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Held at DASA Headquarters
Arlington, Virginia

August 18-19, 1970

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This conference and a prior one in June 1970 (reported in ARPA-TIO-71-13-1) were held to foster communication among the diverse disciplines required to predict the shock effects from nuclear explosions out to teleseismic distances. These disciplines involve the use of rock mechanics, geology, nuclear physics, computer hardware and codes, seismology, and field instrumentation. Results from the conferences included (a) improvement in the communication links between the engineers and scientists engaged in research relevant to the seismic coupling problems, and (b) identification of open circuits at some points along the communication lines. This report presents the August 1970 conference proceedings and a summary paper on the results of both conferences.

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OPENING REMARKS

Rudy Black
ARPA

This meeting is a follow-on to the ARPA Seismic Coupling Conference held at IDA on June 8 and 9, 1970. At the conclusion of that meeting the participants were asked to send us their comments on the meeting, its merits, and its principal shortcomings. We received comments from some of you who are at this conference and from others who attended the IDA meetings. Many of these comments concerned the apparent lack of a tie between the work of the rock mechanics and computer code people and the seismologists who subsequently use their data.

We decided to attempt to close the loop with a follow-on roundtable discussion, to review the seismological aspects of the seismic-coupling problems to which ARPA is seeking solutions.

This meeting will consider various topics related to seismic source functions and their seismological applications. We deliberately kept the meeting small to promote an informal atmosphere and information exchange.

I will chair the meeting this morning. Colonel Russell will chair this afternoon's session.

Jack Evernden made up a list of questions that we could ask the seismologists to throw some light on what uses they make of seismic source functions. The questions are as follows:

1. What is the fundamental purpose of the program?

2. What are the seismological observations to be explained?

   a. m_b versus Y versus medium
   b. 1/10/20/50 sec spectrum ratios for explosions versus those for earthquakes
   c. Close-in, free-field measurements

3. What is required of codes to allow prediction of long-range seismic signals (3 cps to 50 sec)? (Note LRL correlation of reduced displacement potential and m_b.)

4. For information, how are reduced displacement potential or equivalent (given at specified distance from explosion) converted into predicted long-distance seismic signals?

5. What is the status of calculations or calculation capability for distant effects of a defined pressure regime (elastic) applied to the surface of a spheroidal cavity? ... a nonspheroidal but analytically desirable cavity (ellipsoidal, say)? ... an arbitrarily shaped cavity?

6. What is the status of understanding of the spectral composition of earthquake signatures? ... explosion signatures?

7. What are the major remaining problems in understanding of earthquake and explosion signatures?

8. How would seismologists suggest furthering explosion source conditions to alter the radiated seismic signature in the direction of earthquake signatures?

These questions will set the basis for our discussion over the next two days.

I suggest that we consider them in this order: No. 1, which concerns the purpose of the meeting; then No. 2 and No. 6, which concern the seismological observations that have to be satisfied by code predictions of earth motion; No. 4 and No. 5: What do the seismologists do with the seismic source functions that are generated by computer codes?; then No. 3: What do the seismologists require of the people who are developing the computer codes?, What sort of source functions do they need?, What are the parameters that they would like to see incorporated into these functions?; then finally, No. 7 and No. 8.

Before beginning our discussion of these topics, I would like to introduce the participants of this meeting.

The seismologists are Nafi Toksöz, MIT; David Harkrider and Charles Archambeau of Cal Tech; Shelton Alexander from Penn State; Stuart Smith from the University of Washington; and Clint Frasier from MIT.

The rock mechanics community is represented by John Handin of Texas A and M, Wayne Brown of the University of Utah, and Bill Judd of Purdue.

The code calculation community is represented by Jack Trulio, Applied Theory; Chuck Godfrey, Physics International; Dave Riney of SSS; Ted Cherry of LRL; and Hank Cooper of the Air Force Weapons Lab.

Mannie Rotenberg, who is a member of our ARPA-DASA Decoupling Panel, is also here to participate in our discussions.

Howard Rodean is here from LRL. Howie is project leader of the joint ARPA-AEC project concerned with seismic detection and evasion research. One of the major topics they are considering is seismic coupling.

Jack Whitener is here from Rand. Jack was the technical director for our enhanced decoupling experiment Diamond Dust, and he is the technical director for the follow-on experiment, Diamond Mine. Bill Perret, from Sandia, is here to discuss close-in measurements. The DoD representatives are Colonel Pearce, Colonel Russell and Don Clements of ARPA; John Lewis, Marvin Atkins, Colonel Barker, and LtColonel Circeo of DNA; and Colonel Klick, AFOSR.

I would like to comment very briefly on the first question: "What is the fundamental purpose of the program?" The purpose of the ARPA research in seismic-coupling is to develop the capability to predict ground motion resulting from underground nuclear explosions in various geologic environments. We need to be able to predict for tamped shots the close-in motion ranging from tens of feet out to thousands of feet. In connection with experiments that we conduct with nuclear weapons and with HE, where we have very small charges, we are unable ordinarily to get seismic measurements at much more than a few kilometers. We have to rely on the close-in data for low yield tests and extrapolate this kind of data to the larger yields that are of interest to us in our program. We have to develop a computational capability to predict ground motion that duplicates the measurements we actually obtain (the close-in measurements) and having done this, scale to larger yields.

We need to be able to predict for tamped shots in any particular geologic source media the strength and the character of the seismic signal that will be recorded at teleseismic distances. We need this capability to evaluate what yield or range of yields could be detonated by potential evaders without detection by any real or proposed seismic-detection network.

We need to be able to determine quantitatively the amount of degradation of seismic coupling that is produced by either fully decoupled or overdriven shots in cavities. Finally, we need to be able to define the seismic source, explosion versus earthquake, and the yield (if it is an explosion) based on the distant seismic signals.

We clearly need to know a great deal more than we currently do about seismic coupling, and it is for these reasons that ARPA has supported theoretical work to develop and test computer codes to predict ground motion from underground nuclear explosions.

Rock mechanics enters the picture because the codes require knowledge of the source-rock properties. The ARPA Nuclear Monitoring

Research Office supports about a million dollars worth of work annually in rock mechanics, and about the same level of effort in code calculations. We have been working on these problems for several years, and a considerable amount of money has been expended for this research. We hope that the discussions we are initiating here this morning will help us to achieve our objectives.

Question No. 2 concerns the seismological observations that we must eventually explain or duplicate from computer-code calculations. Jack divided this question into two areas: one concerning body-wave magnitude versus yield as a function of geologic medium, the second concerning the power spectral ratio in the 1, 10, 20, and 50-sec period range for earthquakes versus explosions. I would like to add a third category to these: the close-in, free-field measurements.

Shelton Alexander has volunteered to lead off on body-wave magnitude versus yield. I think Howie Rodean of LRL also has something to say on that subject. I believe Shelton also wants to talk about the second area, power spectral ratios, and Clint Frasier also has something to say on that particular subject. Finally, with regard to Question No. 2, Bill Perret from Sandia will discuss the close-in measurements.

BODY-WAVE MAGNITUDE VERSUS YIELD

Shelton Alexander
Pennsylvania State University

What I will do is start off with a figure of Jack Evernden's. Figure 1 shows the P_n magnitude (m_b) versus yield for various types of media. I will have to call on Rudy to comment in detail on this, but I believe the objective of the illustration was to show the variations observed for different types of media. You can see the valley alluvium has the lowest P_n magnitude for a particular yield, and we go on up in tuff and hardrock, which appear to be not too different, at least in the one to 20 or 30 kt region. However, when you go on up to higher yields, they do seem to separate in the vicinity of 100 kt. The values in parentheses are for shots below the water table, and in the upper left of the figure are presumably underwater shots.

This point is for valley alluvium below the water table, and far up to the right are shots below the water table also. I believe Jack's contention is that the water table may make a significant difference in yield (or magnitude) depending on whether or not the shot is above or below the water table.

MR. RINEY: Could you give us some idea what the error bars are on those measurements?

MR. ALEXANDER: I cannot. I will say that while the standard deviation of the mean for body-wave magnitude determined using many observations typically may be quite small, individual station magnitudes commonly deviate from the mean by as much as half a magnitude unit. I will show data relevant to this question a little later. Unfortunately I do not have these same events plotted versus the shot medium, but I do have some typical plots of surface-wave versus body-wave magnitude which presently is one of the best discriminants for identifying nuclear explosions.

MR. CHERRY: Are you going to talk about how those magnitudes are determined?

MR. ALEXANDER: Typically, for the body-wave magnitude, we use the first portion of the seismic signature which consists of a periodic pulse lasting several seconds and which, at teleseismic distances, has a predominant frequency of 1 Hz or thereabouts. At closer distances you get higher predominant frequencies. Typically what you do is measure the maximum amplitude of this first wave packet. The body-wave magnitude then is proportional to the log of this measured amplitude divided by the predominant period. The formula is $m_b = \log(A/T) + B(\Delta) + C$ where B is a distance correction factor and C a constant.

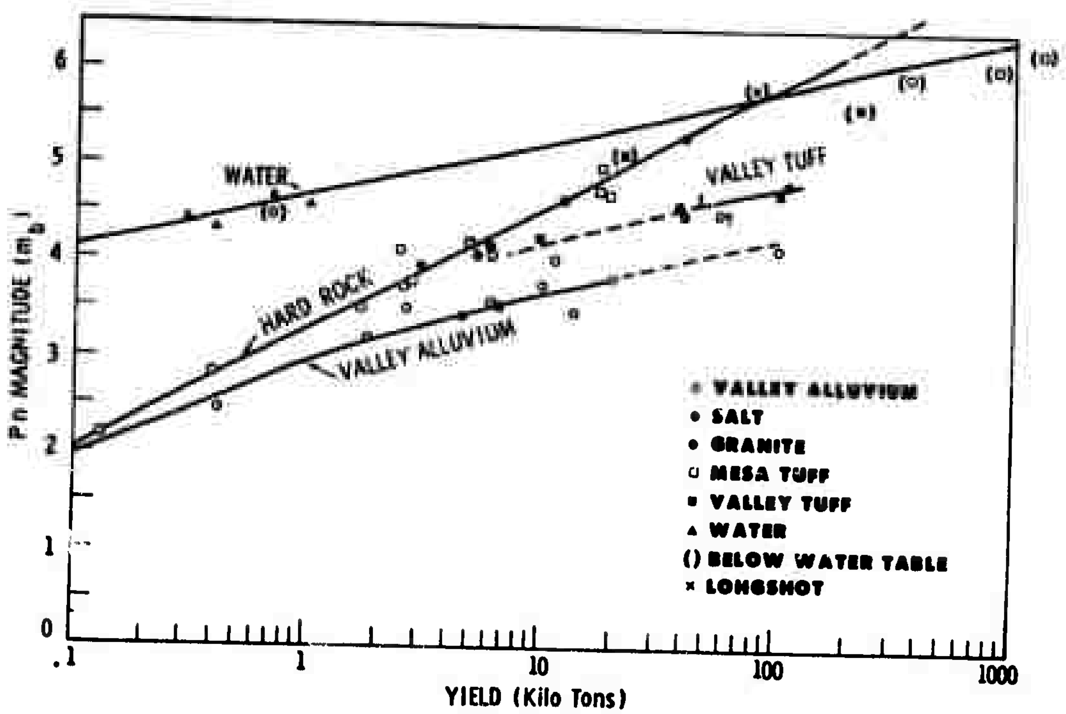

Figure 1. P_n Magnitude (m_b) Versus Yield for Various Types of Media.

After the first half cycle you begin to see the surface reflection coming in, and that alters the amplitude. Therefore the first motion amplitude or the amplitude of this first peak, I think, would be the most reliable in terms of body-wave magnitude although the maximum in the first two or three cycles is used routinely in the calculation of P-wave magnitude by many workers.

MR. CHERRY: What distance is that?

MR. ALEXANDER: At any distance, this is how magnitude is measured. Then there is a distance correction factor, and that is different depending on whether the receiver is close in, that is, less than 3,000 km.

MR. COOPER: How does the range factor into the way the data are presented?

MR. ALEXANDER: There is a distance correction factor which I mentioned associated with the magnitude determination. Jack Evernden, for example, developed an invariable sort of relation for the close-in, the so-called P_n-magnitude estimates, and these are the ones, for stations less than about 3,000 km at the Nevada Test Site. So you take the mean of all of the individual magnitude estimates for each event and that is what would be plotted in a typical curve such as you see here. All of the azimuths that you have recording stations for are included.

MR. BROWN: What is the magnitude here, A over what?

MR. ALEXANDER: Period. It is like frequency times the amplitude. In effect, it is ground motion in microns.

COL. PEARCE: What are A and T normalized?

MR. ALEXANDER: It is not really dimensional. This quantity turns out to be microns of ground motion, microns per cps.

COL. PEARCE: There are supposed to be overall dimensions products, so that A and P and A over A_0, and T over T_0 ....

MR. ALEXANDER: A_0 is one micron. You standardize on the whole set.

COL. PEARCE: I thought there were some standard sources on which you based these.

MR. ALEXANDER: No. The original definition of magnitude went back to what was seen for an earthquake at a fixed distance (100 km) on a fixed instrument (Wood-Anderson torsion seismometer).

MR. RODEAN: Shelton, one of my favorite quotes on this is by Richter, I believe. Magnitude was invented by the seismologists to take some of the nonsense out of earthquake statistics, and so it has this amplitude and period.

Then you get into these station and distance corrections. The ultimate intent is to describe the strength of the source.

MR. ALEXANDER: Right.

MR. RODEAN: What we attempt to do with the distance correction factors, for example, is to eliminate the propagation effects of distance, geometrical spreading, and the effects of the propagation medium itself.

MR. ALEXANDER: Some of the curves I will show you demonstrate that. What I have done is an experiment to try and take out as many of these factors as possible to look at the explosion sources.

MR. TRULIO: Is there some ideal medium that would not have any correction factors?

MR. TOKSOZ: One-dimensional, nonattenuating rock.

MR. TRULIO: Or elastic.

MR. TOKSOZ: Or elastic, one dimensional, or if you work in two dimensions, you have to talk about plane ones.

MR. GODFREY: How does T vary? Is it to the function of yield?

MR. ALEXANDER: A very weak function of yield at teleseismic distances. It turns out that practically all events, at least shots, turn out to have predominant frequencies of the order of 1 Hz.

MR. GODFREY: So why is it in there at all?

MR. RODEAN: It is about 1 sec, because on the narrow band instruments that is about where the response curve is centered.

MR. ALEXANDER: The instrument is taken out before that. There is a correction for the instrument response as far as gain is concerned.

MR. RINEY: I wanted to ask you about amplitude. Is that in any particular direction?

MR. ALEXANDER: Normally it would be taken from the vertical instrument.

MR. BROWN: Why do you take the first cycle peak instead of the second, which is higher?

MR. ALEXANDER: The first energy that is seen at teleseismic distances leaves the source at an angle of less than 35 deg with respect to the vertical. Following onto that is the surface reflection, arriving on the order of a half second later or perhaps less. The crustal structure at the receiver also strongly influences the character and duration of the waveform. For example, Milrow and Longshot were very large events that were very well recorded; the first portions of the signal show up with consistent relative levels at the high gain stations while the later part is quite variable from station to station. However, the maximum amplitude in the first three cycles of motion is commonly used in routine magnitude determinations in spite of these complications.

MR. COOPER: Shelton, could you comment on the source region? I gather these events are primarily at NTS.

MR. ALEXANDER: Except for Longshot, that would be true, I believe.

MR. RUBY: Wouldn't the lower ones be coupled?

MR. ALEXANDER: Except for these and Longshot.

MR. COOPER: What I am questioning, I guess, is the dependence on the path with respect to velocity. Do you have relevant data?

MR. RINEY: In the paper from which this came I think these standardized the path from west to east. Even Longshot was standardized in that way, if I remember right from reading the paper.

MR. BLACK: I think there is about 0.3 of a magnitude difference for paths from NTS to the east compared with paths to the west.

MR. ALEXANDER: Different source areas do have different distance correction factors. For example, NTS structure attenuates energy significantly as compared to certain other source areas. The same size event at NTS and another source area would show up with a different magnitude if you used the same distance correction factor. In effect, what you have to do is calibrate each source region as far as the signal levels vs yield are concerned. Most of these data involve first of all the same source region and pretty much the same set of receivers. I do not think he had common receivers for all of these events, simply because the history of the program is such that the recording stations have changed. Nonetheless, many of the stations are in common, so relatively speaking these relationships are reasonable. The paths represented in the magnitude determination do not change appreciably from event to event.

MR. LEWIS: Could I ask what these data points are up here in the upper right hand corner in parentheses?

MR. ALEXANDER: Shots below the water table. I do not know what particular events these are, however.

MR. LEWIS: I just wanted to ask a question about drawing those curves, the philosophy of drawing curves from data like that.

MR. BLACK: There are several things that I think we ought to keep in mind about this illustration. First, it is an unclassified figure, and there are other points that were used to help define these lines which are not on this graph.

Second, most of this data is NTS data. The points on the left, those triangles, are chemical explosions in water. Because most of these shots were fired at NTS, we are limited in source material to either alluvium, some form of tuff, or some form of volcanic hard rock or granite.

Nevertheless, there are great differences in seismic coupling even in the limited geology in which we have shot. Alluvium turns out to be the lowest coupling material, but it also shows the greatest range in magnitude for a given yield.

Evernden pointed out, at the Coupling Conference at IDA in June, that the difference in coupling as a function of source medium, is small at yields below 1 kt. At higher yields the differences in coupling due to source medium are quite pronounced for the hard versus the soft, unconsolidated rocks. Evernden also stated that the coupling of dry versus wet porous materials is quite different. Note, for example, the shot in alluvium fired below the water table, which lies nearly on the water line.

MR. LEWIS: I interpreted what he said to mean that everything was sort of path dependent; therefore calculations of things happening close to the device or the explosion got washed out in a hurry because of propagation path characteristics.

MR. CHERRY: One of the interesting things is that hard rock coupling line; at the higher yields it looks like you are getting better coupling than in water.

MR. BLACK: I am sorry Evernden is not here to discuss that point, because he has developed an explanation for that bend. When he plots surface-wave magnitude versus yield, it plots on a straight line.

MR. CHERRY: He thinks what is plotted on a straight line?

MR. BLACK: The surface-wave magnitude versus yield.

MR. COOPER: Is the surface-wave magnitude defined the same way as for body waves?

MR. ALEXANDER: Yes, except now you are talking about Rayleigh waves in a later portion of the record and the amplitude A over T where the period is about 20 sec.

MR. ROTENBERG: I would like to ask what the labels really mean, hard rock, valley tuff, and so on. Does that mean where the shot actually took place?

MR. BLACK: Yes.

MR. ROTENBERG: It does not mean the material over which the wave ...?

MR. ALEXANDER: It is the shot-point environment.

MR. ROTENBERG: It does not even mean, for example, that is the material in which the inelastic region was, necessarily.

MR. LEWIS: That is hard to say. Pahute Mesa, for example, where a lot of the larger yield things are shot, is a very complicated volcanic mass. It is a combination of various kinds of ashes, tuffs, and then there are rhyolite sills which may be hundreds of feet thick. Some of the shots, and I don't know whether they are on this curve, were fired in those sills. It depends on how large the yield is as to whether or not the elastic limit would be contained within the sill or whether it got out into the material above and below. It is a very complicated system. About all you can say for sure is that for the really large ones, you probably are below the water table.

MR. BLACK: I would like to make one more point. There are many ways of determining body-wave magnitude. The one that has been used in this graph, involves the maximum amplitude of the first three cycles rather than the first initial pulse. I think LRL does it differently.

MR. RODEAN: For our magnitude versus yield or amplitude work for our yield determination of shots we use close-in stations two or three hundred kilometers away, and what we call the A, B, and C amplitudes: the first positive pulse, the first negative pulse, and then the second positive pulse. We correlate the amplitudes of these versus yield, and then we factor in the location of the shot within the test site. This is what we do to try to get estimated yields in the afternoon from a shot in the morning. We don't calculate magnitudes as such; we just take the measured amplitudes.

One other thing, Shelton, maybe to put things on an even keel, could you just describe briefly the seismic noise as a function of frequency, and then the different, shall we say windows in the seismic spectrum that seismologists look at?

MR. TRULIO: I wanted to ask, are the low ends of these curves based mainly on HE shots?

MR. ALEXANDER: No.

MR. TRULIO: The HE and nuclear shots pretty much fall together?

MR. RODEAN: I think that, with the exception of the four triangles in the upper left hand corner around the water data, everything else is a nuclear shot.

MR. LEWIS: I think that is an important point, because it seems to me that HE, on a pound-for-pound or a kiloton-for-kiloton basis, should couple better than the nuclear. I don't have any data on that.

MR. BLACK: Certainly the spectrum is different.

MR. LEWIS: So you really don't know how much of that curve on the left hand side called the water curve is really influenced by the fact that it is HE.

MR. BLACK: As I said before, there are other points on that curve which are not shown here because they happen to be classified. They do fit the curve very nicely.

MR. ALEXANDER: Back to this other point about the noise factor, there is a noise peak, not at every site necessarily but at many sites, at a period of about 0.3 sec. There is a notch in the neighborhood of 1-sec period which accounts for why the instruments are usually peaked there. Very fortuitously the signals happen to be bigger there as well, because the higher frequencies are attenuated very rapidly with distance so that at teleseismic distances (3,000 km) the 1 Hz energy is dominant. Then there is a very large noise peak in the neighborhood of about 6- to 8-sec period. There was some question raised also at the Woods Hole conference about whether or not there may be a notch in the vicinity of 40-sec period. The idea is that the noise does drop off significantly at the longer periods, particularly in the vicinity of 40-sec period, so that the signal-to-noise ratio may be relatively high. However the surface waves are usually measured at 20-sec period where there is an Airy phase in the dispersion curve resulting in more pulse-like propagation with a smaller decay with distance than for other periods.

Unfortunately, the work that I have been doing has been primarily on the M_s vs m_b type criterion so I do not have the yields shown here, but these are 39 NTS explosions for which this information is available (Figure 2). Figure 2 is simply to show the consistency or lack of consistency, however you want to view it, between the surface-wave measurements and the body-wave measurements. You can see that they do follow a linear trend over quite a large magnitude range. Those at the far end of course would be the large yields, of the order of hundreds of kilotons.

Figure 3 essentially shows the same data plotted in a different way, along with some earthquake data, and also some smaller magnitudes. These data are the same as far as the explosions are concerned, and some smaller earthquakes are included. There is a fair amount of scatter as you get down to the very small body-wave magnitudes and you see an adjusted M_s value for them. What was done in this case was to use the bigger NTS explosions to derive a distance correction factor which is appropriate for the close-in measurements. The surface wave magnitude is proportional to log (A/T) at 20 sec plus 1.66 log Δ, where Δ is the distance. This was the old Gutenberg formulation, with perhaps a constant added on for different source areas.

It turns out that Gutenberg's formula only applies for distances greater than 15 deg (1600 km). This distance correction factor just does not hold at the nearer distances. I do not have a figure to show it, but if you plot the observed amplitude decay with distance, it becomes asymptotic to 1.66 log Δ at about 15 deg. What was done was to define empirically the near-in curve using the large NTS explosions and the many stations that recorded them. The resulting best-fitting single curve gives the distance correction factor.

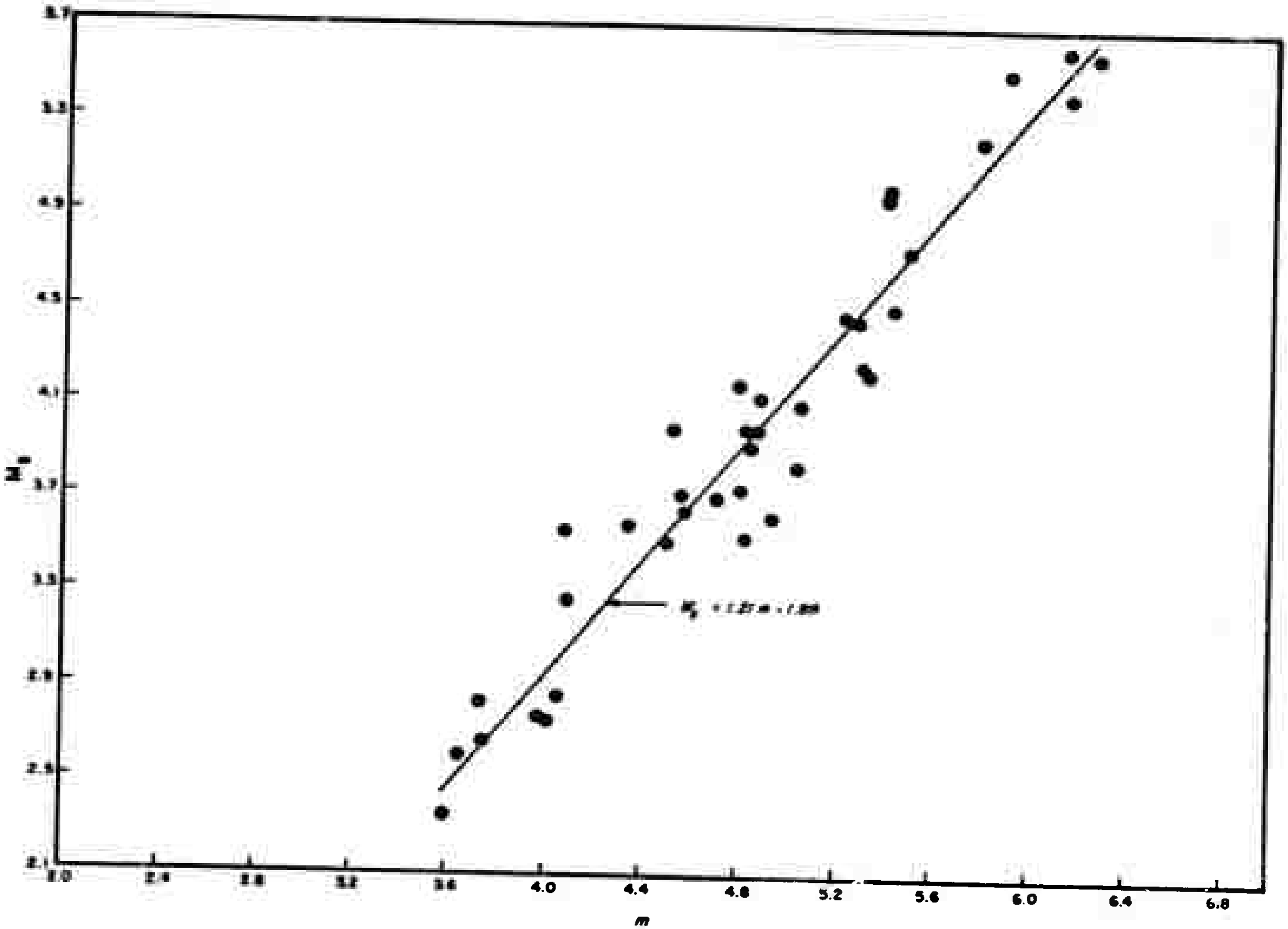

Figure 2. Least Squares Fit to M_S (Gutenberg) Versus m for 39 NTS Explosions.

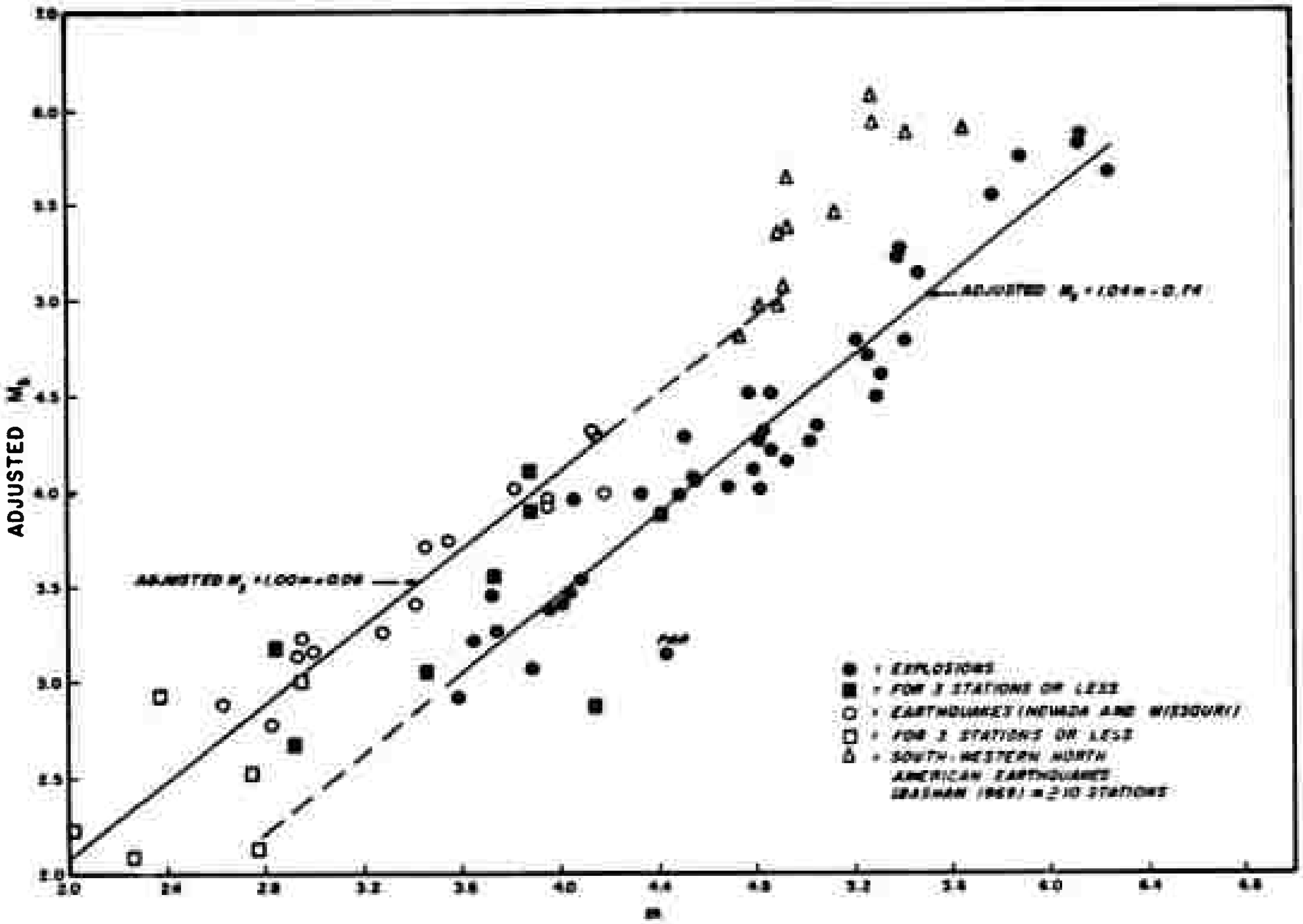

Figure 3. Adjusted M_s:m for NTS Explosions and Nevada and Missouri Earthquakes.

For the small events the surface waves just are not seen at large distances; we must rely solely on the close-in surface-wave measurements. Therefore you do get a bias unless a correction such as this is made to get rid of the propagation effects.

There is a lot of overlap here, but it turns out that even when you get down to the smaller magnitudes, the mean explosion and earthquake curves still tend to be separated from one another. However there is a definite overlap for the small events so you cannot draw a line that completely separates explosions from earthquakes. The straight lines you see are least-squares fits for explosions and earthquakes taken separately.

You still see scatter here, and the question arises as to what it is due to. Earthquakes of course scatter still more at particular stations. We wanted to see whether or not this scatter in M_s vs m_b for explosions was due to the P waves that were received or to the surface waves. Therefore, the next experiment was to try to eliminate some of these propagation effects by looking at a suite of events from a local source area recorded at a single station so that they all have almost the same transmission path. In Figure 4 are shown the P-wave amplitudes of NTS explosions and earthquakes observed at the station KN-UT in Utah versus the Rayleigh-wave amplitudes. The solid dots are the same set of explosions that were plotted in the previous figure. You see there is a lot of scatter. The paths for most of these shots are very, very similar, so that the medium is invariant in the problem, and the station itself is invariant; yet there is still significant scatter.

Figure 5 is a curve obtained by plotting the observed individual surface-wave signals at this single station versus the "expected" P_n amplitudes, based on averages of different stations' P_n or body-wave magnitudes for each event. The scatter is considerably reduced compared to the previous plot. There are still a couple of points down here to the left of the figure. I do not think this apparent curvature here is meaningful considering the overall scatter. The point is that the scatter in the surface-wave magnitudes seems to be quite a bit less than in the previous figure. What I claim is that, as seen at this receiving station, it is the body waves, the P_n's that are quite variable, leading to a great deal of scattering in the body-wave magnitude at this particular single station, whereas the surface waves seem to be more consistent.

This plot (Figure 6) shows the opposite thing, taking the mean of all of the individual surface-wave magnitudes, and plotting the "expected" surface-wave magnitude versus the observed P_n. This length on the plot would be essentially equivalent to one magnitude unit, and you see there is lots of scatter. This I attribute to the scatter in the P waves reaching this station.

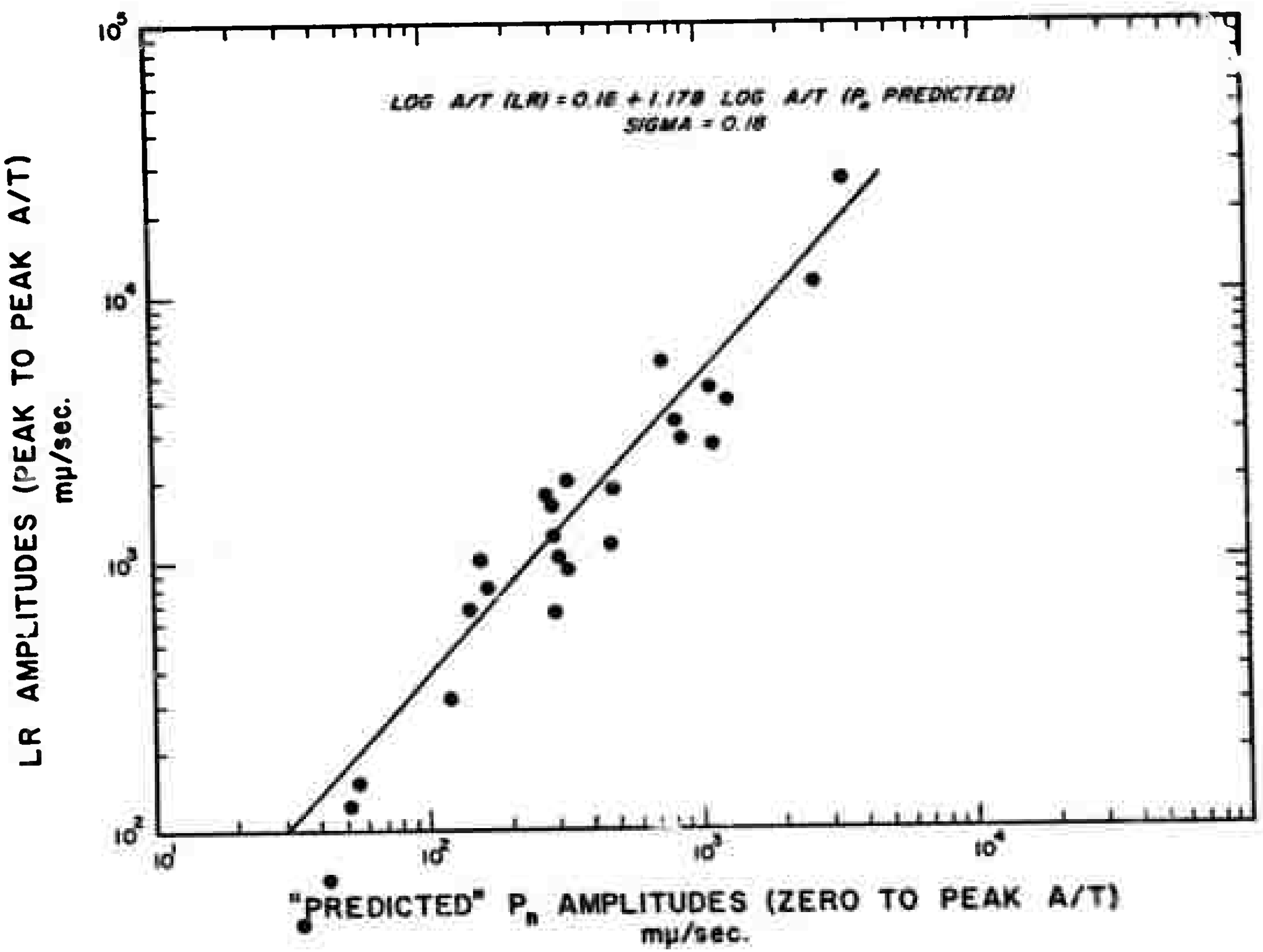

Figure 5. Predicted P_n-Amplitudes From Teleseismic m_b for Explosions at KN-UT.

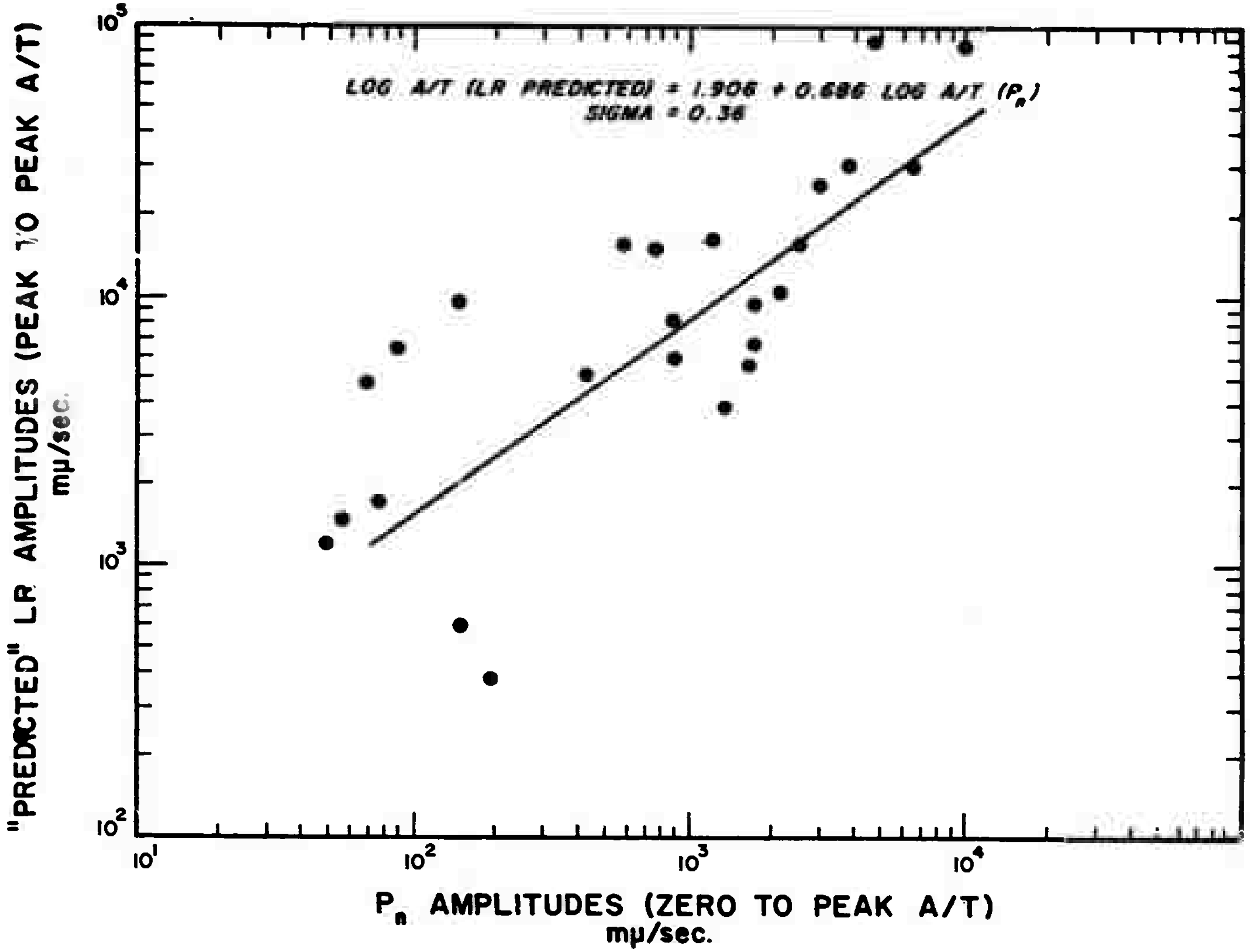

Figure 6. Predicted LR Amplitudes From Teleseismic M_S for Explosions at MN-NV.

What I conclude from these results and similar ones for other stations is that the P waves, as received at a near-in station, are quite variable, whereas the surface waves are much less variable. I think this is something that perhaps the code calculations may show up, and I would be interested to hear whether or not you expect significantly different high frequency or body-wave characteristics as a function of take-off angle at the source, for example, in the different source regions.

MR. CLEMENTS: You are attributing this only to a wavelength effect, are you not?

MR. ALEXANDER: Perhaps. I don't know what to attribute it to. I claim that, as nearly as I can, I have eliminated effects of propagation, because the station is the same and the paths to the particular receivers are almost identical. The attenuation properties of the medium and all types of distortion due to propagation are equalized out; they are common to each and every one of these events. I attribute the scatter, therefore, to behavior right at the source.

MR. SMITH: This basin is how far away?

MR. ALEXANDER: I think this one is about 500 km.

MR. GODFREY: I am a little uncertain as to what is being measured here. Is P_n the amplitude of the first cycle?

