates sudden sliding and produces a transient perturbation in the strain field, which—if fast enough—generates seismic waves in the frequency range detectable by seismographs. All continuum models belong to this category [*Ben-Menahem*, 1961; *Ben-Menahem and Toksöz*, 1962; *Kostrov*, 1964, 1966; *Aki*, 1972; *Tse and Rice*, 1986; *Rice*, 1993]. Such models depict earthquakes as fracture plus a slip transient on a preexisting plane of weakness.

The benchmark for such models is their capability to reproduce the observed wavefield, at least in some (usually very limited) temporal and spectral domain. Discriminating among models is difficult, since essentially any model capable of producing a transient slip in the form of a ramp of finite duration will produce acceptable results [*Bizzarri*, 2011]. Research on this category of models is focused on the minutiae, typically one specific earthquake (selected, perhaps, because it caused considerable damage or is of tectonic interest), with a reasonable fit ensured by having a large number of adjustable parameters, and with considerable variation in estimates of the source parameters depending on the choice of data set and the details of the inversion method [e.g., *Mai and Thingbaijam*, 2014]. While these models can fit waveform data for individual earthquakes, they cannot reproduce the basic empirical laws of earthquake recurrence, that is, the GR and Omori laws, unless further parameters are added to the model. For example, *Hillers et al.* [2007] incorporated *ad hoc* spatial heterogeneity in the characteristic slip distance of the model and obtained a relation between earthquake magnitude and frequency similar to the GR law.

The second category uses simpler models, typically in form of mechanical analogs or cellular automata. Their constitutive laws are simplified to the point that they are often called "toy" models. An example of this category is the Olami-Feder-Christensen model [*Olami et al.*, 1992]. The rationale for such simplifications has its roots in Occam's razor (the principle of choosing the simplest possible model that can explain the data) and aims at reproducing—with a small number of parameters—the dynamical phenomenology that continuum models fail to reproduce (i.e., the GR and Omori laws). On the other hand, this approach is incapable of reproducing the mechanics of the system, that is, the wavefield.

The Burridge-Knopoff (BK) model, a discrete assembly of blocks coupled via springs [*Burridge and Knopoff*, 1967], takes an intermediate approach between these two extremes. It is much simpler than continuum models, but has more parameters than cellular automata models. BK can account for the classical low-velocity friction equations and is generally capable of reproducing the dynamical behavior, but not the radiated wavefield. As such, according to Occam's razor, BK has little advantage over either end-member category of model.

10.3. THE CHARACTERISTIC EARTHQUAKE MODEL

Continuum mechanics models, which are still largely favored in the seismological literature, are basically all variations on the CE model [*Schwartz and Coppersmith*, 1984], which is still widely accepted and used in estimating seismic risk. A typical example is an application to the San Francisco Bay area [*WGCEP*, 1999]. *Schwartz and Coppersmith* [1984] has been cited 750 times (as of 22 Feb. 2015, according to the Web of Science Core Collection database), which demonstrates its high impact on research in this field. These authors (p. 5681) summarize the case for the CE model as follows.

> Paleoseismological data for the Wasatch and San Andreas fault zones have led to the formulation of the characteristic earthquake model, which postulates that individual faults and fault segments tend to generate essentially same size or characteristic earthquakes having a relatively narrow range of magnitudes near the maximum… . Comparisons of earthquake recurrence relationships on both the Wasatch and San Andreas faults based on historical seismicity data and geologic data show that a linear (constant *b*

value) extrapolation of the cumulative recurrence curve from the smaller magnitudes leads to gross underestimates of the frequency of occurrence of the large or characteristic earthquakes. Only by assuming a low *b* value in the moderate magnitude range can the seismicity data on small earthquakes be reconciled with geologic data on large earthquakes. The characteristic earthquake appears to be a fundamental aspect of the behavior of the Wasatch and San Andreas faults and may apply to many other faults as well.

Debate over the CE model has been ongoing since it was first proposed. For some early contributions to the controversy see, for example, *Kagan* [1993, 1996] and *Wesnousky* [1994, 1996]. *Kagan* [1993, p. 7] said that evidence cited as supporting CE could be explained by statistical biases or artifacts:

Statistical methods are used to test the characteristic earthquake hypothesis. Several distributions of earthquake size (seismic moment-frequency relations) are described. Based on the results of other researchers as well as my own tests, evidence of the characteristic earthquake hypothesis can be explained either by statistical bias or statistical artifact. Since other distributions of earthquake size provide a simpler explanation for available information, the hypothesis cannot be regarded as proven.

