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ON ERRORS IN SEISMOLOGY

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ABSTRACT

People are not killed by earthquakes but by mistakes. The basic mistake is committed by the victim who chooses to live in an unsafe structure. Seismologists and earthquake engineers are merely accessories to this erroneous decision. We discuss some of the errors found in seismology —with appropriate irreverence and with an oblique glance towards the peccadilloes of earthquake engineers! It is all in good fun, and the idea is to honor a great friend and teacher: don Rodrigo Flores.

INTRODUCTION

Science, like engineering, stands on two feet: theory and practice. The feet may be pretty or homely, they may be bare or elegantly shod, but they must always be kept on the ground.

Seismological measurements may be supposed to be relatively simple: but this is merely the first of several mistakes. Arrival times, amplitudes, frequencies, to name the basic ones, are perhaps essentially straightforward to measure; but they are also extremely difficult to interpret.

The accepted theory of earthquake locations is old and shaky: it was originally formulated by Geiger (1). The continuing mess of magnitude

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determinations is by now an ancient scandal: yet no one dares to take arms against this particular sea of troubles. Over 50% of the published focal depths are "restricted", i.e. based on depth phases and/or on the intuition of the seismologist instead of on recorded travel times. The table for depth phases is ultimately derived from these focal depth determinations. The circularity of this procedure escapes notice, because there is no theory to hold it in check. The confidence ellipses obtained for epicentral locations are widely known to be meaningless. Quite possibly, so are spectral ratios. Engineers continue to trust and use these data in all good faith.

Similar problems may exist in earthquake engineering. Fond hopes to the contrary, earthquake-proof structures cannot be built to any imaginable set of specifications. More and more often, it is the structures designed and built by engineers that cause the largest death tolls in seismic disasters. If they only knew what seismic motion to expect, the abilities of engineers might be conveniently put to a test; but seismologists don't know or won't tell. There seems to be no available alternative to building structures in which ignorance of the input is compensated by a generous measure of good luck. Conversely, if people wouldn't insist on living in such buildings we seismologists might tend to our business with a minimum of unpleasant interruptions due to disasters. Each profession blames its shortcomings on the other.

Magnitude determinations

The magnitude of an earthquake as measured at a station i is defined as

$$M = \log (A_i/T_i) + \text{constant}, \tag{1}$$

where A_i is the peak amplitude (of some phase) and T_i is the corresponding period.

When the magnitude is measured at more than one station, the question of how to estimate the mean magnitude $\overline{M}$ arises. It is reasonable to define the mean magnitude as that magnitude which corresponds to the mean of the measurements A_i/T_i:

$$E\,[A_i/T_i] = \frac{1}{n} \sum_n (A_i/T_i) \tag{2}$$

Thus if one has several measurements of amplitude and period one may write

$$\overline{M} = \log \frac{1}{n} \sum_n (A_i/T_i) + \text{constant} = \log \frac{1}{n} \sum_n 10^{M_i} \tag{3}$$

which is unbiased with respect to the measured quantities A_i and T_i. The accepted current procedure, however, consists of averaging the station magnitudes M_i:

$$M^* = \frac{1}{n} \sum_{n} M_i = \frac{1}{n} \sum_{n} \log (A_i/T_i) + \text{constant}, \tag{4}$$

a biasied estimator of $\overline{M}$. This can be shown as follows.

For any positive random variable X the following inequality holds between the means:

$$E [\log X] \leq \log E [X] \qquad (X \geq 0) \tag{5}$$

where the left-hand side is $\log \prod X^{1/n}$, the logarithm of the geometric mean, and the right-hand side is the logarithm of the arithmetic mean. But for positive X the geometric mean is always smaller than the arithmetic mean. The same is true for the respective logarithms. It is easy to prove by induction that the fewer the observations, the larger is the error introduced in this way.

Magnitude measurements are few in number —usually between two and for any given event. Therefore the published magnitudes underestimate the true mean magnitudes by a sizeable amount. The error is most serious for small earthquakes, which have a more restricted station coverage.

Example: For a given earthquake the following set of magnitude measurements is available: [4.6, 4.3, 5.3]. The mean magnitude is

$$\overline{M} = \log \frac{1}{3} (10^{4.6} + 10^{4.3} + 10^{5.3}) = \log (260{,}000/3) = 4.94,$$

and the current procedure yields instead

$$M^* = \frac{1}{3} (4.6 + 4.3 + 5.3) = 14/3 = 4.66,$$

thus underestimating the mean magnitude by 0.28, or almost the logarithm of 2.

