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Statistical Theories of Crack Propagation[1]

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The mechanism of crack growth in rocks has been the subject of much recent interest, not only on account of its engineering importance, but also as a background to the study of precursory phenomena for earthquakes. One feature which appears to play a significant role in the fracture mechanism is the formation of microfractures prior to a major failure. Microfractures also play a key role in statistical theories as developed by Weibull and later writers. Some recent work in these two fields is reviewed and the suggestion is put forward that it may be possible to extend the statistical models so as to describe the dynamics of crack formation. As a preliminary step in this direction, it is shown that a branching model for the coalescence of microfractures lead to a simple derivation of the frequency–magnitude law of fracture energies. Other methods of introducing statistical ideas into the dynamics of crack propagation are also briefly reviewed, and compared to deterministic models of crack growth. KEY WORDS: statistics, earthquake prediction, fracture mechanics.

INTRODUCTION

There can be few mechanical problems more complex, more stubbornly evading a definitive physical or theoretical treatment, than the range of phenomena associated with fracture. Insofar as earthquakes involve some form of rock fracture, they may be considered part of this range. Classical seismology has been very little concerned with the detailed mechanics of the fracture process—partly perhaps, because of its inherent unaccessibility, but also because classical seismology has been primarily concerned with earthquakes as a source of elastic vibrations for use in probing the structure of the earth's interior. For this purpose an idealized model of the source is sufficient. In the last few decades, however, the emphasis has shifted towards the earthquake itself, most recently to the study of phenomena which might have application to earthquake prediction. As a consequence, seismology has begun to tangle with the same problems and complexities that have long beset the study of fracture in other contexts.

The intention of this paper is to provide a first look at these problems

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from the point of view of stochastic (i.e., probabilistic) models. I must disclaim at the outset any deep knowledge either of classical seismology or of fracture mechanics. This is a review from "outside" the subject. If there is any excuse for such an exercise, it is the hope that by reviewing the subject from a somewhat novel viewpoint it may be possible to discern new lines of development. I hesitate to claim any such merit for the present paper; on the other hand I do believe that the area of fracture mechanics is one where stochastic models could play an exciting and useful role. I shall be happy enough if the present paper goes some way to indicating the types of problems that might be tackled in this way, and the relation such stochastic models might have to existing theoretical work. The paper falls essentially into two parts. The first part contains a brief account of a simple branching model which I developed recently as a possible explanation of the earthquake frequency–magnitude law (see Vere-Jones, 1976). I do not regard this model as providing a final explanation of this law but I do believe it embodies some elements of the physical situation. Because this model differs rather sharply in character from any of the classical types of models for the earthquake source, in the second half of the paper I have tried to review, inevitably briefly and superficially, some of the wide variety of approaches to crack propagation which currently exist. Here I have included brief comments on Griffith cracks, on more elaborate treatments using elastic theory, on microscopic studies of crack propagation, on the problems of microfracturing and dilatancy in rock fracture, and on statistical strength theories. The aims of this exercise are to gain some perspective on the different models, to indicate in a wider context the role stochastic models can play, and to elucidate which features of the branching model seem satisfactory and which features are deficient.

THE EARTHQUAKE FREQUENCY–MAGNITUDE LAW

The instrumental magnitude M_L of an earthquake is an empirical but useful measure of the size of an earthquake. It is defined in terms of the maximum response of a standard (Wood–Anderson) seismometer, adjusted to correspond to a distance of 100 km from the source. The readings are made on a logarithmic scale. Plotting the total frequency of earthquakes above a given magnitude against the magnitude results in a curve such as that shown in Figure 1, where the frequency is also measured on a logarithmic scale. Over a large range of magnitudes, corresponding to a factor of at least 10^6 in earthquake energies, the curve is approximately linear, implying that frequency decreases roughly exponentially with magnitude. An increase of one unit in magnitude corresponds roughly to a tenfold decrease in frequency, so that the coefficient b in the relationship

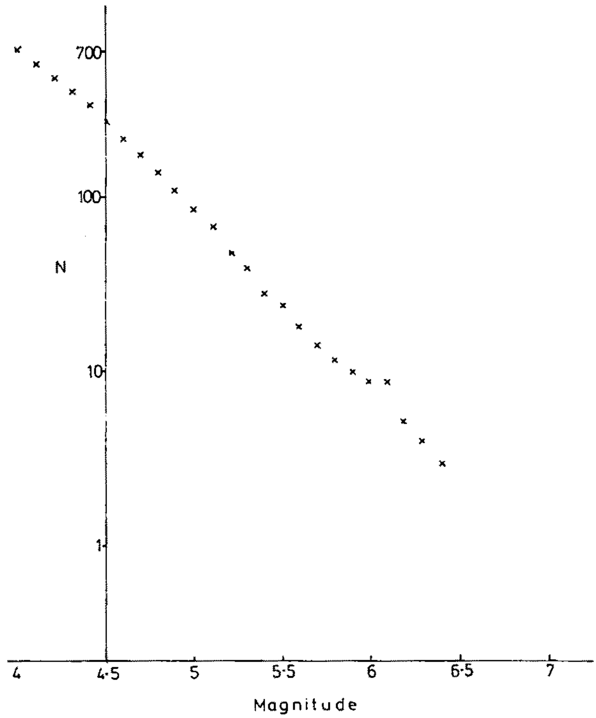

Figure 1. Frequency–magnitude relation for earthquakes in the main seismic region of New Zealand (15/9/54–15/11/65) (after Gilbert, 1974).

$$\text{Prob}(\text{Mag} \geqslant M) = 10^{-bM}$$

is approximately equal to unity.