On the other hand, *Wesnousky* [1994, p. 1940] said that the seismicity on some particular faults was in accord with CE:

Paleoearthquake and fault slip-rate data are combined with the CIT-USGS catalog for the period 1944 to 1992 to examine the shape of the magnitude-frequency distribution along the major strike-slip faults of southern California. The resulting distributions for the Newport-Inglewood, Elsinore, Garlock, and San Andreas faults are in accord with the characteristic earthquake model of fault behavior. The distribution observed along the San Jacinto fault satisfies the Gutenberg-Richter relationship. If attention is limited to segments of the San Jacinto that are marked by the rupture zones of large historical earthquakes or distinct steps in fault trace, the observed distribution along each segment is consistent with the characteristic earthquake model. The Gutenberg-Richter distribution observed for the entirety of the San Jacinto may reflect the sum of seismicity along a number of distinct fault segments, each of which displays a characteristic earthquake distribution. The limited period of instrumental recording is insufficient to disprove the hypothesis that all faults will display a Gutenberg-Richter distribution when averaged over the course of a complete earthquake cycle. But, given that (1) the last 5 decades of seismicity are the best indicators of the expected level of small to moderate-size earthquakes in the next 50 years, and (2) it is generally about this period of time that is of interest in seismic hazard and engineering analysis, the answer to the question posed in the title of the article, at least when concerned with practical implementation of seismic hazard analysis at sites along these major faults, appears to be the "characteristic earthquake distribution."

In a recent contribution to the debate, *Kagan et al.* [2012, p. 952] pointed out that CE had failed many statistical tests:

The seismic gap model has been used to forecast large earthquakes around the Pacific Rim. However, testing of these forecasts in the 1990s and later revealed that they performed worse than did random Poisson forecasts [see *Rong et al.*, 2003 and its references]. Similarly, the characteristic earthquake model has not survived statistical testing [see *Jackson and Kagan*, 2011 and its references]. Yet, despite these clear negative results, the characteristic earthquake and seismic gap models continue to be invoked.

On the other hand, *Ishibe and Shimazaki* [2012, p. 1041] argued that CE more appropriately explained the seismicity on some particular faults:

A total of 172 late Quaternary active fault zones in Japan are examined to determine whether the Gutenberg-Richter relationship or the characteristic earthquake model more adequately describes the magnitude-frequency distribution during one seismic cycle. By combining seismicity data for more than 100 active fault zones at various stages in their seismic cycles, we reduced the short instrumental observation period compared to the average recurrence interval. In only 5% of the active fault zones were the number of observed events equal to or larger than the number of events expected by the Gutenberg-Richter relationship. The average and median frequency ratios of the number of observed events to the number of expected events from the Gutenberg-Richter relationship are only 0.33 and 0.06, respectively, suggesting that the characteristic earthquake model more appropriately describes the magnitude-frequency distribution along the late Quaternary active faults during one seismic cycle.

The failure to reach a consensus about the appropriateness, or lack thereof, of the CE model after 30 years of continuing debate reflects poorly on the earthquake science community. It is comparable to what might have happened in a parallel universe in which the physics research community was still arguing about the existence or nonexistence of the "ether" in 1917, 30 years after the Michelson-Morley experiment.

Why is the controversy over CE (needlessly in our opinion) ongoing? Before getting into the details, let us recall the basic problem afflicting research on earthquake occurrence. In most fields of physics a hypothesis is formulated and is then tested against experimental data. On the other hand, in some areas of physics, including seismology (and astrophysics, etc.), realistic experiments are impossible and the data must come from nature, over a long period of time. This can lead to a situation in which data from past events are retrospectively sifted to find patterns that match the investigator's preconceptions, while much larger volumes of data that do not match are ignored [cf. *Mulargia*, 2001].

In many cases, including the works of *Schwartz and Coppersmith* [1984], *Wesnousky* [1994], and *Ishibe and Shimazaki* [2012], the study region is divided into many subregions on the basis of what is already known (or believed) about its geology and seismicity. Thus, effectively, a huge number of parameters have been used to choose the subregions. Even if analyses of the datasets for the various subregions produce results that nominally support the CE model, the retrospective parameter adjustment means that the statistical force of such arguments is virtually nil. Furthermore, it is well known [e.g., *Howell*, 1985] that dividing a large region into many small subregions and then studying the frequency-magnitude distribution in the various subregions is essentially guaranteed to produce artifacts.