MR. ALEXANDER: No, the zero to peak.

MR. GODFREY: Oh, the first cycle.

MR. ALEXANDER: Yes.

MR. GODFREY: And the LR is the end of the first peak?

MR. ALEXANDER: This is peak-to-peak at the 20 sec predominant period in the surface wave.

MR. RODEAN: Is that using our data?

MR. ALEXANDER: Yes.

MR. RODEAN: Then that is about 300 km from the test site.

MR. ALEXANDER: Correct. I did several of these different stations.

MR. COOPER: Is the data scatter here of the same order as the scatter that was shown on Figure 1?

MR. ALEXANDER: I am sure that the curves Evernden plotted in the very first one were averages. If you average the P_n amplitudes over an array of stations, even though there is large individual scatter, the means turn out to be much more consistent. The same thing happens for the earthquakes too.

MR. COOPER: Is the scatter that you are attributing to the source region consistent with the scatter that Evernden was suggesting based on whether or not the source is granite or some other rock?

MR. ALEXANDER: Remember what this represents is the energy going out along a pencil ray taking off from the source and seen at one particular distance. Energy represented by each point on this graph went out from the source over a very small part of the focal sphere.

MR. COOPER: I understand, but you have attempted in plotting all of this data to make everything except the immediate source region invariant. You intentionally made it that way to keep the uncertainties in the path constant.

MR. ALEXANDER: Right.

MR. COOPER: If you were to plot the data in the same way as Evernden to distinguish between granite and other source region geologic materials, would similar trends result?

MR. ALEXANDER: That I have not done yet, so I can't answer that question, although I think Carl Romney plotted the individual surface-wave magnitudes as we saw them before as a function of medium. The scatter for each type of media is about the same as you saw in Figure 5. At least for the surface waves there did not seem to be any evident correlation between shot medium and the surface-wave magnitude. I believe you will find the same is going to be true here, but I can't say that definitely right now.

MR. SMITH: Shelton, I think you ought to point out there are two distinctly different things that are operating in different directions to the scatter. First of all, the wavelength of the surface waves is longer, therefore the scattering is less. Secondly, the path of propagation at a distance of 300 km, the body wave is going through a much more homogeneous part of the earth than the surface wave is, which would act in the opposite direction. You would expect less scattering from body-wave type propagation.

MR. ALEXANDER: Yes, that is probably true.

MR. SMITH: The net result is the wavelength seems in effect to predominate.

MR. COOPER: The point of my question was whether or not the suggested data scatter is really scatter. If you plotted the data according to the source region, would you see the same kind of trend that Evernden indicated in Figure 1?

MR. ALEXANDER: I can answer that indirectly, and only qualitatively, by saying that when we looked at the same set of events at another station in the basin range we got scatter also, but the pattern of scattered points was not consistent with this case at all. For that reason I attribute it to something other than the shot medium itself--either geometry at the source or some such effect, which could easily cause these variations because they represent all of the trapped P waves and S conversions in a large range of angles at the source. You would expect any variations with takeoff angle to get averaged out in the surface waves, and indeed that seems to be what this little bit of evidence shows.

MR. CHERRY: Will you explain again what you mean by predicted amplitude?

MR. ALEXANDER: In effect it is the same as the average of all the P_n data for all of the stations available. What we are trying to see is how does this particular station compare with a mean which is presumed to be a better estimate of the actual size.

MR. TRULIO: For all of the points on that last figure, the detecting system was the same?

MR. ALEXANDER: Right. It is the same station, same instruments, the same path. Only the sources themselves are different.

MR. TRULIO: How much scatter would you get from just changes in wave shape?

MR. ALEXANDER: Very little, at least for the surface waves.

MR. TRULIO: Do you have the frequency-response curves for the detecting system?

MR. ALEXANDER: I don't have a slide of them, but they are available in the published shot reports for any of the shots, and they are all consistent. The spectral shapes at least are maintained to be the same. I think they were changed one time uniformly, but they peak around 20 sec for the long-period system, and die off at 12 db per octave, I believe, on either side of that. Then the short-period instruments peak at about 1 Hz. I forget what the die-offs are around that peak, but they are maintained at the same shape for all stations. The levels are adjusted depending on how big the shot is expected to be, so the gains are different, but the shapes of the instrument response are maintained to be the same.

MR. TRULIO: The incoming waves will depend on what the source was.

MR. ALEXANDER: Right, and what I am maintaining in these latter figures is that since the receiver has the same response, and the paths are in common, what is left is the actual variation over a range of yields or magnitudes, and is a true measure of the differences in what is being sent out from the source.

MR. TRULIO: Yes, although they might respond not just to the amplitudes of the waves that arrive at the detector, but to the entire wave shape.

MR. ALEXANDER: This is something that may be a factor, particularly for these P_n waves. I would not necessarily expect the source-time functions for them to be invariant with azimuth from the source region if there are any kinds of homogeneities in the vicinity of the shot point. But that is what the close-in measurements ought to be able to tell you, that is, how asymmetric are these source-time functions.

MR. TRULIO: You are not thinking of the spatial shapes of the pulses as much as their time variation.

MR. ALEXANDER: Yes, that would be the same kind of thing. In other words, this should be reflected in the variations you observe from shot to shot at a given range, let us say. Suppose you had the same size, same yield event, and you look at it at a particular range, how different are they one from the other? That would be an analogous measurement to what we are doing here.

MR. CHERRY: Do you have any feeling for what that Rayleigh-wave arrival really is at like 300 km? Is it sensitive to a particular waveguide, or is it really the surface Rayleigh wave?

MR. ALEXANDER: I think it is really the surface Rayleigh wave, because it has at least the primary characteristics of one, in that it has elliptical particle motion and is dispersed.

MR. CHERRY: The waveguide would give you that also. Is it a waveguide phenomenon that you are looking at?

MR. ALEXANDER: Yes. It is a fundamental mode Rayleigh wave. It is not a higher mode.

MR. HARKRIDER: It is a combination of both.

MR. CHERRY: Is it dispersive, and has it all of the properties of a waveguide Rayleigh wave?

MR. HARKRIDER: It is more like a surface wave, like a nondispersive Rayleigh wave that sees a different half space for each frequency. It is not really a waveguide in which there is trapped P-SV conversion. The higher waves are predominantly trapped P-SV conversion. This is just sort of a weighted Rayleigh wave which sees for each frequency a different half space.

MR. CHERRY: But there is an Airy phase associated with that mode also, isn't there?

MR. ALEXANDER: Typically over the whole Basin and Range, you get a nearly flat portion of the group velocity curve with a true Airy phase minimum in the neighborhood of 16 to 18 sec.

MR. CHERRY: Is that where you are looking?

MR. ALEXANDER: There is a peak at about 10-sec period and another around 50-sec period. What you would see is a waveform developing from these periods. Suppose you were a little bit farther out in distance. What you would see first is a 40 or 50-sec wave if it were well enough excited by the source. Then coming in on top of it would be a pulse starting off at a predominant period of 10 sec and dispersing just a little bit to 20-sec period. At near ranges you see predominantly a Rayleigh wave that starts out with 10-sec period and essentially ends with a predominant period of 16 to 18 sec. Because the dispersion curve is nearly flat in the range 10-20 sec, the signal comes in as a pulse all over the Basin and Range. This is the kind of signal that is measured.

MR. CHERRY: And that is looking at the first 35 km or so. Is that dispersion curve drawn for the first 35 km?

MR. ALEXANDER: This would be everything down to 100 or 150 km, but these measurements are sensitive primarily to the upper 35 to 40 km. Their propagation is controlled almost exclusively by what is going on in the upper 40 km, certainly the upper 50. These tend to be very consistent everywhere, and the wave shapes themselves do not change significantly.

MR. CHERRY: That is consistent with what we have been finding at LRL. We have recently undertaken a program to look at the Rayleigh waves at Mina. We have sort of concentrated on just one area of the test site initially.

MR. ALEXANDER: Figure 7 is an explosion as seen at Winnemucca, Nevada, which is not quite 500 km, and each point here is 10 sec in duration. You can see from what I was trying to explain earlier, the beginning here is about the order of 10-sec predominant period, and this last predominant period you can see is of the order of 15 or 16 sec. These wave shapes tend to reproduce themselves very closely from event to event.

While I am on this, I might as well point out one other thing on this figure. This is a collapse, observed at the same receiver. If you reverse the polarity of the collapse signal and overlay it with the signal for the explosion, they are virtually identical, with perfect scaling. This means that essentially the source-time function is not all that different for the two.

MR. TOKSOZ: Explosion versus implosion type.

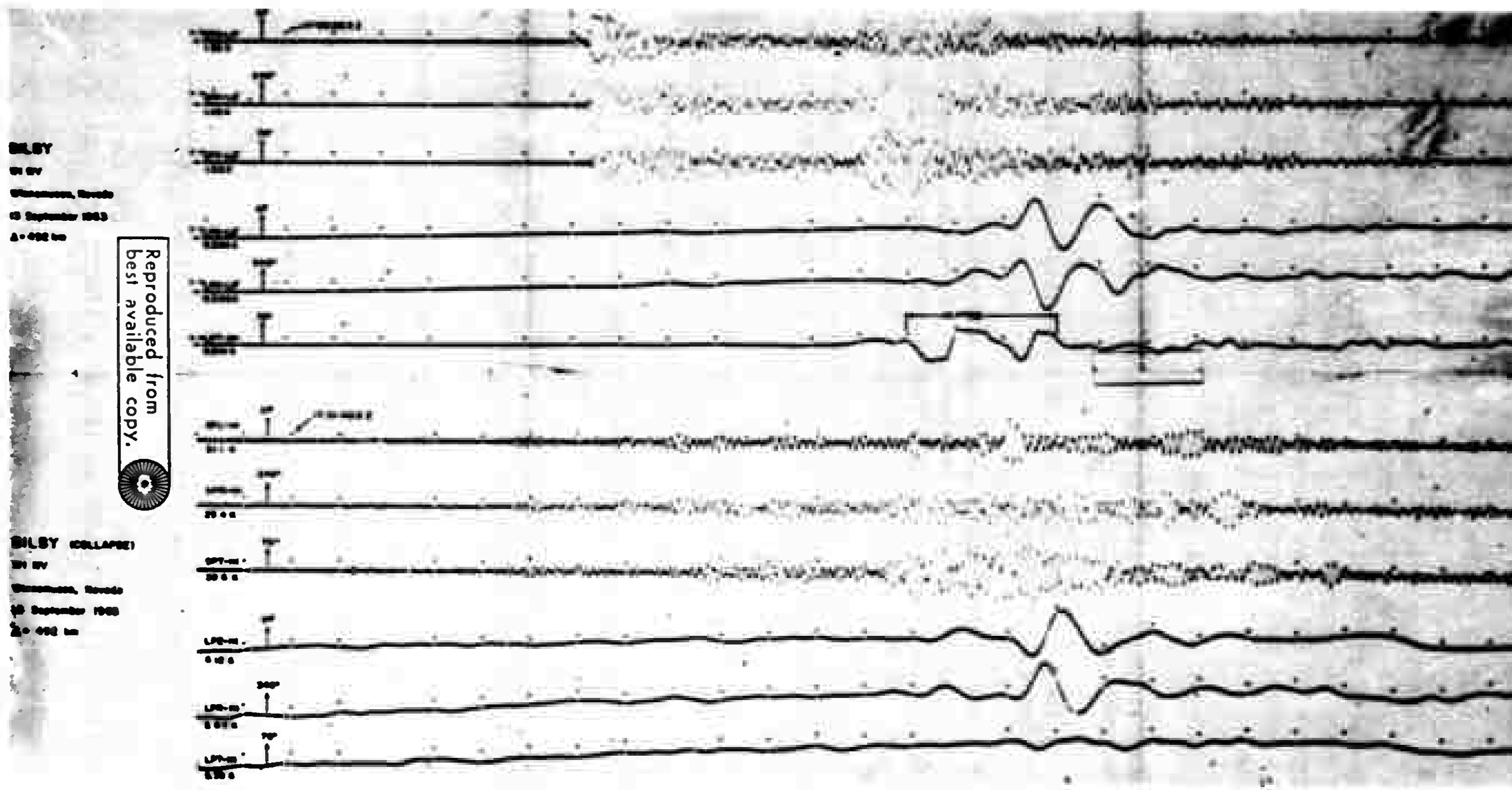

Figure 7. Wave Patterns for Bilby Explosion and Collapse.

MR. ALEXANDER: This is something that has to be explained, too; that is, why do the collapses, at least for the surface waves, look very similar to the direct explosion. There is one other thing I might as well discuss at this time. The components all have a common gain but the gains are quite different for the explosion and collapse. This is a 0.6 K gain here as opposed to a 5 K gain here, so there is a factor of ten difference in the gain. Here the Love waves are clipped, and in principle you should not expect any Love waves. Here in the collapse they are absent.

MR. CHERRY: Is that the same gain?

MR. ALEXANDER: It is the same relative gain. Everything here has been raised by one order of magnitude.

MR. CHERRY: Is that the same gain on the Love-wave channel as on the Rayleigh-wave channel for these?

MR. ALEXANDER: Yes. These gains are 0.596 and this is 0.64. This is 5.38 versus 5.14, so the vertical and the transverse are almost the same gain. Yet the explosion produces Love waves that are clipped at this gain level, and the collapse produces no long-period Love waves.

MR. PERRET: I think I can tell you something more about relative signal amplitudes from the explosion and collapse in and near the crater a little later on.

MR. ALEXANDER: Okay. This behavior is the rule rather than the exception. To my knowledge it always happens. The collapse produces practically no 20-sec Love waves, whereas most NTS explosions do.

MR. ROTENBERG: In principle there should be no Love waves.

MR. ALEXANDER: Right.

MR. ROTENBERG: With the explosion. Can you amplify on that a little bit?

MR. ALEXANDER: The kind of shear waves that are necessary to produce Love waves are horizontally polarized shear waves.

MR. ROTENBERG: Yes, but don't you get mode conversion?

MR. ALEXANDER: If you do, you should get it for the collapse as well as the explosion, and you don't see it. The source points are essentially geometrically identical.

MR. ROTENBERG: Of course, the plots need not be spherical.

MR. PERRET: The collapse signal is definitely polarized vertically in the earth, because in the records we see very strong vertical signals within the subsidence area and very weak ones outside; horizontal signals are weak both inside and outside the crater.

MR. ALEXANDER: Neither of them should give you Love waves.

MR. PERRET: They are quite different mechanisms.

MR. ALEXANDER: Neither one of them should give you Love waves, that is the point.

MR. CHERRY: So what you are saying is that the Love waves in fact are bigger than the Rayleigh waves for this particular shot.

MR. ALEXANDER: Yes.

MR. TOKSOZ: At this particular station.

MR. CLEMENTS: I remember reading recently that somebody was trying to measure SH waves and they were looking for a good generator. They found that a varied explosive gave large SH waves, which it should not.

MR. ALEXANDER: This is a matter of real controversy, and it would be very worthwhile, at least from my point of view, and I think probably that of the other seismologists here, to hear what the close-in calculations were in fact predicting in terms of any sort of SH waves. We can present good seismological evidence that these SH components here are in fact generated right at the source and are not converted along the path. They are generated very near to the source point.

MR. SMITH: What percentage of the explosions that you have looked at actually gave larger Love waves than Rayleigh waves?

MR. ALEXANDER: It depends on the azimuth. I can't answer that categorically.

MR. SMITH: Typically they get up as big as the Rayleigh waves.

MR. ALEXANDER: Yes, of the same order of magnitude. It varies from one shot medium to another. I think Nafi has lots of data on the relative generation of Love versus Rayleigh waves for many different events.

MR. TOKSOZ: I will show those later on, but explosions in harder media such as granite or some of the rhyolites and some of the tuffs have the tendency to give much more Love waves than the explosions in softer media. Then you have the explosions in salt, for example, where there are no Love waves associated with it. They are below the noise level.

MR. ATKINS: Have you observed the event and the collapse that helped discriminate or identify a specific event other than our own shots, or is the collapse too small in this order of magnitude?

MR. ALEXANDER: I think the collapse sizes vary, and I have only a rough idea about these. Is this what you are asking, the size of the collapse versus the size of the explosion?

MR. ATKINS: Well, can you see the collapse from ...?

MR. ALEXANDER: You can see it particularly for the larger ones. You can see the collapse at teleseismic distances.

MR. ATKINS: Has this been useful as a discriminating technique at all by associating the two?

MR. ALEXANDER: Provided you could see it, it would be, because the surface waves are exactly reversed. I think the frequencies involved are quite different, too.

MR. SMITH: But the answer to his question is no, because for those events that are big enough that the collapse should be useful, other techniques work very well.

MR. ALEXANDER: That is right. For the ones I have looked at, the collapse tends to be from a factor of three to about ten smaller than the accompanying explosions surface-wave amplitude. The magnitude of the collapse for surface waves would be anything from one whole magnitude unit to maybe half a magnitude unit smaller than the explosion that precedes it.

MR. GODFREY: Perhaps one comment to make is, although the amplitude of the surface wave is different, as you point out, there is a remarkable similarity in the shape.

MR. ALEXANDER: That is correct.

MR. GODFREY: One comment from a calculational point of view would be then that from the physics the explosion is just a completely different beast from the collapse. The form of the actual physical disturbance you are measuring may not be very important. I think to describe the two in a code calculation would be just vastly different, and yet they give the same shape.

MR. SMITH: Well, no, their high-frequency spectrum is entirely different.

MR. ALEXANDER: That is right.

MR. SMITH: Most of the wave shape you see there is controlled by the instrument, rather than the source. That is the low-frequency lag.

MR. ALEXANDER: What this says is that in the low-frequency limit they are pretty similar.

MR. GODFREY: Are you using the same instruments?

MR. ALEXANDER: Yes. The low-frequency part of the signal spectrum is similar for both. The high-frequency part is demonstrably different. If you look at the P waves, for example, and other high-frequency waves, they are quite different. This is a higher mode signal from an explosion. It may be difficult to see from far away, but the frequencies are quite high. For the same portion of the collapse record the signal is considerably lower in frequency. Thus there are observable differences between the two at the higher frequencies.

MR. COOPER: This is consistent with what you found earlier, too. These surface-wave data are less scattered.

MR. ALEXANDER: Yes. I am really leaping ahead with Part B of this meeting when we talk about these surface waves and spectra, but I think it is true that the spectra for Rayleigh waves seems to be pretty independent of the size of the event. As to the shapes of the spectra, I think in theoretical calculations this is reasonable also.

MR. ROTENBERG: Do you only see a Love wave from an explosion, or just in this particular event?

MR. ALEXANDER: It is the rule rather than the exception. Do you know of any?

MR. TOKSOZ: The water shots do not generate Love waves. The explosions in salt do not generate Love waves, and some in loose alluvium, such as Sedan, for example, did not generate any appreciable amount of Love waves. But all of the larger explosions that we have looked at to some extent have generated Love waves.

MR. CHERRY: And they were as big as the Rayleigh waves?

MR. TOKSOZ: No, no.

MR. ALEXANDER: Well, they may be. They may be comparable for some NTS events.

MR. ROTENBERG: Your argument is saying there should be no Love waves because of a left-right symmetry, but if there is some asymmetry in the medium in which you are shooting, you can get them.

MR. ALEXANDER: Providing the asymmetry is different from symmetry about the Z axis. Anything that is symmetric about the Z axis, including a point source, should not produce Love waves.

MR. CHERRY: I think his point is that the puzzle is why you don't get them on collapse. Is it really the layering or is it some peculiarity in the source?

MR. ALEXANDER: They are essentially the same depth. You see, one cannot use arguments about the medium being responsible for all of this through

P to S conversion because the explosion and collapse occur at the same place.

MR. ARCHAMBEAU: One of the arguments that has been advanced to explain the generation of Love waves has been the relaxation of pre-existing stress. When you think of what will occur in a stressed medium upon shock induced fracturing due to an explosion then you will find that it is possible to produce Love waves of this size and magnitude. In fact, Nafi and I have both done studies on this process, and it seems to be a good working hypothesis at the moment. We can explain pretty well the magnitude of the Love waves in that way. It can also explain why one does not see Love waves from a collapse nor from materials like salt, where prestress levels must be very low.

MR. CHERRY: You are saying it is due to a small earthquake.

MR. ARCHAMBEAU: Well, something like that. If you conceive of introducing a bounded shatter zone with low strength (or rigidity) into a stressed medium, then the surrounding stressed medium has to adjust or relax, which is accomplished by radiation of energy. You can do that either by shattering a roughly spherical zone or by inducing failure along a pre-existing weak zone of lower symmetry. I will show some slides later on this subject and we can discuss some of the details then.

MR. ALEXANDER: There are all kinds of items of evidence to indicate that, whatever the mechanism, it is associated with the immediate vicinity of the source. I would comment also that it is not evident in this case, this particular event, but in some cases the collapse does seem to produce a higher frequency Rayleigh wave, for example, 10-sec Rayleigh waves.

BODY-WAVE MAGNITUDE VERSUS YIELD

Howard C. Rodean
Lawrence Radiation Laboratory

Most of what I am going to say is contained in a paper that is now being prepared for submission to the Journal of Geophysical Research. I am going to pose a number of questions, and at the most propose perhaps partial explanations for some things that I believe are still puzzles in this business.

With respect to Rudy's comments at the beginning (about the need for communication between the rock mechanics people, the code calculators, and the seismologists), I attended both the June seismic coupling meeting here in Virginia and the Woods Hole meeting a few weeks ago. The latter meeting was essentially a group of seismologists--and talk about two different worlds! I thought with a private grin that probably a lot of the people at the June meeting who are concerned with the details of calculating explosions and the resultant seismic sources would have been aghast at the almost cavalier way some of the seismologists talked about seismic source functions with their idealized point sources and couples and so on and so forth, completely ignoring all of the hard work involving rock mechanics, etc. Therefore, I think this meeting is very timely.

Figure 8 here is a plot that I made up myself. It is unclassified as it stands. However, the two dashed lines and the center solid one are also based on a lot of declassified data. What I have plotted here, and also on another (classified) plot that I used in constructing this, were the yields for all of the shots for which I could also find body-wave magnitudes. I selected the latest body-wave magnitude to be published, and so most of the points on there, as well as on a classified version of this, are either the magnitudes done by Evernden or Basham. Evernden mentioned at Woods Hole that Basham uses essentially the same method, so it is quite legitimate to plot the two kinds of points on the one curve.

As we see here, we have the variations in magnitude of a given yield for shots in dry alluvium, tuff, salt, etc. We have a few events that have rather high magnitudes, like Longshot and Milrow. Milrow, which was about a megaton, had a magnitude of about 6.7 (if I remember correctly what Jack Evernden said at Woods Hole). Perhaps this is a regional effect, and if we had the right kind of regional corrections for that particular part of the world, maybe these points would be moved down to match the main population.

One of the principal points I would like to make is that if we talk about shots in competent materials, and forget about the shots in locking solids like alluvium, about 80 percent of all other shots, from a population of over 50 shots, fit within this band, plus or minus 0.2 of a magnitude unit, and the magnitude-yield curve has a slope of about 5/6.

There is a wide variety of rock types in this band: e.g., the tuffs, the salts, and the granites (except Piledriver is a bit high). I have two points shown here for Gasbuggy. The lower was the original AFTAC shot magnitude. Basham calculated a higher magnitude for Gasbuggy.

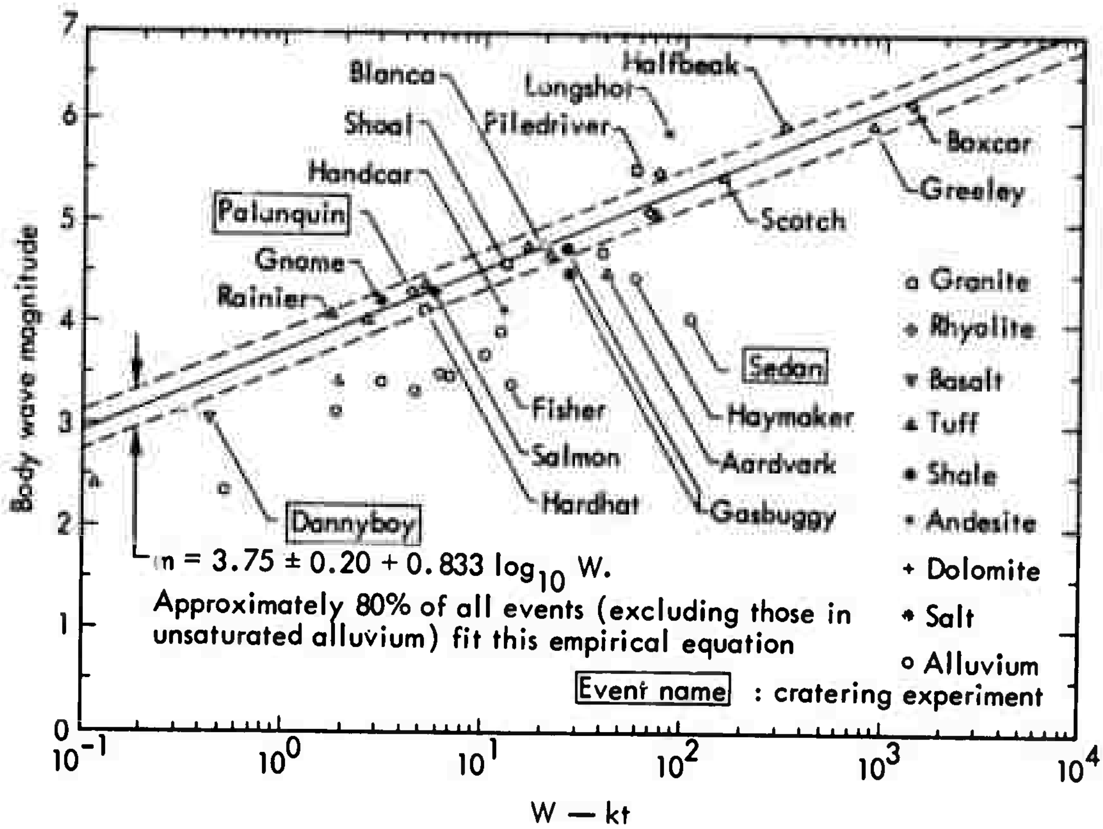

Figure 8. Body-Wave Magnitude Versus Explosion Yield and Rock Type.

So from this point of view, you could say that, at least for the population of shots at the Nevada Test Site including the Pahute Mesa high-yield shots, even though you have a wide variety of shot materials, still a large fraction of the population fits within a fairly narrow band as far as body-wave magnitude is concerned.

With respect to labeling these points with different rock names that the geologists give to the shot-point material, I believe that if we could find a bunch of dimensionless rock-property combinations, sort of analogous to the Reynolds number of fluid mechanics, we would have a more rational way of identifying combinations of shot-point rock properties and could replace such rock labels with a more rational rock description in the magnitude versus yield versus rock property type of plot.

So you might conclude from this that most of the shots fit within a narrow band. We think we know in general why alluvium is a low-coupling material. Maybe it is still a puzzle as to why Longshot, Milrow, and so on coupled high. But otherwise one could assume the attitude "What is the use of getting more detailed". This is dangerous, as I will now try to demonstrate.

Most of the rest of what I am going to say has to do with a problem that I believe is a real puzzle with respect to shots in granite.

One of the things I did recently was to extend some of the work that was done a long time ago by Latter and others in connection with their decoupling studies. I derived a very simple equation for the maximum ratio of seismic-coupling efficiency, where E_W is the radiated seismic-wave energy, and E_X is the explosion energy. My model consisted of a spherical cavity in an elastic medium, and I assumed the explosion is modeled by a step change in cavity pressure. With respect to cavity gas properties, in order to maximize the coupling efficiency I assumed a monoatomic gas with the ratio of specific heats equal to 5/3. The maximum ratio of radiated energy to explosion energy is then a function of Y, a yield function (which is equal to the maximum allowable value of the stress deviator in the rock), divided by the shear modulus μ as follows:

$$E_W/E_X = 2Y/3\mu.$$

We don't have too much data for the maximum value of the Y factor, but shear moduli are more readily available. The interesting thing I found is that when I took this very simple-minded equation and put what I believed were reasonable numbers into it, I got about the same order of magnitude of seismic-coupling efficiency as indicated by experimental data for tamped shots. The SIPRI report (1968), for example, gives ratios of radiated seismic-wave energy to explosion energy based on field observations. The SIPRI report values were contributed by a Russian member of the SIPRI conference.

Another point is that it might be interesting if we could get real good values for some of the upper limits of this strength, shall we say.

MR. GODFREY: May I ask a question there? You spoke of the analysis being based on a spherical cavity in an elastic medium. Why does Y max play any role?

MR. RODEAN: Because I am just trying to get a reasonable number for the maximum stress that you can put on the cavity. It comes out of this analysis for the maximum stress in a cavity, and it is an extension of analysis in one of Latter's early papers.

MR. ARCHAMBEAU: Is it an assumed elastic medium or an elastic-plastic medium?

MR. RODEAN: I am just assuming it is elastic, but that it is just at the verge of failure.

MR. ARCHAMBEAU: But that is the way you define your elastic radius then?

MR. GODFREY: I see. You are just saying there is an elastic zone somewhere.

MR. ARCHAMBEAU: And that it begins at the point or radius where you are just below the yield stress.

MR. RINEY: How does this relate to Haskell's work where he has a zone which is assumed to have failed between the cavity and the elastic zone?

MR. RODEAN: This analysis corresponds to, shall we say, zero thickness of the plastic zone. Anyway, the reason I put this thing in here is to suggest that perhaps we can get a better fill-in on how good this simple-minded equation of mine is if we could get more good data for some of the other rock materials, both strength data and shear modulus data.

Figure 9 is a curve generated by Ted Cherry, Hugh Heard, and others at LRL, and again this is the Y parameter. Ted Cherry, in his most recent paper, has this as Y over 2, but to be consistent with the rest of my work, I changed it to Y. P is a kind of mean confining pressure, and these are the failure curves for three different types of granite: C--dry, solid or consolidated samples, which are strongest; B--dry, cracked; and A--wet, cracked.

The following work was done based on calculations by Ted Cherry after we had been to the Las Vegas Plowshare meeting last January which was sponsored by the ANS and the AEC. The French sent a sizeable delegation to this meeting, and they gave quite a few excellent papers on the post-shot exploration results of their shots in granite in the Hoggar Massif in the Sahara. Their papers were based on the shot program that they conducted in the Sahara before Algeria became an independent country. The French had to discontinue their Sahara tests after Algerian independence.

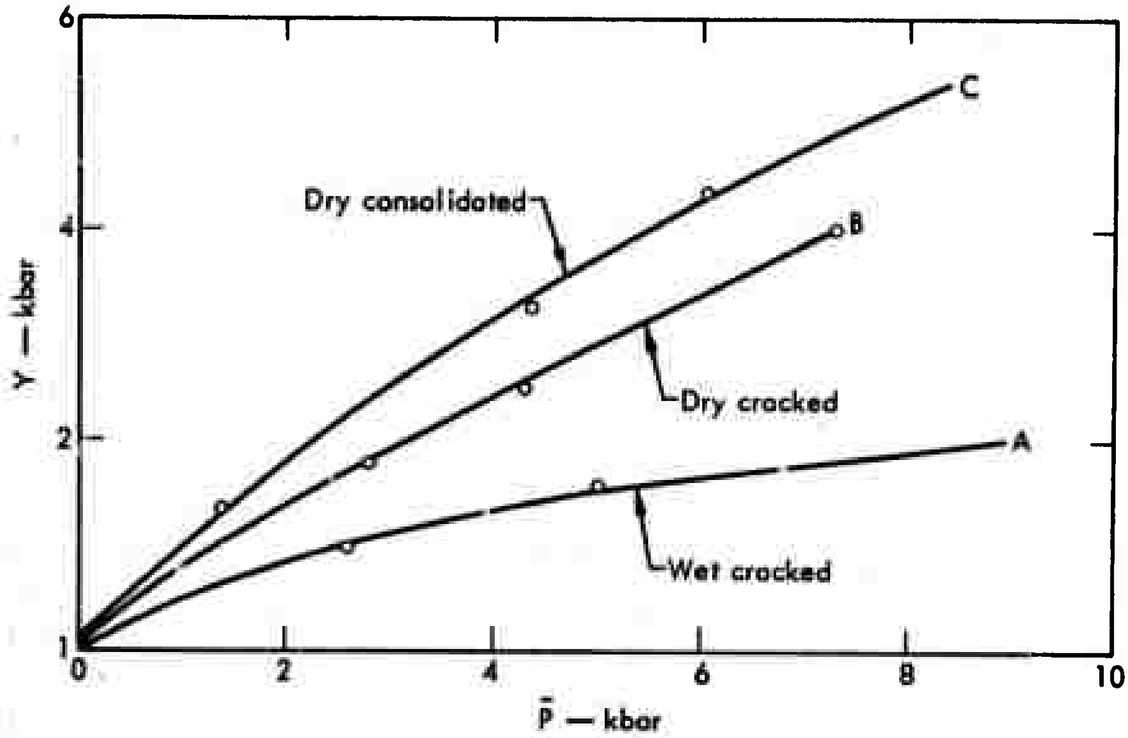

Figure 9. Strength of Hardhat Granite
(*H. C. Heard*, private communication).

One of the puzzles appeared at this meeting when the chief of the French delegation mentioned that for comparable yields in granite the cavities produced by their explosions were only about one-fifth the volume of those of U.S. explosions. This was rather startling to us, so when we got back home Ted Cherry did some calculations using these three strength curves as a basis.

MR. RINEY: What is that cracked granite?

MR. CHERRY: It is a piece of granite that was initially intact and subjected to a triaxial test. The strength was measured, and then we simply redid the experiment with the whole sample of granite in its cracked state in the same container. Nothing was changed.

MR. RODEAN: The curves shown in Figure 10 are, shall we say, the cracking frequency as indicated by the code which Ted used for the A, B, and C materials. A corresponds to the weak, cracked granite (wet); B was the dry cracked; and C was the consolidated sample.

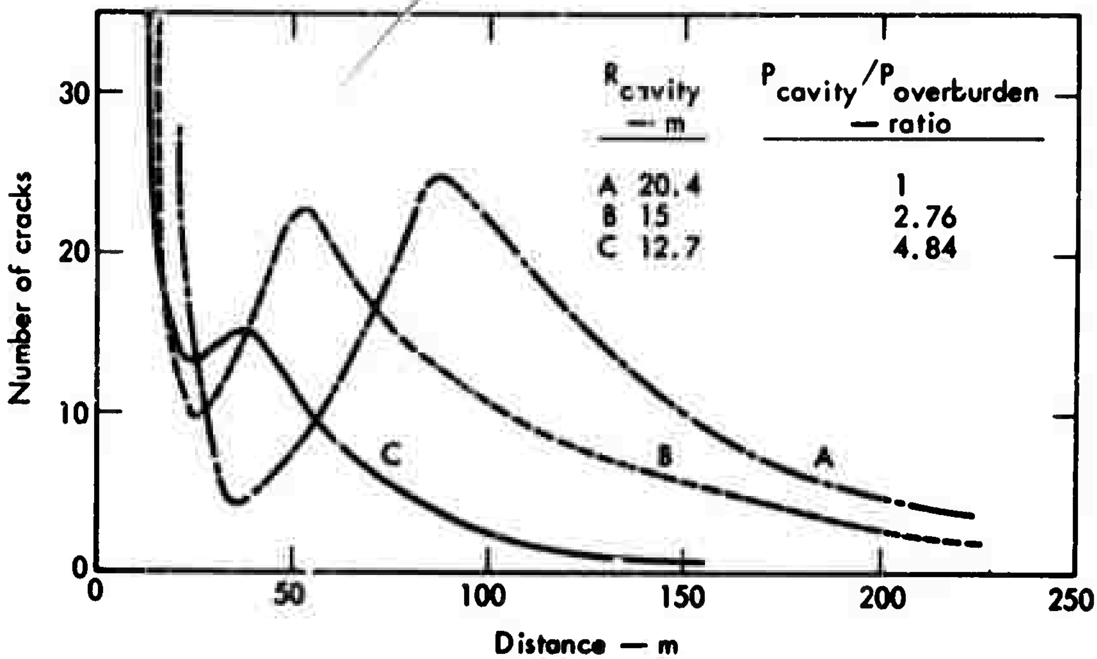

Figure 10. Number of Cracks Versus Distance (5-kt Granite).

What we are interested in here are the calculated cavity radii. These calculations assumed 5-kt yields. Radius A is very close to the measured cavity radius for the Hardhat explosion in Nevada. As you see here, we got a 1 to 4.84 ratio in final cavity pressure to the overburden pressure for the strong granite. In the case of Hardhat, or what we believe to be a good model of Hardhat, we find that the final cavity pressure turns out to be essentially equal to the overburden pressure. The ratio of cavity volumes between samples A and C is more like four-to-one instead of five-to-one, but we believe that this is a good plausible explanation as to why the French results were so different: the French shots were in intrinsically a much stronger granite.

MR. SMITH: In the previous figure, you showed that the wet granite was the weakest, I believe.

MR. RODEAN: Yes, that is the sample with the biggest cavity, A, the wet-cracked granite, which is most representative of the Nevada experience with Hardhat and Piledriver, and C is much more like the granite that the French shot in the Sahara.

MR. COOPER: Why is it C instead of B?

MR. RODEAN: It just gets closer to the French results of about a five-to-one cavity ratio.

MR. COOPER: Maybe I am wrong about the Sahara, but I assume the rock there is jointed, since most rock is. Therefore, the difference would be the water content, so why wouldn't the difference between A and B, rather than A and C, represent the difference between NTS and Sahara granite?

MR. BROWN: Yes, but you can't assume that it is jointed.

MR. COOPER: I can't assume that it is not.

MR. RODEAN: It is jointed, with, I believe, about 20 m between joints.

MR. COOPER: Yes, but what are the wavelengths of interest in this problem?

MR. RODEAN: I don't know.

MR. COOPER: The size of the joint has to be related to something. I believe that the wavelengths of interest are measured in hundreds of feet.

MR. GODFREY: What are the sizes of the joints in Nevada?

MR. RODEAN: About 6 in. Incidentally, the proceedings of the January meeting have just been published, and are available in two bound volumes. They are available from the Clearinghouse and also from Oak Ridge. The French papers are available in English for those who are interested.

MR. RINEY: What about this result?

MR. CHERRY: The results of these calculations were presented informally to a number of people, including you, at LRL. They were presented formally to the scientific community in Vienna at the IAEA meeting on peaceful applications of nuclear explosives in April 1970. The reason I did the calculations was to show the French at the Vienna meeting that a possible explanation of their Sahara granite experience, regarding cavity radius, chimney height, and extent of fracturing, could be obtained if the strength of their granite environment was like our unfractured Hardhat granite. I felt that I accomplished what I set out to do. The French were impressed enough with the calculations that they requested and obtained the slides showing the results.

MR. RODEAN: The curves shown in Figure 11 were also calculated by Ted Cherry. Curve C here is the reduced displacement potential for what we will say is the model of the Sahara granite, and curve A is the calculated result which, in the final steady state, fits pretty close to the Hardhat measurement.

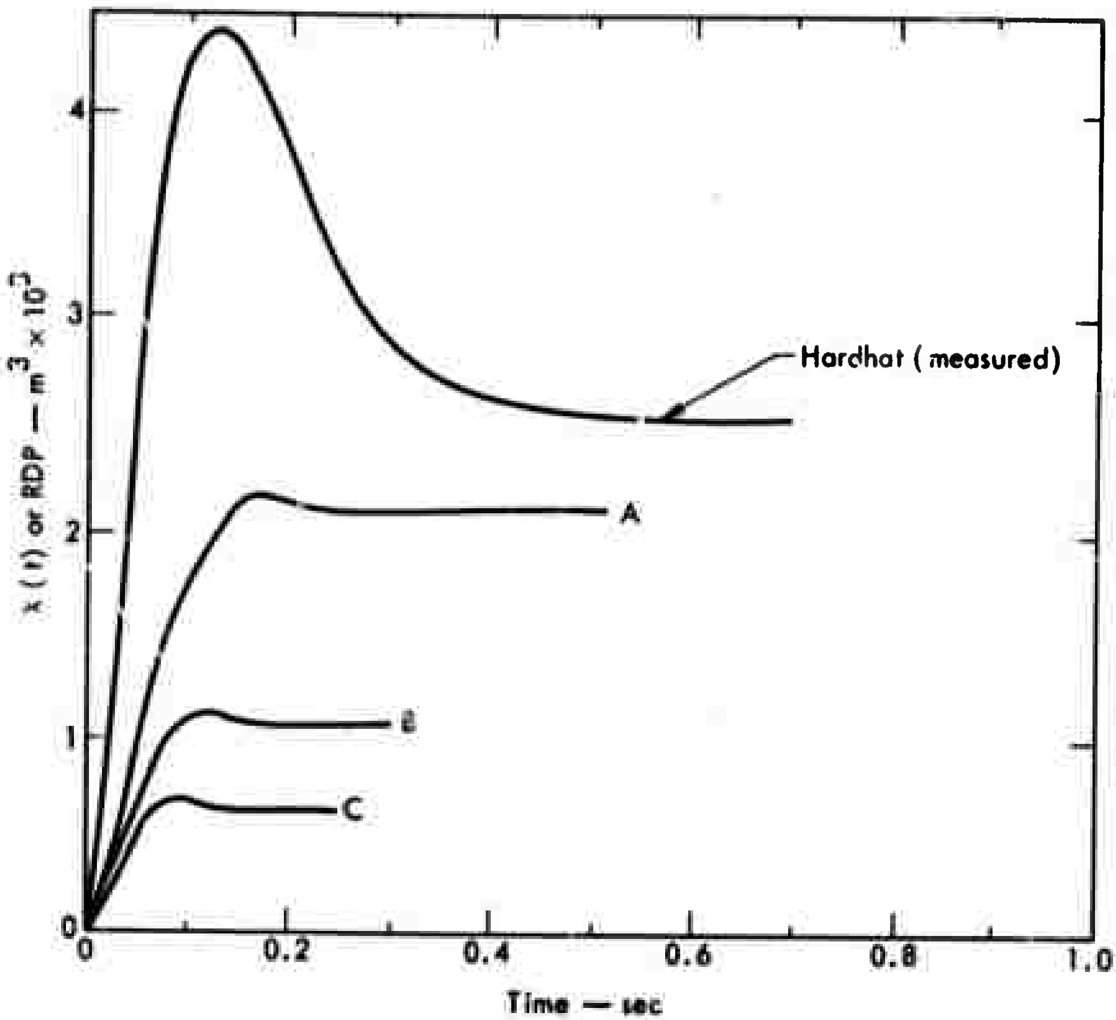

Figure 11. Reduced Displacement Potential (5-kt Granite).