Since the magnitude itself is logarithmically related to the amplitude of the ground motion (and through that to most other variables of direct physical meaning, including ultimately the energy release), frequency–magnitude diagrams such as Figure 1 are essentially log–log plots of the survivor function

$$S(x) = 1 - F(x)$$

of the distribution of energy or related variables. Thus the implication of the empirical "frequency–magnitude law" is that the frequency distribution of such variables is typically of a power law form. The basic theoretical questions are therefore why (and perhaps even whether) such variables follow a power law distribution (rather than, say, an exponential or normal distribution), and why the exponent of this law should have a value close to the value $-1/2$, as it does when the magnitudes are converted into energies. Although the seismological literature abounds in so-called "explanations" of the frequency–magnitude law, I have yet to read one which I find both physically and mathematically convincing. Let me cite two recent examples. Kanamori and Anderson (1975) argue that the magnitude of an earthquake is related to its source volume; since, for different N, a unit volume can contain N distinct sources of volume $1/N$, then we should expect N times more earthquakes of volume $1/N$ than of volume 1. This argument fails to explain how a given volume can sustain an unlimited number of earthquakes of different magnitudes, when it cannot sustain more than a finite number of earthquakes of any fixed magnitude. (If it is argued that distinct earthquakes affect distinct volumes, then the fact that $N \times 1/N = 1$ ceases to be relevant, and an alternative explanation must be found.) A quite different argument is put forward by Nur and Schultz (1973) in the context of a stick–slip mechanism. Here it is suggested that the (spatial) Fourier transform of the fault surface can be related to earthquake occurrence in such a way that the height of the spectral ordinate can be related to the rate of occurrence and its frequency to the magnitude. I am unconvinced of the reality of any such connection, but even apart from this the onus of explaining the law is merely shifted—in this case, from explaining the distribution of earthquake magnitudes to explaining the distribution of spectral ordinates. It would be a rash seismologist who could say with conviction that the latter was more intuitively obvious than the former.

One difficulty in the way of developing more effective models may have been the traditionally deterministic viewpoint of classical seismology, which is inappropriate in dealing with an inherently statistical phenomenon. It may help, therefore, to make a few general comments about what one is trying to do in developing a stochastic model. Stochastic models tend to appear near the boundaries of scientific disciplines, occupying a half-way position between a complete deterministic analysis of the phenomenon, in which the whole structure is supposed known, and a completely descriptive one, in which no structural analysis is offered. A stochastic model incorporates some structural elements, but accepts other elements as random, i.e., as essentially uncertain and unknowable, at the least for the problem in hand. The aim should be to isolate these uncertainties, so that there is a clear separation between the known structural elements and the unknown random elements. Typically this separation is achieved by letting the random elements operate on a microscopic scale (in relative terms) and using the known structural aspects of the process to affect the transfer from the microscopic to the macroscopic scale of interest. A classical example of such a process occurs in statistical mechanics, but other examples occur in engineering, technology, population models, and many other branches of science.

Applying these ideas to the frequency–magnitude problem, we note first that the law itself is of a very simple form, so there is little advantage in transforming it from one variable to another. Another way of stating this is to say that there is very little structural information in the law itself; the phenomenon is characterized by the absence of any obvious characteristic scale (mean energy of earthquake) and in such a context the power law form is the appropriate distribution for a "purely random" phenomenon (see the remarks on maximum entropy in Vere-Jones, 1975). There can only be some point in analyzing it further, therefore, if we are prepared to dig down to a further level of detail in the structural analysis of earthquakes. This means looking at the fracture mechanism itself. Thus the level at which it is natural to seek an explanation of the frequency–magnitude law is at the level of the detailed mechanism of earthquake formation. This in turn is more closely related to the problem of fracture mechanics and earthquake premonitory phenomena than to the traditional preoccupations of classical seismology.

It is perhaps not surprising, therefore, that the paper which I feel comes closest to developing the kind of explanations I am looking for is the paper by Scholz (1968a) which relates directly to the study of microfracturing and dilatancy. Scholz' work is perhaps best known for its demonstration of the effect of stress in altering the value of the b coefficient. Unfortunately I do not believe that the theoretical arguments with which he supports his experimental evidence can be sustained. I have discussed his analysis in detail elsewhere (Vere-Jones, 1976), and do not wish to repeat the discussion here. However, I would like to describe his statement of the problem. He proposes that a fracture propagates outwards from a central point of a two-dimensional region, continuing to extend in any direction until a point is reached (which then becomes a boundary point for the fracture) where the local stress fails to exceed the local strength; both local stress and local strength are assumed to vary randomly from point to point of the area. The whole fracture ceases when there is no direction in which further extension is possible. A very similar idea is incorporated in the "Go-Game" model of Otsuka (1971, 1972a, 1972b). Both models are examples of "percolation processes" of the type introduced by Hammersley and Broadbent (1957) and subsequently described by many later writers (see Shante and Kilpatrick, 1971, for a recent review). Unfortunately, these percolation processes are very intractable analytically. So far as I know no explicit results are available, even of an asymptotic kind, for the distribution of the total area affected. Otsuka has obtained a variety of results by simulation, but for an analytical treatment it seems necessary to have recourse to simpler models such as the branching model described below. This appears to be as true for the percolation context (Fisher and Essam, 1961) as it is for the applications to earthquakes. Even if such models represent a considerable oversimplification of the physical process, it seems that they capture at least some important features of the fracture mechanism. One such model is described in the section below. The remaining sections represent an attempt to relate this model to other discussions of the fracture mechanism.

A BRANCHING PROCESS MODEL

The treatment in this section follows Vere-Jones (1976). Unknown to me at the time of writing that paper, what is essentially a special case of this model had been analyzed some three years previously by Saito, Kikuchi, and Kudo (1973), in connection with Otsuka's "Go-Game" model. The two discussions differ slightly in interpretation and analytical approach; both are essentially applications of well-known results in branching process theory. In the branching model we shall describe, it is supposed that the crack does not propagate in a single continuous movement, but through a series of steps or branches. To capture this idea in simple mathematical terms, suppose that the complete cracking episode can be represented schematically by a branching diagram such as that shown in Figure 2. Each segment or stage in the branching process either terminates in a branch point, or simply comes to an end. The behavior at successive modes is supposed independent of the lengths of the preceding segments and behavior at all previous modes, and the number of branches leading out of a mode is supposed to have a common distribution $\{f_n\}$. In the diagram, only simple bifurcation is shown, so the only nonzero terms would be f_0, representing the probability of termination without any new branches, and f_2, representing the probability of the branch dividing in two. The total lengths of all segments is then

$$T = \sum_{i=1}^{N} T_i$$

where T_i is the length of the ith segment in some enumeration. We need not insist, however, that T_i be a length. In the application to earthquakes it is more natural to think of T_i as the energy released at each step of the process.

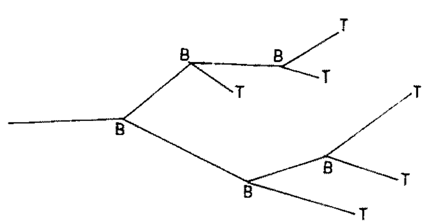

Figure 2. Schematic representation of branching model.