The arguments in favor of CE also have other serious flaws. *Ishibe and Shimazaki* [2012], for example, assume that the CE model is correct, and then use the CE model for each subregion to compute the expected number of earthquakes. They then compare this to the GR curve and argue that an insufficient number of small earthquakes is evidence for the CE model. However, this argument is essentially circular. The apparent discrepancy could also easily be the result of occasional larger-than-characteristic earthquakes that break multiple fault segments at one time. Furthermore, GR is a statistical law that should not necessarily be expected to apply strictly to small subsets. In summary, convincing arguments have not been advanced by the CE proponents.

Two popular variants of the CE model—the time- and slip-predictable models—have also been proposed. These models implicitly are based on the intuitive arguments that there must exist definite and fixed limits to either (1) the breaking strength or (2) the ground level of strain [*Shimazaki and Nakata*, 1980]. However, these models were also shown to fail statistical tests [*Mulargia and Gasperini*, 1995].

10.4. UNCHARACTERISTIC EARTHQUAKES

The real test of a model in physics is its ability to predict future data, not its ability to explain past data. The CE model has had no notable successes, but we cite three failures below.

The first and most serious failure is the 2011 Tohoku earthquake. As summarized by *Kanamori et al.* [2006], a Japanese government agency claimed that characteristic M7.5 earthquakes occurred off the Pacific coast of Miyagi Prefecture at about 37 ± 7-year intervals and could be expected to continue to do so. *Kanamori et al.* [2006] pointed out that the previous off-Miyagi events were much less similar to one another than would be expected if the CE model were correct. What actually happened, however [see *Geller*, 2011; *Kagan and Jackson*, 2013], is that, rather than the "next characteristic M7.5 earthquake," the M9 Tohoku earthquake occurred in 2011. This earthquake simultaneously ruptured many segments that government experts had stated would rupture as separate characteristic earthquakes.

The second failure is Parkfield [for details see *Geller*, 1997; *Jackson and Kagan*, 2006]. To make a long story short, *Bakun and Lindh* [1985] said that repeated M6 characteristic earthquakes had occurred at Parkfield, California, and that there was a 95% chance of the next characteristic earthquake occurring by 1993. Their prediction was endorsed by the US government. *Savage* [1993] pointed out that even if an M6 earthquake did occur at Parkfield within the specified time window, it was more reasonable to explain it as a random event than as the predicted characteristic event. An M6 earthquake near Parkfield in 2004 was 11 years "late" and failed to match other parameters of the original prediction.

Another problematic instance involves a cluster of small earthquakes off the coast of northern Honshu, Japan, near the city of Kamaishi. In an abstract for the Fall AGU Meeting (held in December 1999) *Matsuzawa et al.* [1999] made the following prediction:

> ... we found that one of the clusters is dominated by nearly identical and regularly occurring small earthquakes (characteristic events). This cluster is located about 10 km away from the seashore and its depth is around 50 km. By relocating the hypocenters using JMA (Japan Meteorological Agency) catalogue, we confirmed that small earthquakes with JMA magnitude (Mj) of 4.8 ± 0.1 have repeatedly occurred with a recurrence interval of 5.35 ± 0.53 years since 1957; eight characteristic events in total are identified. ... If this cluster really has characteristic nature, the next event with Mj 4.8 will occur there in July, 2000 ± 6 months.

An earthquake similar to the earlier events in the cluster off the coast near Kamaishi occurred on 13 November 2001, and was claimed by *Matsuzawa et al.* [2002, abstract and paragraph 20] as a successful prediction:

> The next event was expected to occur by the end of November 2001 with 99% probability and actually M4.7 event occurred on November 13, 2001.
>
> ...
>
> Since the recurrence interval was so stable, *Matsuzawa et al.* [1999] predicted that the next event would occur by the end of January 2001 with 68% probability and by the end of November 2001 with 99% probability assuming that the recurrence interval would follow the normal distribution.

We now examine the statistical arguments in the final paragraph of the above quotation from *Matsuzawa et al.* [2002]. (Note that *Matsuzawa et al.* [1999] did not explicitly state the probability values of 68% by January 2001 and 99% by the end of November 2001.) The above probability values rest on unrealistic premises, including "that the recurrence interval would follow the normal distribution" and the implicit assumption that interarrival times are random, independent, and identically distributed (IID). The IID assumption implies, in particular, that interarrival times do not depend on magnitudes, which seems to contradict physical models that support CE.