One thing I would like to mention is that the Hardhat reduced displacement potential has the high peak which indicates that there is an impulse component as well as step-function component which generated this reduced displacement potential. We can't seem to reproduce this peak in any of our calculations. If I remember Bill Perret's measurements correctly--maybe he will have something more to say about this--the Gasbuggy reduced displacement potential had a little bit more, maybe not quite as high a peak as this, but more of a peak than Ted Cherry's corresponding calculations. Anyway, if we look at the computer calculations of explosions together with an equivalent system of spherical cavity in a perfectly elastic material, a step function in cavity pressure will give a pretty good approximation to the computer-calculated reduced displacement potential function for an explosion.

Here we have the measured Hardhat reduced displacement potential together with a calculation (Curve A) which agree fairly well in their final steady state values. The French shot should have a very small, in comparison, reduced displacement potential (about 1/4 that of Hardhat).

MR. ALEXANDER: Do you have any thoughts on what causes that amplitude to peak there?

MR. RODEAN: One of the confusing things is that there was a surface reflection which came into the instrument at about this time. Maybe Bill Perret will have some things to say about that.

MR. CHERRY: The reflection off the free surface arrived even a little earlier than that, I think.

MR. ALEXANDER: Your calculation was for a shot in a whole space?

MR. CHERRY: That is right. There is no surface reflection in the calculations.

MR. RINEY: That measured form is sort of typical of the earlier ones reported between 1961 and 1963 at LRL. There they identify the effective pulse as being to the right of that peak.

MR. CHERRY: To the right? I thought it was to the left.

MR. RODEAN: In that vicinity. That is the Werth-Herbst paper (1963).

MR. PERRET: There is a little question about how much effect any reflection from the surface will have on those things since they were measured horizontally at shot level within a couple of hundred feet, which was like 1/4 of the distance to the surface. So that reflection signals which got in there would probably be down by at least an order of magnitude below the peak of that.

MR. CHERRY: I think it is interesting to point out there just has not been anything I can do to the calculations that will reproduce that peak. It has been a very difficult and kind of disturbing measurement. I just have not been able to correlate it.

MR. RODEAN: Ted can calculate a reduced displacement potential that corresponds very well to that generated by a step function in cavity pressure, but the measured peak implies that there is an additional impulse function, which as he said, he can't seem to manipulate the code to reproduce.

MR. RINEY: Have any parameter studies been made for the peak, you know, this little spike that goes out, where this is buried, and how this might affect the reduced displacement potential?

MR. RODEAN: What it does to the spectrum is just add a little extra amplitude to the vicinity of the dominant frequency.

MR. RINEY: To what part of the spectrum? To the reduced displacement potential?

MR. RODEAN: I am talking about the reduced displacement potential.

MR. GODFREY: You can see that peak had a 0.3 sec kind of variant.

MR. RODEAN: The time derivative of the reduced displacement potential--again this is for a step change in cavity pressure within a sphere in an elastic space--has a spectrum that is approximately flat up to a cutoff frequency. The cutoff frequency is equal to two times the shear wave velocity divided by the elastic radius. If you plot the same curve for an ideal delta-type impulse function, for cavity pressure, you get a curve that peaks at the cutoff frequency.

MR. ARCHAMBEAU: Could you outline very quickly for me just exactly how you are doing this? I am not quite sure what you are doing, and I would like to know. Are you assuming a fluid, or what?

MR. RODEAN: No, this is an ideal elastic solid.

MR. CHERRY: The code plots the displacement of a particle at any requested distance from the source.

MR. ARCHAMBEAU: What are you assuming for the rheology in the near-source zone? You have a shock wave going out being converted into an elastic wave.

MR. CHERRY: Yes.

MR. ARCHAMBEAU: So you are just cranking through this thing?

MR. CHERRY: Right.

MR. ARCHAMBEAU: Okay. What are you assuming about the material for the shot?

MR. CHERRY: Well, he showed you the strength of the material, and we just have the regular low pressure hydrostatic compressibility measurements that we do up to 40 kbar, and then above that we take the Hugoniot data.

MR. RODEAN: The reduced displacement potential value that we showed was based on the behavior out in the regions where, according to the code, no inelastic failure occurs. The material does respond elastically.

MR. ARCHAMBEAU: Yes, what is that distance?

MR. RODEAN: I have that here. Table 1 is based on code calculations. This is the Hardhat measured cavity radius of 19 m. What Ted calculated for his wet-cracked model of Nevada granite is 20.4 m. For the dry cracked, the cavity radius is 15 m, and for what we believe is an approximation to the French Sahara granite, 12.3 m. The corresponding final, steady-state value of the reduced displacement potential measured for Hardhat is about 2500 m^3; Ted calculated about 2100 m^3 (Case A). For the next type of granite (Case B) it is about half that, and it is 600 m^3 for, shall we say, the French experience (Case C). Based on the indications in Ted Cherry's problems, the elastic radius for Hardhat appears to be about 365 m. This I think is consistent with experiment. The measurements upon which the 2500 m^3 value is based were made at some distance greater than this, if I remember the numbers correctly from the Hardhat report. For the other types of granite, we get elastic radii of 276 and 165 m.

If we use Equation 2 (Table 1) for the final steady state reduced displacement potential, it is equal to an equivalent cavity pressure, again assuming our simple elastic model, times the cube of the elastic radius divided by four times the shear modulus. So using this reduced displacement potential, this elastic radius, and the shear modulus value, we calculate an equivalent cavity pressure for these data based on this equation. Equation 7 in this table is based on an equation published by Yoshiyama and another Japanese back in 1935 for the total amount of radiated elastic-wave energy, assuming a step change in cavity pressure. I calculated the radiated elastic-wave energy for these three cases, and then the ratio of it to 5 kt.

It is interesting to note that the quantity $E_W/E_X = 0.00272$ for the Hardhat model, and that this quantity for the French Sahara case is 0.00242, so that the total radiated elastic-wave energy is about the same, even though the cavity radii, the elastic radii, and the reduced displacement potentials are radically different. If we accept this simple model, both shots of comparable yield in Nevada granite and Sahara granite would radiate about the same total amount of elastic-wave energy.

Figure 12 is from the SIPRI report except that I deleted Longshot, Gnome, and Salmon from the curve because they are not applicable to what we are talking about. So these are all granite shots: Hardhat, Sahara, Shoal, Sahara, Sahara, Piledriver, and Sahara. In view of the preceding data we wondered where these magnitudes and yields for the French shots came from. At the Las Vegas meeting last January, the French were very reticent about the yields of their shots. They just said they had so many shots greater than 20 kt and so many less. They normalized all of their data to 5 kt, and all of their papers are based on that nominal yield. But, if you take this figure at face value, you find that the magnitude versus yield curve for Nevada granite and Sahara granite is just about the

Table 1. Experimental and Calculated Values for Cavity Radius and Reduced Displacement Potential for a 5-kt Explosion in Granite. Calculated Elastic Radii and Solutions of Equations 2 and 7.

Medium	R_{cavity}(m)	$\chi(m^3)$	$R_{elastic}$(m)	P(bars)[a]	E_w(kt)[b]	E_v/E_x
Hardhat granite (measured)	19	2500	—	—	—	—
(A) Wet, cracked granite (calculated)[c]	20.4	2100	365	43	0.0136	0.00272
(B) Dry, cracked granite (calculated)	15	1050	276	50	0.0079	0.00158
(C) Dry, consolidated granite (calculated)[d]	12.7	600	165	134	0.0121	0.00242

Eq. 2: $\chi = PR^3/4\mu$

Eq. 7: $E_w = 2\pi P \quad \chi = \pi P^2 R^3/2\mu$

[a]Based on equation 2 and value μ = 251 kbar used by Cherry in his calculations

[b]Solution of equation 7

[c]Model for Nevadu granite

[d]Model for Sahara granite

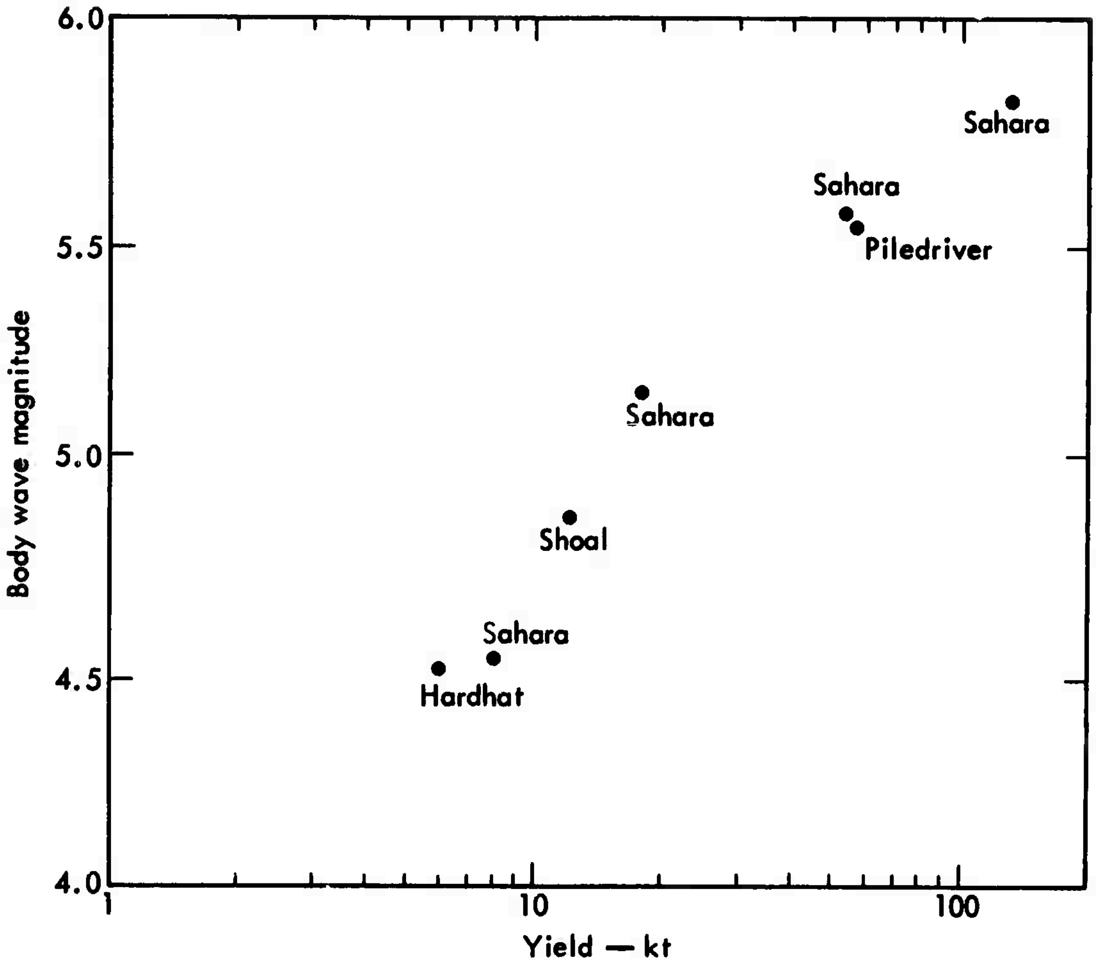

Figure 12. Some Data on Explosions in Hard Rock. Named explosions are the U.S. detonations; those labelled Sahara are the French detonations. No error bars are placed on the magnitude determinations, but error may be taken for the lower yields as ±0.3 and for the larger yields as at least ±0.1 (*SIPRI*, 1968).

same. Yet we have the evidence (based on what the French say) that their cavities (per Ted Cherry's calculations), the elastic radii, and reduced displacement potentials are radically different.

MR. CHERRY: The question back here was is the Sahara point their data, and it is.

MR. RODEAN: I am coming up to that. When I was at the Woods Hole meeting a few weeks ago, I talked to Dai Davies, who is now at Lincoln Labs, and to Peter Marshall from the United Kingdom Atomic Weapons Research Establishment. I learned that the magnitudes for the Sahara shots as published here were determined by the French based on measurements at one station in France, and that the yields were given to the SIPRI conference by a Frenchman. So these are French magnitudes based on one station, and the yields as released by the French on that occasion.

Peter Marshall also told me that he had since taken these French yields at face value, but recalculated the magnitudes based on readings at other stations in Europe. He had a rough pencil version of a magnitude-yield curve with him. For example, he had this Sahara point here some distance below Piledriver, so that perhaps much of the Sahara data came down somewhat below, but not on a magnitude scale terrifically below, the average hard-rock curve. Remember on an earlier figure I showed that Piledriver is somewhat higher than the average for hard and wet rock. So perhaps these values for magnitude, again accepting the French yields, are somewhat below the U.S. experience, but not too much below, especially if you consider the information which I believe is on the next figure.

MR. ALEXANDER: I have a question before you go to that. There seems to be a definite regional dependence on body-wave magnitude. For example, NTS events tend to show up systematically low in body-wave magnitude. Has that been taken into account at all here?

MR. RODEAN: I don't know the answer to that question.

MR. ALEXANDER: That would force at least about half a magnitude unit difference, and low for NTS events as opposed to the French. This I think is because of the attenuation in the upper mantle of the western United States.

MR. RODEAN: All I can say is that Peter Marshall's pencilled version of his magnitude-yield curve showed that one Piledriver-like-yield French shot had a magnitude more like Rulison.

MR. ALEXANDER: If that were true, that would lift up all of the NTS ones above the curve of the other by about half a magnitude.

MR. RODEAN: Yes, but not as much as what Figure 13 leads up to. This uses a lot of Bill Perret's data.

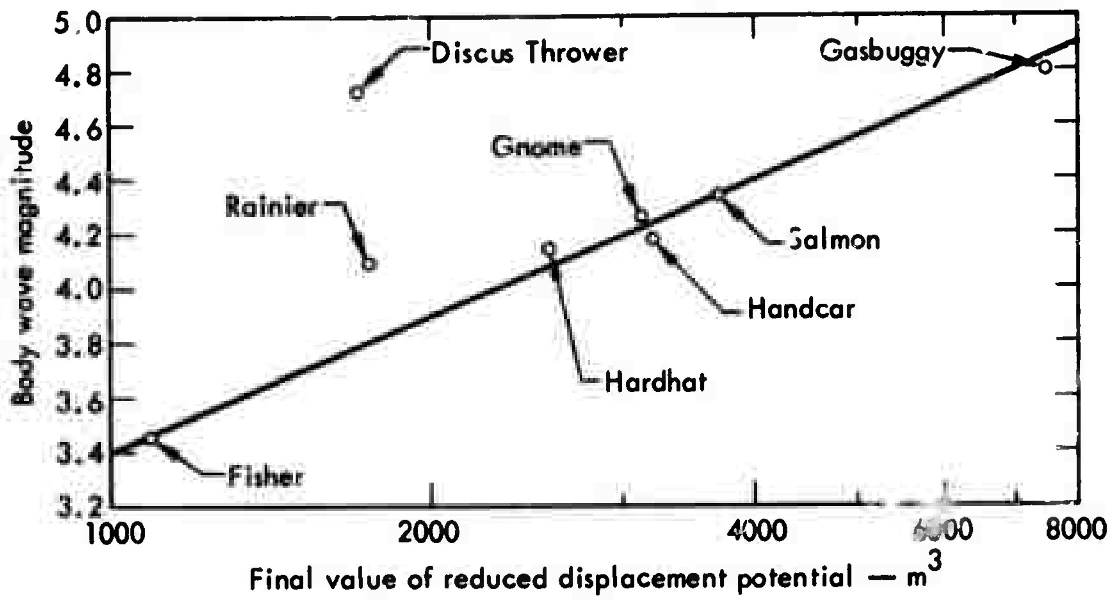

Figure 13. Body-Wave Magnitude Versus Final Value of Reduced Displacement Potential.

MR. TRULIO: Howard, on the previous figure, are the detecting systems there the same? A great many different locations and shots are represented.

MR. RODEAN: The U.S. shot magnitudes are probably based on U.S. stations. As I said before, the magnitude data presented on that figure for the French shots were based on readings at one seismic station in France. This I learned by talking to Peter Marshall. Then he had re-done them and gotten somewhat lower magnitudes using the readings from other stations in Europe, probably mostly in the United Kingdom.

MR. BROWN: He used these same distance corrections that were spoken of earlier.

MR. RODEAN: Yes.

MR. TRULIO: Yes, but I am talking about the group of detectors rather than corrections for the medium.

MR. BROWN: The instruments you assume are comparable, is that right?

MR. TRULIO: Are they?

MR. RODEAN: I am assuming they are. I don't know the real answer to that question, though.

MR. TRULIO: There is an obvious related question. Suppose you made seismic wave measurements for a variety of yields in the same medium, using the same detecting system in each case. The pulses for the larger yields are spread out in time. If you fold the time-scaled (but otherwise identical) pulses for different yields into the frequency response curve for the postulated standard detector what happens to the magnitude-yield curve?

MR. RODEAN: It bends over at the higher yields.

MR. TRULIO: I mean the one that you had on the previous figure. How does it look if you fold in the variation in pulse width as the cube root of the yield?

MR. RODEAN: For the yield range that we are talking about, where we went up to only 200 kt at the most, that effect is not too noticeable. The curves bend over because of the shift in signal spectra with respect to the response of the instrument only when the yield approaches a megaton.

MR. RINER: There are two factors, I guess, if you take that scaling law, and then take the transform of it. You get a magnitude ratio of two-thirds power coming in because this is bending, and then there is also the shift in the spectrum, too. There is also an amplitude-magnitude ratio of two thirds that comes in addition, if you just assume the simple scaling law.

MR. TRULIO: If you assume a simple scaling law, then at corresponding distances with a scale like the cube root of the yield, you get the same pulse except it is stretched out by the same factor as the distance.

MR. RINER: Well, I was trying to quantify that by taking the Fourier transform and re-do that. That two thirds comes in the transform, and that gives you the bending over. That is primarily the reason for it.

MR. ALEXANDER: The question really then comes back to the peak of the instrument. The peak of the instrument is in that flat part of the displacement curve up to a pretty high yield. I think that is what you were saying, isn't it?

MR. RINER: Yes.

MR. ALEXANDER: So it does not matter where that curve bends over at different places as long as your instrument is peaking way out at around one Hz.

MR. RODEAN: I have something on that in some of my later figures.

The curve of Figure 13 is related to the preceding plot of the magnitude-versus-yield for the Nevada and the Sahara granite shots. This one shows body-wave magnitude, from five on down to three, versus the final steady state value of reduced displacement potential. Most of these reduced displacement potential data are in an as-yet-unpublished report by Bill Perret on Gasbuggy, which is the point in the upper right corner. Then we have Handcar, Gnome, Salmon, Hardhat, Rainier, and Fisher. Fisher, Rainier, Hardhat, and Gnome reduced displacement potentials are also given in the four mediums in the Werth-Herbst paper. Salmon values are given in a report on that event.

There is one other point in this figure for Discus Thrower which is also contained in Bill's report. Interestingly enough, I think we have reduced displacement potential measurements for only Merlin and one or two other shots in addition to those listed here. The Merlin magnitude, as far as I know, has never been computed by the seismologists, but I think it would be interesting to get that, especially if we can succeed in getting the Merlin yield declassified.

The main point of this is that, with the exception of Discus Thrower, there seems to be a pretty decent correlation between these body-wave magnitudes and the final, steady-state values of reduced displacement potential. Fisher, Hardhat, Gnome, Handcar, Salmon, and Gasbuggy are very close to or on the curve. Rainier is a little bit high, but as Carl Kisslinger pointed out to us, the Rainier magnitude is one calculated a long time ago by Carl Romney. These other magnitudes are by Jack Evernden or Mr. Basham. The Romney magnitudes, according to Kisslinger, were a tenth or a few tenths higher than those later calculated by Evernden, so if you would assume the same type correction would apply to Rainier, perhaps a corrected Rainier point would come down closer to the curve.

The Discus Thrower anomaly is readily explained because the measurements upon which this is based are in the horizontal plane through the shot point, in roughly the same type of rock material, and Discus Thrower was quite close to or not too far above a discontinuity in the geology. There was a much harder, different type of rock not too far below the shot point. Therefore we can't expect much correlation between the distant seismic signal and the reduced displacement potential.

If we think back to the French data for, shall we say, a 5-kt shot in the Sahara, we calculated that the corresponding final value of the reduced displacement potential is about 600 m^3. If we would extrapolate the curve in this Figure 13 we would expect a 5-kt shot to have a body-wave magnitude of about three. That would be a much bigger shift downward than those indicated by Peter Marshall in his corrected version of the SIPRI body-wave magnitudes.

MR. ROTENBERG: I just wonder how much confidence to put in the slope of these straight lines that go through a selected number of points?

MR. RODEAN: I don't know. As I pointed out, the number of shots for which we have both body-wave magnitudes and also reduced displacement potential measurements is very, very small, so this is the only common population that I could find.

MR. ALEXANDER: Is this because there are few displacement potentials measured?

MR. RODEAN: Relatively few good measurements of the reduced displacement potential have been made. Bill Perret can speak to most of them, which he will do later on.

MR. ALEXANDER: You could remeasure the body-wave magnitudes.

MR. RODEAN: You are talking about instruments being at the right place at the right time.

MR. CHERRY: I think it would be a mistake to throw out the Gasbuggy data. It is probably some of the best that I have seen. The data were very consistent on Gasbuggy.

MR. ALEXANDER: But the body-wave magnitudes would be biased, however, because that is in a different setting than NTS.

MR. CHERRY: Sure, because it is alluvium, and the rest are sort of rockish.

MR. TRULIO: It is also really true that Discus Thrower does not belong in this set at all. It simply isn't a spherical shot; so it can't be put on the same basis with the others.

MR. RODEAN: Yes, that is what I have said.

MR. TRULIO: That is right. If you want to draw a horizontal line, you might be biased by Discus Thrower, and it really is not comparable to the other shots.

MR. RODEAN: What I have put here is the total population that I know of, shots that have both a measured reduced displacement potential and a determined body-wave magnitude.

MR. CHERRY: If you are going to throw anything out, I would throw out Handcar.

MR. ALEXANDER: Handcar is in a very layered geology, too.

MR. PERRET: Except that Handcar was down in the hard rock, and the others were in soft rock.

MR. RODEAN: I think right here we are getting to one of the key points of this whole meeting. Here is where we have the very few experimental links between close-in measurements and the distant seismic measurements.

MR. PERRET: Let us define something about the rocks near these shots. Fisher was in alluvium at about a thousand feet with hard rock and the water table down another 500 or so feet, maybe a thousand feet. Rainier was in tuff in the Rainier Mesa, and it was, I guess, of the order of several hundred feet away from any reasonably hard rock. Discus Thrower was in tuff. It was about 100 ft above the paleozoic rocks which were dolomite and argylite. Hardhat was in the granite, and was not near any kind of an interface except a few fault planes. Gnome was in layered salt, and there was one continuous but thin conducting layer of polyhalite near shot level. Handcar was several hundred feet below the top of the paleozoic carbonates, and the measurements for that were made in the carbonate rock. Salmon was in a salt dome and measurements were all made within the salt dome. Gasbuggy was in the Lewis shale, which is pretty hard shale below hard sandstone. The seismic impedance contrast between those two formations was small.

MR. BROWN: Could you say a little bit more about this reduced displacement potential and how you measure this? I am still a little confused.

MR. PERRET: I will do that later.

MR. RODEAN: Are we done with this? Anyway, this is all of the data of this kind that is available. My point here is that if we believe this kind of slope here, and we extrapolate out to the calculated 5-kt shot in Sahara granite, this would imply a body-wave magnitude of three, whereas Hardhat was almost 4.2. The downward shift that Peter Marshall talked about with his correction to the initially determined French magnitudes was only a few tenths of a magnitude, not a magnitude unit at all. What I am saying here is that if you would then try to take this curve and apply it to the French case, it just does not fit.

To repeat, if we would take Peter Marshall's corrections to these French magnitudes and shift them down (Figure 12), we would not shift them down by anywhere near an order of magnitude. It would be just a few tenths of a magnitude.

MR. ALEXANDER: They might if you do another thing as well, and that is to shift up all the NTS ones by about a half magnitude on account of the differential-attenuation bias in the body-wave magnitudes for that area.

MR. CHERRY: I think that is a good point; instead of shifting the French data down, the NTS data ought to be shifted up.

MR. RODEAN: That is something for the seismologists to argue about. My main point is that there is a bunch of data which does not seem to make sense.

Here in Figure 14 are the normalized spectra for granite-type A, which is our model for the Nevada granite, for C the French granite, and then this is the measured Hardhat spectrum as published by Werth and Herbst. I normalized them to each other. The frequency coordinate is a normalized frequency in terms of what I call the cutoff frequency, twice the shear wave speed divided by the elastic radius.

The main thing I wanted to point out here for these idealized spectra (again based on an assumed step change in cavity pressure) is that the crossover point between, shall we say, the Nevada and Sahara spectra, is at about 5 cps, which is within the bandwidth of the short-period instruments used for measuring body waves.

Then if we do a few mathematical manipulations to these curves, we get Figure 15. This is really the energy spectrum for the Nevada granite and the Sahara granite, and if we remember one of my earlier tables, the total energy under curve C is not too much less than the total energy under curve A. These are the energy spectra as determined experimentally for Hardhat based on the Werth-Herbst data, and also a paper by Berg and Trembly. The difference between the two, perhaps, is just based on my scaling off of the small curves published in the journals. Again, this shows the crossover point between the two spectra is at about 5 cps, which is within the instrument bandwidth. If we assume cube root scaling for the shift in this spectrum as we would go from 5 kt up to, say, 200 kt, which seems to be about the largest yield for the largest of the French shots, this crossover frequency would shift down to on the order of 1.5 cps, which is still in the same bandwidth. Therefore, I am proposing that these explosions--even though these shots are in two types of granite which are so different with respect to final cavity volume, reduced displacement potential, elastic radius, and so on--have approximately the same total radiated seismic-wave energy, with the spectra crossover in the region of the peak response of the measuring instruments. Whether this is the real explanation or not, I don't know. As I said, one of the main purposes of my paper is to pose the question.

MR. ALEXANDER: If those are teleseismic measurements, I don't think you can.

MR. RODEAN: These are theoretical curves.

MR. ALEXANDER: I know, but you are saying that C there peaks in real frequency at about 5 cps.

MR. RODEAN: Yes.

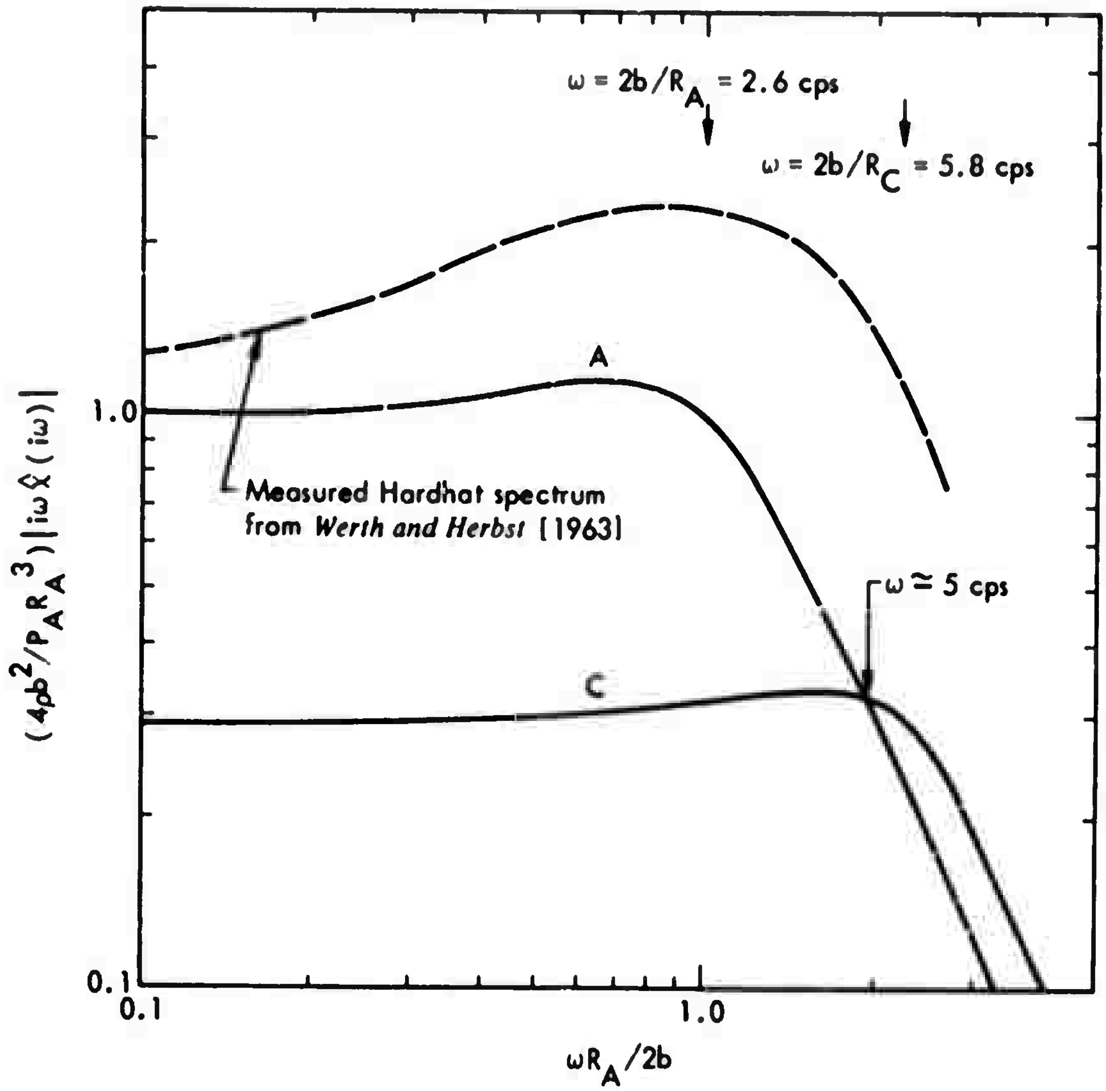

Figure 14. Fourier Amplitudes of the Time Derivatives of the Reduced Displacement Potentials Based on the Values of Elastic Radius and Step Change in Cavity Pressure Equivalent to the Calculated Nevada, A, and Sahara, C, Explosions Listed in Table 1. Measured Spectrum of $\partial \chi(t)/\partial t$ for Hardhat (*Werth and Herbst*, 1963).

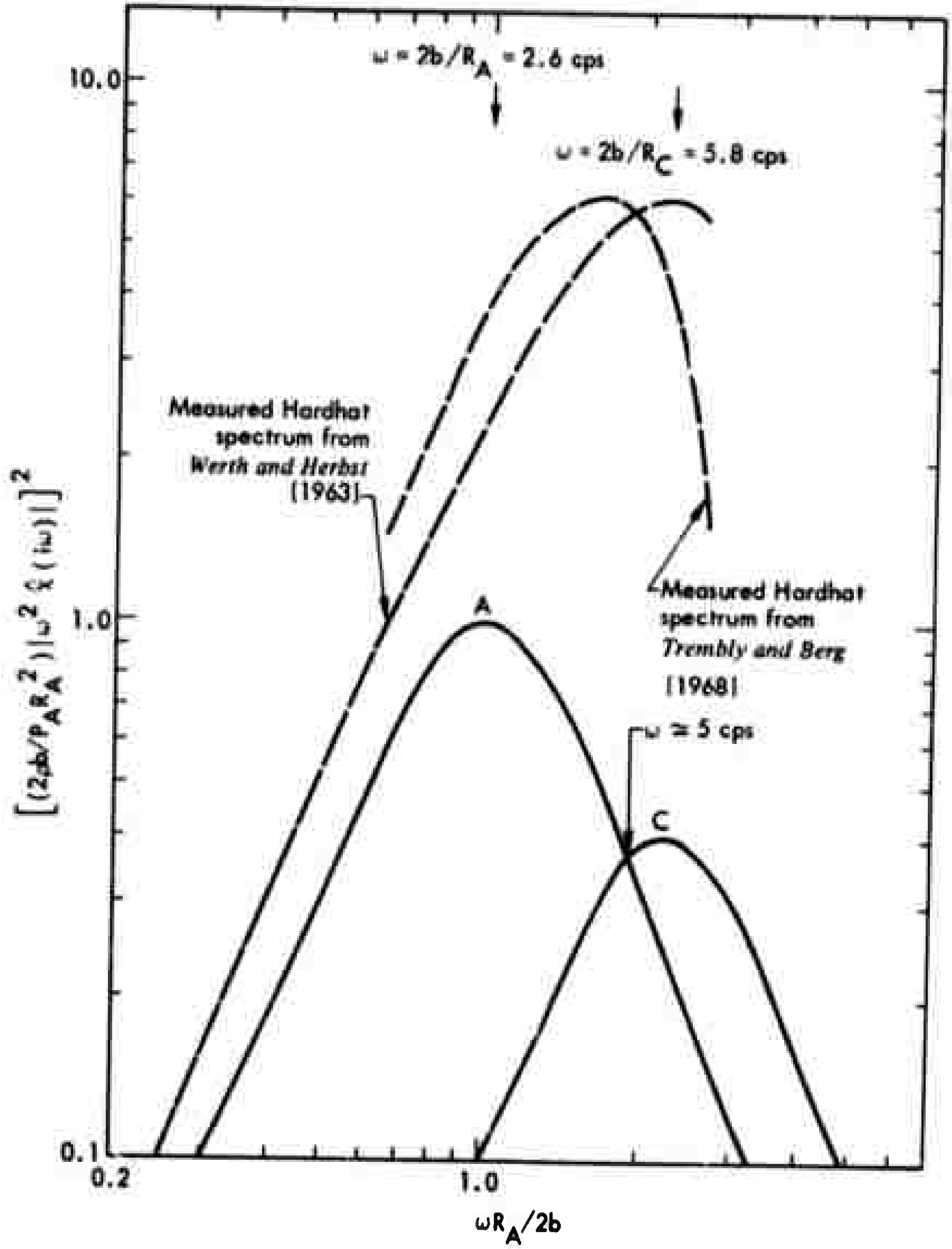

Figure 15. Square of the Fourier Amplitudes of the Second Time Derivative of the Reduced Displacement Potentials Based on the Values of Elastic Radius and Step Change in Cavity Pressure Equivalent to the Calculated Nevada, A, and Sahara, C, Explosions Listed in Table 1. Measured Hardhat Energy Spectrum (*Werth and Herbst*, 1963; *Trembly and Berg*, 1968).

MR. ALEXANDER: It is rare indeed if you ever see teleseismic P waves at that frequency range. You see one cycle energy. The attenuation is enormous at 5 cps, and you just don't see it at teleseismic distances, no matter what the source. You are peeling off that high frequency energy too fast for it to be seen at large distances.

MR. RODEAN: There is no attenuation in this figure.

MR. ALEXANDER: Between A and C you should see an enormous difference in body-wave magnitude.

MR. RODEAN: But I am saying that if we believe the published data, we don't.

MR. TOKSOZ: Then there is one thing left. You would be needing alternations.

MR. CHERRY: The interesting thing about the calculations, at least the ones I did, is that the cavity radius, the chimney height, the amount of fracturing in the French shots seem to be explained by the material properties they encountered. The thing that is a puzzle is that the reduced displacement potential is so low. Well, it is not a puzzle. When I did this I said, aha, we have explained the French data, and people said no, you have not, because their shots are coupling as well as our granite shots at NTS.

MR. ROTENBERG: Ted, when you said you varied everything in your calculations to see how they would fit the French data, did you vary the rate of onset of the pressure pulse?

MR. CHERRY: No, I kept the compressibility the same. The only thing that varied was the strength.

MR. ROTENBERG: You did not program the pressure.

MR. CHERRY: No.

MR. ROTENBERG: If you put that on more slowly than a first step, you get more of a pulse.

MR. CHERRY: That was done the same in all of them.

CLOSE-IN MEASUREMENTS

William R. Perret
Sandia Laboratories

Let me do a little defining first. The seismologists talk about close-in stuff when they are 30 or 40 km out, and where I have been making measurements, we call close in anything inside several hundred meters. The people who have been making hydrodynamic measurements call close in anything within 10 or 20 m. So bear in mind that when I say close in, I am not talking about away out in the elastic or the seismic region, and I am not talking about the hydrodynamic region.

The measurements we have made are quite obviously divided two ways. One of them is what we optimistically call free field, and the other surface measurements. The free-field measurements are made in the environmental rock and are called free field because you would like it to be a simple homogeneous rock with no free surface. Normally none of these things can be realized, but if you are lucky you may be close enough to the explosion in the same rock that effects of the free surface or of overlying or underlying beds of different materials arrive late enough or are small enough that the record you are concerned with is not seriously affected by them.

The other problem that lies in this area is one of dynamic range of the instruments. If you are close enough in so that you must record peak signals in the neighborhood of 10 to 1,000 g's at 5 to 10 Hz, pretty obviously you can't see the low frequency signals in the neighborhood of a tenth or a hundredth of a g. They are down in the noise. Consequently, most of our data are limited in frequency range to somewhere between 1 or 2 cycles and perhaps 10 or 20. The instrumentation has the capacity to record higher frequencies, but there is very low signal strength there. I don't think we are very concerned about the high frequencies, because they don't get very far through the ground.

With that as an introduction, in Table 2 I have put together a list of events from which we have free field data, and from which we can determine reduced displacement potentials or make an energy ratio determination. There are four of them in alluvium: Fisher, Haymaker, Merlin, and Faultless. The first two are unclassified, but I am not sure whether the yields have been unclassified in terms of numbers or just in terms of approximate sizes--the system that differentiates small, intermediate, and large yields.

We hope to get the yield of Merlin declassified in the next few months. The other one, Faultless, probably will never become unclassified.

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Table 2. List of Events and Type of Data Available

ROCK	EVENT	OBSERVATIONS
Alluvium	Fisher	Freefield & Surface
	Haymaker	Freefield & Surface
	Raccoon	Surface
	Aardvark	Surface
	7 others in Area 3	Surface
	Merlin	Freefield & Surface
	Faultless	Freefield & Surface
Tuff	Rainier	Freefield & Surface
	Mudpack	Freefield & Surface
	Discus Thrower	Freefield & Surface
	Agile	Freefield & Surface
	Commodore	Freefield & Surface
	Lanpher	Freefield & Surface
	Cypress	Freefield
	Clearwater	Surface
	Antler	Surface
	New Point	Surface
	Pin Stripe	Surface
Granite	Hardhat	Freefield & Surface
	Shoal	Freefield & Surface
	Piledriver	Freefield & Surface
Salt	Gnome	Freefield & Surface
	Salmon	Freefield & Surface
	Sterling (decoupled)	Freefield
Volcanics	Halfbeak	Surface
	Greely	Surface
	Scotch	Surface
	Boxcar	Freefield & Surface
	Handley	Surface
	Longshot	Freefield & Surface
	Milrow	Freefield & Surface
Sedimentaries	Handcar	Freefield & Surface
	Gasbuggy	Freefield & Surface

In tuff, we have data from Rainier, Mudpack, and Discus Thrower, and from Agile, Commodore, and Cypress. The first three have unclassified yields. The last three do not.

Four of those six were fired in tuff in a vertical hole in the Yucca Flats area, two in Area 8, and two in Area 2. Rainier and Cypress were both in Rainier Mesa tuff.

There are three granite shots: Hardhat, Shoal, and Piledriver. You have seen some of the information on Hardhat and some data from Piledriver.

Three shots were located in salt: Gnome, Salmon, and Sterling; of course, Sterling was the decoupled one that was fired in the Salmon cavity.

There are three that were in rhyolite or andesite, namely, Boxcar, Longshot, and Milrow. Boxcar was in Pahute Mesa, and Longshot and Milrow were on Amchitka.

There were two that I have called sedimentaries; the first is Handcar, in dolomite, and the other is Gasbuggy, which was in Lewis shale.

I have also made a list of those events from which we have surface data. These surface data range from within 50 ft of surface zero out to twice the shot depth in most cases and as far as 84,000 ft.

There are something like 19 sets of surface data from shots that were in alluvium, including Fisher, Merlin, Haymaker, and Aardvark. There are 15 that were in tuff, most of these in tuff below alluvium in Yucca Flats. There were two from Amchitka, two from salt: Gnome and Salmon. Handcar and Gasbuggy were in sedimentaries; Hardhat and Piledriver in granite. Four, and possibly five sets of surface data were from Pahute Mesa, including Halfbeak, Greely, Scotch, Boxcar, and Handley with a question mark after the last because of the distribution of gages there.

This gives you some idea of the kind and distribution of data that are available from the close-in region.