In this example, an amplitude based on the present procedure would be unconservative by a factor of two. Earthquake hazard computations are often extrapolated from the magnitudes of smaller earthquakes. Therefore the hazard of large earthquakes is underestimated. The effect can be considerable. The recorded amplitudes of aftershocks of the 1985 Mexico earthquake were about twice as large as their magnitudes would predict, as scaled to the amplitude of the main shock. This is precisely the result one should expect from the use of M* instead of $\overline{M}$.

The least-square method of earthquake location has been in use for about 80 years. It is based on the assumption that the errors in the measured arrival times t_i are zero-mean normally distributed, since the least-square procedure is optimal only for this special case. But the idea that positive and negative errors are equally likely flies in the face of the truth. All travel-time measurements contain a positive bias or time lag.

a) *Travel-time errors*

Signals from earthquakes invariably emerge from the background noise; that is to say, their amplitudes must grow larger with time until they stand out from the background. Otherwise they remain hidden in the noise and are never detected. Thus the likelihood of reading a later phase as t_i depends inversely on the signal/noise ratio, which must be greater than unity for a reading to take place. Hence the probability of a lag is always greater than zero. On the other hand, the probability of reading some early noise for the beginning of the signal is small. It can be assimilated to human error: a computer would hardly make it.

The human error was estimated by Freedman (2). She subjected a number of seismogram readers to independent tests of readings of a set of records and obtained a Gaussian frequency distribution of their estimates of t_i. Obviously, the true t_i remained unknown; there was no attempt to determine it independently of the readers. The dispersion of the readings represented therefore human error and not the error in the travel times, which should have differed from seismogram to seismogram. For this reason the central value of the distribution found by Freedman was biased with respect to the true arrival times. Hence the normal distribution of the travel-time errors could not have been centered about zero, as Freedman assumed.

b) *Station errors*

Delays due to the local geology (chiefly the underground of the station) are difficult to estimate. Instrumental errors are chiefly due to electromagnetic lags in the transducer circuitry of the seismometer. They are always positive. Usually the electrical time lag is estimated at up to 0.5 s for standard Benioff-type equipment. As the readings of t_i are reported to within 0.1 s these lags are far from negligible; they should be added to the travel-time errors.

c) *Model errors*

Even if one supposed that all errors mentioned thus far were zero-mean normally distributed, it would still be necessary to prove that the location model is linear. This is not the case, since the geometry is spherical and the slope of the travel-time curve $dt/d\Delta$ is a function of the epicentral distance Δ.

Example: For $\Delta = 20^{\circ}$ the value of $dt/d\Delta$ is 16 s/° and for $\Delta = 80^{\circ}$ it is 4 s/°. A one-second error made at 20° would therefore shift the epicentral location by 1/16 degree while at 80° it would shift it by 1/4 degree —a difference by a factor of four. Yet the least-square procedure attributes the same weight to both errors.

To make matters worse, the distribution of stations is biased towards the longer distances. Assuming a uniform density of stations per square degree, the area of a ring of epicentral distance Δ and width $d\Delta$ is proportional to $\sin(\Delta/2)\, d\Delta$. Thus there are 3.7 times more stations at 80° than at 20°. The programs used for epicenter determination neglect this fact and remain unaware of the corresponding bias. In conclusion (as we just saw) the errors at 80° weigh four times more than those at 20° and occur nearly four times as often. It is clear that the errors at distant stations will dominate the least-square solution. Finally, these errors are also intrinsically larger than those at near stations, because the signal weakens as it travels outward while the noise remains independent of the epicentral distance. Thus the signal/noise ratio decreases with distance.

Travel-time tables are compiled on the basis of earlier epicentral locations based on least squares. If the errors in these locations are systematic they contaminate all future leastsquare locations through the repeated use of the travel-time tables. The resulting self-consistency is spurious and is apt to delude the seismologist into thinking that the tables are correct and that therefore this location is also correct. It is true that epicentral locations are quite insensitive to errors in the travel times —*provided that the azimuthal distribution of the observations is homogeneous* Unfortunately, the earth is divided into oceanic and continental areas, and most stations are located on continents while most earthquakes occur at the edge of oceans. Therefore the azimuthal distribution of stations is almost always lopsided.