Because a normal random variable can have arbitrarily large negative values, modeling interarrival times as IID normal variables implies that the ninth event in a sequence could occur before the eighth event in the sequence, and, indeed, even before the first event in the sequence: this is not a plausible statistical model. Moreover, even if interarrival times were IID normal variables, the number *Matsuzawa et al.* [2002] computed is not the chance of the next event occurring by November 2001, but something else, as we now explain.

The observed mean interarrival time for the eight events on which the prediction was based [see Figure 3a of *Matsuzawa et al.*, 2002] is 5.35 yr (64.20 mo), with an observed standard deviation of 0.53 yr (6.36 mo). The last event before the announcement of the prediction took place on 11 March 1995. The date 31 January 2001 is 70.74 mo after 11 March 1995, that is, the observed mean interarrival time plus 1.028 times the observed standard deviation. The date 30 November 2001 is 80.69 mo after 11 March 1995, that is, the observed mean interarrival time plus 2.593 times the observed standard deviation. The area under the standard normal curve from $-\infty$ to 1.028 is 64.8%, and the area from $-\infty$ to 2.593 is 99.5%. Hence, we infer that *Matsuzawa et al.* [2002] arrived at the 68% and 99% figures by calculating the area under a normal curve with known mean 5.35 yr and known standard deviation 0.53 yr.

This calculation ignores the difference between *parameters* (the true mean interarrival time and the true standard deviation of interarrival times, assuming that the interarrival times are IID normal random variables) and *estimates* (the observed sample mean of the seven interarrival times between the eight events and the observed sample standard deviation of those seven interarrival times).

The problem addressed by *Matsuzawa et al.* [1999] was to predict the next observation in a sequence of observations of IID normal random variables. The goal was not to estimate the (true) mean recurrence interval, which would involve calculating a standard Student's t confidence interval for the mean from the seven observations of interarrival times. Rather, the goal was to predict when the "next" (ninth) event would occur. The appropriate statistical device for addressing this problem is called a prediction interval. A prediction interval takes into account both the variance of the estimated mean interarrival time and the uncertainty of the time of the single event being predicted. Hence, a prediction interval is significantly longer than a confidence interval for the mean arrival time.

A proper one-sided $1-\alpha\%$ prediction interval based on seven IID normal observations with sample mean M and sample standard deviation S would go from $-\infty$ to $M+t_{1-\alpha,6}S\sqrt{1+1/7}$, where $t_{1-\alpha,6}$ is the 1 − α percentile of Student's t distribution with 6 degrees of freedom. Now $t_{.68,6}=0.492$, $t_{.95,6}=1.943$, and $t_{.99,6}=3.143$. Hence, if the interarrival times were IID normal random variables, there would have been a 68% chance that the ninth event would occur within 5.35 + 0.492 × 0.53 × 1.069 = 5.629 yr of the eighth event, that is, by 25 October 2000; and there would have been a 95% chance that the ninth event would occur within 5.35 + 1.943 × 0.53 × 1.069 = 6.451 yr, that is, by 22 August 2001; and there would have been a 99% chance that the ninth event would occur within 5.35 + 3.143 × 0.53 × 1.069 = 7.131 yr, that is, by 27 April 2002. Thus, under the assumption that interarrival times are IID normal random variables, the probability of an event occurring by the end of January 2001 was 81.3% (not 68%) and the probability of an event by the end of November 2001 was about 97.4% (not 99%).

The ninth event (the event on 13 November 2001) occurred outside the 95% prediction interval; it was in fact on the boundary of the 97.1% prediction interval. The data thus allow us to reject the hypothesis that interarrival times are IID normal random variables at significance level 100% − 97.1% = 2.9%. In summary, this case should not be regarded as supporting the CE model.

Finally, we note in passing that the above discussion has been deliberately oversimplified in one important respect. A more rigorous statistical treatment would consider the conditional probability of a future earthquake as of the time of the announcement of the prediction—December 1999—given that the eighth and most recent event in the cluster occurred in March 1995 and had already been followed by over 4 years with no subsequent event.

10.5. THE PSHA APPROACH TO EARTHQUAKE-HAZARD MODELING

If the CE model were correct, it could be used to reliably and accurately estimate earthquake hazards, but, as discussed above, CE fails to explain observed seismicity data. Nonetheless, CE is used routinely to generate nominally authoritative, albeit effectively meaningless, estimates of seismic hazards through probabilistic seismic hazard analysis (PSHA). PSHA relies on two unvalidated premises: (1) earthquake occurrence is random and follows a parametric model, and (2) the parameters of that model can be estimated well from available data. Neither of these premises appears realistic.