In general, we measure two things, acceleration and the particle velocity as dictated by limitations of instruments. We do have accelerometers which serve our purpose very well and which are rugged enough to live through any loading through which cables can survive to get the signal out. We have a velocity gage that will, generally speaking, go through the same loading. However, the velocity gage will stand a lot more acceleration than it can accommodate in either frequency or velocity response.

We do not have a good displacement gage. Part of this is due to the fact that we are trying to measure displacements of the order of feet, in a 6-in. diameter boring. We have had some gages that could do this, but results were not reliable. They used either a segment pendulum or a mass riding on a splined shaft to drive a flywheel, with the result that the transducer mass moved only a fraction of an inch in response to a displacement of the order of feet.

The trouble with our displacement gages was extreme sensitivity to tilt. It is fairly obvious that when you have a mass riding on a horizontal shaft to respond to horizontal motion, if friction is reduced as close to zero as possible, it takes very few minutes of arc of tilt to cause the mass to run down to an end stop instead of staying in the middle. The same thing was true of the pendulum. SRI put some soft springs in their pendulum gage to control response to tilt with some degree of sucess, but in processing the data it was necessary to subtract the reaction of the springs from the records.

MR. SMITH: The same criticism is true of the integrated accelerometer records. They are also sensitive to tilt in exactly the same way.

MR. PERRET: No.

MR. SMITH: There is absolutely no way of distinguishing between tilt and horizontal acceleration, without an inertial reference frame fixed on the stars or something.

MR. PERRET: This is possibly true, but the difference is that sensitivity of an accelerometer to such tilts is usually down in the noise.

MR. SMITH: Which brings up the question of reliability of the base line.

MR. PERRET: As I said in the beginning, for the long-period signals you can't see the signal because there isn't sufficient dynamic range in the instrument system to record the peaks and to resolve the long-period signals from the noise.

MR. SMITH: In these various records we see of reduced displacement, how low in frequency do you consider them reliable?

MR. PERRET: Oh, probably one, possibly a half cycle, not much more.

MR. RODEAN: Bill, I got quizzed on this at the Woods Hole meeting: What is the final steady-state value of the reduced displacement potential as inferred from measurements? I indicated that very often the steady-state value was at late times so you had to be very careful on how much you believed the integrated measurements. Could you speak to that, please?

MR. PERRET: I will get to that later on. What we have done, then, is to get our displacements by integrating either the acceleration or the velocity-gage record. The velocity-gage record is essentially an internally integrated acceleration record. The velocity-gages we have used and found most satisfactory are grossly over-damped pendulums, where the damping factor ranges from 75 to 200 times critical. The consequence of this is that displacement of the pendulum gives about 99.5 percent velocity response, and about 0.5 percent displacement response. We have integrated these both digitally, in other words, we have digitized the analogue records off magnetic tape, and we have integrated them electronically before they went on the tape. There the agreement is frequently within 5 percent on the peaks, and in the longer-period signals the electronically or digitally integrated data from a velocity gage are very similar. From the acceleration record the doubly integrated, displacement signal usually agrees with the others at peak motion, but long period or residual data include numerous deviations from integrated velocity-gage data as a result of doubly integrated system noise.

One of the biggest problems we have had in data reduction is location of true signal zero. If you have a record with appreciable noise before the signal, the choice of the real zero is somewhat arbitrary and the integration may include a significant ramp in long-period data.

We get around that problem in part by making the assumption that since the gage remained in the ground, relatively close to where we put it, out in a velocity record beyond the principal signal, we can arbitrarily pull the record back to zero. This can be done with velocity and, of course, with acceleration, but it can't be done with displacement because finite residual displacements may occur at fairly large distances from an explosion. Generally, the purely elastic-response region is beyond really good measurements from our gages.

MR. SMITH: The important point is that it appears as if the reduced displacement potentials are crude enough or long enough to cover the period range that is of importance in the m_b measurements, which is 1 cps at teleseismic distances.

MR. PERRET: I don't want to say that yet.

MR. SMITH: Well, that is really an important thing.

MR. PERRET: Well, that is what is coming up.

MR. ARCHAMBEAU: That is right on the borderline. You are saying at best he is out to 1 cycle.

MR. PERRET: In some cases this is true. and in some cases it is not.

MR. RODEAN: Is what you have just said, and let me put it another way, is that you believe your measurements more in the inelastic region than farther out in the elastic region?

MR. PERRET: Generally this is true. Let us say the precision is better in inelastic regions, because we have big enough signals for our type of instrumentation there. This does not say that we don't ever have a big enough signal in the elastic region to produce usable records, but we don't have the kind of precision that we have closer in.

For displacement potential we do two things. Like everybody else, we use the displacement records and integrate them with the usual computer program for deriving potential from the displacement. We have also developed a circuit which does this to an electronically integrated velocity record before it is recorded on tape; again the two results agree fairly well. The real problem of reduced displacement potential integration is that the part out to somewhere past the peak is pretty reliable, but whether it drops off much or only a little depends strongly on the kind of correction, if any, made in the velocity record to pull it to zero.

MR. SMITH: What time is that line you drew?

MR. PERRET: That depends on whether you are working in hard rock or in alluvium because of the length of the record. In other words, in hard rock, like granite or dolomite, this first maximum duration may be of the order of 0.5 sec or less. In alluvium it may be 2 sec.

I have here in Figure 16 some records of the reduced displacement potentials from Discus Thrower, Hole 9. These are from five stations at different depths. The first, 9A-UR, was in the tuff a little above shot level; the second, 9B-UR, also in the tuff, but about 50 ft above the interface with the paleozoic. 9D-UR was at the tuff-dolomite interface. The last two, 9E-UR and 9F-UR, were in the carbonate and dolomite respectively. The deepest was about 300 ft below the interface. The time ticks are 0.5-sec intervals. Zero time was that of detonation, so the signal arrived at roughly 0.2 sec. Down in the dolomite the period is much shorter, and other signals come in that probably are refracted or reflected motion from the surface.

These illustrate fairly well the problem of reliability of residual potentials.

MR. BROWN: Are those from accelerometers or velocity gages?

MR. PERRET: These are radial vector records from velocity gages. In the geological profile, Figure 17, there was an alluvium-tuff interface and a tuff-paleozoic interface. There was a layer of argillite near the top of the paleozoics in some parts of the section, but seismic impedance of the argillite was very nearly the same as that of the carbonates, the limestone, and dolomite which was below. The shot was in tuff. This instrument hole was offset 1600 ft laterally from surface zero. So you see that by the time a signal gets out here, quite a bit of refraction or reflection may have occurred and

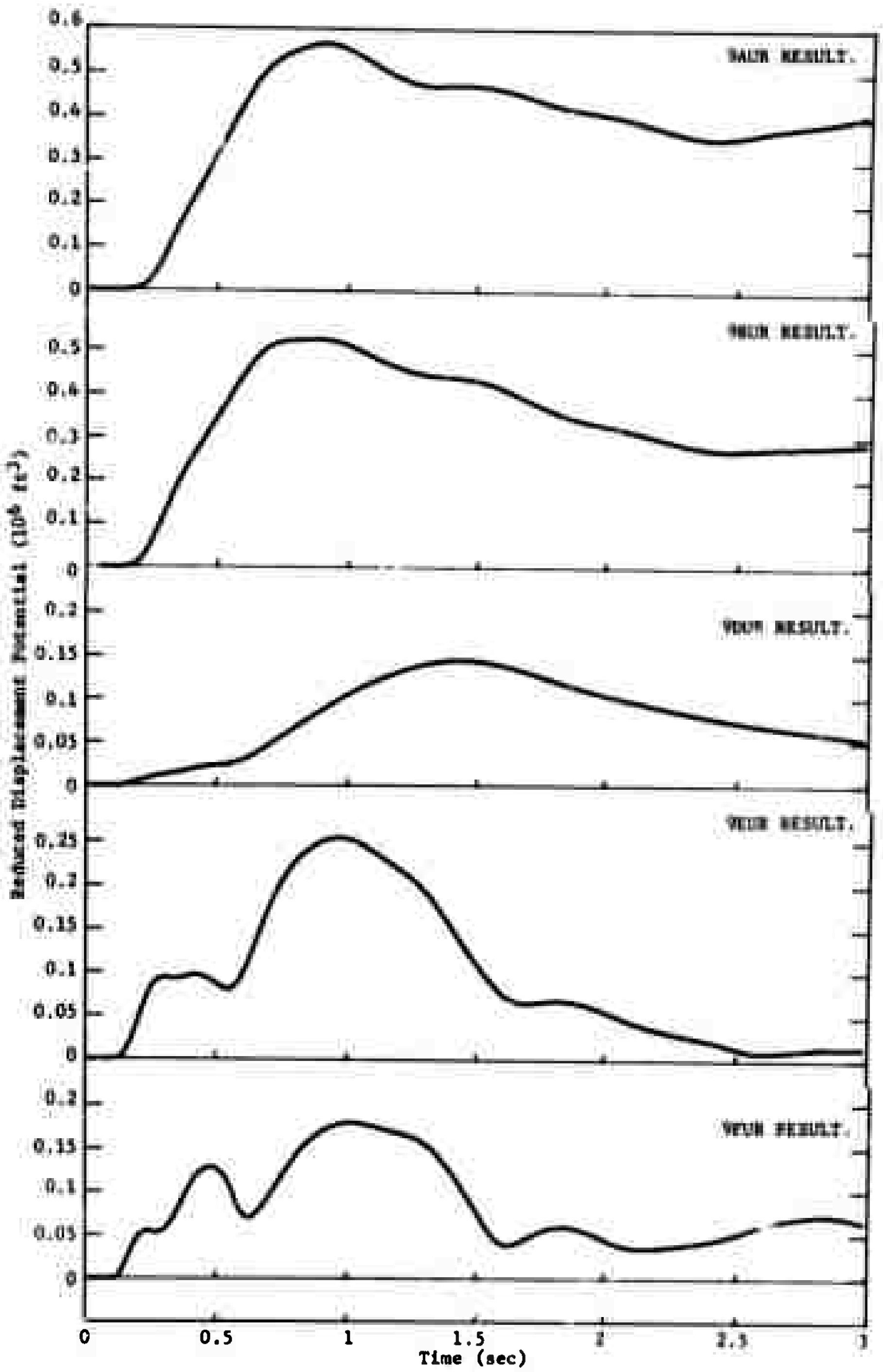


Figure 16. Reduced Displacement Potential, Resultant Data, Boring U8a-9 (Discus Thrower).

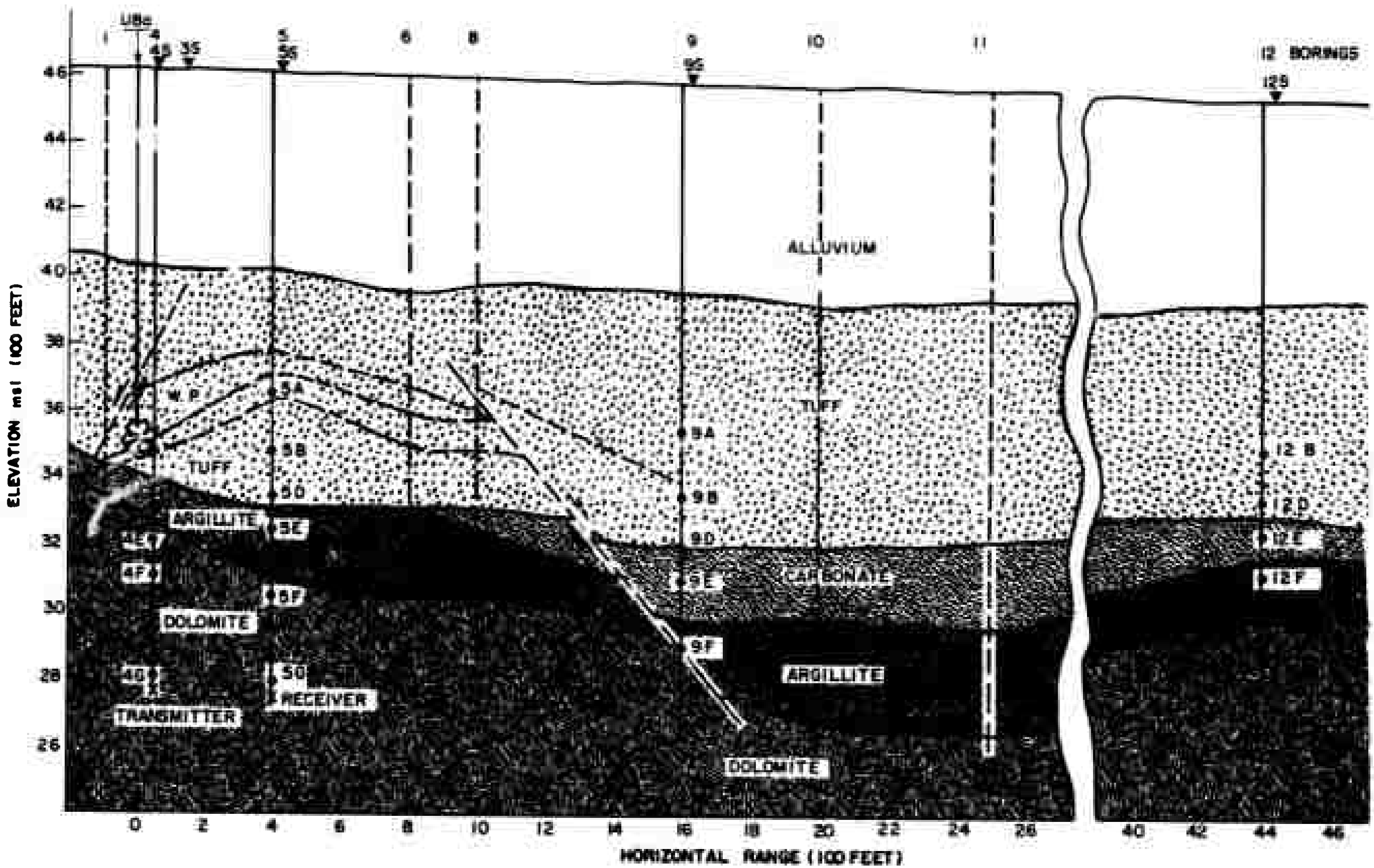


Figure 17. Discus Thrower Site Profile.

disturbed the tail end of these records.

I have here another set of Discus Thrower potential curves from Hole 12 (Figure 18) at about 4400 ft from surface zero. All these reduced displacement potentials are from velocity-gage records, two in tuff, one in carbonate and one in argillite. The peak amplitudes in here are comparable.

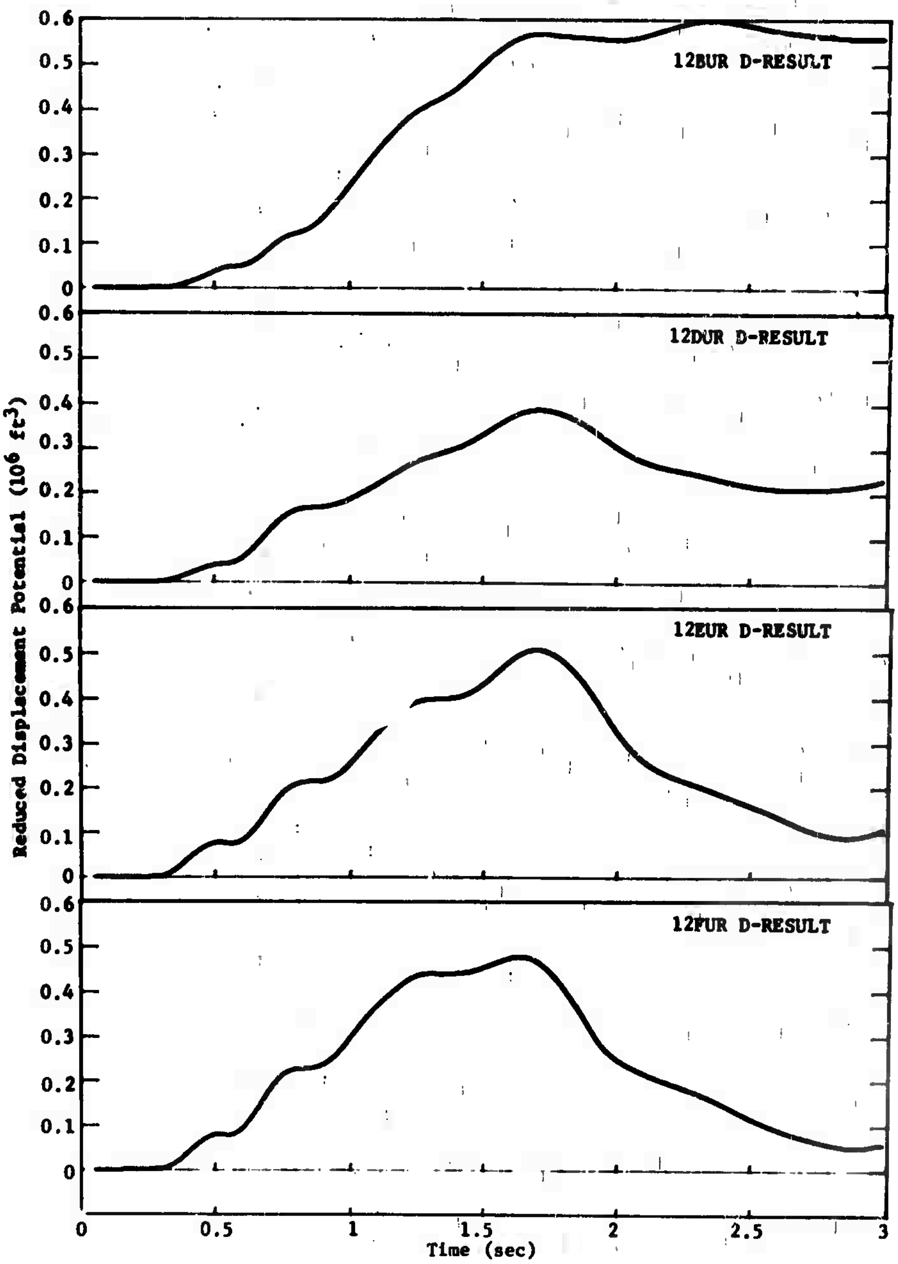


Figure 18. Reduced Displacement Potential, Resultant Data, Boring U8a-12.

MR. CHERRY: Bill, when you go into these various interfaces and calculate the reduced displacement potential from your records, you have to assume an elastic velocity. Do you use the elastic velocity appropriate to that interface?

MR. PERRET: I use the best elastic velocity for the material the section is in. I am not sure how good that is, but I am not quite sure what else you would do.

MR. CHERRY: The reduced displacement potential is not defined for a layered environment.

MR. PERRET: Yes.

MR. BROWN: Yes, what does it mean?

MR. PERRET: This is one of the problems, of course, and this is not a good example of potential data, because it is in a layered medium. It happens to be the one that I had available.

I do have also some data from Gasbuggy, unfortunately this was in a layered environment also. Here are some velocity records from Gasbuggy, Figure 19. You can see they have this characteristic high spike. These also are radial vector records. Velocity gages must be used either in a vertical or a horizontal orientation. Because they are pendulums, for horizontal motion, the pendulum is upright; for vertical motion, the pendulum is supported in the horizontal position by a spring. So for our purpose we have taken the sum of vector components along the radial vector.

These records are the radial vector ones for the Gasbuggy shot which was sited as shown in Figure 20. The Pictured Cliff sandstone contained gas, overlying the Lewis shale, and the shot was 40 ft below the sandstone-shale interface. We had an instrument hole 1500 ft offset from the shot, with stations at 4600 ft in Lewis shale; at 4100 ft in Pictured Cliffs sandstone, and two more at depths of 3600 and 3200 ft. All of this rock above the gas rock was very highly stratified with soft shales, thin and thick coal measures, and hard sandstone. So the fact that the data records in Figure 19 are so clean is remarkable.

Integrations of those velocities to displacement are shown in Figure 21, and the reduced displacement potentials from them in Figure 22.

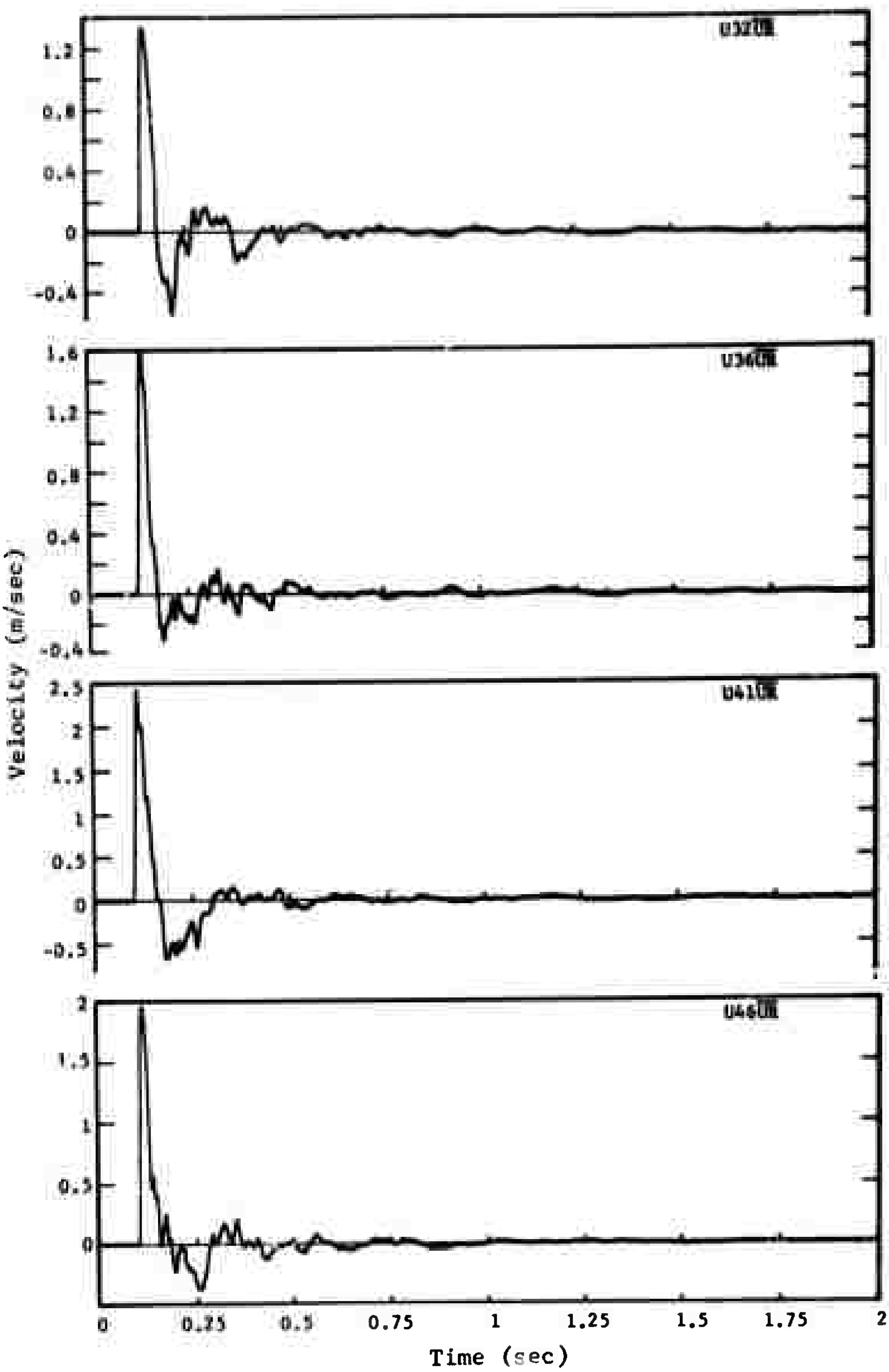
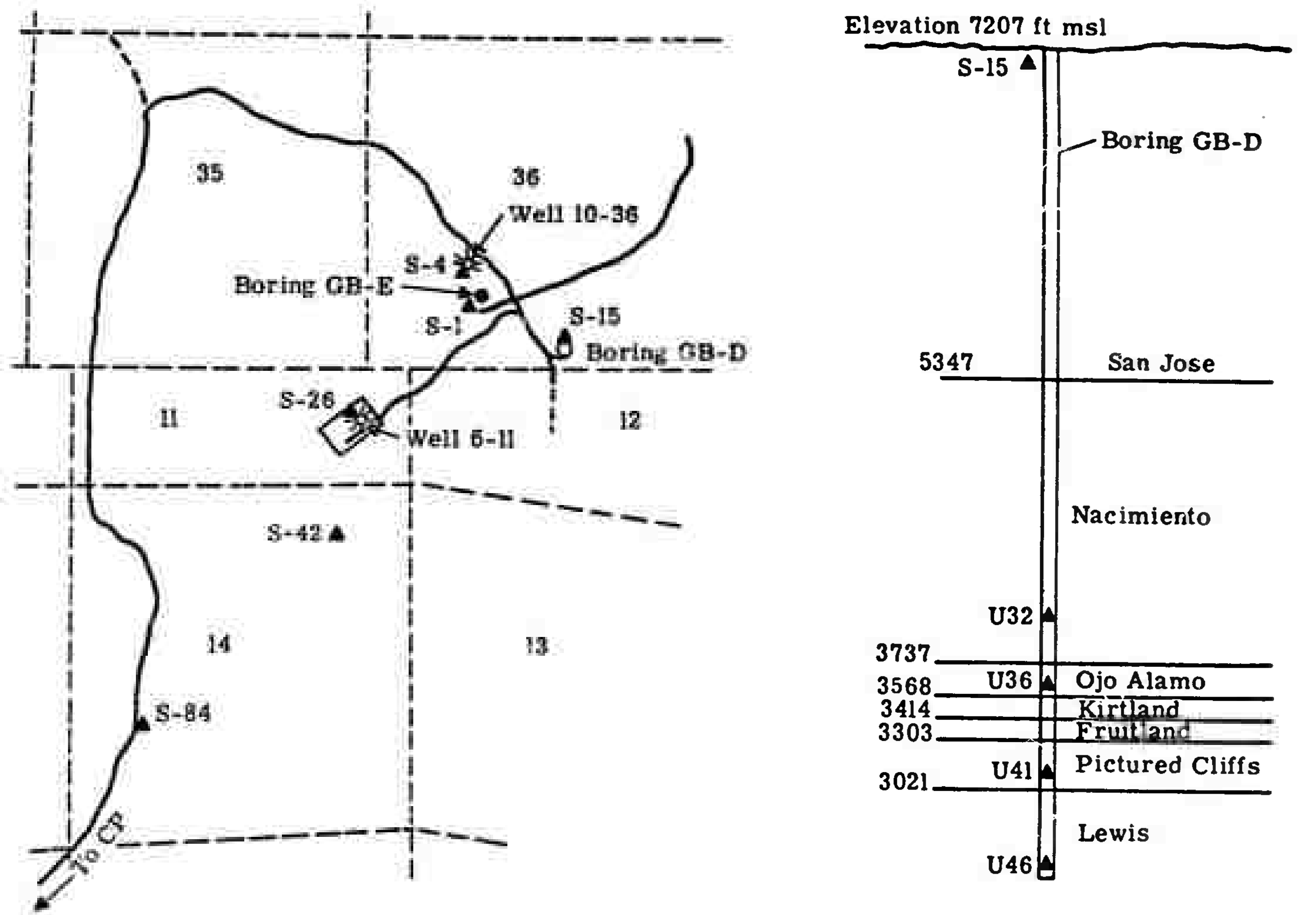


Figure 19. Radial Vector Particle Velocity Records (Gasbuggy).



Site Map

Subsurface Station Locations and Geology

Figure 20. Site Map and Instrument Station Locations (Gasbuggy).

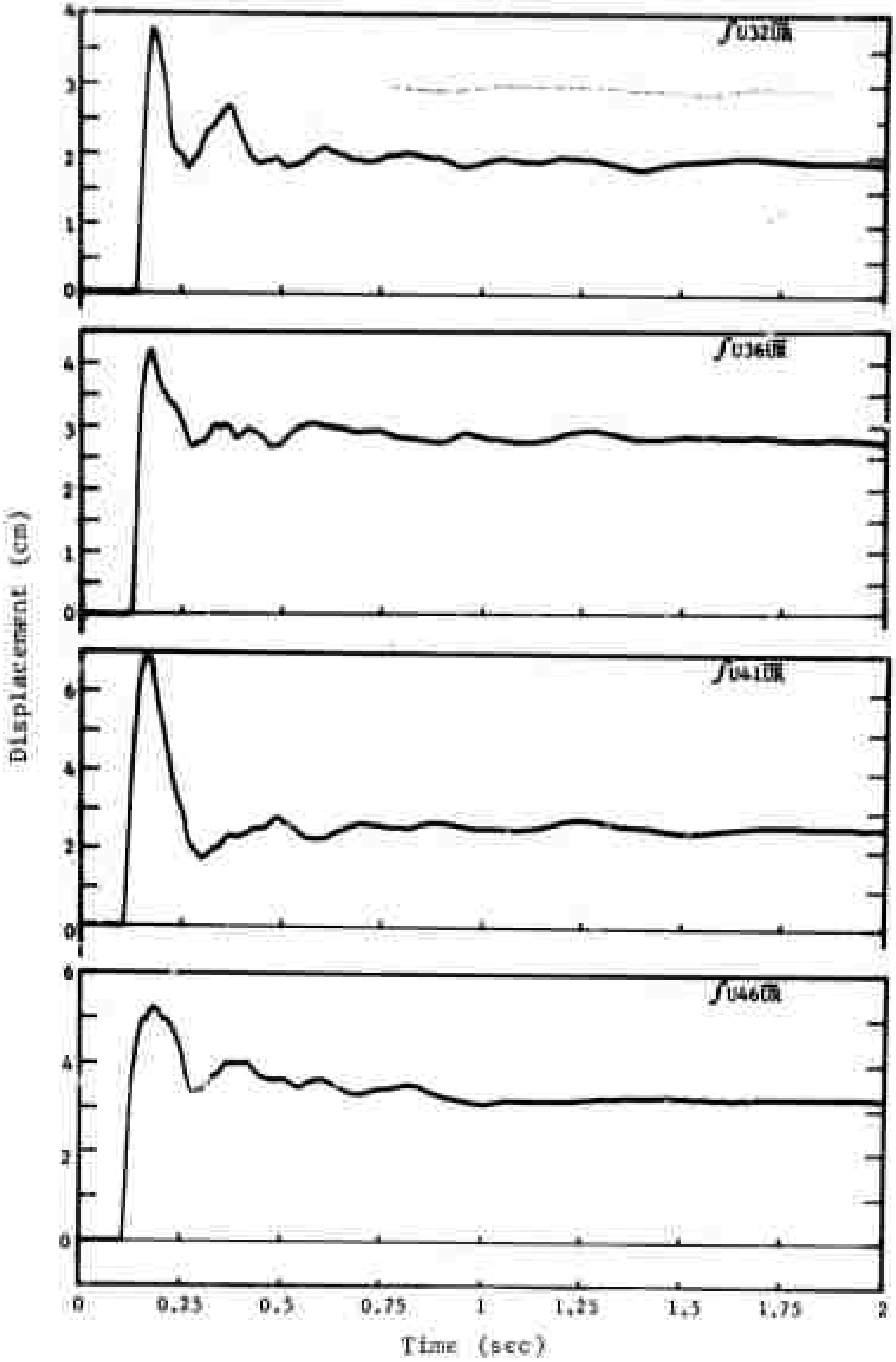


Figure 21. Radial Vector Displacement Records (Gasbuggy).

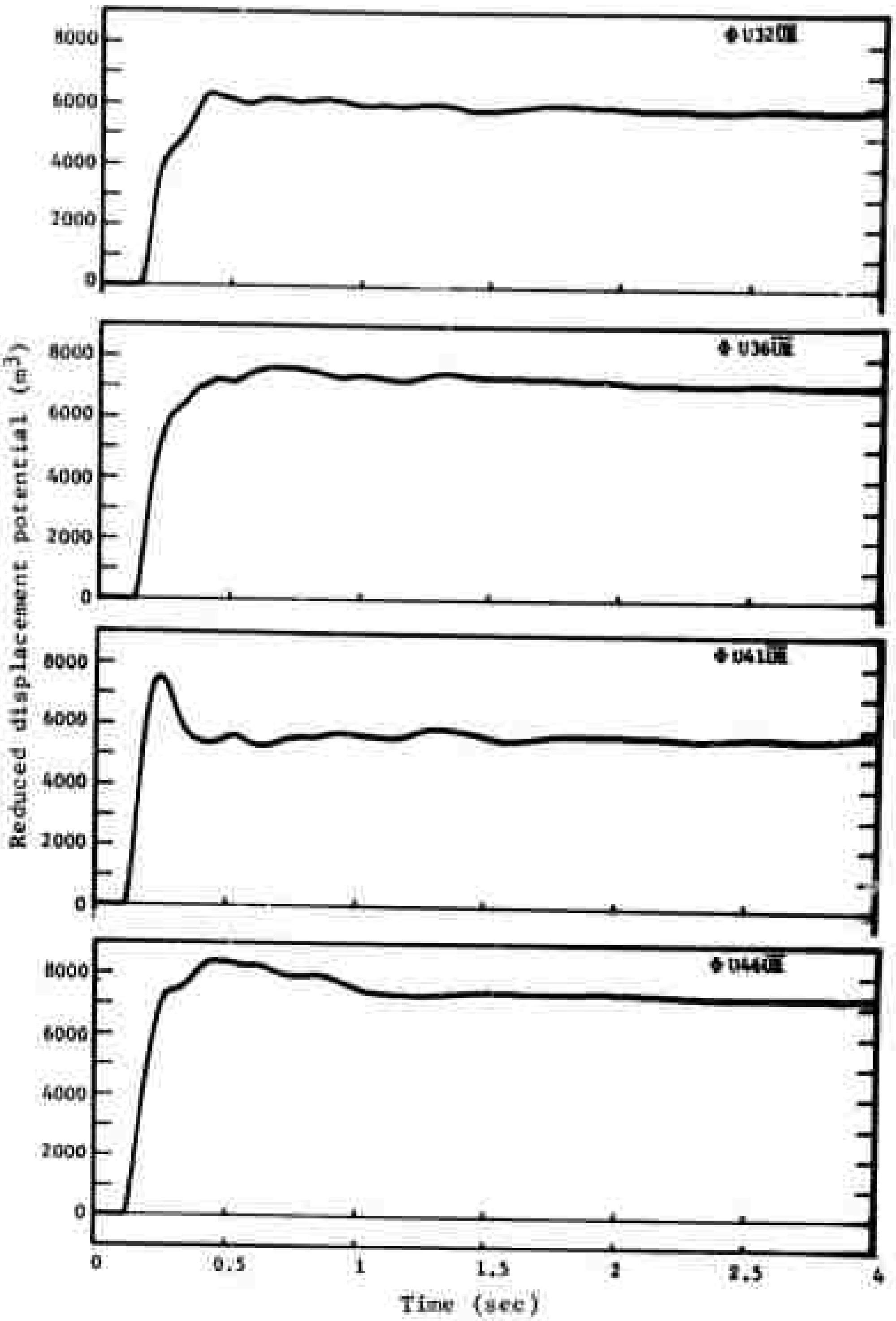


Figure 22. Reduced Displacement Potential Records (Gasbuggy).

MR. ARCHAMBEAU: I still don't quite know what you mean by that reduced displacement potential.

MR. PERRET: Let me get this out of the book. The reduced displacement potential to start with is defined by $\delta(t) = \partial[\phi(t)/r]/\partial r$ where $\delta(t)$ is displacement as a function of time. The digitized displacement is used as input to a computer program that performs the integration

$$\phi(t) = cre^{-ct/r} \int_{t_0}^{t_1} \delta(T)\, e^{cT/r} dT$$

The circuit we used is essentially an integration circuit in which if you make the time constant c/r numerically equal to the RC of an RC-integrating circuit, the output is the reduced displacement potential. It is, however, usually simpler to perform the computer integration.

MR. ALEXANDER: Is the "c" appropriate there, the one right at the receiver, or is that the whole thing along the path?

MR. PERRET: That is the one essentially at the receiver. Generally speaking, this value of c is for essentially all of the material that the signal has traversed because the potentials are most reliable where the travel path from shot to measurement station is within the same material, and such reflections or refractions as may enter the record arrive late enough to add only a few small wiggles near the end of the potential.

This situation was fairly true for Gasbuggy because the impedances of Pictured Cliff sandstones and Lewis shale are nearly equal.

MR. ALEXANDER: You don't get any refractions.

MR. PERRET: You get very small refraction signals in Gasbuggy records.

MR. ROTENBERG: What about the contaminant from dispersion? That is, the wavelengths you are talking about are comparable to the depths of the layers, and therefore I would imagine that the velocity of propagation would be frequency dependent.

MR. PERRET: I think these stations are still too close in to have that bother you much. In other words, in most cases these motions are observed at distances which are at least of the order of magnitude of the distance to any interface. So although dispersion may affect the signals farther out, I doubt if it has much effect on the signals at this distance. There is some dispersion. There obviously is because the rise time of the velocity increases as you go out but not very much within the distances we are talking about.

MR. ALEXANDER: Is that really dispersion or attenuation? You are wiping out high-frequency energy.

MR. PERRET: Well, it can be either wiping it out or spreading it out. I think it is possibly some of both.

These potentials from Gasbuggy (Figure 22) except for that at U41, were derived from velocity records. At Station U41 we lost the horizontal velocity gage preshot and used as displacement the doubly integrated horizontal acceleration. These potentials run around 8,000 m^3, except for the shallowest, which is about 6,000 m^3, but the travel path to it included about 10 or 20 percent of very soft coal, and soft shale strata.

MR. GODFREY: Could someone comment on the fact that Howard and Ted spoke of measured reduced displacement potentials as having characteristically this peak and dropping off to some value like half of that, and that this does not seem to have that?

MR. PERRET: These particular ones don't, except for the U41 potential. I would not say they generally decrease to half peak value, but they drop further than these suggest. This is why I don't have great faith in numbers that seek to describe residual RDP's.

MR. ARCHAMBEAU: All that depends on how far out you go.

MR. PERRET: Yes, it depends on how far out you trust your record. The peaks I think are pretty reliable.

MR. ALEXANDER: Is there any theoretical basis for a peak as opposed to just an asymptotic value?

MR. PERRET: I think the best definition of that is the fact that you have, generally speaking, accelerations which tend to have single positive spikes and smaller negative ones, velocities which tend to have single positive half cycles and a longer negative one, and consequently displacements which tend to peak broadly and reach a residual value with minor oscillations. But if you are at great enough distances, displacements may come down and oscillate about zero.

MR. ARCHAMBEAU: How do you define your elastic zone?

MR. PERRET: I will get to that a little later on.

To have real meaning, the reduced displacement potential must be measured in the elastic zone and there it should be constant.

Evidently from what we have seen there can be displacements which have residual values out in the elastic zone, although theory implies no residuals should occur there.

MR. ARCHAMBEAU: In the elastic zone?

MR. PERRET: Yes, and that is because the measurement is really in a pseudo-elastic zone, characteristic of natural rather than textbook materials.

If you assume an explosively generated spherical cavity in the rock and that no net change in density occurs out to the position where you measured displacement, then this displacement can represent a spherical shell, the volume of which is equal to that of the cavity. This calculation has been checked out on four or five different shots and is within 10 or 20 percent of the volume measured by gas-pressure methods and by drilling.

MR. ALEXANDER: That would be the maximum you could ever hope to observe, is that right?

MR. PERRET: This is essentially the maximum, yes. Following the Salmon shot a 17.4-m radius was measured and calculation gave about 21 m. Bill Wells at LRL calculated how much one would expect this kind of cavity to shrink because of the plasticity of the salt. The result was within 10 percent of the measured value.

So how much you can trust a residual displacement depends partly on how much doctoring (zero correction) you have done to your data, and how far from the source a measure was made.

MR. CHERRY: What does the reduced displacement potential look like on Salmon? Did Salmon have the peak in it?

MR. PERRET: I believe it did. Salmon also was an experiment where I doubt that we ever got to elastic response. Let me talk about elasticity

MR. BROWN: Before you go into that, did you ever try to take into account bulking effects when you make these kinds of calculations, looking at the volume of the crater, and then the final displacement?

MR. PERRET: No, we have not, and part of the reason we have not done this is because where they have mined back into cavities, they have usually found only very localized compaction of the rock. I believe that is true, isn't it, Ted? I am thinking of things like the Rainier and Hardhat reentries.

MR. CHERRY: I think there was a real density change on Hardhat when they went back and looked at the rock post shot.

MR. GODFREY: By bulking effect, you mean the opposite of compaction, don't you, and it will occupy more space at the same residual pressure.

MR. PERRET: You get that when you have a chimney, I know; as much as anything because you have already pushed the ground up. However, net change between the cavity and gage station averages out local bulking and compaction near the cavity.

MR. BROWN: You can take a nice little intact specimen and go over it carefully under controlled conditions and find this bulking.

MR. PERRET: I am sure they did this with Rainier.

MR. RINEY: I wonder if in the LRL calculations they used Stevens crush-up data for tuff. There you would expect to have some permanent compaction. In the code calculations did you predict that the volume displacement out in the elastic region could be accounted for there by the cavity? Did you look at that?

MR. CHERRY: I don't think we have looked at tuff, but we have looked at alluvium, and that is a locking solid.

MR. RINEY: The volumes are accounted for for that, so that they do recover?

MR. CHERRY: We match the reduced displacement with the locking solid model.

MR. TRULIO: But the volume of that cavity is not equal to the volume swept out by a shell of alluvium that experiences only elastic deformation as it moves.

MR. RINEY: Isn't that what I understood you to say?

MR. CHERRY: Not for alluvium.

MR. RINEY: No, it should not be, but I understood that every time they go in and look in the tuff and alluvium and so forth, that is what is seen.