Then $T = \Sigma T_i$ represents the total energy released. The total number N of segments corresponds to the total number of individuals in the ordinary branching process (see Harris, 1963, or Vere-Jones, 1976). It is well known that the probability generating function

$$H(z) = \Sigma\,\text{Prob}(N = n)\,z^n$$

for this total is given as the smallest nonnegative solution of the functional equation

$$H(z) = z\,F[H(z)] \qquad [F(z) = \Sigma f_n z^n] \tag{1}$$

This total number is finite with probability one if and only if m, the mean number of new branches per mode, is less than or equal to unity (subcritical and critical cases). If $m > 1$ there is a nonzero probability that the crack will propagate indefinitely (supercritical case), i.e., that the total number of branches becomes infinite. The Laplace transform of the density function for the total length (or energy) T can be found from the equation

$$m_T(s) = E[\exp(-sT)] = H[\phi(s)] \tag{2}$$

where

$$\phi(s) = E[\exp(-sT_i)]$$

is the Laplace transform of the density function for the length of a single branch, it being supposed that the lengths of successive branches are independent with the same distribution.

The simplest special case corresponds to treating the developing crack as a birth and death process, each segment having probability μdv of terminating in $(t,\ t+dv)$ given that it has already reached length t, and a similar probability λdv of bifurcating (thus μ and λ correspond respectively to the "death rate" and "birth rate" of segments). Then $F(z)$ corresponds to the two point distribution described earlier, with

$$f_0 = \mu/(\mu+\lambda) \qquad \text{and} \qquad f_2 = \lambda/(\mu+\lambda)$$

while the length of each branch has an exponential distribution with parameter $(\lambda+\mu)$. Equation (1) now takes the form of a quadratic equation, which can be solved explicitly for $H(z)$; then substitution of

$$\phi(s) = (\lambda+\mu)/(\lambda+\mu+s)$$

in (2) yields the following expression for the Laplace transform of the total length T:

$$m_T(s) = \{(\lambda+\mu+s) - \sqrt{[(\lambda+\mu+s)^2 - 4\lambda\mu s]}\}/2\lambda$$

The corresponding density function is the Bessel density

$$f_T(x) = \sqrt{(\mu/\lambda)}\exp[-(\lambda+\mu)x]\,I_1(2\sqrt{(\lambda\mu)}\,x)/x \tag{3}$$

where I_1 is the modified Bessel function of first order. This distribution is well known in other contexts, for example in queuing theory, where it represents the density function for the busy period in a simple queue (see Feller, 1966, p. 414). Using the asymptotic relation

$$I_1(t) \sim (2\pi t)^{-\frac{1}{2}} \exp(t)$$

we find

$$f_T(x) \sim C_{\lambda,\mu} \exp\left[-(\sqrt{\mu}-\sqrt{\lambda})^2 x\right] x^{-3/2}$$

where $C_{\lambda,\mu}$ is a constant. The distribution, therefore, has a power law form in the critical case $\lambda = \mu$. Even for subcritical processes, the distribution

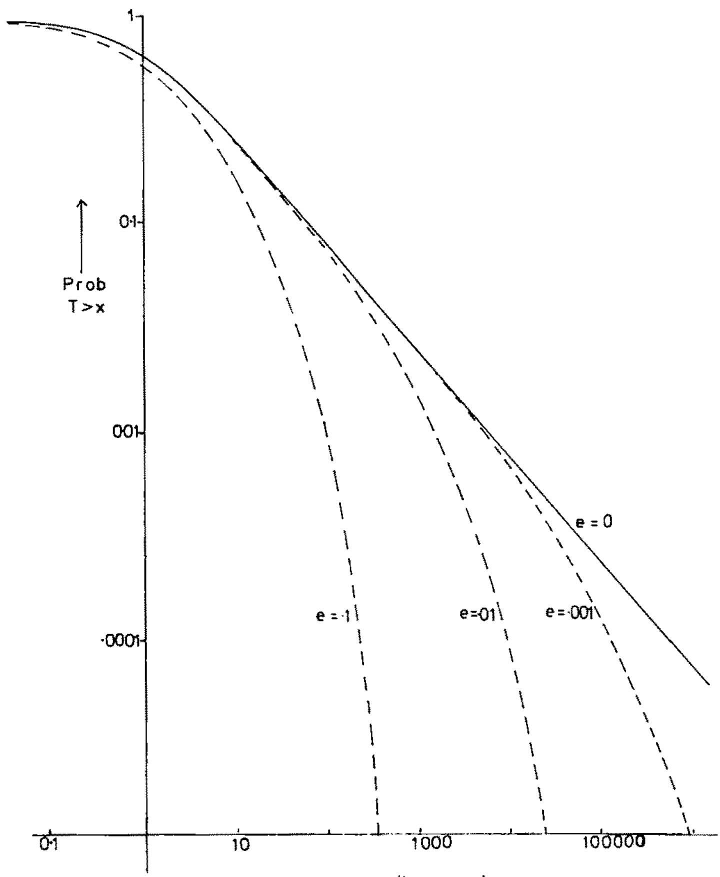

Figure 3. Graphs of survivor function Prob ($T > x$) for the distribution with density $f_T(x) = \sqrt{(\mu/\lambda)} \exp\left[-(\lambda+\mu)x\right] I_1(2\sqrt{(\lambda\mu x)})/x$ for different values of $\varepsilon = \sqrt{\mu} - \sqrt{\lambda}$ and $\sqrt{\mu} + \sqrt{\lambda} = 1$.

may approximate a power law form over a considerable range. This is shown in Figure 3, where the survivor function of the distribution, viz.

$$\text{Prob}(T>t) = \int_t^\infty f_T(x)\,dx$$

is shown on a log–log plot for different values of the parameter

$$\varepsilon = \sqrt{\mu} - \sqrt{\lambda}$$

assuming the normalization

$$\sqrt{\mu} + \sqrt{\lambda} = 1$$

As $\varepsilon \to 0$ the curves approximate to a power law form with

$$\text{Prob}(T>t) \sim \text{const} \cdot t^{-\frac{1}{2}}$$

In Vere-Jones (1976) it is shown that this type of behavior is not peculiar to this example, but holds whenever the distribution for each segment length has an analytic Laplace transform at $s = 0$, a condition implying finiteness of all moments. If only some moments are finite, the behavior is similar but the exponential decay term is replaced by a power law term of higher order ($>\frac{1}{2}$). If the segment length distribution has infinite first moment, the behavior of $f_T(x)$ is dominated by the length of the largest segment, and other types of power law behavior can also be obtained. It will be seen that these curves are qualitatively similar to the frequency–magnitude curve illustrated previously, but before we can proceed to a quantitative comparison we have to know how to interpret the parameters of the model in terms of the parameters of an earthquake, in particular the magnitude. The most appropriate procedure is probably to equate T with the total energy release. The energy suggests itself naturally because we are looking for an additive variable whose total can reasonably by identified with one of the gross parameters of an earthquake. This identification also leads to the most satisfactory agreement with the observational results.