Epicentral locations

The fact that least-square epicentral location works at all can be understood as follows. Let $[S_1, S_2, \ldots S_i, \ldots S_n]$ be a set of n stations ordered by increasing arrival times. Thus $t_1<t_2<t_3$ and so forth. The meridian which bisects the arc S_1S_2 defines two hemispheres, one of which contains the epicenter. It is the same hemisphere which contains the earlier stations S_1. If we repeat this procedure for all sequential station pairs we obtain a set of n hemispheres $[H_1, H_2, ... H_n]$ each of which contains the epicenter. Therefore the intersection

$$A_n = H_i \cap H_2 \ ... \cap H_n \tag{6}$$

contains the epicenter.

A_n is a spherical polygon of at most n sides, whose area converges to zero as n increases. Thus it is possible to locate earthquakes to any desired accuracy by order statistics, without using either arrival times or travel-time tables.

If travel-time tables in epicentral location are redundant, what is the effect of their use? The answer depends on the procedure and on the error. When A_n is smaller than the mean error in the tables no added accuracy can be provided by using travel-time tables. This is quite often the case, especially in regional location work.

Example: When $t_1 = t_2 = t_3$ the hemispheres H_1, H_2, H_3 intersect at a point which is the epicenter. Thus $A_3 = 0$. No possible improvement can be achieved by using travel-time table in this case. If the procedure jointly estimates the epicenter and the focal depth the use of travel-time tables merely serves to contaminate the focal-depth estimate with the error in the tables.

This is the point we wanted to make. The two remaining parameters (focal depth and origin time) are strongly model-dependent. They cannot be estimated without travel-time tables at all. The reason is the fact that all observations are necessarily confined to the earth's surface and the focal depth is unknown: hence no information about the velocity structure at depth can be obtained in this way. Inversion procedures need some starting point, usually provided by controlled explosions.

The better the epicentral determination (latitude and longitude), the more the travel-time errors contaminate the least-square focal depth estimate. In other words, the errors in the least-square procedure are not evenly partitioned between the four variables involved, as the location program assumes.

Since most rays exit through the lower focal hemisphere, any positive bias in the arrival times tends to raise the estimated focus into the air. The program will rarely generate a spurious deep-focus estimate. Comparisons using controlled seismic sources suggest that the error in the Jeffreys-Bullen travel-time tables averages about −2 s, or up to −16 km in terms of focal depth. This error tends to compensate the bias in the travel-time measurements. Quite possibly, one is caused by the other.

Amplitude enhancement on soft ground

It is only with considerable trepidation that I approach the subject of ground amplification. In my opinion the entire matter needs rethinking, but unfortunately I cannot suggest a better approach at this time.

Consider a bundle of seismic rays emerging at the earth's surface at two adjacent locations, one on firm ground α ("rock") and the other on soft ground β ("soil"). Let $R = v_\alpha/v_\beta$ be the rock/soil shear velocity contrast. In a simple-minded way let us calculate the amplitude enhancement of the horizontal component of SV-waves at the surface of the soil, as compared to the adjacent rock (Fig. 1).

Each ray may be thought of as a channel carrying a unit of seismic energy. When the rays are refracted upwards the horizontal component of the SV amplitude vector is enhanced as