MR. PERRET: It was on the Merlin shot.

MR. RINEY: I am just repeating what I heard.

MR. BROWN: It seems strange you would need to do it for hard rock.

MR. RINEY: I would not expect it from a code calculation modeling, but apparently that is what they are saying they observed.

MR. PERRET: Another thing we have done with these data recently is to try to get some measure of how much energy gets out into the elastic region. To do this, we derive the energy flux at the position of the gage from the equation,

$$E_f = \rho c \int_0^{t_1} u(t)^2\, dt.$$

This procedure has assumed elastic response because of the assumption that the kinetic energy and the potential energy are equal. The data include all of the recorded motion. This flux multiplied by $4\pi R^2$ where R is the distance from source to gage is a measure of the amount of energy that passes through that spherical shell.

MR. JUDD: Bill, where do you get the values for ρ?

MR. PERRET: Either from core samples or from 3-D logs. We take the average ρ as measured by the log in the vicinity of the gage; again we are concerned with the value of ρ and c at the gage because this is where the energy flux is measured. The total energy which traverses this spherical shell divided by the energy yield of the explosion is an index of how much of the source energy has reached the observation station. When this energy becomes constant with distance, the elastic response region has been reached. I think this is the best definition of the elastic region.

On the Salmon event in a salt dome, gages at shot level were 166, 320, 620, and 740 m from the explosion. Energy at these stations divided by the yield gave us 25, 11, 5.6, and 3 percent at the respective stations. This suggests that if response were elastic at the most remote station there is no evidence to verify it, but you may be certain that the rest of the stations were not within the elastic region.

MR. SMITH: Of what frequency would you be talking principally in there?

MR. PERRET: I think it is around 2 or 3 cps, something like this.

MR. SMITH: So attenuation is going to be negligible over these short distances.

MR. PERRET: I think so, yes. The edge of the salt dome was at roughly twice this distance in the direction of gage line; in the other directions it was still farther away.

MR. ALEXANDER: If I remember correctly, some of the data presented earlier had the same kind of energy ratio calculated. It was around 0.2 percent.

MR. PERRET: That is correct. I will discuss that shortly. I have derived this ratio for four or five shots. This list that I show here, Table 3, is from a report that is currently being reviewed before publication. Incidentally, Figure 23 shows the kind of curve that you get for this integral of u^2. The very slight slope at the top of the curve is a measure of the system noise.

Table 3. Energy Ratios for Explosions in Various Rocks

Event	Environment				Energy ratio (%)
	Rock Type	Porosity (%)	Seismic impedance (gm/cm^2-sec)	Depth (ft)	
Merlin	Dry alluvium	30	3.23×10^5	980	0.10
Discus Thrower	Dry tuff	20	3.99×10^5	1106	0.25
Mudpack	Dry tuff	20	4.14×10^5	507	0.12
Handcar	Dolomite	11	1.52×10^6	1320	2.01
Gasbuggy	Shale	6	1.05×10^6	4240	1.77

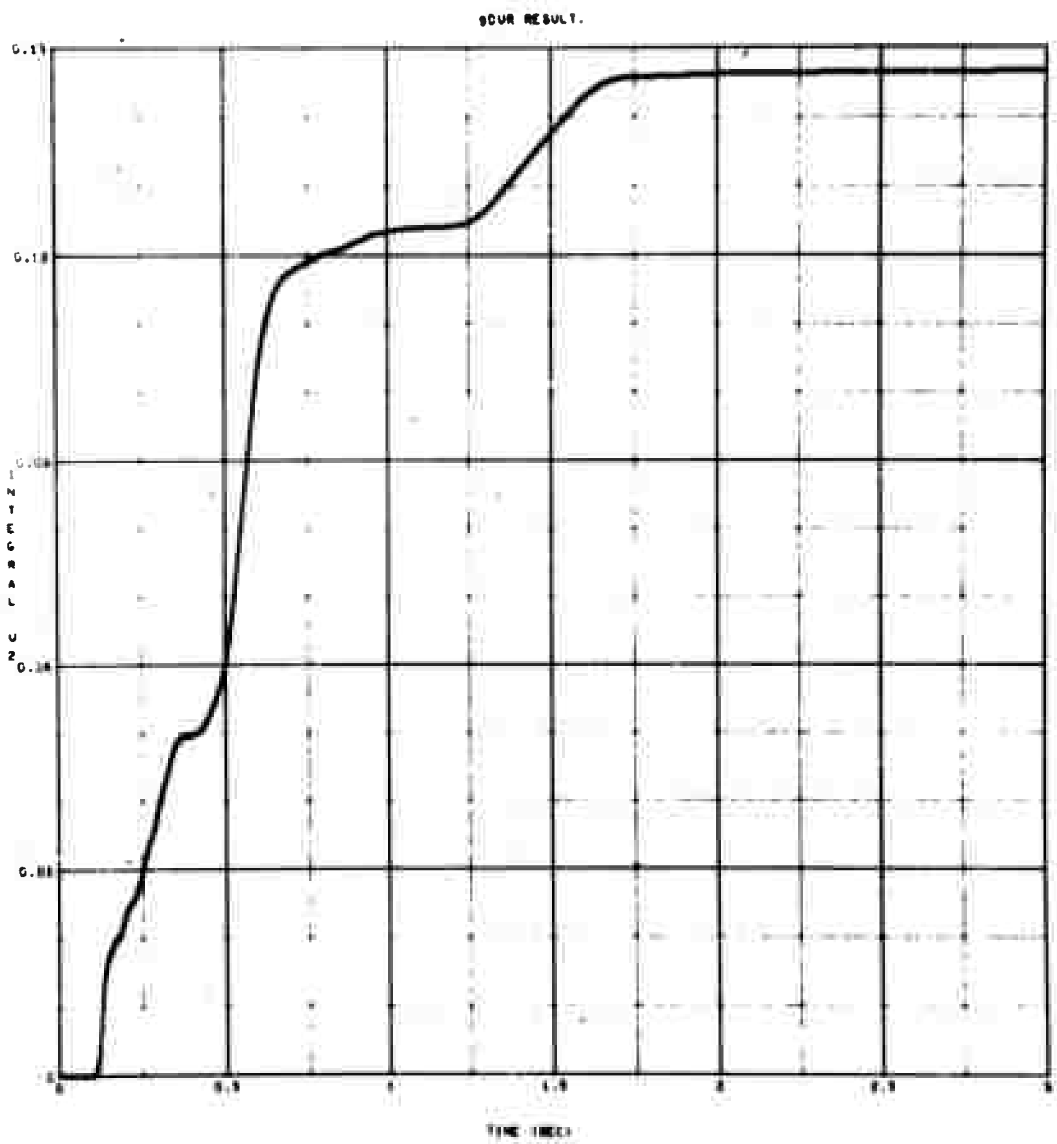


Figure 23. Discus Thrower $\int u^2\,dt$.

MR. PERRET: The total time for this record is 3 sec. The integral has nearly reached its maximum in 1 sec. This curve happens to be for Discus Thrower.

Now, let me discuss the values of this ratio we found at different Discus Thrower stations. For the five stations at various depths in tuff and alluvium 1600 ft from Discus Thrower, the ratios are 0.30, 0.32, 0.23, 0.20, 0.14. The last two were in the underlying dolomite. The third one was at the tuff-carbonate interface, and the first and second in tuff.

Now, if we go to the stations out at 4400 ft, the two in tuff gave ratios of 0.19 and 0.14, and the two in dolomite 0.29 and 0.45. The record from which the last ratio was derived included a lot of trash, probably from late reflected arrivals. I am inclined to throw that one out because of the influence of these late phases.

For shots in different types of rock, the mean energy ratios show for Merlin in alluvium 0.1 percent; for Discus Thrower, in dry tuff, it was 0.25 percent; for Mudpack, in dry tuff and very much the same geometry and geology as Discus Thrower, but about one tenth of the yield, the ratio was 0.12 percent. Handcar, which was within a thousand feet of Mudpack, but was detonated in dolomite, the ratio was 2.01 percent and for Gasbuggy, deep in shale, the ratio was 1.77 percent. Thus, in a hard rock, coupling of energy is roughly ten times better than in tuff, and twenty times greater than in alluvium.

MR. ALEXANDER: This is all independent of frequency, is that right?

MR. PERRET: That is right. It is from the whole record.

MR. ALEXANDER: As far as the seismological record, what would that percentage be at around 1 Hz?

MR. PERRET: You see, you are faced with a record in which perhaps 95 percent of the energy arrives within 1 sec. These records are squared and integrated and beyond that first second there is essentially no signal.

MR. ALEXANDER: Suppose you just band-pass filter that so you reject everything higher than, say, 1.5 cps and then do the same calculation?

MR. PERRET: Then you would have nearly nothing.

MR. ALEXANDER: But you see something at large distances at those frequencies.

MR. PERRET: Here again is the problem of the difference between close-in measurements and teleseismic measurements. In order to see these peaks in the close-in measurements the longer period signal is forced into the noise.

MR. SMITH: How much dynamic range would you need to recover one cycle energy?

MR. PERRET: Oh, probably an increase of a thousand. We have some systems with dynamic range of the order of 1000, but the problem is really in signal-to-noise ratio which may be improved but probably not enough by increased dynamic range.

MR. SMITH: I am talking about the amplitude for a voltage dynamic range. A thousand to one would be required.

MR. PERRET: I suspect so.

MR. SMITH: That is only 60 db, and that is attainable.

MR. TRULIO: Maybe it is not right to just re-analyze that close-in record. Don't you have dissipation all the way out in what is supposed to be the elastic regime?

MR. ALEXANDER: Oh, yes, but it is going to be a lot less for one cycle.

MR. TRULIO: I know, but I mean dissipation spreads the pulse and changes the wave shape.

MR. PERRET: Oh, it does, yes.

MR. SMITH: But it is minor.

MR. TRULIO: Even over the distances of travel that interest you?

MR. PERRET: That is not necessarily true this close in because a lot of spreading occurs in these close-in ranges.

MR. ALEXANDER: We are talking about the elastic zone.

MR. PERRET: Yes.

MR. TOKSOZ: Could I just clarify the dispersion. If we believe the medium is behaving somewhat linearly, it does not transfer the energy from high frequencies to low frequencies, but what it will do is that it will eliminate the high frequency.

MR. PERRET: That is right. The high frequencies are eliminated.

MR. ARCHAMBEAU: It will cause a slight amount of dispersion, but it is minor.

MR. RINEY: We are not concerned about the dispersion. It is just the amplitude.

MR. TOKSOZ: But if the medium behaves nonlinearly in these ranges, then it is possible to get some energy in, but I am sure it would be very small.

MR. CHERRY: Does anybody have the capability in the seismological field to take a given source function plus a set of layering and make synthetic Rayleigh-wave seismograms.

MR. HARKRIDER: Yes. We use transparent sources, in which the reflected waves don't see the source. We start out with the whole-space solution for a cavity. Sometimes we also take observed displacements at some distance, essentially in the linear zone, and we use that to calibrate our outgoing wave. The reason we call them point sources is because they are transparent. Reflected energy does not bounce off them.

MR. CHERRY: What sort of source description do you require?

MR. ARCHAMBEAU: What we are sitting here waiting for is a description of what the pressure pulse looks like in the elastic zone.

MR. CHERRY: So you want the pressure pulse.

MR. PERRET: The velocity pulse should be the same.

MR. CHERRY: The trouble I have is, what is the appropriate source function for a layered source geometry? It seems for some of these shots Bill could give you almost anything you want, depending on what layer he chooses to look in.

MR. ARCHAMBEAU: Yes.

MR. GODFREY: Aren't we still in the dilemma that we have no way of measuring the one cycle?

MR. ARCHAMBEAU: You have to get signal definition out to longer periods.

MR. HARKRIDER: It might not look all that different from one layer to the next.

MR. PERRET: These gages and the recording system have a capability of responding to signals down to about a tenth of a cycle. But unless the initial signal is clipped and the system can recover in time, then the gage sensitivity can probably not be increased sufficiently to differentiate late, low-frequency signals from noise.

We have never tried this because we have been concerned not with the seismological problem but with the gross close-in motion. I am quite sure one can do this sort of thing.

MR. ALEXANDER: What you predict here is something like one magnitude unit by these figures. We would expect one magnitude unit difference from one type of medium to the next.

MR. PERRET: Yes.

MR. ALEXANDER: And we don't see that, I don't think. There is certainly not one magnitude unit difference in the magnitudes when you plot them versus the medium.

MR. PERRET: This method was used first on the interpretation of the close-in Sterling data, and we arrived at a decoupling factor of the order of 90 or 100 compared with the Sterling yield. In other words, we had a reduction to 0.02 percent, and this is roughly the factor found from seismic records at stations about 100 km from Sterling.

MR. ARCHAMBEAU: Those figures you are listing, you are sure you are in the elastic zone? Those things could keep on going down.

MR. PERRET: Yes. Here I quote the percentages found for the four Gasbuggy stations, 1.52, 1.79, 1.99 plus a value of 3.56 percent from the station which was essentially at shot level. This latter value was derived from an integrated acceleration, all others were from velocity-gage records. I don't have as much faith in this last one as I have in the other measurements. The mean of the first three data is 1.77 percent.

MR. ARCHAMBEAU: That average then is the last one out that you measured.

MR. PERRET: That is the only place we have any instruments.

MR. ARCHAMBEAU: But you did not go to greater distances, so you didn't know how they changed with distance.

MR. PERRET: The span of ranges here is not very great, from 556 to 468 m. There is maybe a slight indication that the ratio decreased with range, but again unfortunately these are the only stations we had for Gasbuggy. For Discus Thrower, however, the ratios are derived from data at ranges between 1600 and 4400 ft. The average ratio is derived over this span of ranges and those from 4400-ft stations were a little greater than from the closer stations.

MR. LEWIS: Are all of the data that you are looking at here at shot depth?

MR. PERRET: No. This from Station U41, Gasbuggy, is the only one near shot depth. It is about 100 ft above shot depth but at 1500 ft horizontal distance. From Discus Thrower one was something like 200 to 300 ft below shot level and another about 400 ft above it. But in all cases data used were those derived as radial vector records.

MR. LEWIS: These shots, though, varied in depth over quite a range.

MR. PERRET: Oh, yes. Gasbuggy was 4200-ft deep; Discus Thrower about 1300 ft, Mudpack about 500 ft, and Merlin, 1,000-ft deep.

We have done the same kind of thing for about half a dozen other shots, but have not yet completed analysis.

MR. ALEXANDER: Do you get figures like this?

MR. PERRET: Roughly, yes. That list that I read first were the ones in alluvium, tuff, granite, salt, rhyolite, and sedimentaries. There were 21 shots for which we had data we can do this with, but in some cases there are only one or two stations. From Boxcar data, for instance, we could possibly find ratios at two stations, at 8,000 ft and the other is 24,000 ft from the explosion. I say we can do this, meaning that we do have velocity records; the results might not permit conclusive interpretation.

MR. ARCHAMBEAU: Actually that discrepancy in numbers is not as bad as it might appear at first. The spectrum for the harder materials is peaked at a shorter frequency, so that when we look at them, we would not see this kind of difference.

MR. ALEXANDER: Not only that, but you have the attenuation.

MR. ARCHAMBEAU: Yes, in addition, the attenuation, so that as far as we are concerned, there is no real discrepancy here.

MR. ALEXANDER: We just can't tell because we are looking at different parts of the spectrum. He is looking at high frequency and we are looking at low.

MR. ARCHAMBEAU: Well, he is sort of on the edge of where we are looking, near one cycle, so that he is integrating up to one cycle energy. But for those hard materials you have a peak in the spectrum way out in the high frequencies, and that is where most of the energy is, so it does not matter what that efficiency is.

MR. PERRET: Records of the integrals of u^2 versus time generally show that the system noise begins to dominate in the neighborhood of 1.5 sec. This same slope projected from zero represents noise energy of the order of 1 percent of the total energy in the record. Thus, any of the data beyond this time or the start of the noise dominance will be negligible compared to noise energy.

MR. RODEAN: But this is based, though, on data for shots like Discus

Thrower, which is about 20 kt, and Gasbuggy is about 26 kt. If you jump from there to about a megaton, then you would have to multiply the time scale by the ratio of the cube root of the yields.

MR. PERRET: It will stretch out, because the records stretch out.

MR. RODEAN: There is one problem with the Boxcar data. I have looked at the geometry. You would want to look at those data for perhaps several seconds to get the complete signal, but after only a fraction of a second you also get the surface reflection coming in on top of the direct signal.

MR. PERRET: At those distances you must begin to get such extraneous signals. At 4400 ft from Discus Thrower, we ought to see some of them, and we did have a high value of nearly 4 percent for the deepest station at that distance. I think this gives a reasonable measure of how much of the energy from the explosion actually gets out into the elastic region, if the data derive from near the elastic region.

MR. ARCHAMBEAU: You are looking at the whole record now. You are making no attempt to try to subtract out reflections when you know what the geometry is.

MR. PERRET: No.

MR. ARCHAMBEAU: You could perhaps include these numbers.

MR. PERRET: You might be able to, yes, except that generally the reflections are relatively small.

MR. ARCHAMBEAU: If you are attributing to reflections the kinds of anomalies you are talking about, for example, you had one where you said 4 percent, you are talking about pretty small things.

MR. PERRET: That is right.

MR. ALEXANDER: One test of that is the true radial. Do you have three components that you resolve?

MR. PERRET: Yes. These are all for slant-range radial records.

MR. ALEXANDER: Then you should be able to just maximize that output and get the direction.

MR. ARCHAMBEAU: Get the direction of the arrival as a function of time.

MR CHERRY: You did not always have that, Bill. I don't think you had that on Hardhat.

MR. PERRET: No, because Hardhat gages were at shot level, so it did not matter.

MR. CHERRY: They are worried about the reflection from the surface.

MR. PERRET: Oh, yes, but there again I don't think such reflections were a problem. The Hardhat gages were at ranges something like 700 ft or less, and the surface was 900 ft above them. I think most of the signal had gone by before reflections from the surface arrived. Perturbations in these records were more likely from the tunnel floor, only 100 ft above the gages. Although the tunnel is not very big compared to the wavelength, we might have gotten some perturbations from it.

MR. TRULIO: In hard rock media spherical symmetry is lost pretty early anyway.

MR. PERRET: Most of the teleseismic signal is very different from the signal that we record close in. The teleseismic signal derives from that propagated downward within a small cone and has generally been refracted at the Moho or deeper. Hopefully there was not much difference initially between that signal and the one observed by our gages close in, near or above shot level. Concerning that component of close-in, free-field radial records that comes from surface reflection in the direction of gage response, it must be relatively small for stations near shot level except perhaps for the shear wave.

MR. ARCHAMBEAU: Actually that is a minor correction.

MR. PERRET: Yes.

MR. SMITH: Do you have any idea of how much variation there is in azimuth around the source, how much asymmetry?

MR. PERRET: We have not found anybody with enough money to let us observe that factor. These are expensive measurements because a 1000-ft hole is expensive. Such data were taken on Shoal where three holes at more or less 120 degrees were instrumented at ranges of about 1950 ft. As I recall, asymmetry was of the order of a factor of two. I believe the low value was on the far side of a fault zone 10 ft or more thick and full of clay gouge. So it would be very surprising if asymmetries like this were not fairly common. Similar measurements and results were obtained from Piledriver, POR 4000.

MR. ALEXANDER: How about the wave shape, though?

MR. PERRET: I can't answer that. I didn't take the data and I have not looked at it recently enough to really remember it.

MR. RODEAN: If I remember correctly, that is in a report by Wendell Weart.

MR. PERRET: Yes, Wendell Weart has a report on that. It is a Vela Uniform report, I think, VUF-2001.

MR. LEWIS: We have some data on this asymmetry from nonnuclear shots.

MR. PERRET: Yes, I think we have some also from Piledriver.

MR. LEWIS: From nonnuclear shots we have looked at near the surface, there has been a factor of two in amplitude, if the wave was propagating with the grain of the rock versus perpendicular to the grain of the rock. This is in hard rock.

MR. PERRET: Generally speaking, though, we have not been able to make such measurements because, as I said, these instrument holes cost as much as $100,000 to $200,000 anyhow.

MR. PERRET: You also get similar variations on the surface. Surface measurements at distances equal to shot depth in different directions may give differences of at least a factor of two.

MAJOR CIRCEO: It is my understanding that as you increase the scale depth of burial down to where the Gasbuggy shot was, there is a change in the frequency that you get. In other words, you get an increase in the amplitude of the high-frequency signal. This was presented at the Plowshare symposium, and I was wondering if this was taken into account in looking at these percentages, whereas in Gasbuggy it was at 1,000 ft instead of 4,000 ft that we would get a drastically changed percentage.

MR. PERRET: You are talking about seismic data, I think. I don't think we see it in free-field observations.

MAJOR CIRCEO: I am not sure.

MR. RODEAN: I think, if you are talking abou Mueller from ERC, that it is based on the total seismic-motion records measured some kilometers away. This work is concerned with the prediction of seismic damage. He has come up with a theory which says that you do get this spectral change as a function of shot depth.

MAJOR CIRCEO: But looking at the records of what was predicted, what we would expect at NTS as compared to what we got at Gasbuggy, there was a significant difference in the high versus low frequencies, and it would seem to me that this would affect this.

MR. PERRET: But it is not in the free-field records.

MAJOR CIRCEO: Oh, it is not?

MR. RODEAN: As I pointed out in my January 1970, Las Vegas, paper, there is one other theoretical paper by Fuehs that (at least as I read it) seems to say the opposite of Mueller as far as the effects of depth on the spectra are concerned. What Bill here is talking about is the free field, assuming that the shot is in the middle of an infinite medium, and what Mueller and others are talking about is an explosion in a half-space. Eventually, in the real world, seismic waves hit either layers or the free surface so there is wave conversion. This introduces notches and whatnot into the spectrum that you observe some distance away.

MR. PERRET: I don't think that affects these free-field records. I don't recall any notable difference in the frequencies. Gasbuggy and Discus Thrower ought to be good for comparison because Discus Thrower was about the same size as Gasbuggy, but about one fourth the depth. If there is such a difference, it is much less than 50 percent in periods, and probably less than 10 percent. It could be there; I have not looked for it. But if there, it is small.

MR. COOPER: One more comment on the question about data variation or scatter in different directions. It has been our experience that velocity and displacement data scatter in rock (even in a single given direction) is considerable, easily a factor of two. This scatter is probably due to local cracks, joints, and in-situ inhomogeneities.

MR. PERRET: Yes, you have a lot of factors that affect these data, such as the fact that the rock not only is not uniform in itself, but it may have tilted strata. It may include fault zones. It has preferably oriented joint systems, and when you get into materials like alluvium, there are caliche layers or lenses at depths at least as great as a few hundred feet. All of these things can give strong changes in local rock properties that you hope have been ironed out by assuming some kind of mean value for ρ and c locally.

MR. FRASIER: The thing is that seismic signals are extremely frequency sensitive to source and receiver environments. It turns out that if you look at seismic data for high frequencies, above $\sim$ 2 Hz, there is tremendous variation from site to site for a given event, but if you low-pass filter the data, much of this variation disappears. So a crucial objective is to try and predict what seismic signals in the pass band 0.5 to 2.0 Hz get out into the elastic zone.

MR. ARCHAMBEAU: It would be very useful to us to look at the spectrum, because that is how we are thinking. We have to do the Fourier analysis in our heads while we are looking at the time-domain data you've been showing us.

MR. CHERRY: We have kind of a different point of view, I think, or at least I do. I would like to be able to design an experiment that looks like an earthquake, design a shot that looks like an earthquake, and until I know how you people discriminate, I don't think I am going to be able to do that.

MR. PERRET: I think if we ever get an event like the Rulison shot was supposed to have been, with one shot above another, you may be able to build some strong shear waves.

MR. CHERRY: That is right. I think you can develop a mach stem between two sources that radiates a great deal of shear energy.

MR. ARCHAMBEAU: You already get shear energy, of course.

MR. PERRET: Not from the shot itself.

MR. CHERRY: I don't want to get converted shear energy.

MR. ARCHAMBEAU: That is not what I was showing you there.

MR. RODEAN: In this connection we should consider the paper by Molnar, Savino, et al.* They reported the results of these new long-period measurements. They mention one event, the double Blenton-Thistle event--I don't know the configuration, but I understand they were in separate holes some distance apart, which looked more like an earthquake than any of the other explosions as far as both the M_s-m_b criteria and the spectral ratio within the long-period spectra were concerned.

MR. ROTENBERG: What about the phase?

MR. RODEAN: I am just reporting what they said.

MR. HARKRIDER: Did they set them off at the same time?

MR. RODEAN: They were simultaneous.

MR. PERRET: The only place where we have identified shear waves in close-in, free-field measurements was from the Sterling shot, Figure 24. There we had very distinct shear phases in all of the stations at shot level and in some of the gages above and below shot level. At shot level the shear phase was definitely polarized vertically. There it was not legible in the horizontal gage record. In the records from stations above and below shot level a shear phase showed up in both horizontal and vertical records. Of course, there was built-in asymmetry in the source, because there was a recrystalized lens of salt in the bottom of an almost spherical cavity with the shot positioned at the center of the spherical cavity; i.e., the shot was closer to the floor than it was to the roof by about 7 m. In addition, all of the shock-developed cracks in the floor of the cavity were filled with melt and recrystalized, and all of those in the ceiling were

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* Small Earthquakes and Explosions in Western North America Recorded by New High Gain, Long Period Seismographs, *Nature (London)*, v. 224, p. 1268-1273 (27 Dec 1969).

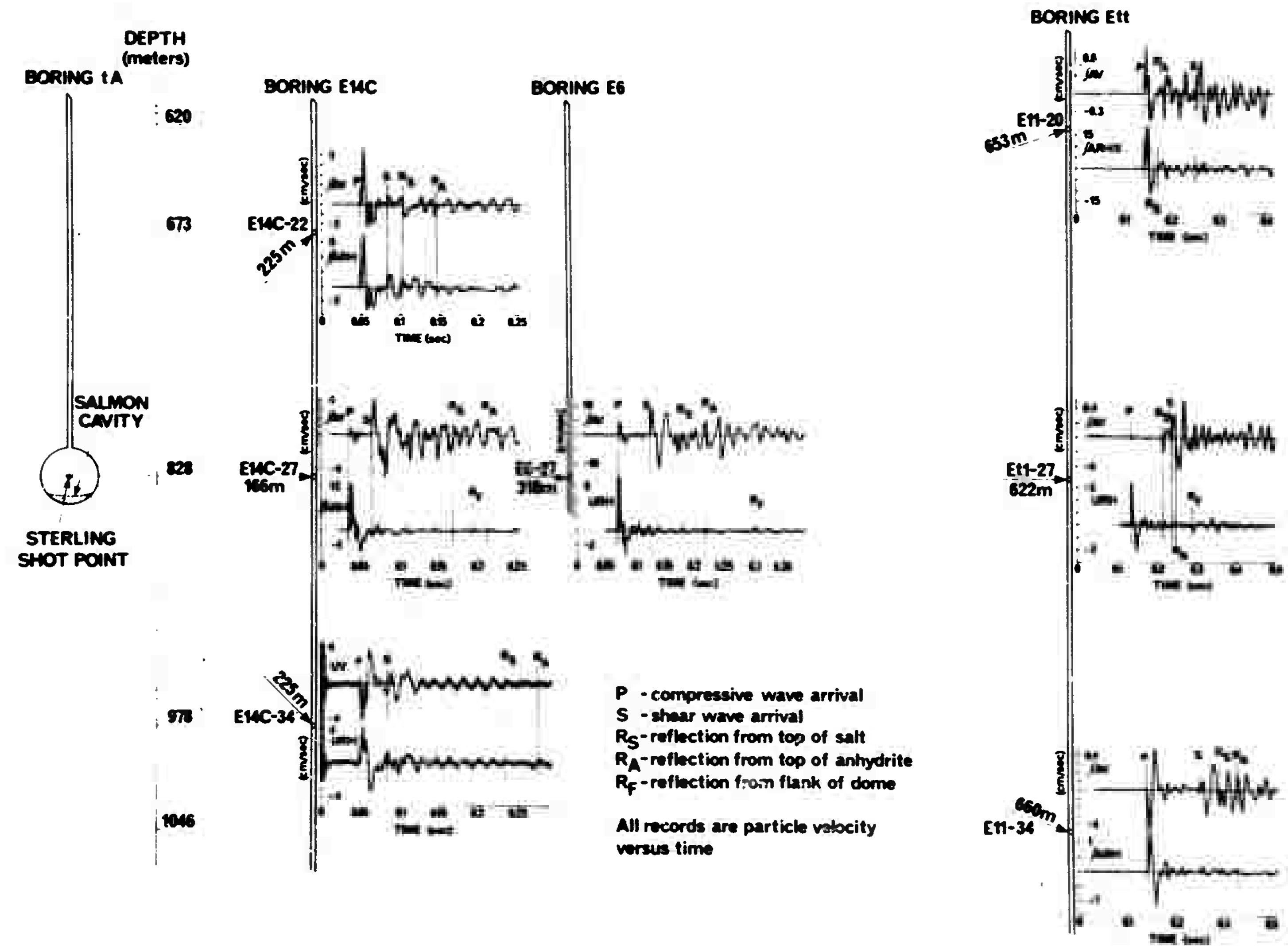


Figure 24. Compressive and Shear Wave Records in Sterling Experiment Profile.

still open, so there was a distinct vertical asymmetry in the environment of the Sterling shot.

MR. ALEXANDER: I think in regard to that question, spoofing is a whole process in itself. Presumably, with enough fiddling around with the geometry, you can spoof many of these criteria for this. The one you can't spoof would be the SH component of energy, the horizontal polarized wave. It is not produced by any of the shots to begin with, and setting off a lot of them is not going to produce SH energy.

MR. CHERRY: Is that what you are using for discrimination?

MR. ALEXANDER: We are using an array of different criteria. M_S-m_b is one. That one you can spoof. You also have the radiation pattern to simulate. I have not talked about anything but point sources. If you have a double-couple type source, you get a radiation pattern both from the P waves and the surface waves. The surface waves exhibit frequency-dependent radiation as well. You have to account for all of those things when you simulate an explosion. You can spoof any one of them, but to spoof them all at once is going to be tough. Maybe it can be done.

MR. ARCHAMBEAU: It's probably not easy to do.

MR. PERRET: One more item mentioned this morning which I did not discuss might be germane here. This is comparison of the surface motion above a shot at the time of detonation with that at the time of cavity collapse.

Above a shot in almost any material, the acceleration record looks like the upper one in Figure 25. The period of -1 g represents the development of a spall opening below the surface. It is the ballistic or free-fall period and is terminated by a positive spike when the spall closes. It probably has some influence on surface waves, but I don't presently know what. It is given here so that you may compare it with the collapse signal, to be shown later.

The particle-velocity record below the acceleration in Figure 25 looks generally like a capital N. These positive and negative peaks are nearly equal. Sometimes the negative one may be the greater by as much as 50 percent, but generally they are about equal.

MR. ALEXANDER: What is that time interval?

MR. PERRET: It depends on a number of things, such as the kind of material near the surface, the kind of rock at the shot point. The impact spike has no relationship to the yield but depends on how fast the spalled mass stops at impact. Does it meet something coming up, at rest, or going down? Was the impact at a broken rock surface, in soft alluvium, or a hard rock surface? The duration may range from

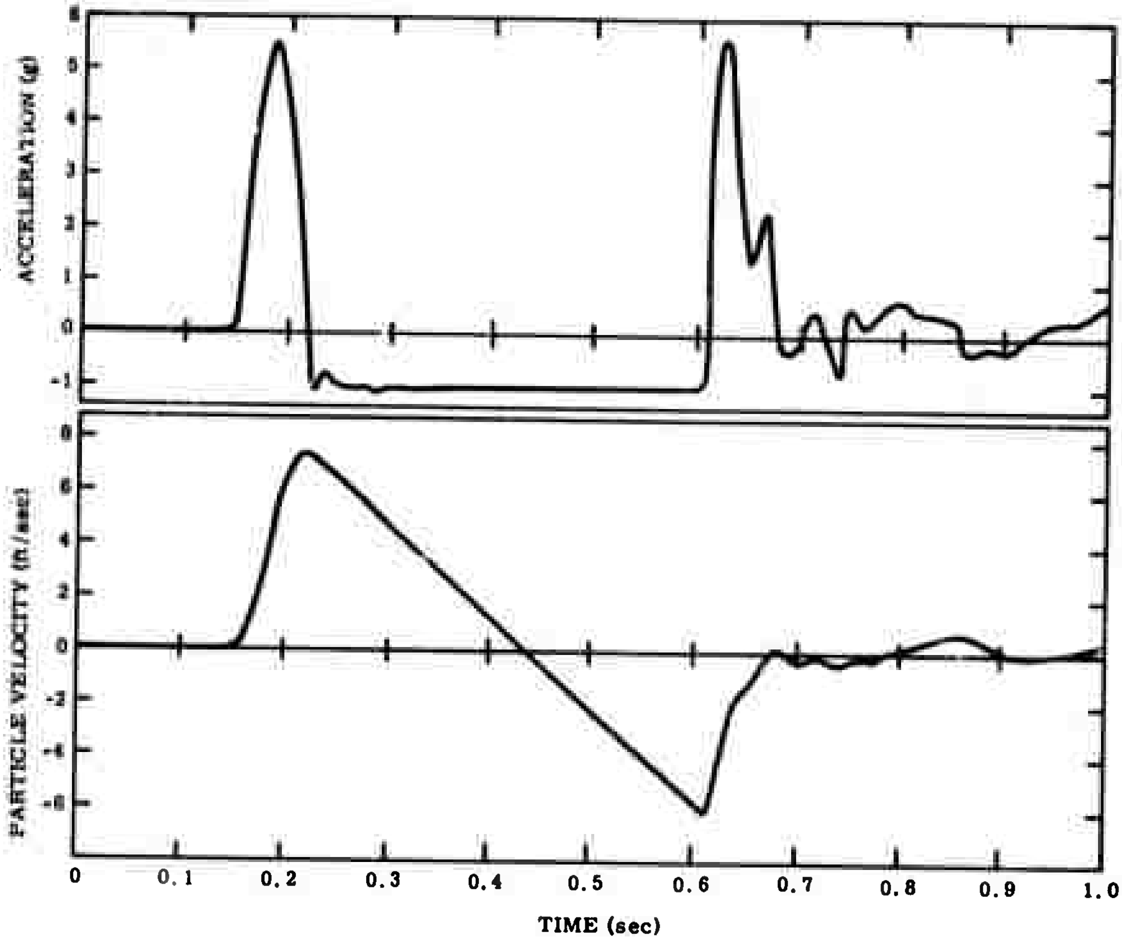


Figure 25. Surface Zero Motion, Rainier Event.

about 0.1 sec to, on some of the big shots, 1.5 sec or more. So on the big ones it could well influence the one cycle or half cycle signals.

This leads us to the kind of signal you get when the cavity collapses and the surface subsides. We have made measurements of this event on several shots. In most cases it was fortuitous. The collapse occurred before our recorders ran out of tape. In a few cases, we have run recorders for 9 or 12 hr to record these signals, and in some the collapse occured the next day.

In general, the acceleration, velocity, and displacement look like that in Figure 26. We have reasonable faith in these, because on the Racoon shot where recorded displacement was 23 ft, the contour maps made from the aerial photographs flown 2 hr after the shot show 27 ft; results were similar in the Merlin collapse data.

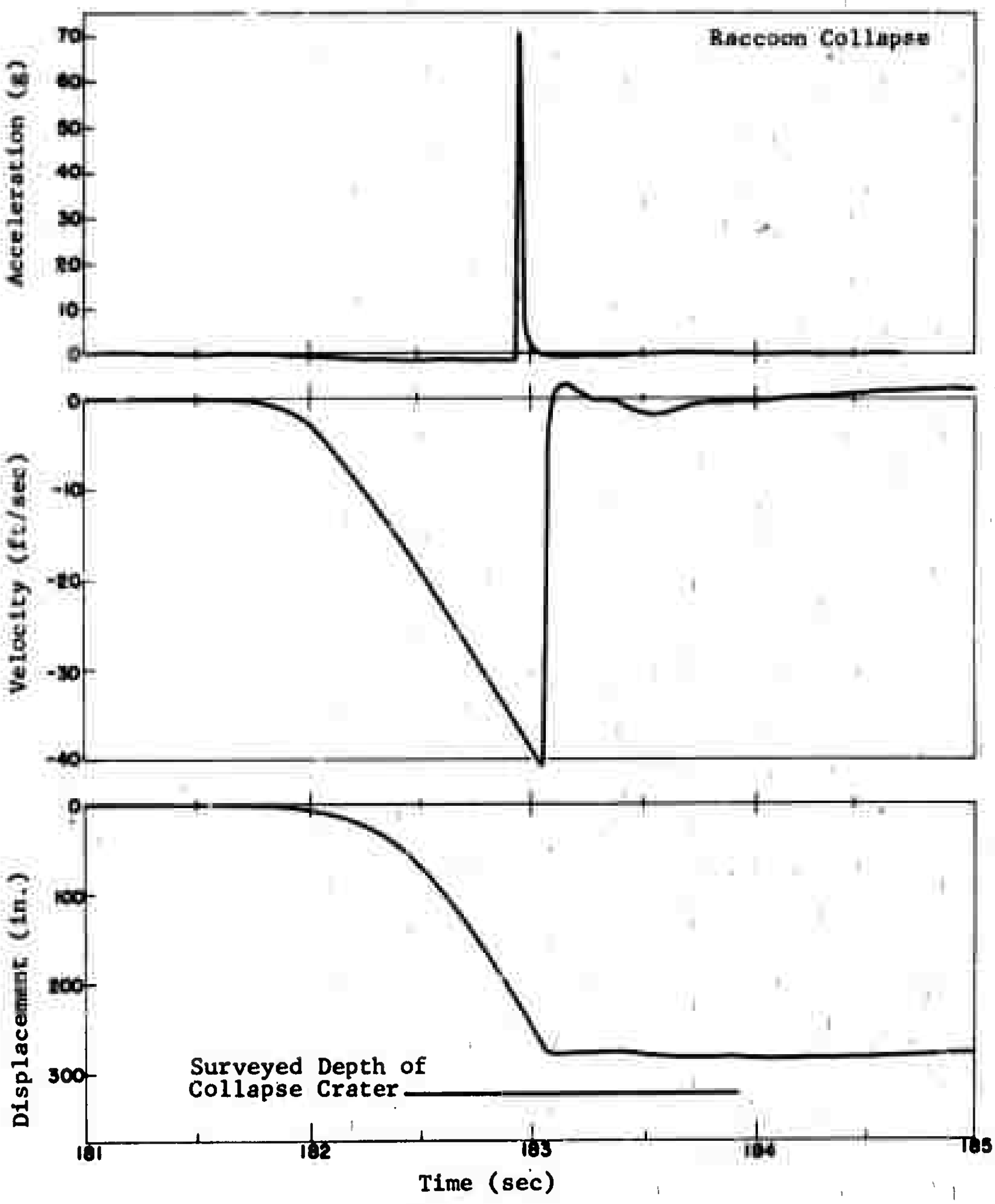


Figure 26. Raccoon Collapse Records.

On the Merlin shot, we had a series of gages from surface zero out to about 2500 ft. Three of the stations were inside the collapse area. The rest of them were beyond it. The peak impact acceleration near surface zero was about 80 g's, and the maximum we have ever seen from collapse was about 130 g's. The maximum negative or downward velocity which occurred at the time of impact was 40 fps. Near the edge of the subsidence, peak acceleration was about 12 g's. Out beyond the edge, all stations recorded about 0.5 g. Horizontal accelerations were appreciably less than 0.5 g.

The horizontal motions in subsidence areas were generally inward and small. The pertinent thing to this meeting is that motion is very strongly polarized in a vertical direction. The other thing that is pertinent from these Merlin collapse data is the sequence of initial collapse motion on gages in the vertical array above the explosion. The deepest station was at about half shot depth, and the start of downward motion there was 2 sec earlier than at the surface. The start of motion at the other stations was sequential during this 2-sec period, suggesting that this whole 500-ft high block, at least a half shot depth, dropped essentially as a body.

Initial collapse signals occurred between 1155 sec at half shot depth and 1157 sec at the surface after detonation.

MAJOR CIRCEO: Bill, if the subsidence occurred due to just the force of gravity, how do you get such a high g pulse, 80 g's?

MR. PERRET: That is the value of the impact peak. It drops at -1 g, and is stopped by impact with an upward acceleration spike of 80 g. The duration of this spike is about 10 to 15 ms. It defines the rate at which the downward motion was stopped.

MAJOR CIRCEO: So in fact the 0.5 g is possibly just a surface spall.

MR. PERRET: No, there is no spall. I think that 0.5 g signal is actually a transmitted signal from the impact up through loose soil.

MR. ALEXANDER: The whole phenomenon is over in how much time? Has anyone measured that?

MR PERRET: A couple of seconds. The duration in the subsidence area is about 2 sec, and outside it the duration is about 0.3 sec. I think this has pertinence to the teleseismic signals of the collapse.

COL. RUSSELL: Thank you very much, Bill.

For the next portion of the conference, Shelton Alexander is going to talk about essentially questions 2-b and 6, which are concerned with the spectral propagation of our seismic signal.

SPECTRAL PROPAGATION OF SEISMIC SIGNAL

Shelton Alexander
Pennsylvania State University

What I will speak to here very briefly, is the Rayleigh-wave spectrum as seen for several NTS shots, and for Milrow and Longshot. In the very beginning I will summarize the conclusion. If you keep the receiver and path pretty much fixed over quite a large range in yield, I can't really say about the medium completely definitely, but it appears that the spectral shapes are pretty much invariant with these parameters. The transmission path, however, does have a very significant shaping effect. Even if you might look, say, within the Basin and Range in different directions away from NTS, and shot after shot will appear to have a similar shaped spectrum, it will not necessarily be the same shape at one azimuth as another. I believe this is primarily due to transmission.