If this identification is made, then we can use one of a number of postulated relationships between energy and magnitude to determine the value of b predicted by the model. The simplest such relationship is the familiar one of Gutenberg and Richter (1954) that

$$\log_{10} E = \text{const} + 1.5\,M \tag{4}$$

In the critical case we have the relationship

$$\text{Prob}(T>t) \sim \text{const} \cdot t^{-\frac{1}{2}}$$

Substituting for M in terms of $T = E$, we find

$$\text{Prob}(\text{Mag}>m) \sim \text{const} \cdot 10^{-0.75\,m}$$

Thus the model together with the relationship (4) leads to the conclusion $b = 0.75$.

A more elaborate analysis of the relation between instrumental magnitude and energy has been undertaken by Randall (1973), who takes into account the response of the Wood–Anderson seismometer used in determining magnitudes. He concludes that

$$\log_{10} E = \text{const} + 3\,M$$

for large earthquakes ($M > 5$) and

$$\log_{10} E = \text{const} + M$$

for small earthquakes ($M < 5$), the difference being attributable to the higher high-frequency contribution to the amplitude in smaller earthquakes. In both cases the constants depend on the stress, so that a family of parallel curves rather than a single curve is in view. As a first approximation, however, these results, together with the branching model, suggest a frequency–magnitude diagram with a change of slope from about $b = 0.5$ for $M < 5$ to about $b = 1.5$ for $M \geq 1.5$.

Soviet authors have preferred to work directly with the variable

$$K = \log_{10} E$$

rather than with the magnitude, so that the frequency–magnitude law takes the form

$$\text{Prob}(K \geqslant x) = \text{const} \cdot 10^{-\gamma x}$$

The model leads directly to the conclusion $\gamma = 0.5$, which appears to be in good agreement with observation (see Gaisky, 1970, for example, who reports a mean value of $\gamma = 0.48$ over a wide range of observations).

In summary, the model provides remarkably good agreement with typical seismological observations—too good, in my opinion, for such models to be dismissed as having no relevance to the seismic process. Nevertheless, it is not easy to accept the above model as a completely satisfactory explanation of the frequency–magnitude relation. One objection might be its apparent dependence on a linear conception of crack growth. This difficulty may be more apparent than real, for the diagram in Figure 2 is deliberately schematic. As Professor Harary has pointed out to me, many apparently more complex graphs can be mapped into this form, provided there is a well-defined starting point, and a defined reference direction. The more significant assumptions, from a physical point of view, are the representation of the whole episode in terms of the sum of a number of more or less independent steps, and the possibility at each step of an extension in one of several different ways, i.e., of branching. In the sections to follow, a first attempt is made to see how far these ideas are compatible with other work on crack propagation.

An important weakness is the failure of the model to accommodate differences in *b*-value. The reality of observed differences is hardly to be doubted. Scholz (1968a, 1968b), following earlier work by Mogi (1962), provides clear evidence that in the laboratory context at least there are systematic variations of the observed *b*-value both with the type of material and its stress environment. On the tectonic scale, differences in *b*-value have been quoted by many authors, and attributed to different geological structures, depths, stress environments, etc. As an example, Figure 4 shows the variation of *b*-value with depth for two sets of New Zealand data, the first for the main seismic region (covering most of the North Island and the northern part of

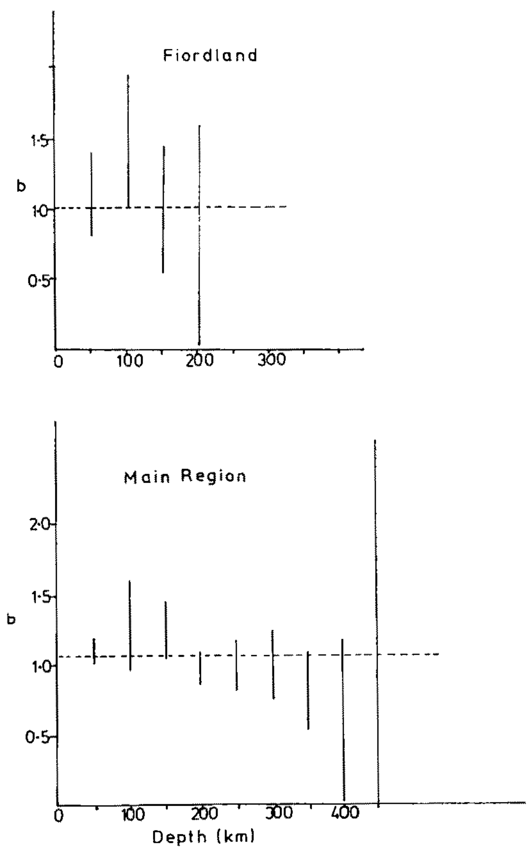

Figure 4. Variation of *b*-value with depth for New Zealand earthquakes [after Gilbert (1974)]. Vertical lines represent 95% confidence intervals.

the South Island) and the second for Fiordland (a smaller but highly active region covering the Southwest of the South Island). As with most work of this kind, some caution is needed in accepting observed differences at face value because of the statistical uncertainty involved in estimating the b-value from limited data. Here 95 percent confidence intervals (of the order of two standard deviations although the data is not normally distributed) are shown. It is not quite impossible, but perhaps stretching credulity a little far, to suppose that such variations could occur solely as the result of chance. Suggestive changes of b-value along the course of large aftershock sequences and earthquake swarms have been found by Gibowicz (1973a, 1973b, 1974) and related by him to episodes in the stress history of the source region. Such observations appear to contradict the assertion of "magnitude stability" by Lomnitz (1966), but I believe the differences are to be attributed chiefly to the much larger data sets (several thousand points) used by Gibowicz, which allowed him to distinguish relatively fine variations in b.