No standard definition of probability allows one to make meaningful statements about "the probability of an earthquake" [cf. *Stark and Freedman*, 2003]. The frequentist approach fails because the "experiment" is not repeatable. The Bayesian approach separates probability from the underlying physical processes, becoming a pronouncement of an individual's degree of belief; moreover, most attempts to quantify "the probability of an earthquake" do not use Bayes' rule properly. The only viable interpretation of PSHA equates probability to a number in a mathematical model intended to describe the process [cf. *Stark and Freedman*, 2003; *Luen and Stark*, 2012]. There are two potential justifications for a probability model and the forecasts derived therefrom: (1) intrinsic physical validity (e.g., as in quantum mechanics or thermodynamics), or (2) predictive validity. Thus, justification for PSHA implicitly hinges either on the physical validity of the CE model or the ability of PSHA to make reliable and accurate predictions. As discussed above, the CE model has not been validated and conflicts with observed data. Furthermore, hazard maps based on PSHA have failed as a predictive tool [*Stein et al.*, 2012].

Present discussion of testing PSHA is phenomenological and ignores the physics. Given the formidable complexity of the problem, progress may require the collaboration of many groups, which the Collaboratory Study for Earthquake Predictability (CSEP) of the Southern California Earthquake Center (SCEC) is attempting to facilitate [*Zechar et al.*, 2010]. But there is a more fundamental question than collaboration and consensus building: namely, does the time interval covered by available data allow accurate parameter estimation and model testing? We suggest that the answer is no, as data from seismicity catalogs, macroseismic data [*Mulargia*, 2013], and geological studies of faults all fall far short of what is required.

In summary, the basic premises of PSHA lack foundation, and it has not been validated empirically or theoretically. PSHA therefore should not be used as the basis for public policy.

10.6. PROBABILISTIC FORECASTS OF INDIVIDUAL EARTHQUAKES

Finally, we consider studies that argue for increased probability of individual earthquakes in the aftermath of a large earthquake. *Parsons et al.* [2000, p. 661] calculated:

> We calculate the probability of strong shaking in Istanbul, an urban center of 10 million people, from the description of earthquakes on the North Anatolian fault system in the Marmara Sea during the past 500 years and test the resulting catalog against the frequency of damage in Istanbul during the preceding millennium. Departing from current practice, we include the time-dependent effect of stress transferred by the 1999 moment magnitude M = 7.4 Izmit earthquake to faults nearer to Istanbul. We find a 62 ± 15% probability (one standard deviation) of strong shaking during the next 30 years and 32 ± 12% during the next decade.

And *Toda and Stein* [2013, p. 2562] forecasted:

> We therefore fit the seismicity observations to a rate/state Coulomb model, which we use to forecast the time-dependent probability of large earthquakes in the Kanto seismic corridor. We estimate a 17% probability of a M ≥ 7.0 shock over the 5 year prospective period 11 March 2013 to 10 March 2018, two-and-a-half times the probability had the Tohoku earthquake not struck.

There is apparently no definition of "probability" for which this statement is meaningful: it is statistical gibberish. The probability is assumed, not inferred and estimated.

Statements of the above type should be abjured, because they cannot be tested objectively. If a large earthquake occurs in the specified space-time window it might be claimed as a success, but as *Savage* [1993] pointed out it could also be an event that just happened anyway. Also, note that the above statements do not precisely specify the bounds of the spatial area of the prediction, the magnitude scale being used, and so on. This invites potential controversy over whether or not the predictions were successful, of the type which has often accompanied controversies over the evaluation of earthquake prediction claims [e.g., *Geller*, 1996].

10.7. CONCLUSION

The CE paradigm is clearly at odds with reality, but it inexplicably continues to be widely accepted. As CE serves as the justification for PSHA, PSHA should not be used operationally in public policy. The "inconvenient truth" is that there is no satisfactory physical model for earthquakes and none is in sight. However, this is scarcely acknowledged, with a few notable exceptions [e.g., *Ben-Menahem*, 1995]. A new paradigm of earthquake occurrence is clearly required. However, the fact that we don't yet have a new paradigm in no way justifies continued reliance on the old and discredited elastic rebound paradigm and CE model to estimate seismic hazards.

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