Let me just run through a few of these figures very quickly. Look at the top curve of Figure 27. This is a set of NTS shots. The ones you will see on subsequent figures are for various stations. This is station BMO. This scale goes from about 10 sec to 50 sec. The magnitudes are listed. You may not be able to identify the symbols. The one event that shows a big excursion here is suffering from signal-to-noise problems. What was done here was to take a given signal velocity window at this particular station, that is the same for all events. Notice all of the distances are very close to one another, about 868 to 870 km, so the paths, distances, etc, to this station are nearly invariant, and the shapes are too. I do not think you can really trust the data out here around 50 sec for many of these. Overall, the shapes tend to be very consistent. These have been normalized just to illustrate the comparison of the shapes.

MR. RODEAN: They are normalized at the 0.02 sec?

MR. ALEXANDER: It is normalized by the total energy, actually, so they are not normalized to one frequency.

MR. SMITH: Is that ground motion?

MR. ALEXANDER: Yes, this is corrected back to ground motion. This is power displayed by the way. It is not amplitude.

I believe all of these are in tuff, Bourbon, Bronze, Corduroy, Cup, Dumont, and Par. Par is the smallest one, and it is the one that shows up with the excursion.

MR. RODEAN: Bourbon is at some interface, or close to one.

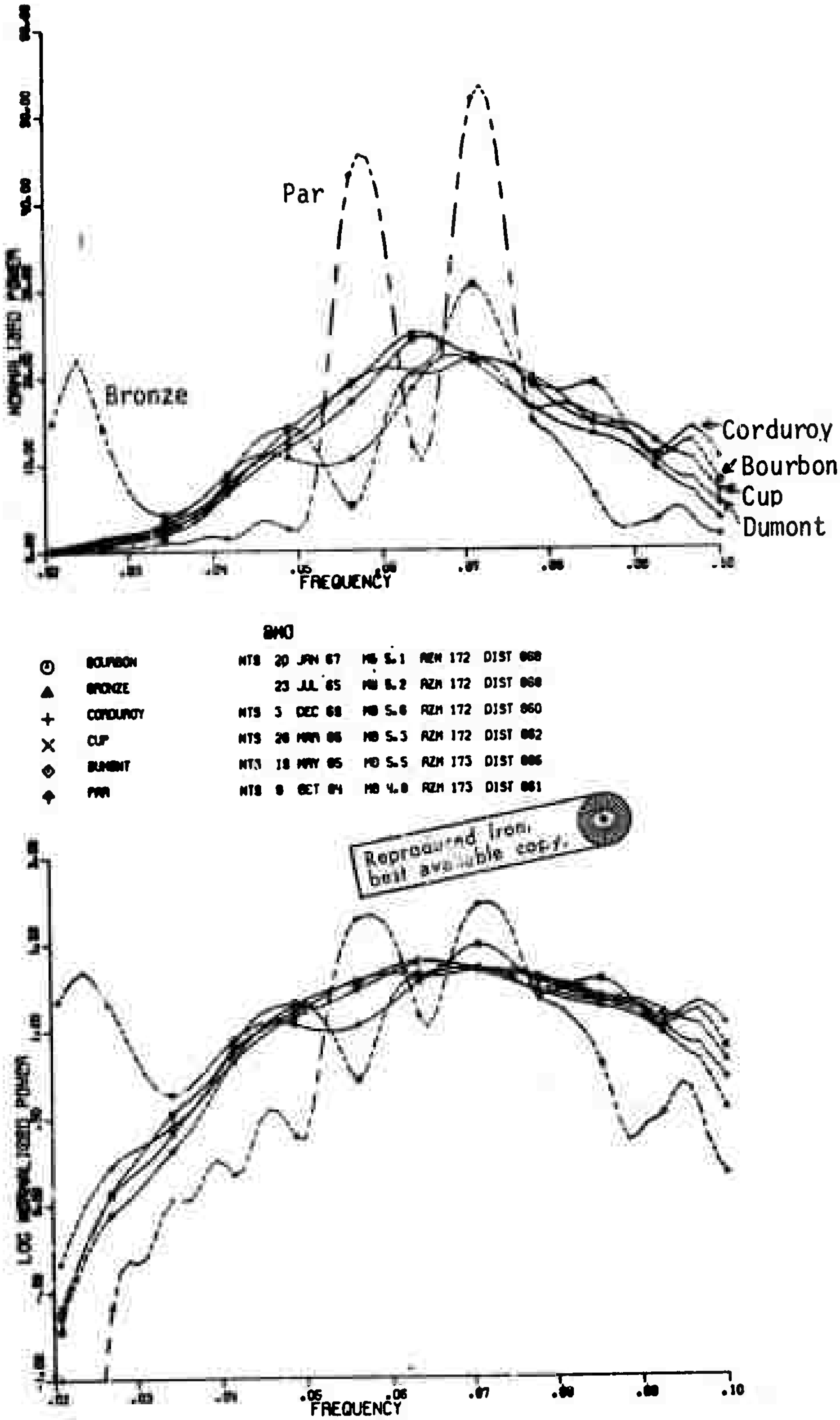


Figure 27. Rayleigh-Wave Spectra at Station BMO.

MR. ALEXANDER: Par, too, tends to show up a little atypically here in Figure 28. But for the kind of measurements that we would intend to make, these are very similar spectra. At Las Cruces (Figure 28), a thousand kilometers distant, I should point out the spectral peak shown on Figure 27 has shifted over slightly. The shapes again tend to reproduce themselves. There is a good deal of spectral character, but it is a reproducible shape.

MR. ROTENBERG: How are these measured, with accelerometers?

MR. ALEXANDER: These are all from velocity instruments. You make an ω correction so you don't have to get back to ground motion in each case to compare events. Again these are ranging the magnitude. In this case it is 4.8 to 5.2. Despite this, the shapes are quite similar.

MR. CHERRY: How does the phase part of the spectrum show?

MR. ALEXANDER: I do not have it plotted here, but I can illustrate it with one example of an actual seismogram. I think you can see that the wave forms tend to be very similar. I will show you that shortly.

Figure 29 is two completely different events. This is Greeley and Bilby at almost teleseismic distances as recorded at CPO. Very interestingly, in this case they are peaked at quite a different (and higher) frequency than the previous examples even though they are considerably larger events. Nonetheless, the two events have similar spectral shapes at this station. So the medium through which it is being transmitted to different directions is very important, but events tend to show up very similar to one another if the paths are invariant.

This Figure 30 gets a little bit back to the point you were mentioning earlier. These are all actual traces observed at PG-BC normalized to the peak value which in this case is at about the point where you would make the surface-wave magnitude measurement. These are all Rayleigh waves. This whole interval is one minute.

The traces are aligned so that this Airy phase on which you would make the surface-wave measurements is nearly coincident in time for each event. You see that by and large the waveforms are essentially reproducible for these events. These are all located in Pahute Mesa, I believe.

MR. RODEAN: Faultless is central Nevada.

MR. ALEXANDER: Faultless is from a different distance, too, so the dispersion alters the waveform somewhat. In this band between 10 and 20-sec period used for the surface-wave magnitude, there is essentially no phase difference, because the waveforms agree in shape over the whole time interval. If the shapes agree with one another, then the phases have to be the same. If the trace amplitude and shape agree, then the phase spectra have to be the same.

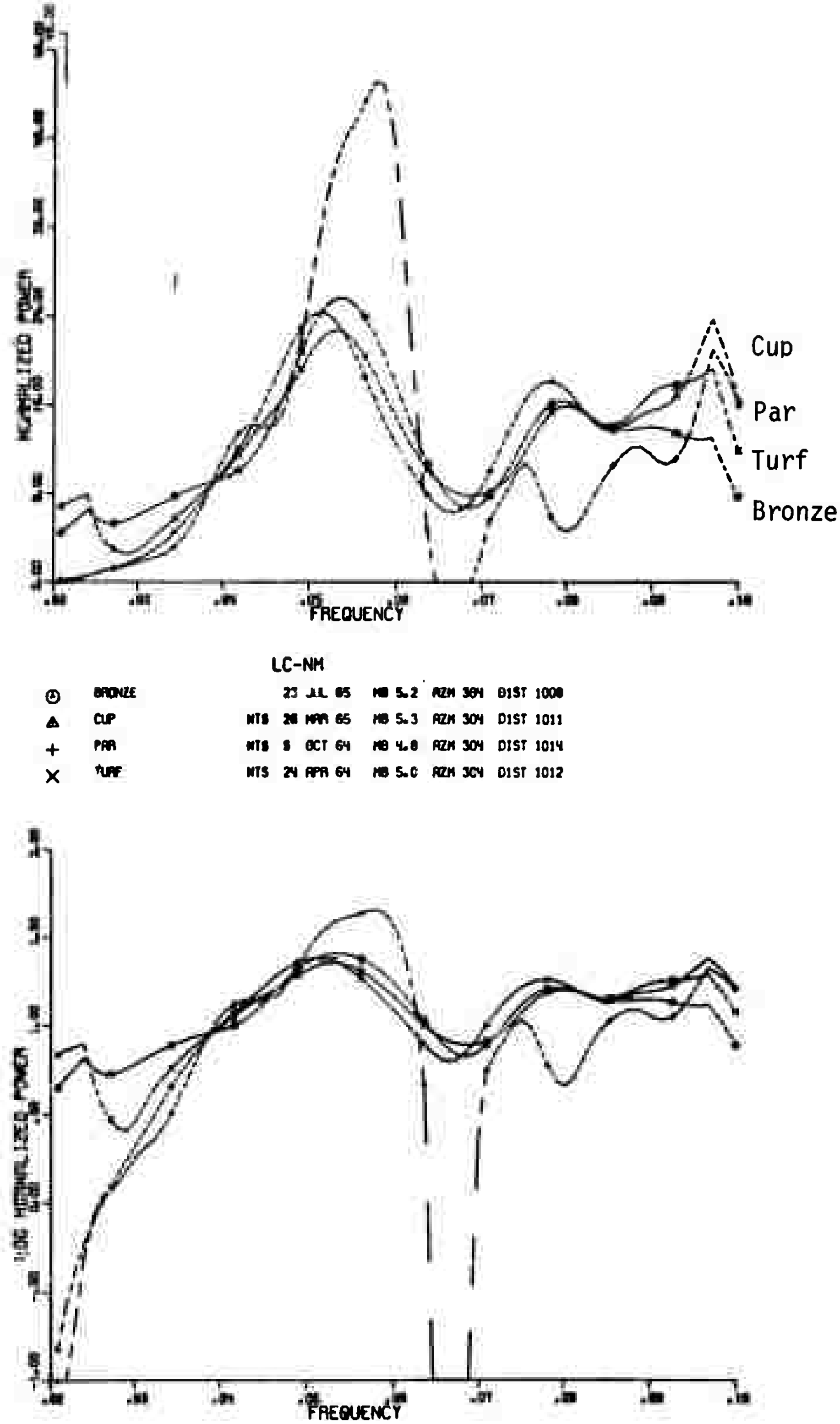


Figure 28. Rayleigh-Wave Spectra at Station LC-NM.

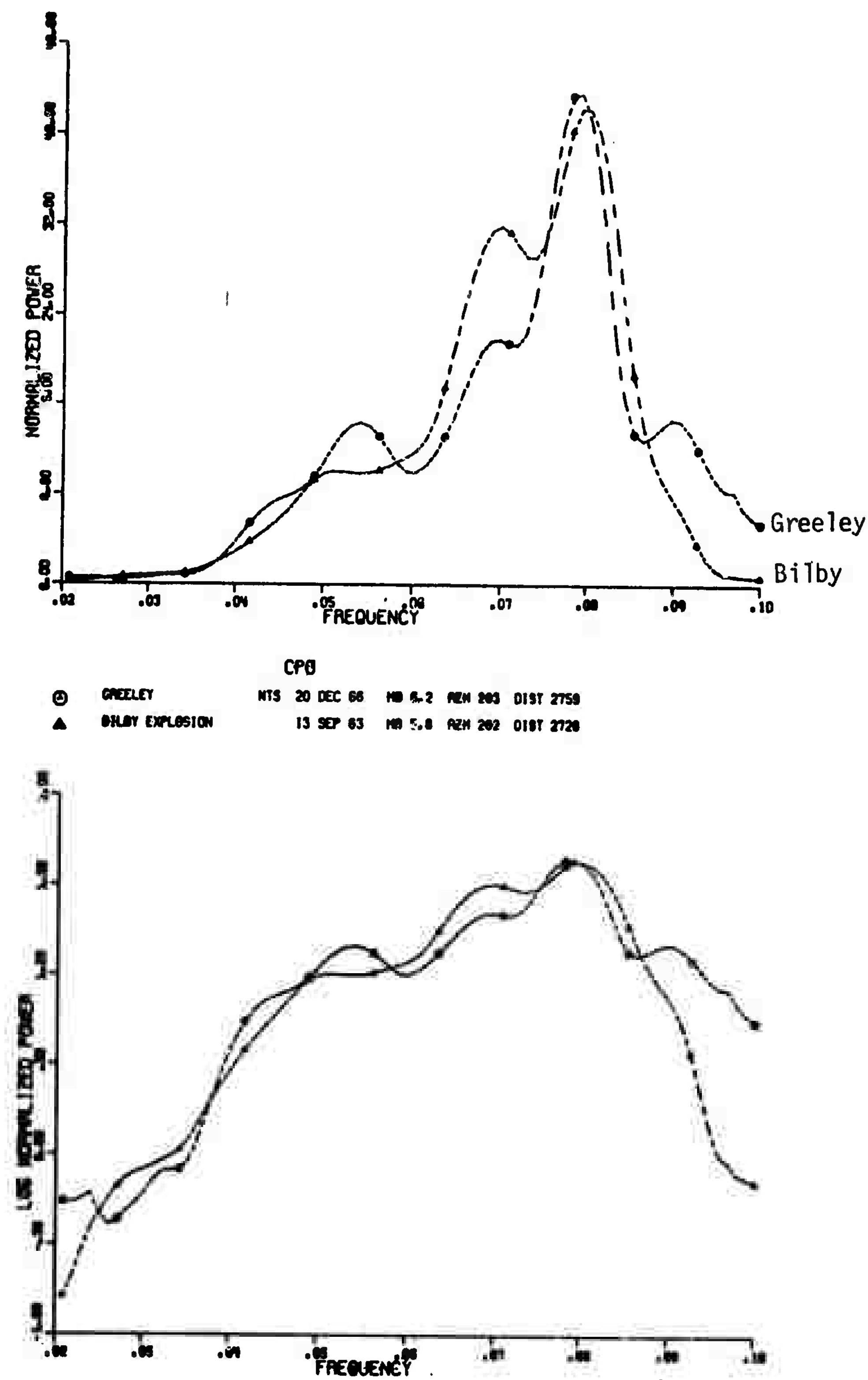


Figure 29. Rayleigh-Wave Spectra at Station CPO.

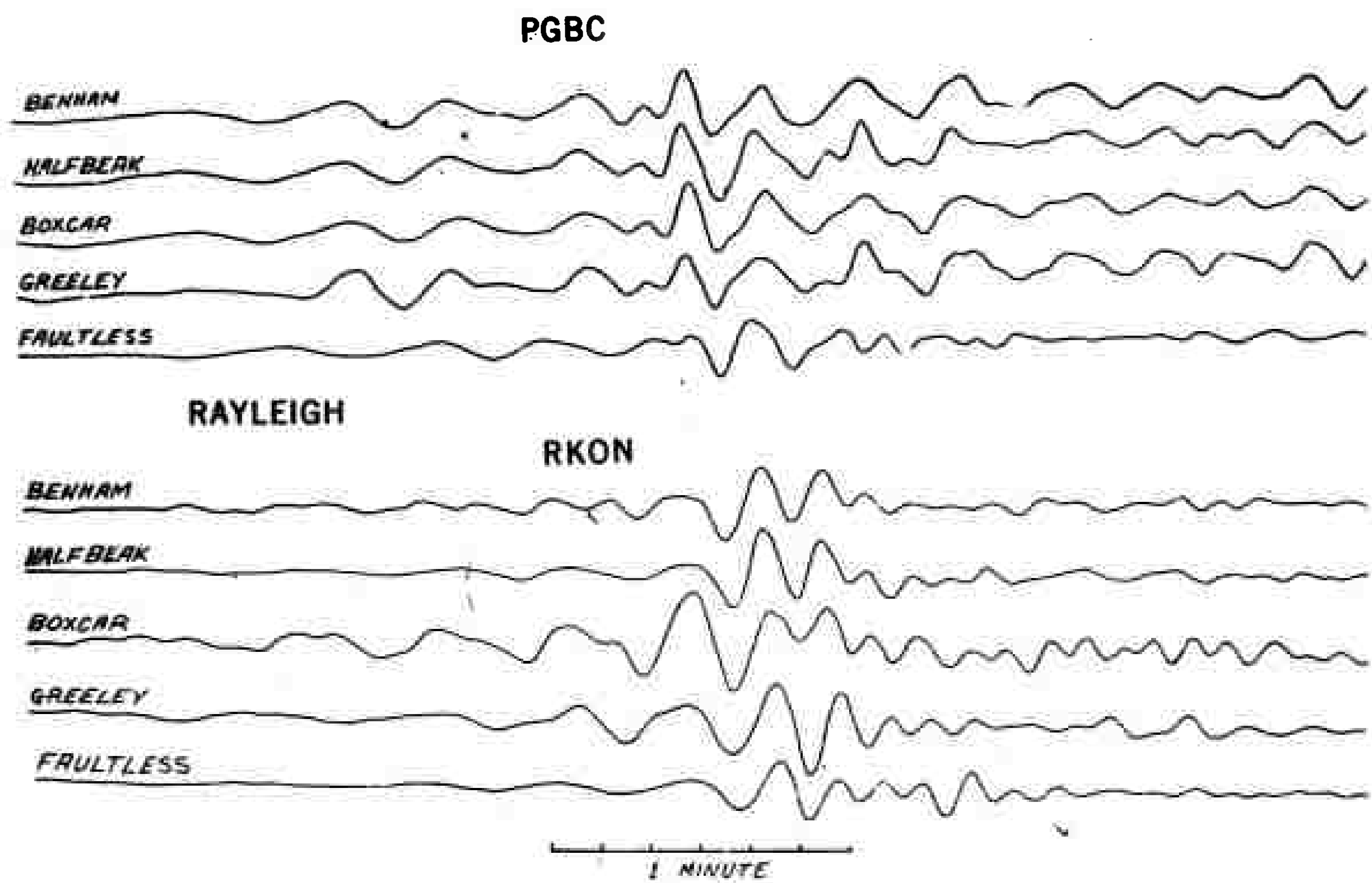


Figure 30. Rayleigh-Wave Spectra at PG-BC and RK-ON.

Now, there are some differences in the long-period portion, particularly for Greeley. Apparently there are other lines of evidence that this event had a significant amount of tectonic release compared to the other ones.

The lower frequencies also, however, are quite comparable. This portion is about 20 to 30 sec in period. Particularly these first three are remarkably similar as to spectrum, shape and everything, as seen at this azimuth PG-BC.

Now, look at the same set of events from a different azimuth, also a very good station, RK-ON, on this same Figure 30. Here you begin to see some azimuthal effects that are somewhat important. Basically there is a similar sort of waveform here at the Airy phase for these four events, but in this case Boxcar seems to be a little bit peculiar. I think the essential point, though, is that even though these events are in different media, and do vary somewhat in size, the spectral characteristics as revealed by the wave shapes are very similar.

MR. ROTENBERG: Is Boxcar timed?

MR. ALEXANDER: Yes, it may be shifted off by one cycle. What he is saying is that this may be off in time slightly, and should be shifted over to normalize it down a little bit, but it still has a bigger amplitude in the earlier part of the Rayleigh wave. They are not all that different from one event to another however. It is remarkable that they are so consistent. This is one generalization that one can make--that the wave shapes are consistent. Also from a theoretical point of view you don't expect that the shape should be different until you get into the real big ones (in the absence of tectonic release) and indeed they should be quite similar. Whereas with earthquakes, you can get any spectral shape. You can shape the spectrum however you wish, depending on how one orients the fault, and at what depth it is located. You can shape the spectrum in any one azimuth more or less any way you choose.

Let me move very quickly then to the last set here (Figure 31) which is a comparison between Milrow and Longshot, and an earthquake very near to the same place. This is seen at PG-BC. The dashed lines with the circles is Milrow. I don't know if you can see the dots. It does not matter, because they are essentially overlays of one another. These events differ in level by not quite a whole order of magnitude. Longshot has a body-wave magnitude of 5.9 and Milrow is 6.5. They are significantly different magnitudes at any rate. Yet the spectra, the dashed lines, are simply coincident. This earthquake in the same area has the shape shown by the solid line, and it was a magnitude 5.0 event. That earthquake happened to have the same surface-wave magnitude as Longshot. In this case at this particular station, at 3800 km distance, the peak is at 20 sec, 0.05 cycles.

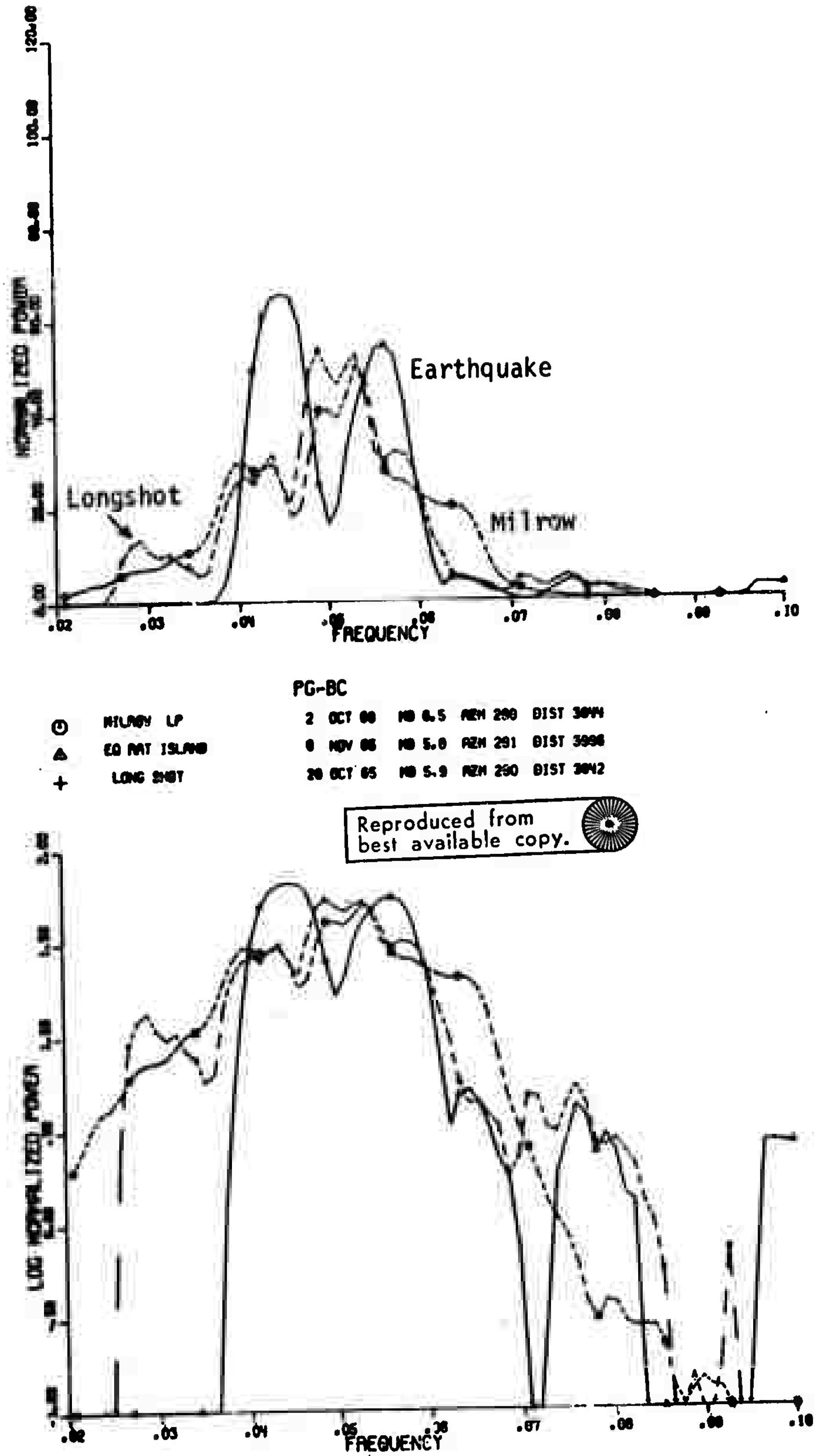


Figure 31. Rayleigh-Wave Spectra at Station PG-BC.

Figure 32 is for another station, Kanab, Utah, which is a little more distant, 5400 km. This is a different earthquake now, but the same Longshot and Milrow, shown by the dashed lines. They are quite different in shape than you saw before; nonetheless the shapes of the two explosions are more or less overlays within the accuracy of the observation. The earthquake in this case peaked at a lower frequency than the shots.

MR. ROTENBERG: Do you mean you overlaid the two earthquakes?

MR. ALEXANDER: No, the dashed lines are Longshot and Milrow, the two explosions. In the sequence of figures not all of the earthquakes are the same earthquakes from station to station, but always the same explosions, Longshot and Milrow.

MR. FRASIER: How deep is the earthquake?

MR. ALEXANDER: I am not sure about the one on Figure 31, but the one on Figure 32 I think was shallow, approximately 20 km.

MR. CHERRY: Were the earthquakes in the same general area, did you say?

MR. ALEXANDER: Very close, yes, within 50 km of the explosions.

Here in Figure 33 is a similar plot for TFO. Here you see two different earthquakes. They are actually smaller earthquakes. One of them is magnitude 5.0, shown with the triangles, the other one is 5.5. These earthquake's results are just the opposite of what some people have predicted in the past, that is, the bigger the event, the more it should be peaked to a lower predominant frequency. In this case you get to see just the opposite effect at this particular azimuth, with the smaller event peaked at a lower frequency. It can go either way. The only thing consistent is that the explosions reproduce themselves in shape over a wide range in size.

MR. HARKRIDER: But you are not suggesting, though, that if someone showed you just a single one of those records that you could discriminate between the explosion and the earthquake?

MR. ALEXANDER: No, I am not suggesting that at all. In fact, what I am claiming is that you really cannot use spectral shape as a means of discrimination, at least not at one azimuth. If you take into account many azimuths, you may be able to.

The other thing is that since the explosions tend to reproduce one another in shape, you might be able to discount an event that was sufficiently unlike an explosion, and say that is not an explosion because an explosion tends to repeat the shape.

MR. ARCHAMBEAU: That is Rayleigh-wave spectra?

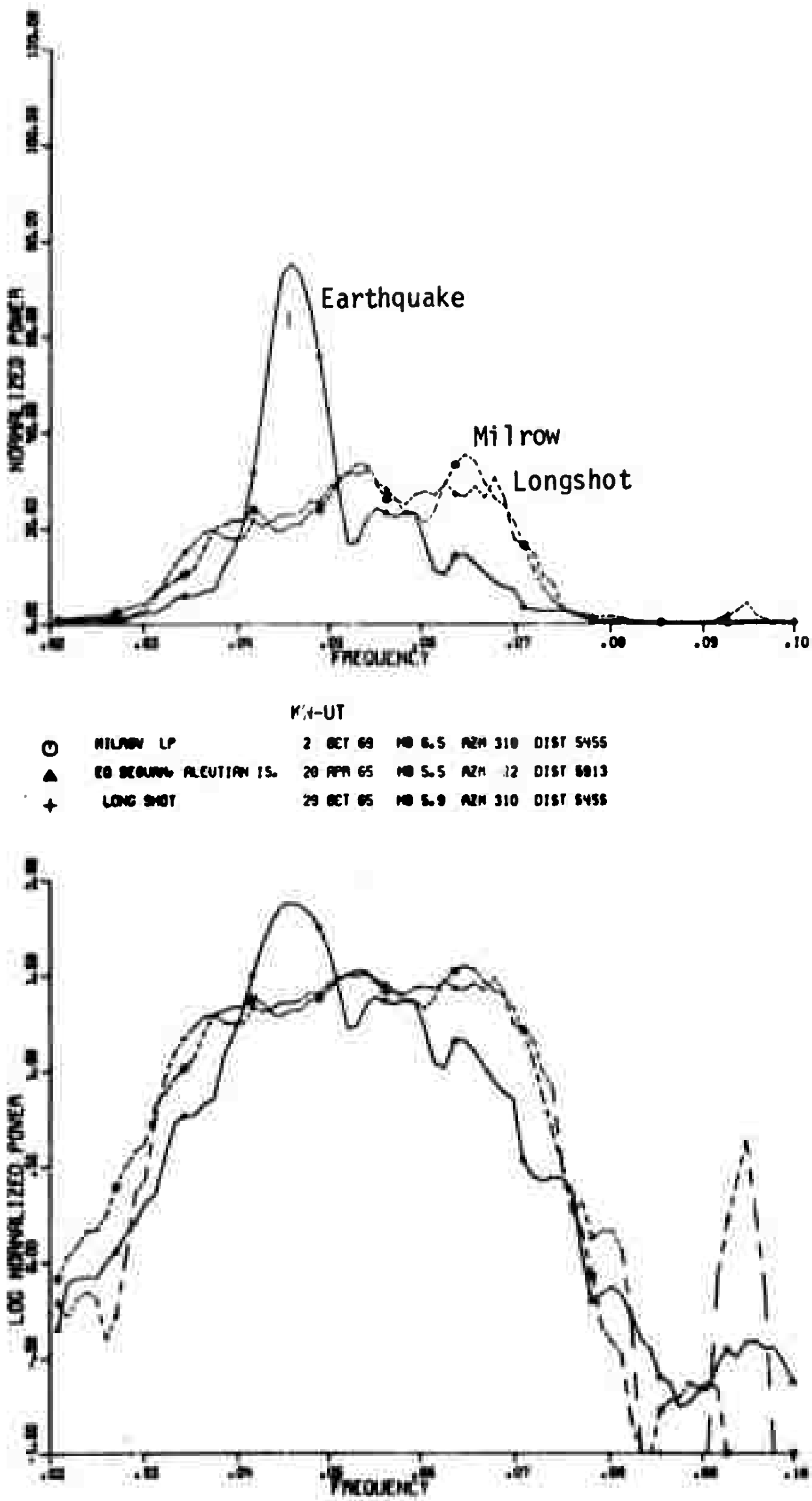


Figure 32. Rayleigh-Wave Spectra at Station KN-UT.

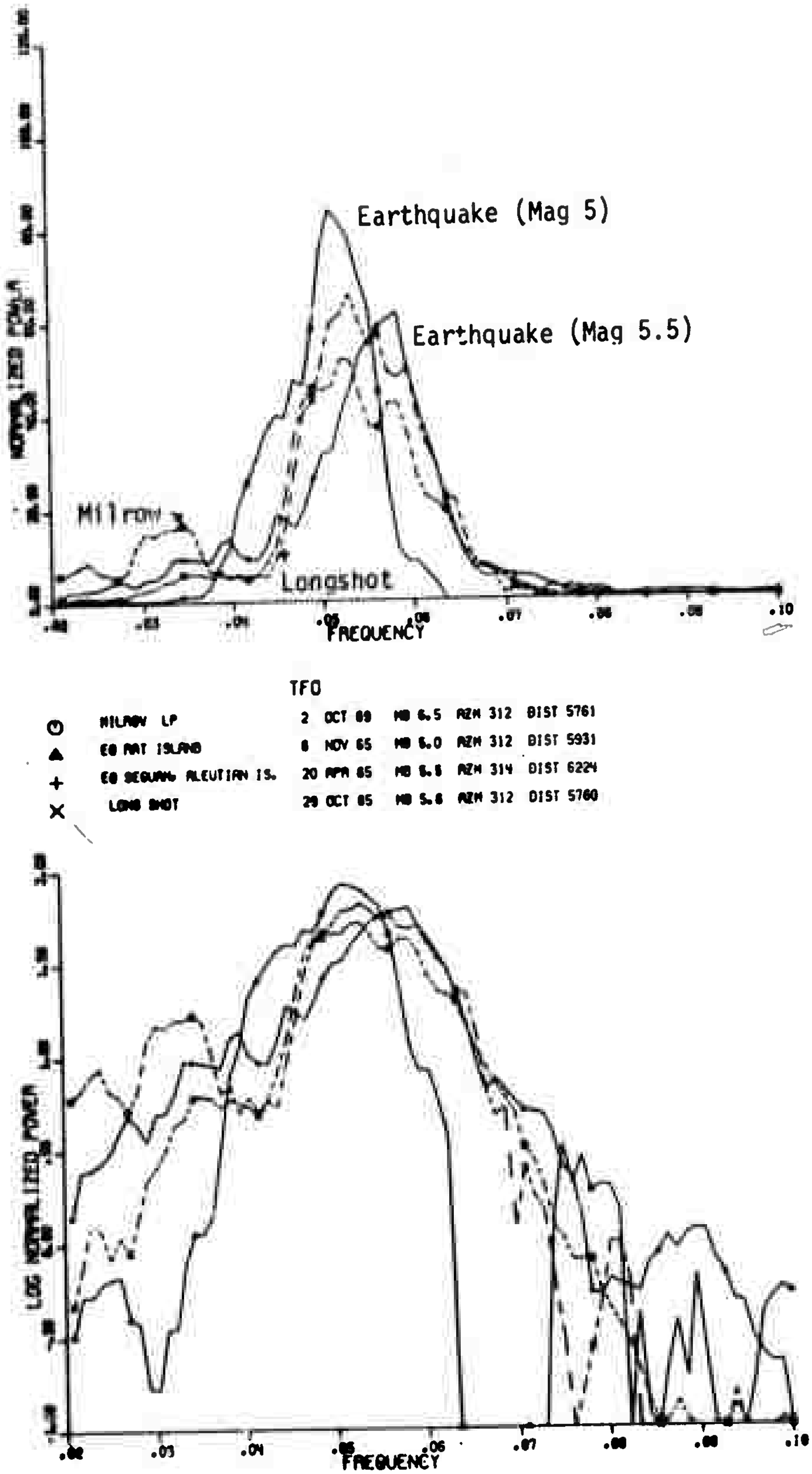


Figure 33. Rayleigh-Wave Spectra at Station TFO.

MR. ALEXANDER: That is right, all of this is Rayleigh waves.

MR. ARCHAMBEAU: You are comparing magnitudes now.

MR. ALEXANDER: I have normalized out all of the amplitudes. This is just considering the spectral shape.

MR. ARCHAMBEAU: Yes, but you said previously you referred to the magnitude of the event. You made the statement that larger events were thought to have a peak in the spectrum at longer periods, and this does not show that. But that depends on how you measure the magnitude, the body-wave magnitude. Maybe the event really was not bigger in terms of energy. Okay?

MR. ALEXANDER: Okay.

MR. ARCHAMBEAU: With respect to body-wave magnitude, but that may not be a measure of energy.

MR. ALEXANDER: Let me clarify that. What you say is true. I don't really believe that the shape of the Rayleigh-wave spectrum in the case of earthquakes is much of a measurement of what the source-time function is like. It is much more a reflection of the geometry of the source, that is, the fault orientation and depth. If you represent it as a point double couple, you can go through a theoretical calculation and show that, depending on how you orient the double couple, you can shape the spectrum making it peak any place you wish. Primarily it is that kind of effect as opposed to the source-time function that is doing this shaping and that is independent of the size of the event. That is purely a source geometry effect which I think overrides in some respects the source-time function in the case of earthquakes. Explosions tend to be very consistent.

I think I have one or maybe two other stations. This Figure 34 is Holton, Maine, a smaller kind of thing. In this case two earthquakes are practically coincident with one another, and again the explosions are the dashed curves. They overlay one another. One earthquake is a 5.0, and the plus symbols denote the 5.5. Seen at this azimuth the two earthquakes tend to reproduce one another also and have a different shape than the explosions in the same general magnitude range.

Here is another example (Figure 35), again showing these same types of events, in fact, the same events. In this case, the peak is away over at 25 sec for the explosions. Remember in the previous cases, a lot of them, they were peaking more or less in the same position. Here the explosion is peaking at about 25 sec, while the same earthquake is peaking over in this region here, the same earthquakes you have been looking at.

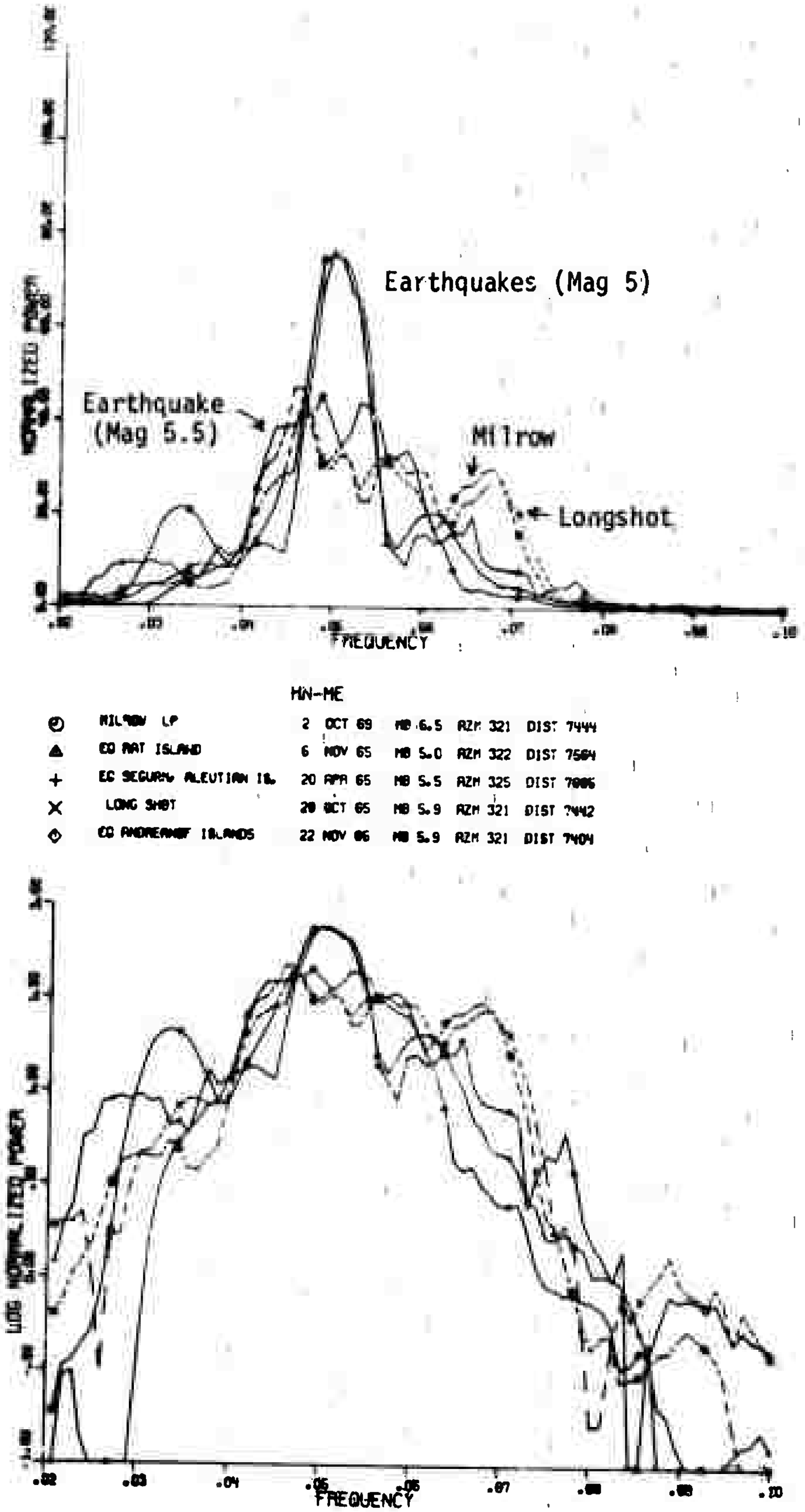


Figure 34. Rayleigh-Wave Spectra at Station HN-ME.