$$f = \cos\beta/\cos\alpha$$

where f may be called the soil enhancement factor. The total energy flux is

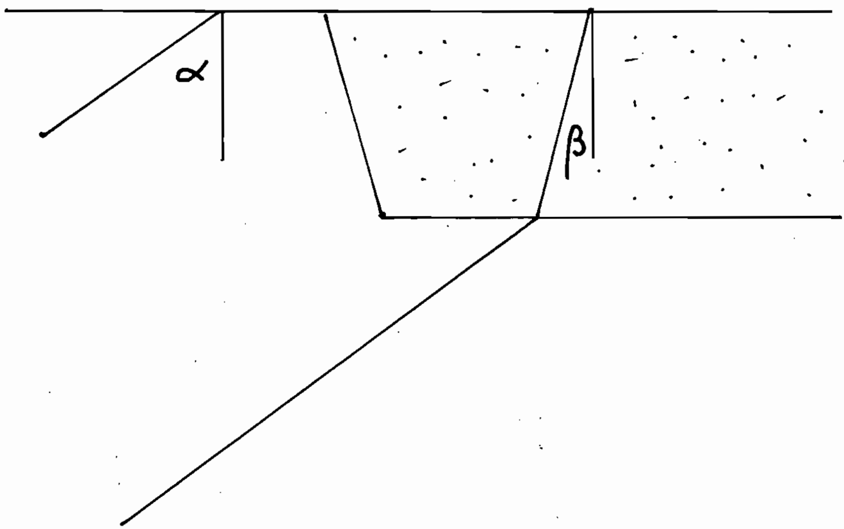

Fig. 1. Two rays of SV waves incident to the free surface: α, on rock, and β, on soft soil.

the same on soil as on rock, except for losses due to reflection, scattering and conversion at the rock-soil boundary. It does not matter that there are intermediate layers of other materials γ, δ, ..., ω between the soil and the rock at depth, because both f and R are transitive:

$$\frac{v_\alpha}{v_\beta} = \frac{v_\alpha}{v_\gamma}\,\frac{v_\gamma}{v_\delta}\cdots\frac{v_\omega}{v_\beta} \tag{8}$$

and similarly for f. The soil enhancement factor f depends mainly on the angle of emergence α on hard ground:

$$f = \frac{\cos\beta}{\cos\alpha} = \frac{\sqrt{R^2 - \sin^2\alpha}}{R\cos\alpha} \tag{9}$$

Thus if we knew R and α we could presumably compute the soil enhancement factor f on soft ground. Actually it is not even necessary to know the velocity contrast R very precisely, since it turns out that Eq (9) may be approximated for soft ground as

$$f = 1 / \cos\alpha \qquad (R > 3). \tag{10}$$

where f is overestimated by less than 5% as compared with the exact formula (9). The soil enhancement factor is shown in Fig. 2 as a function of the angle of emergence α. Note that f is unity for vertical incidence and increases rapidly for oblique incidence.

Now everybody knows that real sediments do not behave in this fashion. First, for $\alpha > 30°$ the loss of SV energy due to conversion increases markedly. This in itself should not deter us from using this approach since the net value of f will still be between 1 and ∞. But the observations also show that amplitudes in sedimentary basins depend very little on the angle of emergence. Amplitude ratios of between 5 and 10 are routinely observed on any sediment at all —not necessarily in extremely soft soils. In the 1985 Mexico City strong-motion records the enhancement was seen only in the horizontal components and not in the vertical.

The seismic intensity on sediments is consistently three degrees higher than on adjacent sites on hard ground. This basic fact remains unexplained. All soils in a given area (say California, or central Mexico) enhance the amplitudes in about the same ratio, no matter what kind of rock the soils are compared to. It hardly matters whether the amplitudes on soft ground are referred to the amplitudes of an adjacent granite or of an adjacent conglomerate or tuff. Amplitude differences between adjacent locations on soft ground are considerably larger than the differences caused by the basement/soil impedance.