From the theoretical side, it is my conjecture that in the critical case not only the branching model but also such variations on it as the percolation model all lead to the same fixed value for b. (As mentioned earlier, I believe the dependence of b-value on stress claimed by Scholz for his model is due to a faulty step in his argument.) Then either the model must be changed, or some secondary mechanism must be introduced to account for variations in b. One possible secondary mechanism, mentioned in Vere-Jones (1976), relates to the distribution of the lengths (or energies) of individual segments in the branching process. If these also have a power law distribution, this can influence the form of the overall energy distribution. Another possibility is that the energy–magnitude relation, rather than the energy distribution itself, could take different forms with different materials and in different stress environments. For example, Randall's (1973) discussion suggests that different correlations of stress drop with magnitude could give rise to apparent differences in b-value. Otsuka's simulation studies, on the other hand, suggest that, in more complex processes, variations in b-value may arise naturally as the result of variations of the other parameters of the process (Otsuka, 1971, 1972a, 1972b).

CLASSICAL MODELS FOR THE EARTHQUAKE SOURCE

The elastic rebound model for earthquake mechanism, put forward by Reid (1910) after examination of field and geodetic observations following the San Francisco earthquake, still holds a predominant place in the seismological literature. In broad terms it envisages the gradual build-up of stress in a region until a point is reached where the stress exceeds the strength of the rocks making up the crust. At this point the material fails by slipping along a plane (fault), the resulting earthquake relieving, at least in greater part, the accumulated stress, and hence allowing the process to start again. Quantitatively, the earthquake can be described in terms of the length L, the depth D, and the orientation of the fault surface; the relative displacement d of the two sides; and the stress at the time the earthquake occurs. The length L can be compared with measurements of the fault trace on the surface; an estimate of D can sometimes be obtained by sounding techniques; while the fault area $S = LD$ can be independently estimated from the boundaries of the region in which aftershocks occur. Although there are considerable uncertainties concerning the relation of surface faulting to the situation at depth and the precise interpretation of geodetic measurements following a major earthquake, such measurements seem generally to produce results which are reasonably consistent with themselves and with deductions from the data on the seismic radiation (see for example, Aki, 1966a, 1966b). Nevertheless it should be borne in mind that it is only for a relatively few large shallow shocks that such a direct approach is possible; by far the larger proportion of instrumentally determined shocks are either too small or too deep to produce any measurable surface effects, and the applicability of the elastic rebound theory to such earthquakes is an hypothesis only.

However, the position of the elastic rebound theory has been strengthened over the last few decades by developments which have brought a wider range of source parameters within the scope of observational determination. Methods have been developed for estimating the energy release by integrating the area under the squared modulus of the seismogram trace or its Fourier transform (spectrum). Such estimates can be compared with the expression

$$E = \sigma S d$$

for the energy release in the elastic rebound model, and are found to be compatible, at least to within an order of magnitude, with reasonable values of the stress σ. Another parameter which has rapidly assumed importance in recent years is the *seismic moment*, equal in this model to the product $\mu S d$, where μ is the rigidity (Aki, 1966b). This quantity has the advantage that it is directly proportional to the amplitude of low frequency signals at a distant observation point and can therefore be determined rather accurately. By combining these measurements it is also possible to estimate the stress (or more probably the stress drop) σ. The far field radiation can also be used to estimate a further source parameter, namely the fault dimension L. It is found that the spectrum for seismic signals always has roughly the same shape, being near constant for low frequencies and decaying roughly as ω^{-2} at higher frequencies. The transition between these two modes of behavior determines a corner frequency which in simple models is inversely proportional to a characteristic fault length or fault radius (see Brune, 1970, or

Randall, 1973). For all of these quantities the elastic rebound model provides an interpretation which is consistent within itself and with the previous results. However, it should be borne in mind that estimates based on measurements of the seismic radiation have an indirect character; so far as I know there are no alternative methods of estimating quantities such as the seismic moment which can be used to corroborate those obtained via the spectrum and an interpretation in terms of the elastic rebound model. Other models could also lead to internally consistent interpretations, also involving some characteristic length, stress-drop, etc., at the appropriate places, but not necessarily carrying the same interpretation as in the elastic rebound theory.

A more cogent argument for accepting the general validity of the rebound theory is in terms of the radiation pattern it predicts. For simple assumptions concerning the propagation of the fracture along the fault, it is possible to obtain, from the general solution of the elastic equations, expressions for the observed displacement at a distance [the vector $\mathbf{r} = (r, \theta, \phi)$] from the center of the fault. These are typically of the form (Haskell, 1964; Aki, 1967):

$$U = (1/|r|)\,C(\theta,\phi)\int_0^L D[\xi,(r-\xi\cos\theta)/v]\,d\xi \tag{5}$$

where $C(\theta, \phi)$ describes the radiation pattern, $D(\xi, t)$ is the prescribed displacement of the fault at a point ξ from the center of the fault and time t after the initiation of the fracture, where $t = (r-\xi \cos\theta)/v$. The factor $C(\theta, \phi)$ can be identified with the factor resulting from the action of an equivalent double couple (with no fault) instantaneously introduced at the fault. This sort of identification is very convenient for the further study of the radiation, for it allows the problem to be stated and solved by classical techniques of multipole theory, and indeed it is from this identification that the concept of "seismic moment" is derived. What is important, however, is that the radiation pattern can be directly studied by combining information on the direction and amplitude of first motion obtained from observation points at many different azimuths around the source. While the observed patterns are consistent with both single and double couple models, it is generally accepted that the appropriate model is a balanced double couple (Kostrov, 1970), and on this basis procedures have been developed for relating the observed pattern to the orientation of the fault plane and the direction of motion along the fault. Again, these results are generally consistent with field observations where these are available.

In summary, most seismological work is in terms of, and is consistent with, a single major movement among a particular fault plane. The possibility of more complicated source mechanisms is not ruled out by the evidence; it would be a matter of reinterpretation rather than of direct conflict.

Expressions for the total energy, for example, or for the seismic radiation (5), could be used in conjunction with the hypothesis that the fault motion proceeded in a series of steps or phases. One would anticipate that the resulting spectrum would be the superposition of the spectra from the different phases. The observed corner frequency would presumably be that associated with the largest step. On the other hand, first motion studies would presumably relate to the initial phase. Insofar as the general orientation of the motion was likely to be determined by the general stress pattern, the different phases would show a consistent mechanism, and there seems no overriding reason why the data should not be interpreted consistently in this style. There is, indeed, a more basic reason why it should not be possible to distinguish between such models on the basis of information derived from seismic waves. This is that the waves cannot "see" features which are small by comparison with the wavelength. Since the frequency of seismic waves is typically of the order of a few Hertz, and elastic wave velocities are of the order of 5–10 km/sec, the ultimate limit of resolution is of the order of a few kilometers; branching effects on a finer scale than this are in principle not capable of detection by such methods. It should also be borne in mind that in "classical" terms seismology has been seen principally as a tool for obtaining information about the internal structure of the earth; for this purpose simple source models which can be expressed in terms of the radiation pattern from an equivalent couple are entirely adequate. For evidence which really bears on the source mechanism, it is necessary to observe the situation at the source itself, a problem which seems likely to remain beyond our technical resources for some time. The more detailed study of crack phenomena on the laboratory scale, and the correlation of this evidence with evidence relating to earthquake premonitary phenomena, seem the most fruitful approaches to this problem at present.