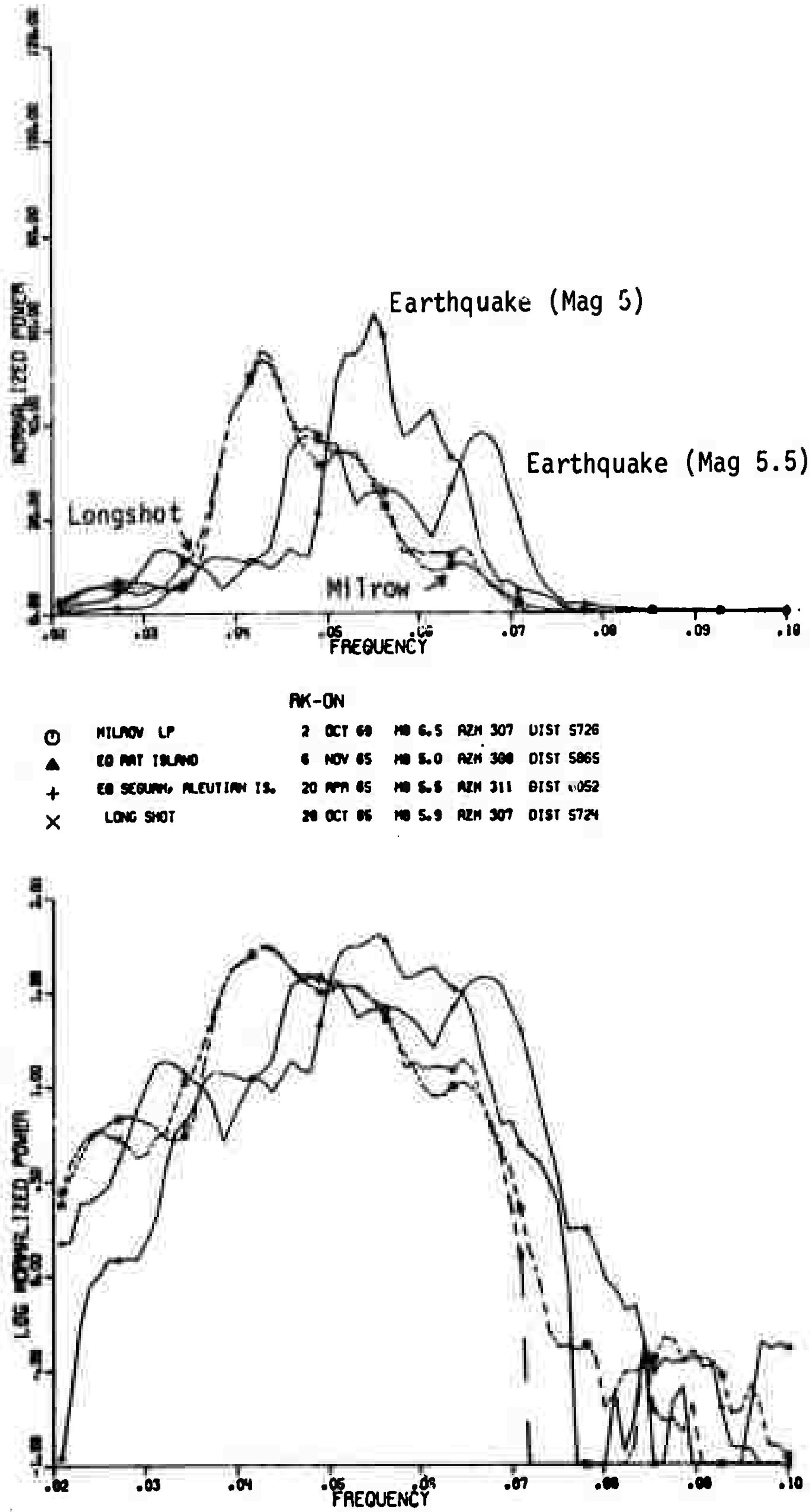


Figure 35. Rayleigh-Wave Spectra at Station RK-ON.

To summarize what I wanted to say, it appears that for at least two different source regions, the NTS and the Amchitka area, the spectral shapes as seen along given paths to particular receivers tend to reproduce one another over a fair range in yield. I am not sure whether it is true over a large range in shot medium, because we don't have that many. Most of what I have been showing you were in tuff, but they were located at somewhat different positions.

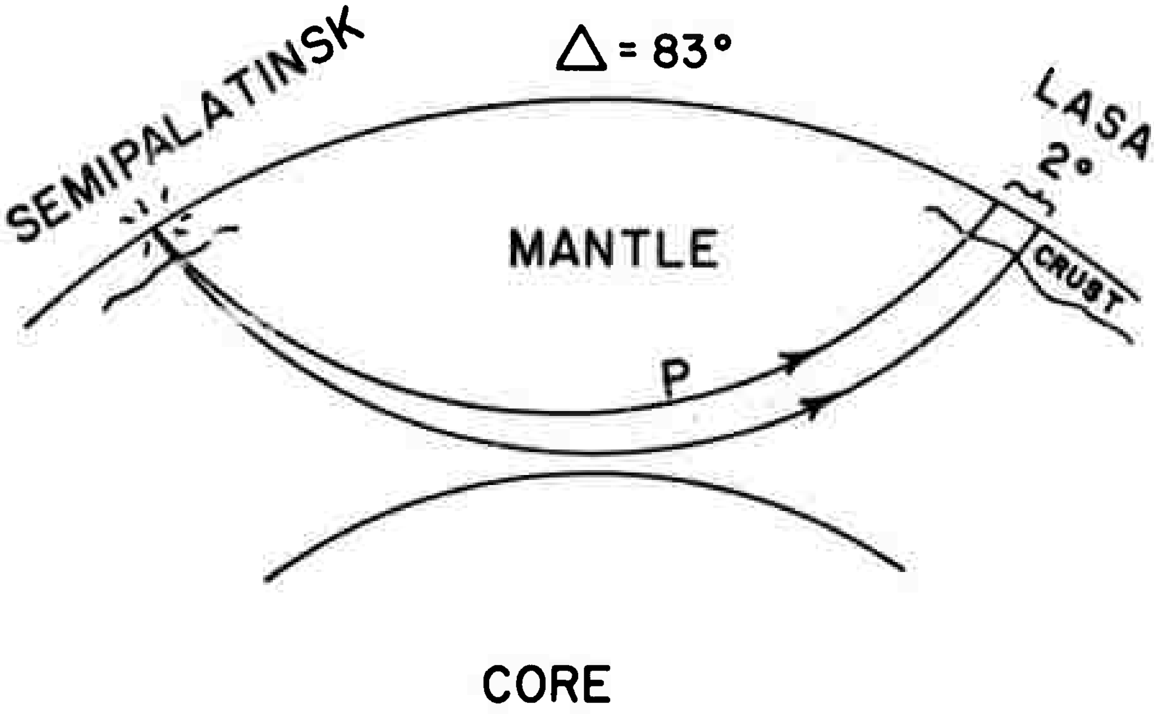
MR. SMITH: Would a fair paraphrase of what you have said be that at long periods explosions are clearly point sources, and the entire structure of the spectrum is determined by the path of propagation?

MR. ALEXANDER: I don't want to go quite that far. I am not sure you could consider them point sources. What I am saying is as far as the low frequency spectral energy being put out, all of the explosions look very similar to one another and a point source representation is a good one from a theoretical point of view. I do not know if anyone here has short-period data, but there are some differences I think perhaps in the short period. I think Clint Frasier has been working in that area, and also Tom McEvilley at Berkeley has some short-period spectra. This essentially completes my discussion of the Rayleigh waves.

POWER SPECTRAL RATIOS - SHORT PERIOD DATA

Clint Frasier
Massachusetts Institute of Technology

The sketch below presents a quick look at four presumed explosions from Kazakh as recorded at LASA.



LASA, the Large Aperture Seismic Array, near Billings, Montana, has an aperture of about two seismic degrees, and the Semipalatinsk area in eastern Kazakh is about 83 degrees distant. This distance is about as far away as you could ever use primary teleseismic data at and try to interpret the signal shapes in terms of source functions. At larger distances, core phases--namely PcP--arrive just behind the P phase.

What I did here was a relative study of explosive sources recorded at the same sites. A difficult problem in short-period seismic data is the tremendous signal variation from site to site for the same event. Along a specific take-off angle direction, the effects of attenuation and layering in the earth are not well known and can only be estimated statistically. So you really are not sure what happens to the short-period signal shapes between the time they start off here in eastern Kazakh and are detected at LASA.

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One thing you can do to get a relative measurement of signals of different magnitudes is to take several shots from practically the same test site. Then by computing spectral ratios of one event to another event at the same site, you can eliminate all the unknown yet common effects of the ray path through the earth from source to receiver.

Since LASA has a two degree aperture, the takeoff angles of the rays from each source to the different LASA sensors are essentially identical. Since the four events are very closely located we are really sampling the source radiations along one specific take-off angle from the Kazakh site.

TABLE 4

SCALING OF CAVITY RADII FROM LASA MAGNITUDES FOR FOUR PRESUMED EXPLOSIONS FROM EASTERN KAZAKH

Event	m_b (LASA)	a_i/a_5	a_i(m) Assuming $a_5 = 500$	a_i(m) Assuming $a_5 = 750$
1	5.4	0.585	293	438
2	5.6	0.681	341	511
4	5.8	0.781	391	586
5	6.1	1.000	500	750

Table 4 shows the data I looked at, and I labeled the events 1, 2, 4, and 5. The magnitudes shown here are those recorded at LASA, and they are increasing in the same direction as the CGS magnitudes, which are averages of many worldwide magnitudes.

The eastern Kazakh test site is thought to be in hard rock, i.e., granite, and probably these explosions were shot in shallow holes. I assumed that for large magnitude events in hard rock the displacement-potential amplitude is directly proportional to the yield, which is proportional to the radius cubed of an equivalent elastic cavity. If a cavity radius is assigned to the largest event, then equivalent cavity radii for the smaller magnitude events can be obtained by cube root scaling of the amplitudes obtained from the magnitudes.

For a magnitude 6 event in hard rock, yield estimates of about 100 kt and an equivalent cavity radius of about 700 m have been made. Two sets of scaled radii are shown here, for a_5 equals 500 m and 750 m respectively.

MR. SMITH: Those are elastic radii, not cavity?

MR. FRASIER: Oh, yes, these are equivalent elastic radii.

MR. SMITH: You have cavity radii there.

MR. FRASIER: Yes, they are supposed to be equivalent elastic.

MR. RODEAN: And that is meters, not kilometers.

MR. FRASIER: Actually this is meters.

Let's look at Figure 36. Here is the problem. I have numbered these events 1, 2, 4, and 5, and arranged them in order of increasing magnitude here. These are the various subarray locations of LASA: the F ring, which is the outermost ring and about 200 m across, and the E ring, and so on. I am just showing you the two outermost rings of data here. This is short-period information. These vertical lines here are 1-sec timing lines. These are Hall Sears instruments, and the velocity response of these is flat from about 1 cps out to about 5 cps. Thus the P waves shown are essentially displacement-velocity records.

Now the really frustrating thing here is that there is much more similarity from event to event at a single site than there is for a given event from site to site. In other words, as we examine the same event at the different sites, we see large variations in both the signal duration and amplitude, which must be due to complex layering in the upper mantle, and below the receiver locations at LASA rather than any variation in source radiation, because there is no difference in the angle of the rays going to the different arrays. This variation you see here for the F and E ring, persists for all 21 subarray sites. These seismograms here are recorded by the deep-hole, 500-ft buried seismometers, single seismometers. I did not want to take the subarray sums, because this introduces other filtering problems in the data.

A crucial point is that the effect of magnitude shift on the short-period signals is overwhelmed by the station's site characteristics. If you accept this idea of equivalent elastic cavity, then a decrease in the high-frequency spectral content should be observed from lower to higher magnitude events. If you look very hard here at some sites there appears to be a slight decrease of high-frequency energy from events 1 to 5. But it is not very obvious.

I computed transfer functions which when convolved with a low-magnitude event, say event 1, give as output event 5. This was done at each of the 21 subarray positions of LASA Since this is equivalent to computing the spectral ratio of event 5 over event 1, all of the unknown transmission path effects cancel out. This yields transfer functions to shape the source radiation of event 1 to that of event 5.

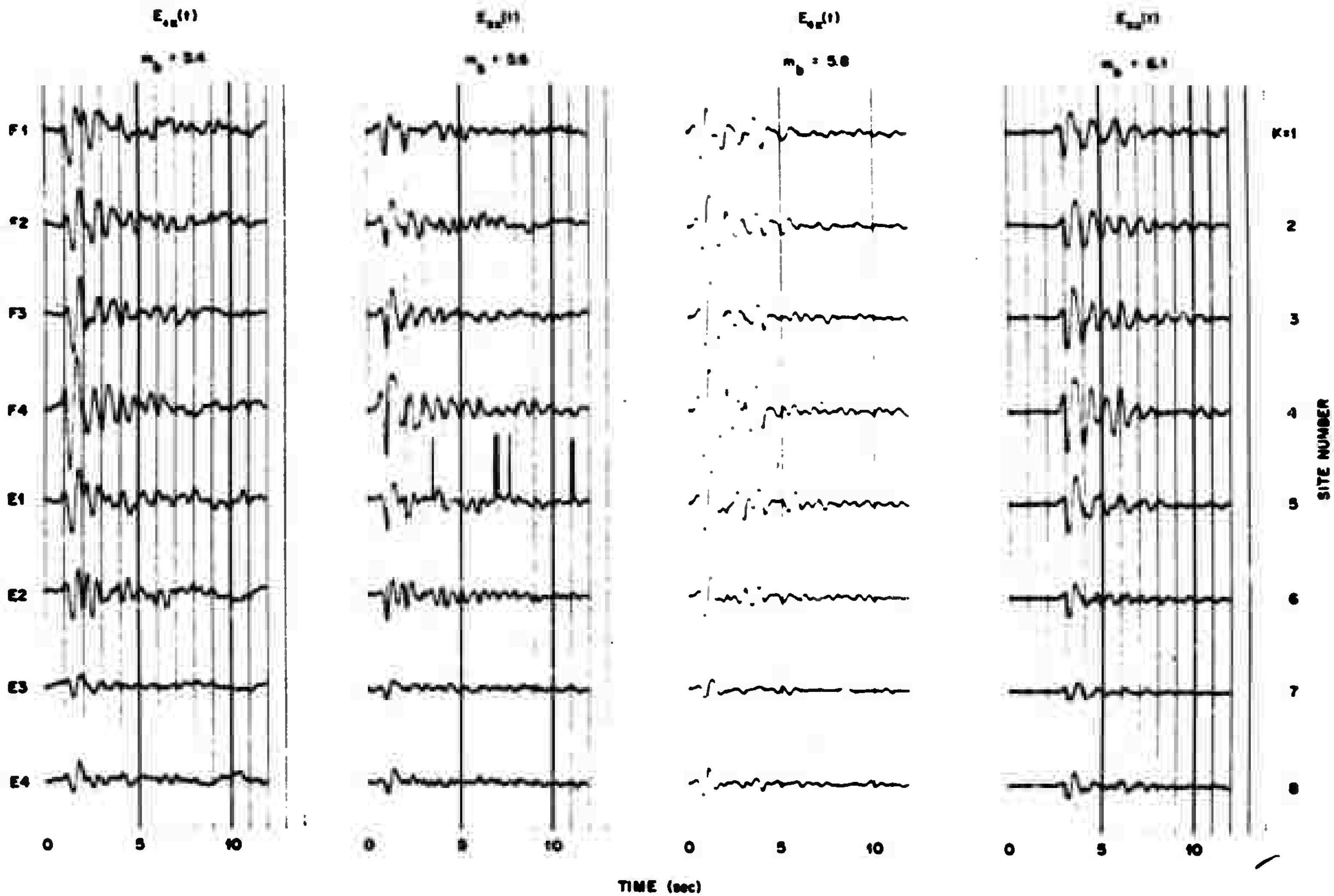

Figure 36. LASA Recording of the Four Events $E_{ik}(t)$ from Eastern Kazakh at Subarray K. The F and E ring records are shown.

Now, with an array, you can calculate transfer functions at each subarray site, and in spite of signal variations you should get the same transfer function when you do this over and over again for each subarray site. I did that for the lowest to the highest, and next lowest to the highest, and so on, so I am going from lower magnitude to higher magnitude.

MR. RINEY: Would you define the equivalent elastic radius again, please?

MR. FRASIER: That is just a hypothetical spherical radius around the source, outside of which things are behaving elastically, such that you can do elastic-wave calculations.

MR. RINEY: That is all you mean by that?

MR. FRASIER: That is all.

MR. ALEXANDER: Is that uniquely defined then?

MR. FRASIER: I don't know. It is probably governed by the lithostatic pressure. When the actual pressure generated by the source has been reduced to the extent that it is not any more than lithostatic pressure, then it will probably go elastic. This is a conjecture and I can't prove it, but I am just saying that far away these four different events which are from the same area look like they are radiating from elastic cavities of different size. So I am postulating that.

Figure 37 shows the transfer functions computed at each of these 21 LASA subarray sites for shaping event 4 to event 5. Event 4 has magnitude 5.7 and event 5 has magnitude 6.1. The vertical lines are 1-sec timing marks here now, so these oscillations are about 2 Hz or so. In spite of the extreme variations you saw in the original data, the transfer functions come out to be very, very consistent from site to site. These transfer functions are computed in time by a least-squares technique.

Figure 38 shows the transfer functions at each site to shape event 2 to event 5, which is a larger change in magnitude. Now things are less consistent from site to site, but still there is a consistent negative swing in the transfer functions which shows up in the average transfer function.

Figure 39 shows the transfer functions from the lowest to highest magnitude events, i.e., event 1 to event 5. Again if I had done a better job of aligning the data more details would show up in the average transfer function. I don't know what this detail is, but the general shape of positive and then negative swing is consistent and shows up in the average transfer function.

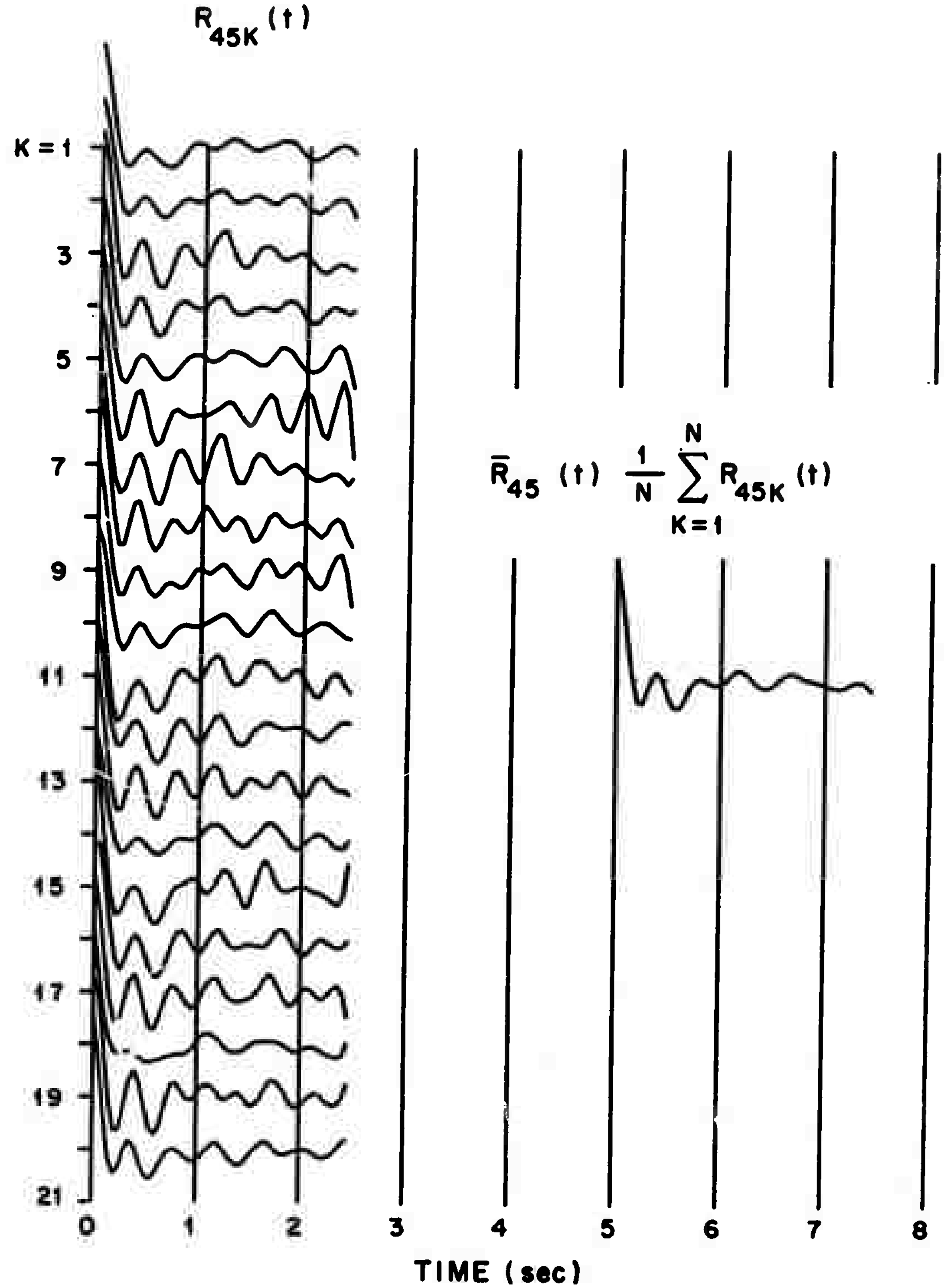

Figure 37. Transfer Functions $R_{45k}(t)$ at Subarray K at LASA. Each function is equivalent to the spectral ratio $E_{5k}(\omega)/E_{4k}(\omega)$ in the frequency domain.

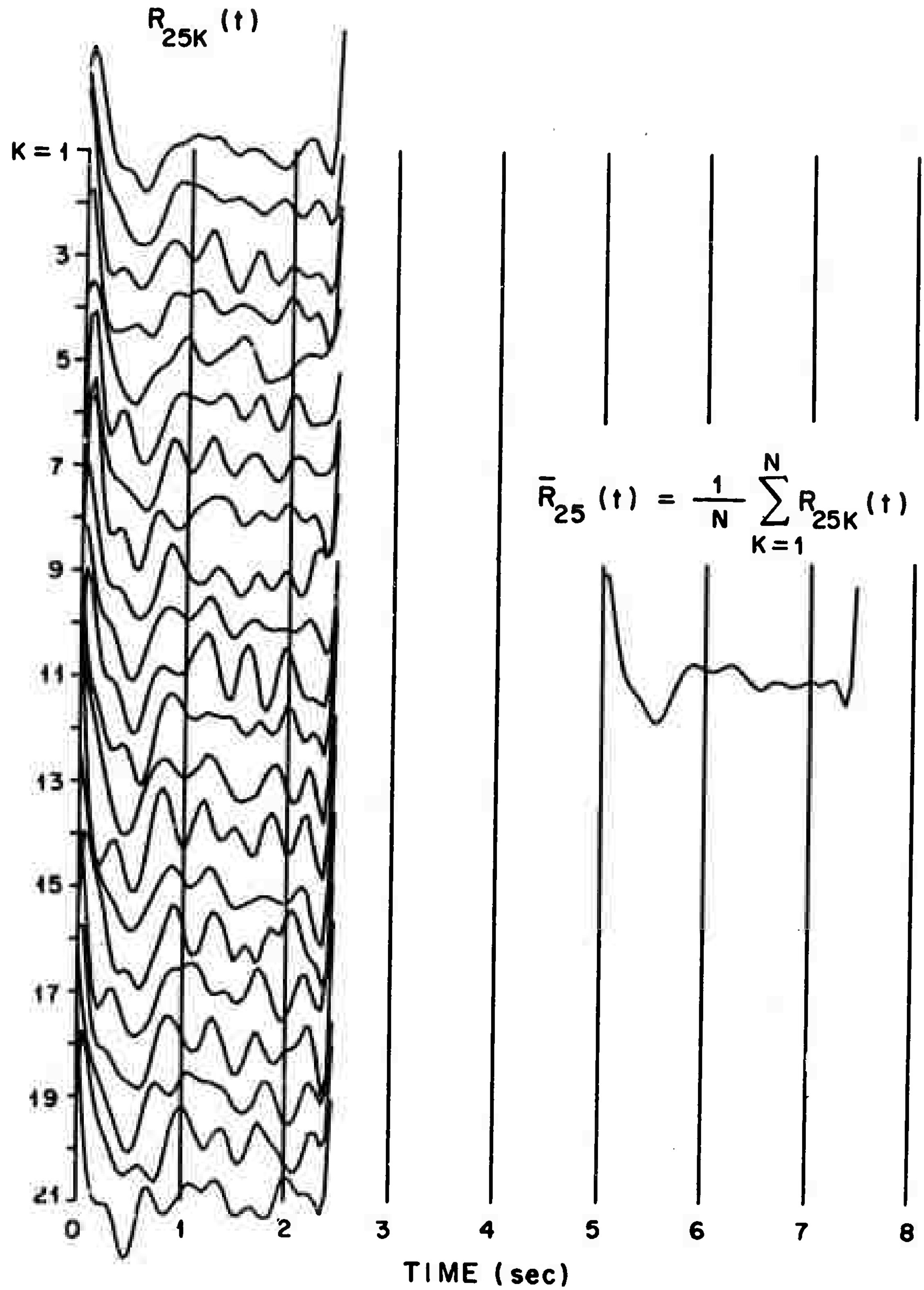

Figure 38. Transfer Functions $R_{25k}(t)$ at Subarray K at LASA.

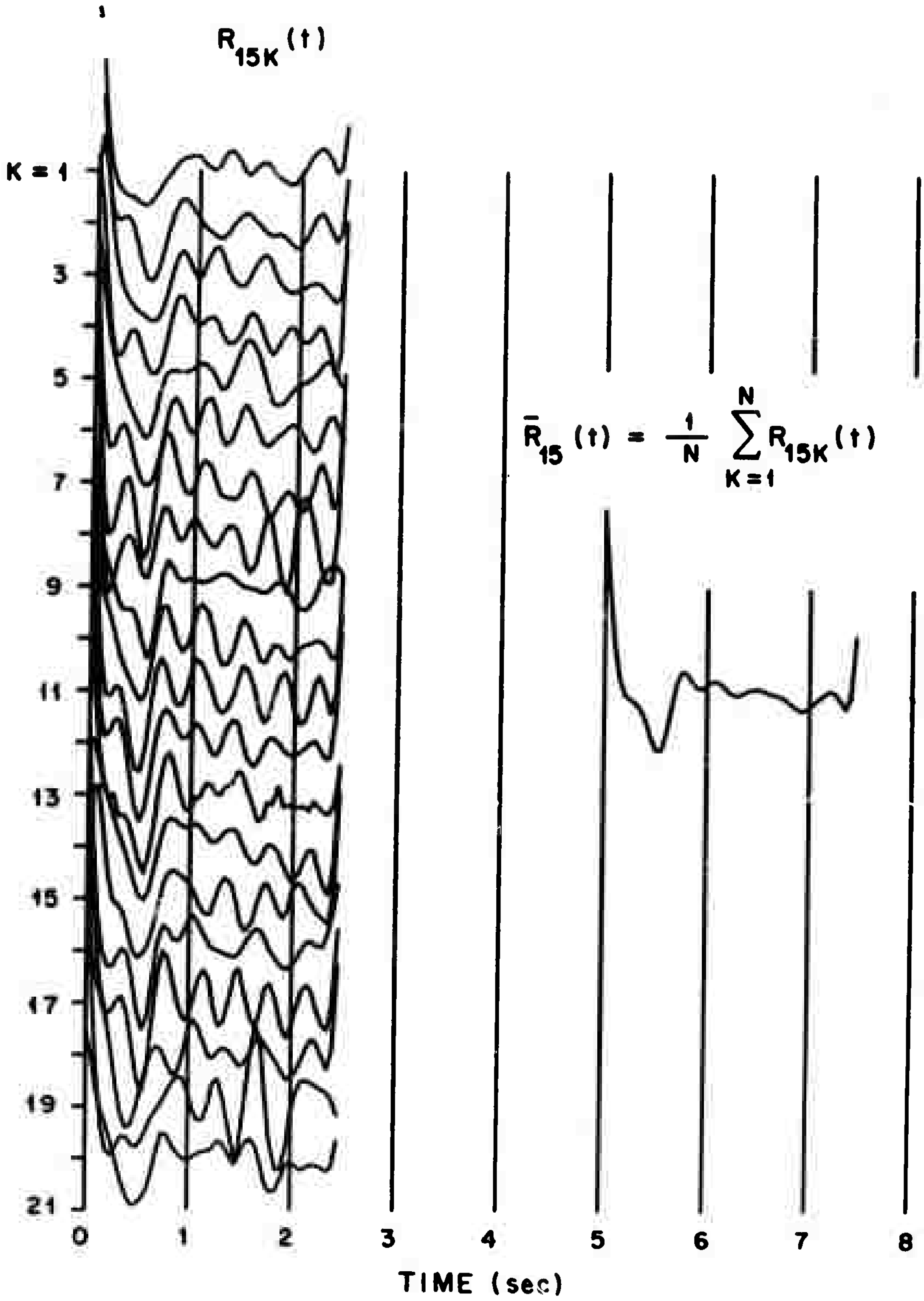

Figure 39. Transfer Functions $R_{15k}(t)$ at Subarray K at LASA.

This is the observed data. Since these are transfer functions, the Fourier transforms of them give spectral ratios of the largest event over smaller events.

If we take the spectral ratio of the radiation from a large cavity divided by the radiation from a little smaller cavity, we should get a definite degradation of higher frequencies. That is what the spectra of the transfer functions show. Figure 40 shows the frequency response of the three transfer functions. The top curve is the frequency response of the first average transfer function that I showed you, i.e., $E_5(w)/E_4(w)$. It shows less degradation of high frequencies than the other two transfer functions. These three spectral ratios are normalized at 1 Hz to show the relative slopes at higher frequencies.

MR. HARKRIDER: What is that frequency scale on the bottom?

MR. FRASIER: This is one cycle, two, three, four, five cycles out here, and you probably can't believe anything beyond about four cycles. The degradation of higher frequencies in the spectral ratio is the important trend.

If you take the scaled cavity radii that I showed you before in Table 4, and take the spectral ratios of the far-field particle velocities produced by a step function of pressure in each cavity, you will get the same type of degradation of high frequencies. Again this is not a proof that an elastic cavity is a good model for the source. It is just a possible explanation for this observed slope in the spectrum of each transfer function.

MR. HARKRIDER: You got the radius from Sharpe's solution that fit this? How did you get the radius again?

MR. FRASIER: I assumed a yield from the largest event of about 100 kt and an equivalent elastic radius of 750 m. I then estimated the yields of the smaller shots by assuming that magnitude varies as the log of the yield. This was done using LASA magnitudes. From the yield estimates, cavity radii were obtained by cube root scaling down from 750 m, the largest cavity radius for event 5.

MR. ALEXANDER: So you think the ratios of the cavity radii computed in this way should be at least approximately correct, but the actual sizes depend on the assumption about that biggest one.

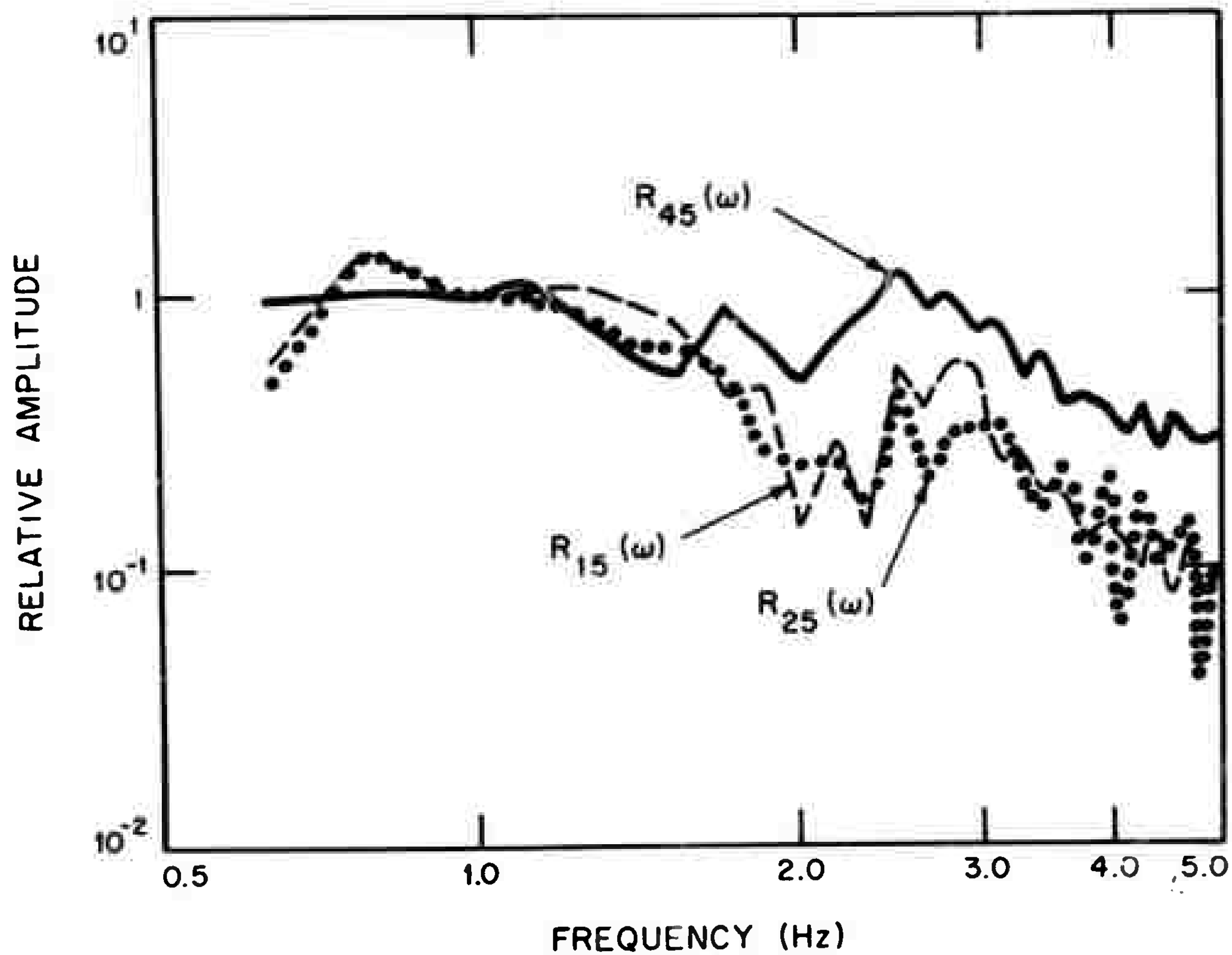

Figure 40. Amplitude Spectra of $R_{15}(\omega)$, $R_{25}(\omega)$, and $R_{45}(\omega)$. Spectra are normalized to 1 at 1 Hz.

MR. FRASIER: Right. All I am looking at is ratios here because I have to eliminate the unknown ray path effects. This is really a difficult problem for short-period seismology. I just don't think you can look at anything absolutely because of the tremendous signal variation you are getting, so I am using this ratio as a gimmick, which everybody else has used to divide out all of the unknown things.

MR. TRULIO: What does your transfer function convert to what?

MR. FRASIER: The transfer function is a filter. If I convolve the low-magnitude event with the filter, the output is approximately the high-magnitude event, just the actual seismograms that I showed you in Figure 36.

MR. RINEY: Are these transfer functions from a given event to different places?

MR. FRASIER: No, what I said was the only thing I can do is compare data of different events at the same site. I can never compare one site to another, because there is too much signal variation.

So I compute transfer functions to shape low-magnitude events to high magnitude, at each sensor, and if I really am getting something that is diagnostic of the source spectral ratio, then these transfer functions should look alike as I go from site to site all across LASA. In other words, if the transfer functions were not coherent from site to site, then I would have no statistical basis for interpreting the data at all. The point is that the average transfer functions show detail seen on each transfer function for a given pair of events.

MR. TRULIO: Do you get a transfer function like a kernel of an integral?

MR. FRASIER: Yes.

MR. TRULIO: But then you can't find it from the conversion of just one signal to another.

MR. FRASIER: Sure, it is a least-squares, digital filter.

MR. TRULIO: Yes, but it is like trying to get a matrix from a single set of linear equations. You know the left-hand vector and the right-hand vector, but you can't get the matrix.

MR. FRASIER: You can't get it exactly, but you can get it in the least-squares sense.

MR. TRULIO: Okay. Why is that the right thing to do if there is no uniqueness about the transfer function?

MR. FRASIER: But it does a very good job. I don't have the examples, but what I do is take the event-time traces and convolve them with the transfer functions and they do a very good job of fitting the desired event seismograms. The transfer functions look like what they call minimum delay in the electrical engineering business. They are well behaved and stable filters.

MR. ALEXANDER: If the two signals look exactly alike, that transfer function would be a delta function, right?

MR. FRASIER: Right. But they don't look alike.

MR. ALEXANDER: All he is getting is something that amounts to the spectral difference from one event to the other. In other words, if they looked alike, this would turn out to be a straight line all the way across.

MR. TRULIO: Okay, the transfer function does not cause much transformation of a signal, therefore it is all right to determine it from a single pulse even though the true function is then not unique.

MR. ROTENBERG: You go back to the frequency and see what the problem is.

MR. ARCHAMBEAU: Just write that as a Fourier transform.

MR. FRASIER: You see, I did not want to divide spectra because when you divide spectra, you get very wild looking ratios. These time-domain calculations effectively smooth the spectral ratio. The point is that I have an array. I recomputed this filter individually for each sensor, and I was able to get a fairly consistent set of filters, and then I took the average of these to get the average transfer function. Then if I convert that to the frequency domain, I just get a spectral ratio.

MR. ARCHAMBEAU: I just wonder about your frequency range here. It seems rather narrow.

MR. FRASIER: It is probably good out to three cycles. I would not push it any farther.

MR. SMITH: So you showed that the larger yield explosions are in fact relatively richer in low frequencies as a result of this exercise

MR. ARCHAMBEAU: Or they had larger radii in the elastic region.

MR. SMITH: What was the conclusion from this exercise?

MR. FRASIER: So far my conclusion is there is source information in short-period teleseismic P waves from explosions.

MR. SMITH: The most impressive thing about the seismograms you showed was the fact that there was some kind of a repeated delayed pulse arriving, and the first thing one would think about was a multi-path phenomenon, rather than a source phenomenon. I think that also shows up in the filters, which are also characteristic of delay in some types of operations.

MR. ALEXANDER: But you might get that just from the source.

MR. SMITH: Yes, it could be a surface reflection, even. That is 1-sec delay, which is pretty big.

MR. FRASIER: Let me finish. Here is the spectral ratio $E_5(w)/E_4(w)$. This spectral ratio is much flatter at higher frequencies due to the magnitudes being more nearly equal.

MR. SMITH: Wouldn't it be exactly equivalent? I know you don't want to divide raw spectra, but if you calculated the outer correlation and the power spectra, I think you would be safe in dividing power spectra, and you would get smooth results.

MR. FRASIER: Yes, that would be the same, but that is the least-squares solution, too.

MR. ALEXANDER: But you have to smooth them a lot first.

MR. SMITH: That is what you do when you calculate power spectra by definition.

MR. FRASIER: You see, I am actually getting more information in the time domain because this has no phase spectra. I really want to keep the phase information, because I want to compare that time-transfer function with what I would get if I just assume two cavity radii for sources. This is shown in Figure 41.

What I did here was use the Sharpe or Blake solution for the far-field particle-velocity response. It turns out the velocity response has amplitude and time constants which are proportional to the cavity radius you assume. I started out with a radius of 750 m for the largest event, and I scaled it down by cube root scaling to get the radius for the smallest event. Again the timing lines are 1 sec apart. This assumes a step-function pressure inside a cavity and an infinite homogeneous medium. $E_1(t)$ and $E_5(t)$ are far-field velocity responses for events 1 and 5.

MR. ROTENBERG: Are those the transfer functions for the Sharpe solution?

MR. FRASIER: The trace on the right (Figure 41) is the least-squares transfer function. It starts with a big spike that goes up and then swings negative. To check it out I actually took the filter and I

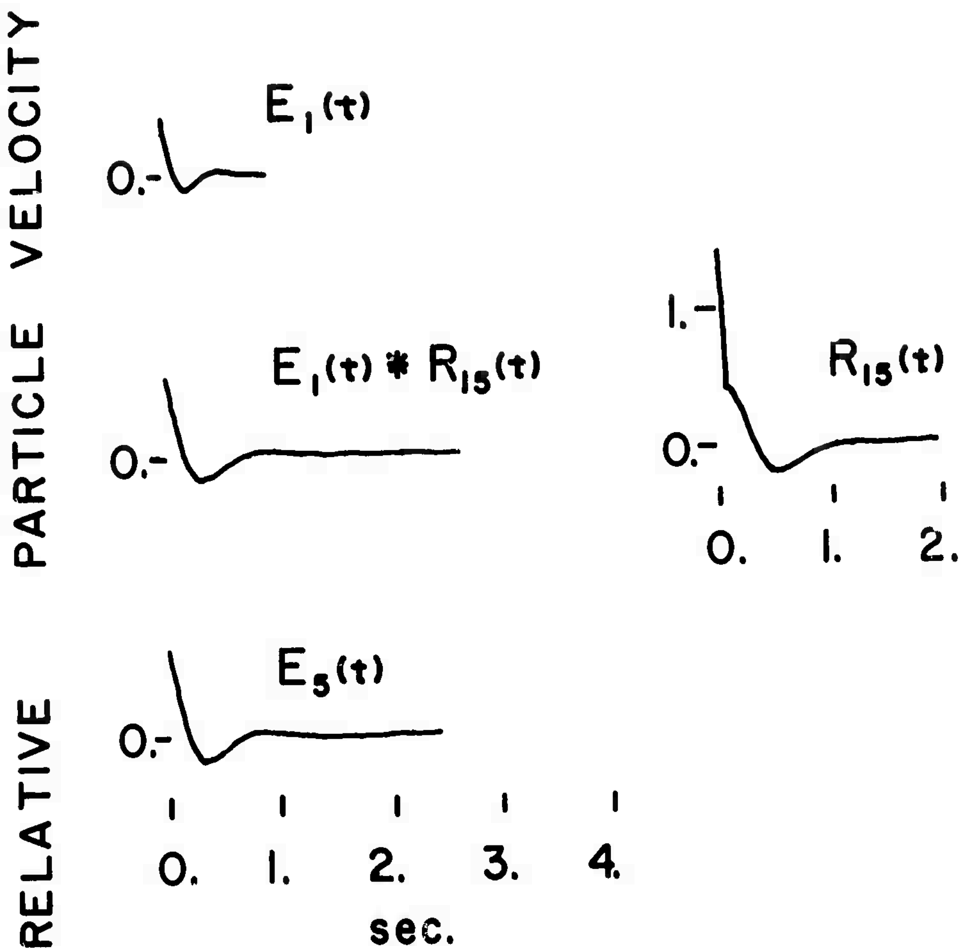

Figure 41. Calculation of Theoretical Least-Squares Transfer Function $R_{15}(t)$ Assuming Blake's Source Model for $E_1(t)$ and $E_5(t)$.

convolved it, the low-magnitude solution, and I get an output which matches the large magnitude solution. It is just a check on the accuracy of the least-squares filter.