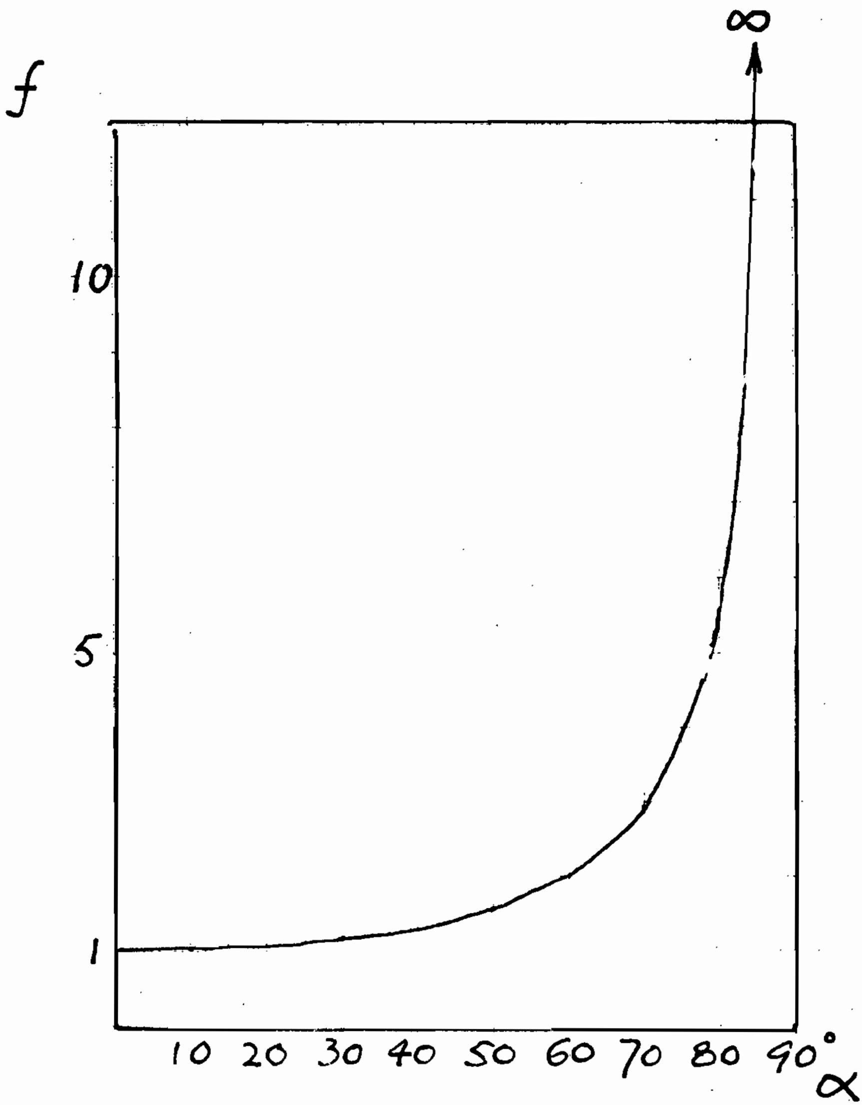

Fig. 2. The enhancement factor f of the horizontal SV component, as a function of the angle of emergence α on hard ground. Note that the enhancement is unity at vertical incidence and becomes very large for oblique incidence.

In downtown Mexico City, at an epicentral distance of some 400 km, the reported horizontal soil amplifications were of the order of 5 as referred to adjacent sites on welded tuff. The ratios of horizontal to vertical peak amplitudes were of the order of 4:1 to 5:1. This suggests an angle of emergence of $\alpha = 76^{\circ}$ to 79°. From Fig. 2 we find $f = 4$ to 5 for this angle, which seems consistent with the observed soil amplitude enhancement —except that such a low angle of emergence disagrees with the data!

The possibility of constructive interference of multiple SH reflections has been often mentioned as a possible mechanism of amplification; but this effect may have been overestimated. Unless the input signal is coherent in the phase domain it seems difficult to get an amplification of more than 2 in this way.

Enter the spectral seismologist. I can give you any amplification you need, he tells the engineer. You need fifty, I give you fifty. All I need is spectral ratios.

The trouble with spectral ratios is that they refer to amplification of the spectrum, not of the signal. Suppose one has a monochromatic yellow filter that passes only the spectral line of sodium. At this frequency the spectral ratio for an incident white light beam is obviously infinite. Yet no increase in amplitude takes place. Conversely, if instead of a filter we take a yellow lens we may observe a strong increase in amplitude at the focal point of the lens as compared to the input. Yet there is no increase in the spectral ratio.

Is it then a coincidence that spectral ratios seem to account for the changes in amplitude on soft ground? Hardly. But the matter is far from clear. If the sedimentary layer acts as a filter, as is normally assumed, then the transfer function alone cannot account for ground amplification. Amplitude increases across the spectrum are observed in sediments; and such effects cannot be explained in the frequency domain.

One thing seems certain: the collapse of high-rise buildings and other reinforced-concrete frame structures on soft soil cannot be blamed on spectral ratios. Structures are calculated as harmonic oscillators and are expected to oscillate. If resonance occurs it is the fault of the building, not of the excitation. One might argue that a random input was expected and not the kind of nearly-monochromatic surface-wave train that random seismic signals seem to generate in the soft soils under Mexico City. But one cannot have it both ways. If resonance is to blame, structures capable of resonating with the known frequency of the soft ground cannot be expected to survive and should not be built.