CRACK PROPAGATION IN AN ELASTIC MEDIUM

In principle the problem of crack growth in an elastic medium can be formulated as that of finding the time-dependent solution of the elastic equation with suitable boundary conditions imposed to correspond to the external forces on the one hand and the presence of the crack or zone of weakness on the other. This is a problem of great mathematical difficulty. Even apart from the question of determining what are physically appropriate boundary conditions, the equations become very complex in all but the simplest cases. To obtain even approximate solutions it is necessary to make very drastic assumptions, such as in the previous section where it was supposed that the displacement along the crack is specified (instead of being determined as part of the solution of the problem) or by imposing a condition such as selfsimilarity or constant rupture velocity which is of dubious physical standing. The reviews of Archambeau (1968), Burridge (1968), and Kostrov (1970) provide useful perspectives on the status of such models, and their relation to more realistic theories.

The first steps towards providing a physical basis for crack extension were taken by Griffith (1921, 1924), whose ideas still play a dominant role in the theory of fracture mechanics. In order to account for the low fracture strength of glass, at least by comparison with its theoretical strength calculated from atomic data, Griffith postulated the existence of small flaws or microfissures which greatly magnified the applied stress in the immediate vicinity of the tip of the crack or flaw. Fracture of the specimen occurs as a result of local failure near the tip of the crack with the highest stress concentration; in tension at least, this local failure increases the stress concentration still further and hence leads to a catastrophic extension of the crack.

Perhaps the most important idea arising from Griffith's work is the concept of an energy balance which determines whether and how a given crack will extend. Work is done against the cohesive atomic forces in separating the two edges of the crack; this must be balanced by the release of stored elastic energy as the crack extends. Consider first the case of a crack in tension, with the crack at right angles to the tension (two-dimensional problem). The stress in the vicinity of the crack tip can be shown to exceed the applied stress σ by a factor of $2\sqrt{(c/r)}$, where c is the length of the crack and r is the radius of the crack tip, supposed equal to the atomic dimension. If it is assumed that a separation of the order of an atomic dimension is needed to pass beyond the range of the atomic forces, the elastic energy released per unit extension of the crack will be $2\sigma^2 c/E$, where E is Young's modulus. Also, the work done against the atomic forces per unit extension can be expressed in the form 2α, where α is the surface energy per unit area of crack surface. Equating these two expressions, an approximate equation of balance is

$$c = \alpha E/\sigma^2$$

This expression gives the critical crack length for a given applied stress σ; a longer crack will start to extend, and will then continue extending until the material is ruptured; a shorter crack will not extend.

By balancing the rate of energy release against the rate of work done against the atomic forces, the same approach can be extended to the dynamical context, and used to provide an expression for fracture velocity. A simple theory of this kind is given by Jaeger and Cook (1969) following work of Berry (1960a, 1960b).

The application of these ideas to the failure of rock in more complicated stress fields is problematical. It is not clear, for example, how much of the stored elastic energy is available for release as the crack extends. Griffith (1924) himself reverted to an alternative approach in dealing with materials under compressive stresses; he determined the tangential stresses around an elliptic crack, and assumed that failure would occur when the maximum tangential stress exceeded the theoretical strength of the material. This approach has been followed in much of the later work on fracture criteria, although it suffers from the disadvantage of being more seriously dependent on the validity of the geometric assumptions concerning the shape of a crack. For a material in simple compressive stress, it predicts that the crack with the most vulnerable orientation will be inclined at about 30° to the direction of compression, and that the initial extension will not be along the axis of the crack, but in a direction roughly bisecting the angle between the axis of the crack and the direction of the applied stress. This last result is of considerable significance, for it implies that crack growth in compression is not of itself unstable.

A rather different approach to the role of atomic forces is contained in the discussion of the equilibrium *shape* of a crack given by Landau and Lifshitz (1965) following work by Barenblatt. This work may be regarded as a first attempt to solve the elastic equations (equilibrium equations in this case) without taking the form of the crack as given, but supposing instead that it is determined by the applied stress and the interatomic forces. The conclusion reached is that the end of the crack will be broadly parabolic in form

$$y \propto \sqrt{(L-x)}$$

where $2L$ is the crack length, x is the distance from the crack center, and y is the displacement, except for a tiny notch at the tip itself, which takes the form of a cusp

$$y \propto \pm (L-x)^{3/2}$$

for $L-x$ of the order of an atomic dimension. Such a sharpening of the crack tip is indeed a feature of some electron micrographs (see, for example, papers in Averbach *et al.*, 1959). It may be regarded as the means by which the interatomic forces are balanced against the elastic forces to produce an equilibrium configuration.

Barenblatt's analysis for the static problem was extended to shear cracks in Barenblatt and Cherepanov (1961) (see also Sih and Liebowitz, 1968). A possibly more important development is due to Kostrov (1966), who was able to solve the simplest case of a shear crack in Mode II (propagation at right angles to the direction of shear stress, applied parallel to the fault) to obtain an "equation of motion" for the crack tip. This equation determines the position of the crack tip as the crack extends in response to specified initial tractions along the crack, and given assumptions concerning the forces acting at the crack tip, effectively equivalent to assumptions concerning the dependence of the surface energy on the velocity of crack propagation. Most recently, Husseini *et al.* (1975) have used this approach to ask the question, "How does a crack stop?", examining two possible mechanisms, increase in fracture strength (surface energy) and decrease in elastic stress (seismic gaps), and obtaining expressions which allow the fracture energy to be related to other seismic parameters such as the stress drop and the seismic moment.

All the work reviewed in this section is committed to a view of crack propagation as a single movement. It does not of itself bring forward any new evidence as to whether one or more steps are involved, and indeed any claim to accurately mirror the physical process of fracture would probably be discounted by the authors we have cited. From the point of view of a more realistic theory, the principal deficiencies are probably the restriction to unnaturally simple geometries and stress configurations, and the omission of any discussion of the effects of flaws and inhomogeneities, which may exert a controlling influence on crack growth in rock.