CONCLUSIÓN

Errors in seismological measurements may range from a few percent in terms of travel times, to over 50% in terms of energies or focal depths. Frequently dismissed as of little significance, they can nevertheless cause certain categories of engineering designs to be unconservative, with disastrous results.

As an example, consider Fig. 3 which compares the response spectrum of the 1985 Mexico earthquake with the new 1987 Mexico City building code. Apparently, a deficiency of nearly 60% exists at a period of 2 seconds; this deficiency will presumably be compensated by ductility. Further ductility allowances can increase the deficiency to around 80% in terms of response spectral accelerations.

This approach may seem plausible enough when the ground motion can be accurately predicted. The trouble is that the real ground motion seems quite different from the ground motion assumed in the code (Fig. 3). Engineers were dismayed at the high spectral accelerations generated

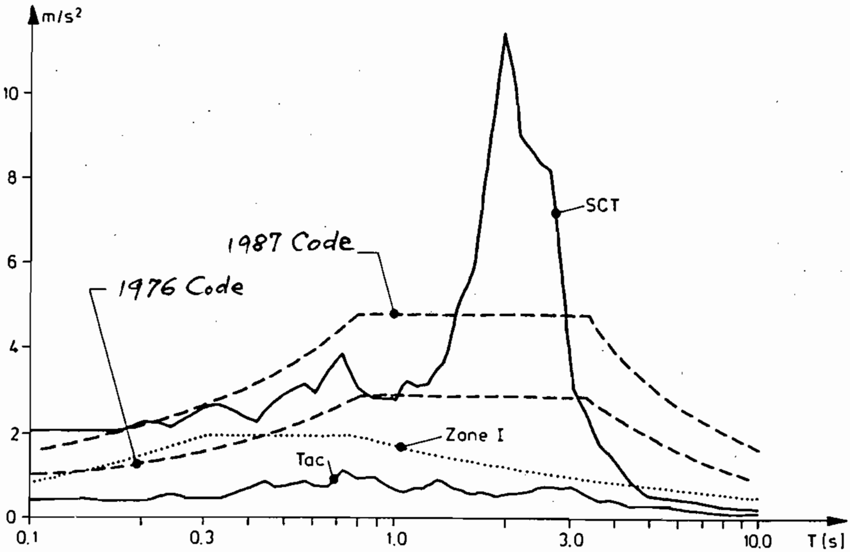

Fig. 3. A comparison of the prescribed acceleration response spectra (5% damping) in the 1976 and 1987 Mexico City Building Codes, with the actual response spectra recorded in the 1985 earthquake. The flat part of the 1987 response spectrum corresponds to 0.4 g. Note that the recorded accelerations on hard ground ("Tac") are well within the prescribed spectrum for the hard zone (Zone I), while the recorded accelerations on soft ground ("SCT") exceed the prescribed spectrum by a significant amount.

in the 1985 earthquake; yet an earthquake of this magnitude and epicentral distance should have been anticipated. In fact it was. The frequency range turned out as predicted and the amplitudes were consistent with available attenuation curves and with error bars in ground amplification. The peak measured amplitude of ground acceleration was below the flat spectral peak of 0.24 g in the 1976 version of the building code.

Obviously, the large peak in the response spectrum had not been predicted; yet the 1987 version of the design spectrum seems a bigger sister of the 1976 version in this respect. Then how is one to decide whether the old building code was inadequate, or whether the new one is adequate?

The critique of measurements represents a permanent basic task of the theorist. The same is true of engineering. There is a part of the puzzle still missing. Is it rotational accelerations? They certainly played an unexpectedly important role in the Mexico earthquake. Then should not recording the rotational components of ground motion become a priority? Yet one keeps hearing that wavelengths in earthquakes cannot be shorter than the dimensions of a building. No wonder short wavelengths are never reported!

About 15.6% of all high-rise buildings on soft ground collapsed in the 1985 Mexico City earthquake. Some engineers point out that the remaining 84.4%, though damaged, are still standing. A proud record, perhaps, until it is compared to the performance of neighboring Colonial masonry structures in the same area. None of these beautiful old buildings collapsed; and there are hundreds of them all over the area on soft ground. If we only found better and more reliable ways to measure and predict ground motion, perhaps we could understand why. Then the performance of reinforced concrete-frame construction on soft ground could be further improved.

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