MICROFRACTURING AND DILATANCY

In this section I shall comment briefly on some of the direct experimental and observational work on the nature of the fracture process. I have included in this heading microscopic and ultramicroscopic studies of fracture; engineering studies of the behavior of rock under stress; as well as the information, on quite a different scale, relating to dilatancy preceding a major earthquake. These may seem strange bedfellows, but I suspect they have more in common with each other than with the idealized fracture mechanisms discussed in the preceding two sections.

There is an enormous literature relating to technological aspects of fracture in metals and other materials, which in turn has prompted a large amount of fundamental research on fracture. The most detailed work, particularly at the microscopic level, has been done on the fracture of crystalline metals, usually in tensile stress conditions. In such materials dislocations of the crystal lattice typically precede fracture and there is a considerable theory relating to the growth and accumulation of such dislocations (see Nicholson, 1972), where both statistical and physical aspects of this process are discussed. From our point of view, such dislocations manifest themselves as a zone of plastic deformation immediately in front of and around the crack tip. To this extent, the behavior of such crystalline solids differs from the purely brittle behavior described in the preceding section. Some aspects of plastic yielding can be accounted for relatively simply by assuming a larger effective surface energy of crack formation. With this modification the energy balance approach may still be applicable. However, the detailed picture of fracture propagation may be significantly more complex. Cottrell (1959) describes the process as follows: "Propagation often occurs by the separate nucleation of small cracks in a plastically deformed zone in front of the main crack . . . The general picture . . . is one for which a number of glide dislocations . . . become converted at some place in the crystal into cavity dislocations which then spread and multiply in the form of a growing crack." Metals frequently exhibit a transition between cleavage fracture or brittle fracture at low temperatures, and ductile fracture, at higher temperatures, in which the above processes dominate and the metal is literally torn apart. It appears likely, therefore, that there are at least two stages to the process—initiation of cracks, either at crystal boundaries or from dislocations forking and moving within a crystal, and their subsequent propagation. Brittle fracture occurs if the cracks produced by the dislocations are long enough to propagate directly as Griffith cracks. If this is not the case, they may grow by a more gradual process of plastic deformation leading to crack linkage. Petch (1959) and Hahn, Karrinen, and Rosenfeld (1972) provide more detailed reviews of these processes.

Major uncertainties arise if we ask how far the above concepts are applicable to rock, or to materials subjected to more general stress fields. The term "rock" covers a wide variety of materials, and one must beware of assuming blithely that the same mechanism operates in materials of different types. Cracks may originate within crystals, at crystal boundaries, or from existing flaws. The behavior of rock under compression has been extensively studied but a definitive interpretation of the results is missing. In conventional tests rock specimens fail chiefly by cleavage along a diagonal plane. This appears to support the Griffith theory, but the effect may be spurious in that it may relate to the testing machine more than to the intrinsic properties of the rock. Catastrophic failure occurs because the machine continues to unload energy into the specimen even after it has begun to deform. Using stiff testing machines, or machines controlled by a servomechanism, it appears that rock retains some load-bearing ability even beyond the normal fracture point (e.g., Wawersik and Fairhurst, 1970; Brown *et al.*, 1972). Vertical splitting, parallel to the direction of the compressive stress, is commonly observed but its explanation appears obscure. The relative stability of crack growth under compressive stress has already been referred to. This effect has been studied by Brace and Bombolakis (1963) and Hoek and Bieniawski (1966) using specially constructed specimens (plastic) containing annealed cracks. They suggest that crack linkage may be necessary to secure the formation of a major fracture.

A new phase of experimental work on rock specimens was ushered in by the discovery of dilatancy effects in rock under compression and its possible implications for earthquake prediction. Dilatancy is a term borrowed from soil mechanics, where it refers to the stress-induced increase in volume (relative to the volume changes predicted by elastic theory) caused by grains of the material moving against each other in response to the applied stress. If the material is initially in a closely packed state, any change must be to a less closely packed state, and hence should be accompanied by an increase in volume. If the material is initially water-saturated (wet sand for example), the increase in void space causes a partial drying out, increasing the effective friction between particles and hence increasing the strength of the material (dilatancy hardening). Brace and co-workers at MIT (Brace *et al.*, 1966; Scholz, 1968a, 1968b) carried out an extensive series of measurements on the physical properties of rock specimens undergoing compression. As in earlier work by Mogi (1962), they found that the compressive process was accompanied by the emission of small acoustical signals from the rock specimen; these were attributed to the formation of microfractures in the specimen. This process was accompanied by an increase in volume, the extent of the observed dilatancy appearing to vary proportionately to the cumulative number of observed microfractures (Scholz, 1968b). Changes in the "b-value" were also reported, with a progressive tendency towards lower b-values at higher stresses. Brace and Orange (1968) reported changes in conductivity which they also associated with the onset of dilatancy. More recently, measurements on the P- and S-wave velocities have been carried out in similar conditions (Hadley, 1975).

Interest in this work was heightened by the observation of Soviet workers that changes in a number of these variables, particularly the ratio V_P/V_S, occurred prior to some shallow earthquakes in Central Asia (see Saverensky, 1968). Similar changes were subsequently reported from the United States, New Zealand, and other seismic regions. In some cases it was found that, after an initial reduction in the V_P/V_S ratio, a recovery to normal values just preceded the earthquake itself.

Explanations of these results remain at a qualitative and rather tentative level (a review of the situation as of 1974 is given in the PAGEOPH, Vol. 113, special issue on earthquake prediction). A popular explanation (e.g., Scholz, Sykes, and Aggarwal, 1973; Press, 1975) supposes the following sequence of results. Increasing stress causes the onset of dilatancy, which leads to a lowering of the V_P/V_S ratio and dilatancy hardening, the rock being supposed initially saturated. The ratio remains low until sufficient water has diffused back into the region to fill the extra void space. At this point the material is again saturated, the V_P/V_S ratio returns to its normal value, the rock is weakened, and the earthquake occurs. It is supposed that the size of the earthquake will be related to the size of the dilatant region. An important feature of this theory is that it leads to an estimate, in terms of the diffusion parameters, of the time delay between the onset of dilatancy and the occurrence of the earthquake. Anderson and Whitcomb (1973) report a satisfactory agreement at this point. There remains some doubt as to the status of this theory, however. Soviet writers appear to have developed a "dry dilatancy" theory based on a nonlinear process of microfracture formation and stress intensification within the material (Sobolev, 1975). An apparently similar theory has been proposed in a series of papers by Brady (1974). He suggests that microfracturing within a region causes a reduction in the elastic constant within that region; this leads to the development of tensile stresses at certain points around the boundary of the region, hence to further microfracturing and a further increase in stress. The picture here is qualitatively similar to the microscopic picture of crack propagation in a crystalline material quoted earlier in this section. In both cases, however, the formulation of a quantitative theory appears to pose formidable difficulties.

STATISTICAL STRENGTH THEORIES

Statistical strength theories represent a natural extension of the Griffith theory, in that they seek to provide a quantitative relation between the observed strength of a specimen and the statistical distribution of the flaws or microfissures existing in it. For tensile tests, it can be assumed that the material will rupture if the material contains at least one crack of such a length and orientation that the stress at its tip exceeds the theoretical strength of the material. The statistical problem is therefore to determine the probability that the given material contains at least one crack of this kind. In general the dimensions of the material may be supposed large with respect to the lengths of the cracks and the distance between them. Hence the distribution can be approximated by a Poisson process. The probability $S(L)$ that the material contains no crack of length L or greater is then given by an expression of the type

$$S(L) = \exp[-\mu V F(L)] \qquad (6)$$

where μ is the crack density, V is the volume of the material, and $F(L)$ is the cumulative distribution function for the crack lengths.

Discussion from this point hinges on the choice of the distribution function $F(L)$ and on finding methods of extending the model to take into account nonhomogeneous stress fields, cracks of specific shapes and orientations, etc. The first explicit development of a theory of this kind appears to be that of Weibull (1939a, 1939b), which in my view remains one of the most satisfactory treatments. Weibull chose the power law form

$$1 - F(L) = cL^{-\alpha}$$

which on substitution in (6) leads directly to the Weibull distribution for the strength of the specimen. It should be noted that the theory predicts not only the scatter of values for the strength of a specimen of given dimensions, but also the variation of mean strength with specimen size. Thus it follows from Weibull's assumptions that the mean strength decreases with volume according to $V^{-1/\alpha}$. Such a decrease of strength with volume is well known and is referred to as the size effect. But while the statistical theory gives a good qualitative agreement with observational work, it is my impression that the quantitative agreement is somewhat more variable. Weibull himself, in the second of the papers cited, was obliged to postulate the existence of additional structural complexities in the material to account for observed deviations from the simple law in some cases. On the other hand, Anderson (1959) gives a favorable report on the statistical theory in Griffith's original context of the strength of glass.

Many variations on the theme exist, apparently discovered and developed independently in relation to different materials. Fisher and Holloman (1947) developed a statistical theory for the fracture of certain metals (pearlitic steels) in which the defects initiating fracture are thought to be plate-like carbide particles embedded in the steel. This feature gives rise to an important orientation effect, and a dependence of strength on stress, for insofar as the material deforms plastically the plates tend to align themselves parallel to the stress as the stress increases. This example also shows how important specific material properties may be in determining the form of an appropriate fracture theory (see also Charles and Fisher, 1959).

Russian work on statistical strength theory is expounded in the monograph by Volkov (1962), although this is so badly translated that it is often hard to follow. This work reviews a much wider range of applications of statistical ideas, starting from the effects of randomly distributed variations in stress concentration and strength on the material properties (effective elastic constants, for example) and leading to detailed discussion of failure in specimens of different geometries. I would be interested to know just how widely such an approach has found application in engineering. From a theoretical point of view, Volkov's approach differs from Weibull's in that it does not start from a physical picture of the microfissures but takes as given a normal distribution for the variations in stress and strength on a microscopic scale. This approach appeals to me less, and it appears to me that there are numerous places in Volkov's discussion where the argument needs rephrasing (at least) to be expressible in rigorous form.

A completely different type of statistical model dates back to a paper by Daniels (1945) on the strength of fiber bundles. In this paper Daniels supposes that the strengths of the individual threads making up the bundle vary statistically; that the load is distributed evenly over the threads in the bundle;

and that if one fiber fails, its portion of the load is evenly redistributed over the other fibers. On these assumptions he shows that the strength of the bundle is approximately normally distributed and finds expressions for the mean strength and the variance. This theory has recently been modified to describe the strength of fiber composite materials (see the review by Kale and Kelly, 1972).

All of this work relates to static strength theory. More recently Coleman (1958), Yokobori (1965), and Hori (1959, 1962) have initiated, in different contexts, the study of stochastic models for time-dependent aspects of fracture where the time to failure as well as the stress at the fracture point is treated as a random variable. Coleman, for example, describes a time-dependent version of Daniels' theory in which it is supposed that each fiber in the bundle has a time to failure which is exponentially distributed with a mean life inversely proportional to the stress applied across the fiber. The overall process is then Markovian, and can be handled by solving the forward differential equations. The immediate relevance to crack propagation is not clear, but some combination of these ideas with the approach of Scholz and Otsuka would seem a fruitful area for further study. In the meanwhile the works on statistical strength theory are perhaps of main interest to us for the support they lend to the notion that microfractures and other weaknesses play a basic role in the fracture process.

CONCLUDING REMARKS

In this paper I have tried to put into perspective a variety of approaches to the problem of crack development in rocks, with a particular view to assessing the plausibility of the branching-type model. It seems to me that no clear-cut conclusions emerge. The extensive material on crack propagation in an elastic medium is acknowledgedly an idealization of the physical picture, at least on a microscopic scale. Most seismological work is based on the assumption that an earthquake represents a single motion, and although this is consistent with the seismological data, it seems unlikely that there is at present any evidence that would rule out a more complex process. Direct studies of fracture on the microscopic or laboratory scale also support the idea that fracture is a complex process, which in certain circumstances may proceed through crack linkage. It seems possible that statistical models for the development of a fracture could provide a useful compromise between a purely qualitative model and a full theoretical treatment. If this represents a program for the future, I see some of the more immediate steps as follows:

(a) In relation to the frequency–magnitude law, to confirm whether other models, e.g., the percolation process model, lead to the kind of asymptotic behavior exhibited in this paper for the branching model. It might be possible to formulate and prove a general asymptotic result that is independent of the detailed structure of the model.

(b) To find a theoretical mechanism to account for variations of *b*-value. I do not regard this problem as having been adequately treated at the present time.

(c) To investigate quantitative models for the process of stress intensification and microfracturing near the tip of a developing crack.

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