It is the positive and negative swing which I tried to interpret on the transfer functions from the actual data. Those are all superimposed on Figure 42.

What I did was take the scaled radii that I assumed and computed the transfer functions. These transfer functions are shown by the dashed lines, and they are superimposed over the actual observed transfer functions of the data. The only thing I can say is that the negative swing of the transfer function increases with magnitude difference between the events being compared and that this is also predicted by Blake's solution. I could very easily have taken a different elastic radius for the largest event and scaled it down to obtain a different set of radii, but this would only change the scale factors for the transfer functions, not their shapes. It would be interesting to know what these oscillations are, whether or not they are caused by surface reflections at the source or perhaps nonspherical oscillations of the cavity. But the fact that many of these high frequency oscillations are consistent over all 21 sensors when I compute the transfer functions, and remain there in the average, indicates that such effects are due to the complex source radiation coming out at that particular take-off angle from Kazakh towards LASA.

MR. CHERRY: Are those theoretical transfer functions independent of the source-material properties?

MR. FRASIER: I assumed that the site was just granite in Kazakh. I took a typical velocity and Poisson's ratio for granite, and I assumed that the cavity pressure is the same for each event, the differences in radiation being produced by the cavity radii, which were scaled for each event. The depth of burial of each explosion was not known, but was assumed to be shallow due to the granitic source region. So that is all I did, not knowing anything more about the test site.

MR. ARCHAMBEAU: You assumed the step function.

MR. FRASIER: The step function of pressure. Now, if there is not a step function of pressure, this would also work if the time history of pressures are the same in both, because they also would divide out in the frequency domain. This is just a possible interpretation I have, and it does seem these transfer functions do show the right degradation of higher frequencies with increasing magnitude that would be predicted if you did use this.

MR. ALEXANDER: In those transfer functions, like E-5 to E-1, even, at frequencies lower than one cycle, they were flat.

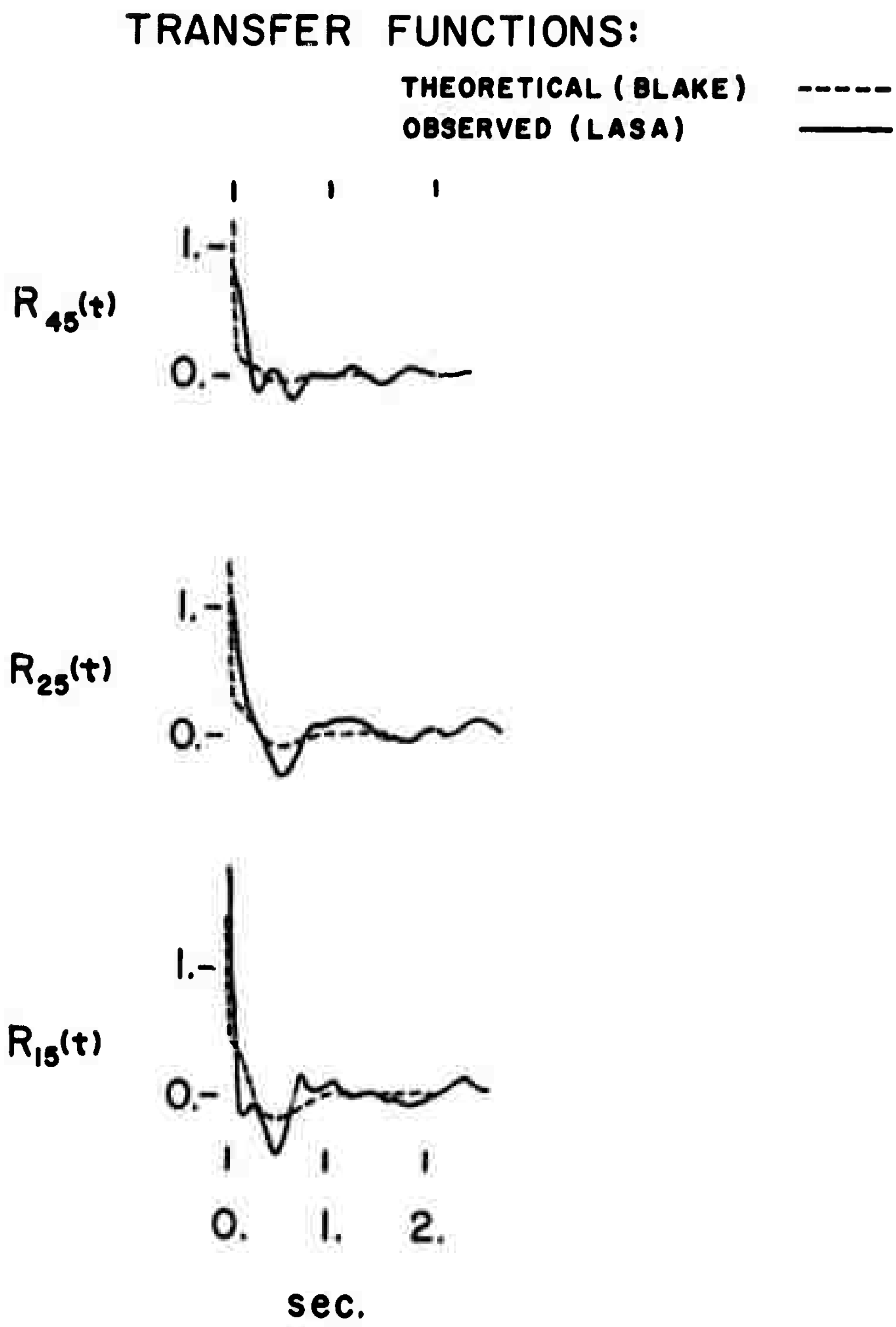

Figure 42. Comparison of Theoretical and Observed Transfer Functions.

It is telling you that the energy content at low frequencies, lower than one cycle where we are normally measuring magnitudes, is pretty independent of the magnitude over that range where you looked. Whereas all of the variations you are looking at are really ones that are of a higher frequency than one cycle.

MR. FRASIER: Yes, I think so.

MR. ALEXANDER: Even this scatter that you are getting on the ripples.

MR. FRASIER: I am working with John Filson on this. He was the one who suggested to Evernden that the reason that the m_b versus yield curves for explosions in hard rock taper off at high magnitudes, whereas the M_s versus yield does not, is that the overshoot in the Haskell displacement spectrum moves across and out of the frequency band of short period instruments. This causes the m_b at 1 Hz to increase slowly relative to M_s for large yields. In other words Haskell and Blake's solutions have frequency-dependent displacement spectra which cause m_b versus M_s to not fall on a straight line at high yields.

Now, this ignores all of the problems Ted Cherry was talking about, the Sahara shots and whether the source rock is cracked or not. But you can take a lot of points and plot them, and notice this type of thing for hard rock. So this is another possible explanation. Of course, none of these things is unique. It is probably a combination of all of these factors that cause this. John Filson does not think it is the water table that is causing this turnover, but simply as I said, this effect of moving the Haskell spectrum through this narrow-band instrument.

MR. SMITH: You are saying that some of the source characteristics, such as cavity dimension, that information is preserved in the spectrum around one cycle, whereas Shelton is saying that none of the source characteristics, or very few of them, are presented at 0.05 cycles, that it is all path.

MR. FRASIER: Of course, he is looking at the surface waves. But you notice also that when he goes to another station his spectra change.

MR. SMITH: That is all path effect.

MR. FRASIER: Yes. I completely eliminate the path, because I deliberately divide out all of the stuff I don't know.

MR. ALEXANDER: Each individual plot that I showed had that same characteristic. They were all the same station, same path, so each individual one was analogous to what you have done to the short period. This was a somewhat different result. I get completely consistent spectral shapes, and you do, too, up to about one cycle, and then they start to vary from one shot to the next.

MR. FRASIER: These are the types of problems that we are stuck with in short-period seismic information. I think that a next step is to estimate the effect of free surface over the source. This should be done numerically. In short-period data, attenuation, spherical spreading, and layering seem to distort the data more than for long-period data so that we have a very hard time using the absolute signals to determine source parameters.

COL. RUSSELL: Thank you very much, Clint.

Next we are going to take a look at Questions 5 and 7 on your list, and Nafi Toksöz will speak on those points.

CONVERGING CLOSE-IN AND FAR-FIELD CALCULATIONS

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Massachusetts Institute of Technology

The problem in bridging the gap between the near-source calculations and the far-field studies which are trying to come back toward the source, is that a common ground has not been reached. Theoretically the seismic observations made at some distance from the source, when corrected for all of the propagation effects, should give ideal source properties. Furthermore these properties should agree with what one computes starting at the source and taking into account the explosion history and the behavior of the medium.

The reasons for the lack of complete convergence between the inward and outward approaches are manyfold. Some are connected with the close-in phenomena and what happens to the pressure pulse within a few kilometers of the source. Others are related to the far-field observations and the propagation effects on the seismic pulse. It is not possible to correct for the exact medium response.

Still another complexity, which we will not go into at this moment, arises when we compare earthquakes and explosions in conjunction with discrimination phenomena. We know quite a bit about the explosions, both theoretically and from measurements. For the earthquakes, our knowledge of the source is very sparse. There has been no direct measurement of ground motion at the hypocenter nor has there been an exact theoretical formulation of the source. Progress is being made in these areas theoretically, by improved modeling and numerical calculations as well as by expanded and improved field measurements.

Let us get back to the explosions and start from field observations. What are some of the difficulties that we face as seismologists in getting back to the source? Those who are working with seismic surface waves face a number of things. If the medium (crust and upper mantle) can be characterized by plane, parallel layers of known velocities and densities, we can compute the amplitude and phase responses and determine, for example, what a Rayleigh wave should look like at a distance of 1,000 or 5,000 km. Inversely, given a surface wave observed at some distance, we can theoretically correct for the propagation effect and get back to the source. The limiting factors here are the insufficient knowledge of the structure and deviation from plane, parallel layering. Generally we do not know the velocities and densities exactly as a function of depth, and we do not know the attenuation properties of the medium. The problems of lateral nonuniformity of the structure (where the layers are dipping, the surface topography varying, and the velocities varying laterally) introduce theoretical limitations. In these areas very little progress has been made in exact computational schemes. Some calculations have been made for Love waves and some are being carried out for Rayleigh waves, but these are still far from modeling all the crustal heterogeneities.

For a moment let us look at problems of seismic body (P and S) waves. For a point source in a laterally homogeneous earth (the parameters vary only as a function of the radius), we can compute the exact geometric spreading. We can include the attenuation effects if Q is also known. Thus for a given source function we can compute theoretically what the m_b value should be. Inversely, given an observed P wave, we can determine the pulse at the source.

The difficulties arise if we do not know the layering exactly, if there are very sharp variations in the velocity, and if there are lateral heterogeneities. If one adds to this some of the near surface complexities that affect the pulse shape as Clint Frasier showed, then this problem becomes more complicated.

As more observations become available, the effect of attenuation gets to be more and more significant. Earlier this morning Shelton Alexander mentioned how the Q might affect the body-wave magnitudes. In North America, as a result of variations in attenuation in the upper mantle, the body-wave magnitudes may vary by as much as 0.3 to 0.5 magnitude units. Because of attenuation effects, an NTS event would have a lower body-wave magnitude than the identical source detonated in a shield area. Similar problems apply to the observing station sites.

At the moment there are a number of organizations working on collecting data and evaluating the effects of these factors on the magnitudes of explosions as well as earthquakes.

The most important aspect of this conference is that investigators working with the far-field data and those working with the near-field measurements and computations are present. From the information that I have been able to gather, the code calculations go to a certain limit and beyond that the medium is assumed to behave elastically. The problem for seismological purposes is that the computations are not carried far enough.

In seismology we deal with the wave equation, derived with certain assumptions. One of the most important points is that the strains are assumed to be very small. This limits how closely we can approach the source from data recorded at far field.

We would like to see the code calculations extended radially far enough so that the strains due to the explosive source become very small (i.e., 10^{-5}). If this can be done, then the seismological and code calculation results can be compared directly.

The second problem that we must face results from the complexities in the near field. If we assume homogeneity of the medium for near-source calculations, we ignore a number of geologic factors. Examples of these are: crack formation and growth, and movements and adjustments along existing faults and boundaries.

A third problem that comes to mind is what happens if there is existing stress (prestress) in the medium. What happens to the radiated energy? These problems have to be ironed out if we expect to be able to match near-field measurements and far-field results.

Let us now go into the far-field results. Without going into details, let me show the source-time function for a typical explosion--Bilby (Figure 43). This is the source-pressure function at, what we call, the boundary of the elastic zone, that is, the hypothetical zone where the medium is behaving elastically and the strains are very small. To obtain this, the observed Rayleigh waves have been corrected for all of the propagation effects and then carried back toward the source. This pulse is similar to what we saw before from Bill Perret's data, except it may be decaying a little more rapidly.

MR. SMITH: This is an assumed pulse form for which you have fitted the parameters? This is a perfect fit with an assumption about what it should look like?

MR. TOKSOZ: It fits the amplitudes exactly and fits the phases to the accuracy that we have. The pulse form has been characterized by $p(t) = te^{-\eta t}$. η is a parameter we have varied to fit the data.

MR. COOPER: How big is the cavity radius in this problem?

MR. TOKSOZ: We assume a point source, but this time function would correspond to the pulse at distances larger than a wavelength from the source. The main reason for this is that we take the asymptotic expansion of $H_0^{(2)}(kr)$ and neglect terms of the order $(kr)^{-3/2}$ or smaller. Note that $k = 2\pi/\lambda$ is wavenumber and r is horizontal distance.

MR. RINEY: We should be able to determine that, shouldn't we--the equivalent elastic radius from the reduced displacement potential? Shouldn't there be some relationship between these two?

MR. ALEXANDER: Aren't you saying that if you assume a point source and this goes into the elastic zone, it matches up with what the actual one does out in the elastic zone?

MR. RINEY: That is right.

MR. ARCHAMBEAU: You are making kind of a misleading argument when you say point source. You are using Sharpe's solution for something aren't you?

MR. TOKSOZ: No, we are not using Sharpe's solution.

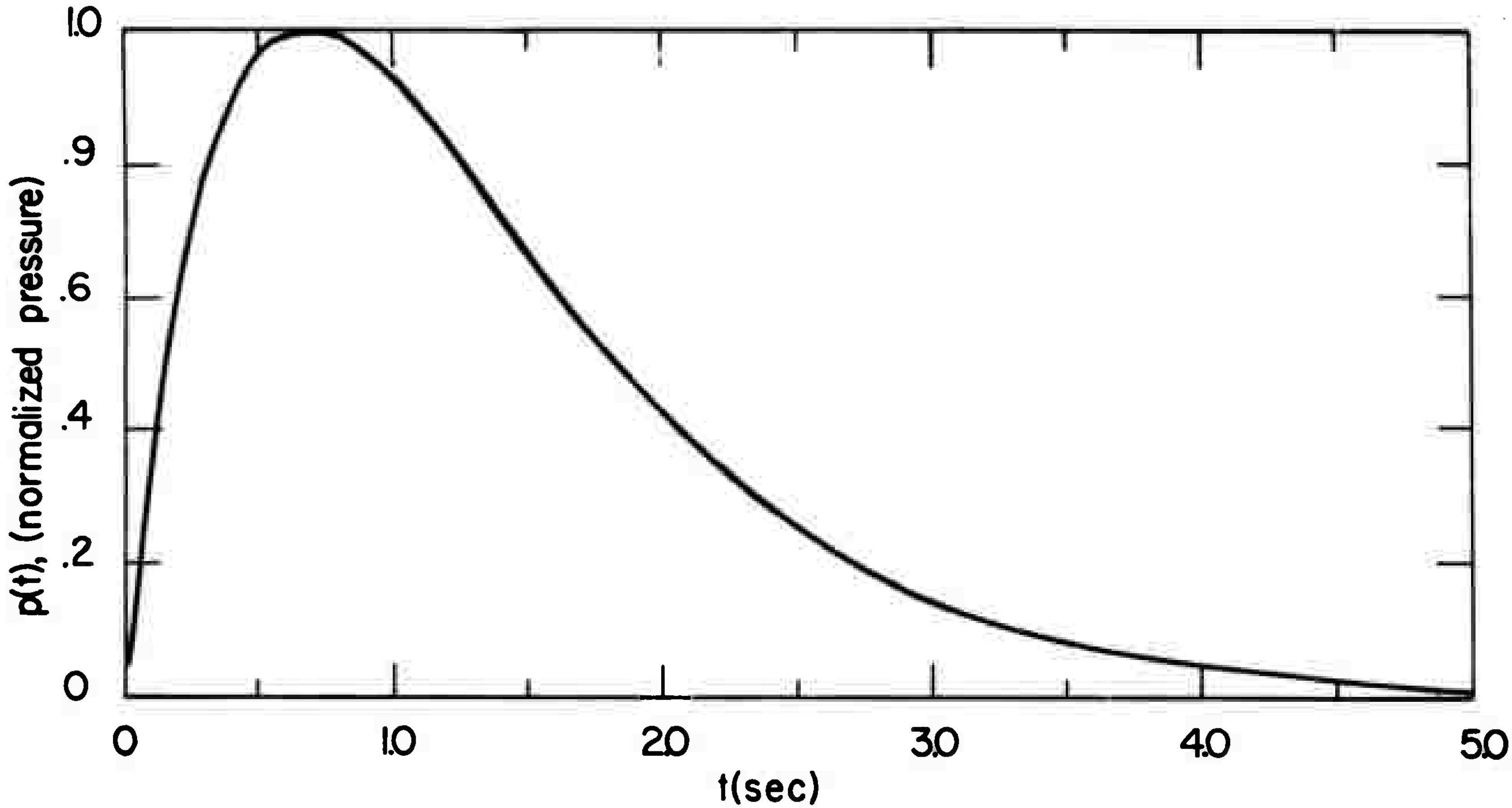

Figure 43. Source-Time Function of Bilby Explosion.

MR. CHERRY: Then the pressure has to be equal.

MR. COOPER: Oh, this is your result of equalizing back?

MR. TOKSOZ: Yes. To where the asymptotic expansion is valid, such as one wavelength away.

MR. COOPER: My question really has to do with the situation of a finite cavity in elastic material, loaded by some pressure pulse. If the pressure pulse duration is reasonably long with respect to the cavity response time, it does not matter what the pulse shape is anyway. The cavity response controls the far-field response. So I am still confused as to what the cavity radius in your analysis means. Is this a cavity radius to which you apply a pressure-time history? Do you treat it as if no signals reflect from the cavity wall?

MR. TOKSOZ: We take a small, hypothetical cavity, and this is the pressure we put at this cavity. Once the pressure is applied, the cavity is assumed to disappear. It does not oscillate nor scatter waves.

MR. RINEY: Could you define for us nonseismologists exactly what you mean by point source? Isn't that our problem around here?

MR. COOPER: Consider a step pressure on a finite cavity wall. The far-field response to this step pressure is really not very different from that associated with some decaying time history whose positive phase is reasonably long with respect to the response time of the cavity. I am trying to understand why the loading pulse shape matters.

MR. TOKSOZ: Your first statement is exactly what we did.

MR. COOPER: I see a pulse shape here.

MR. FRASIER: It is normalized pressure.

MR. RINER: It depends on time, so it is not a step pulse.

MR. COOPER: Somehow it does not depend on the cavity radius. I don't understand, because the cavity radius (in terms of the Blake solution or the Sharpe solution) appears in the far-field response.

MR. RODEAN: And determines the frequency spectrum.

MR. COOPER: Yes, it does. It determines the frequency spectra rather independently of the detail of the pressure pulse on the cavity wall, I think.

MR. TOKSOZ: Let us take the case of a layered half space, and you have a relatively small cavity, a cavity that is small compared to the wavelength. Then you put in a pressure pulse, and immediately afterwards you remove the cavity. Thus the cavity does not oscillate nor do you have the waves coming to the cavity and scattering.

MR. ROTENBERG: This is the pressure signal that you are putting on the walls of that little cavity.

MR. COOPER: I still don't understand. The cavity radius, if I remember the problem, is one of the dominant determining factors of the response in the frequency spectrum.

MR. ARCHAMBEAU. You take the radiated field and you just divide out the propagation effects in a layered medium.

MR. HARKRIDER: All the way back to the source.

MR. ARCHAMBEAU: In the radiated field you get a 1/R singularity because you expand the field outside the source zone. You just took out the 1/R, so you have A (the elastic radius) in there somehow. The real A has to be in this equivalent time function. How do you use this with Sharpe's, then?

MR. SMITH: At the risk of confusing things, I understood that this cavity is made so small that all of the oscillations that come out of an analytic solution are all much shorter periods than that, and really are not of any concern. It is this long-period waveform that is controlling what we see at great distances. In fact, it does not matter a lot what short-period things you superimpose on that, because you don't see them at a distance anyway.

MR. CHERRY: I think the question is what is it, what does that thing really represent? If you are asking us to give you something, you have to tell us what to give you, and to just put a curve like that up on the board and say, okay, this is what we want, does not help.

MR. SMITH: No, he did not say that. This explains what we see at great distances.

MR. ARCHAMBEAU: Yes, he is going to say what he means later.

MR. COOPER: But I don't understand what that is.

MR. ARCHAMBEAU: If you consider a spherical cavity, and distribute on the cavity a pressure like that, take this configuration to the limit as the sphere becomes very small compared to the wavelength, then this source will reproduce the long wavelength field. That is what they have done.

MR. SMITH: That is an equivalent source for what kind of a far-field situation?

MR. RODEAN: Surface waves. This is very interesting because you (Harkrider) first published this in a series of two papers on Hardhat and other events. What you are saying is that when you take this mathematical model, which you described in your BSSA paper several years ago, and you calculate backwards to determine source, you get an impulse function for your pressure--that is sort of a pressure impulse inside your cavity, and let us forget about how big the cavity is for the moment. If we take Ted Cherry's calculations for the close-in elastic response, or Bill Perret's measurements, there we get, instead of an impulse function, a step function as the dominant input to the equivalent elastic system.

MR. FRASIER: It is a band-limited step function. You see, he does not have infinite frequencies. He is stuck since his instruments only have certain bands.

MR. RODEAN: On the other hand, if you just throw away the long-period frequencies in a step function, then you are left more with that.

MR. TOKSOZ: From all the preceding questions and discussions it is clear that there is some confusion. Let me try to explain the problem again. There are several factors that must be considered: (1) the shape of the pressure pulse we apply to the small, hypothetical cavity; (2) the geometric effect of what happens to a spherical wave as it propagates outward; (3) rheological effects of the medium on the pulse; and (4) long-range propagation effects of the layered medium on Rayleigh waves.

In this study, all we correct for is the long-range propagation effects. Thus the pulse shape we obtain incorporates in it the shape of the pressure function, geometric effects near the source as formulated in Sharpe's or Blake's solution, and the attenuating effects of the medium as described by the stress-strain curve. If one measured the radial stress at a distance of about 20 km from an explosion source, we contend that it should look like our pulse.

MR. RODEAN: Isn't another thing that is happening here is that this source puts out only compressional waves or body waves? But what you are looking at at a distance and then calculating backwards from is the result when some of these body waves have been somehow converted into the surface waves in your layered medium?

MR. TOKSOZ: That is one way of looking at it, but a source of pure body waves in a half space will generate surface waves.

MR. CHERRY: Would it be fair to say that the units on that normalized pressure might be stress times distance?

MR. COOPER: How much difference would it make in terms of your Rayleigh waves and the rest of the solution if you were to put a step function in as the source function, as opposed to the curve that you actually have used?

MR. TOKSOZ: We cannot explain the observed data. To the best of our knowledge the observed spectra has more higher frequency components than you would get from the step response in the period range of 10 sec to 40 sec.

MR. ALEXANDER: This is due to the pressure history on the wall of the cavity, right?

MR. TOKSOZ: Don't mention the cavity, because we have different interpretations of the cavity.

MR. HARKRIDER: As far as convenience goes, as to your question of what we would rather have, we would rather have the reduced displacement potentials for the outgoing waves. They are easier to work with, but I can take the pressure if you have it.

MR. CHERRY: In the linear zone.

MR. HARKRIDER: In the linear zone. I would rather have that of all of the things.

MR. CHERRY: Sure, but you would also like the reduced pressure.

MR. ARCHAMBEAU: This is just the time function.

MR. RODEAN: What you are saying is that the DC value does not seem to make much difference for your surface waves.

MR. SMITH: It can't, because it is a band-limited system. It does see zero frequency anyway. I think you can look at this, and this is simply a function which if you put it into this operator that describes the response of the layered earth, what comes out is the seismogram. So this is one of a collection of an infinite number of possible functions at the cavity which would give the same seismogram.

MR. RINEY: But if this is due to different models of his layered earth, you would get a different function.

MR. ALEXANDER: No, he has equalized out the earth, and it leaves this function.

MR. SMITH: Say something about the frequency range that you used for the inversion, because I think that is pretty crucial.

MR. RODEAN: What is your bandwidth of surface waves that you used to get that?

MR. TOKSOZ: 10 sec to 40 sec.

MR. RINEY: I am still confused. This comes out, and if you wish it is implied using your layered model. I was just curious about the sensitivity of this implied function from the variations in the layers, thicknesses, and parameters that you assumed for layers, and energy or anything else that you put into your model, would you always come out with this function?

MR. TOKSOZ: This function depends on the layering response, such as the layer parameters, but for any realistic thing within the general range of layers that we have, it is relatively insensitive to small variations in the layer parameters (velocities and densities), if our assumption of flat parallel layers holds. The second thing is that we use the phase or the dispersion properties of the medium from phase and group velocity data, we know within certain bounds what the structure is, and within these bounds the amplitude response is not going to change very much.

MR. RINEY: The second question I have is, did I understand you to say you would just as soon have the reduced displacement, but you would like to have a different one from the one we gave you, because it is not consistent?

MR. HARKRIDER: I don't know. That is what we are here to find out. I just make these things. They look at them.

MR. TOKSOZ: The thing we would like to have is for you to give us the reduced displacement potential at some distance like 10 km from the source itself, and also computed not only to half a second, but to about 5 sec or 10 sec.

MR. RINEY: That would be just as good to you.

MR. TRULIO: How about giving it at the farthest range at which material ever becomes inelastic, and that might only be ten cavity radii.

MR. TOKSOZ: Inelasticity is not sufficient. The assumption we make is very, very small strains, so you can drop those. We do not know the attenuation behavior or the material behavior for finite amplitudes, and the material will not break down. It will still behave elastically. Whether it would continue on attenuating heavily like we saw in Bill Perret's data is a function of distance. If this excessive attenuation is still taking place, we know that the pulse shape is still changing. The calculations must be carried out to a distance where the pulse changes very little, if any, in successive steps.

MR. ALEXANDER: The observed spectra I showed for Rayleigh waves all had the same shape where the path is fixed and you have different sized events in various media. Therefore, whatever the code calculations tell you, they should tell you that these long-period signals ought to look alike for all of these different media.

MR. CHERRY: As far as the Rayleigh wave is concerned, I think that is certainly reasonable. I don't see why periods that long ought to be sensitive to how many beer cans you throw in the emplacement hole. You have a cavity 20 m in radius versus a cavity 5 m in radius. Why should wavelengths as large as the Rayleigh wavelengths be sensitive to a cavity that size?

MR. COOPER: That is exactly why I asked about the detail of the pressure pulse in the first place.

MR. ALEXANDER: That is what he is talking about. He is getting back those low-frequency components in terms of some time function you could put in there equally well with the explosion, and get out the same thing.

MR. ARCHAMBEAU: It is not the cavity size. It is the radius of the nonlinear zone that matters.

MR. RODEAN: The elastic radiator.

MR. SMITH: I think there is a big difference of opinion about that zone. I think the zone may be as large as 10 km in some cases, because any time that you have, for example, surface cracking and permanent strain offsets, then demonstrably you are in the nonlinear zone. I think one of our basic differences of opinion is the size of this zone. I think the seismologists would tend to assign a much larger zone.

MR. TRULIO: Also, present models of these materials, including their inelastic behavior, lead to no magic effects at the boundary of that zone. If, as a driving condition for seismic motion, you were to use the history of motion at a smaller range than the boundary of the nonlinear zone, you would not see much difference in the seismic waves generated by a given explosive source.

MR. SMITH: Oh, right, if we knew what attenuation and constituent relationships to use in there, yes, but we are using information about the earth determined from infinitesimal strains, so we would like to be out in the region where those are valid, and maybe strains of a tenth of a percent are too big for those to still be valid.

MR. TOKSOZ: I would like to mention here that the pulse shape we computed depends upon both the shot size and the medium itself. Figure 43 happens to be for Bilby. For a shot in a granitic medium (Hardhat for example), the pulse shape would be somewhat sharper. For alluvium, it would be broader. The dependence is not a very strong one. For a given medium, the pulse for a large explosion is broader than that for a small explosion.

Figure 44, taken from Bishop, shows schematically what happens near the source. There is crushing, extensive cracking, and finally radial cracking before we reach a zone of no cracking. If the material were perfectly homogeneous there would be no problem, but in cases of either existing weaknesses (pre-existing joints or faults) or a prestress field, the cracking will tend to follow the direction of least resistance. It could extend to fairly great distances.

In Figure 45 we give the result of a laboratory test of a shot in prestressed medium. We conducted a set of experiments at Stanford Research Institute putting explosive sources in glass plates and observing both the time history of cracking and the radiation of P waves and S waves. In the cases where plates were unstressed, the cracking pattern and the history looked very much like the theoretical case that was illustrated in Figure 44. But in the stressed medium, initial cracking near the source started in all directions. As the shock wave expanded, however, the radial cracks which continued to grow were directed by the direction of the stress. Figure 45 is the case for about 150 bars of tensile stress, and here the extensions of the cracks are illustrated. The light areas show where there was radiation taking place, and the darker areas correspond to relaxed zones where there is no radiation. So one sees the extension of these cracks, and the contribution of the radiation from these cracks into the total energy field.

The reason we are interested in cracking phenomena is that we are very anxious to understand the mechanism of the SH or Love-wave generation by some explosions. Just about all of the explosions that were denotated in hard medium showed some sign of Love waves. Theoretically if one takes a half space with a perfectly symmetrical source, one does not get Love waves. Then something else must be happening in the source region, and Dr. Archambeau will probably discuss the theoretical aspects of this subject tomorrow. The prestressed medium and in particular the region where extensive cracking and relaxation of the medium takes place are extremely important. If the code calculations can be carried out for the cases where there are existing heterogeneities or prestress fields, taking into account the cracking strength and other properties of the material, these could contribute significantly to our understanding of the problem and the interpretation of Love-wave generation.

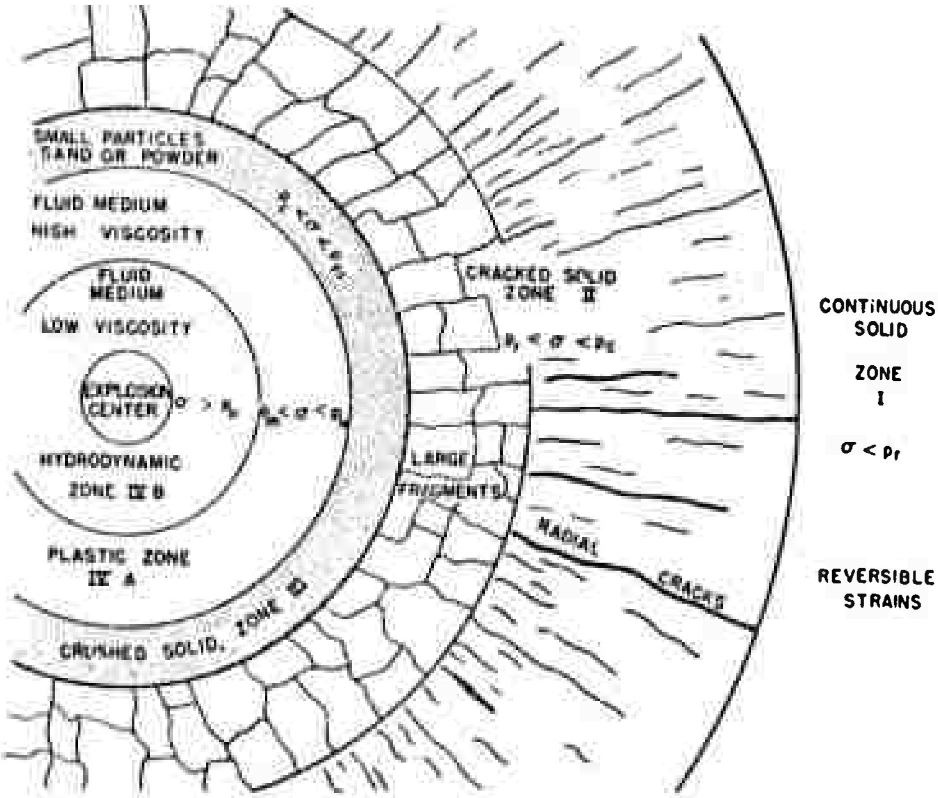

Figure 44. Schematic Diagram of the Source Region of an Explosion.

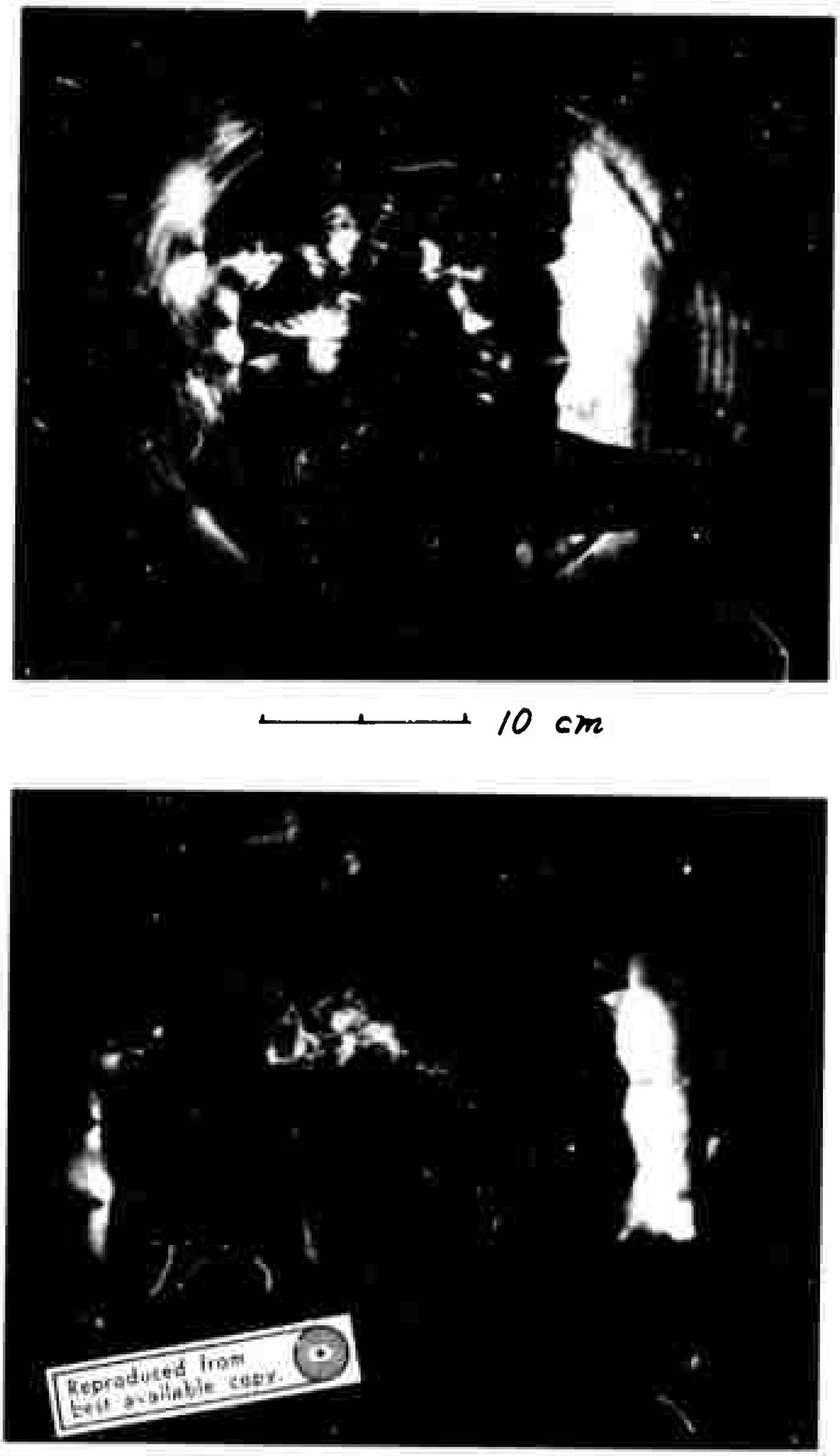

Figure 45. Cracking Due to an Explosion Source in a Glass Plate Stressed Under Tension (114 bars). Stress axis is up-and-down direction. Note the growth of the cracks in a direction perpendicular to the stress axis.

The generation of Love waves by the explosions is illustrated in Table 5. Most of the events are large explosions. The reason for this is that at large distances, it is much easier to get a better signal-to-noise ratio. The important factor in the table is the relative strength of the double-couple component (strength of the Love waves measured relative to the explosive strength). This is denoted as the F value and is proportional to the square root of the energy ratio of the double-couple component to the explosion.

We tried to correlate F values with a number of phenomena. One outstanding result is that F seems to correlate with the medium strength. For explosions in granite (Hardhat, Shoal, and Piledriver) the F values are greater than 0.90. Then comes the rhyolite in the range of about 0.95 down to about 0.6, and then the tuffs anywhere from about 0.55 down to about 0.3 or 0.4, and then below that are the alluviums, which from one example we have (Haymaker) is about 0.3. The explosions in loose alluvium (Sedan) and in salt (Salmon and Gnome) generated no Love waves.

We know that the generation of these waves is taking place in the source region. Shelton Alexander gave a good example and a good justification for that. We do not know the exact mechanism of Love-wave generation, although the laboratory results give us some ideas. Theoretical calculations for these cases, especially near the source, to my knowledge, are nonexistent. If code calculations can be made, they may help us understand the seismic observations.

That is all I have. Now we can entertain questions.

Table 5. Source Characteristics of a Sampling of Underground Nuclear Explosions

EVENT	MEDIUM	(F) DOUBLE COUPLE STRENGTH	FAULT AZI.
Piledriver	Granite	3.20	340°
Hardhat	Granite	3.00	330°
Shoal	Granite	.90	346°
Chartreuse	Rhyolite	.94	353°
Duryea	Rhyolite	.75	355°
Halfbeak	Rhyolite	.67	345°
Boxcar	Rhyolite	.59	346°
Greeley	Zeol. Tuff	1.60?	355°
Benham	Zeol. Tuff	.85	345°
Corduroy	Quarzite	.72	347°
Cup	Tuff	.55	200°
Bilby	Tuff	.47	340°
Tan	Tuff	.39	347°
Buff	Tuff	.31	208°
Bronze	Tuff	.33	185°
Faultless	Sat. Tuff	.50	344°
Haymaker	Alluvium	.33	340°
Sedan	Alluvium	0	-
Salmon	Salt	0	-
Gnome	Salt	0	-

MR. COOPER: I don't like to beat a dead horse, but Shelton started off the morning by showing us data that suggested Rayleigh waves and surface waves were relatively insensitive to the source region. We think we understand why that is. Yet you say that by theoretically looking at those differences you can come back with a rather detailed pulse shape for a source function to represent the seismogram at large distances. I am wondering how much difference it makes if you vary that pulse shape widely? If I believe what I heard you say, and it seems reasonable, I would not expect the Rayleigh waves at far distances to be terribly influenced by the pulse shape. I would like to know how the energy gets coupled consistent with this intuition.

MR. TOKSOZ: If one compares the spectra of Rayleigh waves from different explosions (Sedan and Haymaker, for example), one sees the difference. If one takes Haymaker and compares it with Hardhat, one would see a difference. In all of these, the similarity of the spectrum does remain, but there is a tendency with the larger explosion in a given medium to go more to the longer periods.

MR. RODEAN: You mean the longer, more drawn-out source function?

MR. TOKSOZ: More drawn-out (broader) source function.

MR. CHERRY: Are those data from the same station? Was the source station held invariant while the pulse shapes were determined?

MR. TOKSOZ: In some cases, yes, like Sedan, Haymaker, and so on. These data were obtained from the same four stations in four different quadrants.

MR. ALEXANDER: The differences in the actual spectra are not really great. They are there, but they are not pronounced. What percentage variation would you expect?

MR. TOKSOZ: The percentage variations that would exist for all of the shots that we looked at, the maximum variation actually is $\omega/(\omega^2 + \eta^2)$. The frequency range we are talking about is 0.1 cps to 0.025 cps, and this value of η seemed to vary somewhere between a minimum of 0.8 to a maximum of about 1.6 or so in the cases that we have looked at. So it is a very slow variation, and this parameter does not change very much. But there is something indicating that longer periods are associated with the larger explosions.

MR. TRULIO: In the computational models we use for close-in calculations, there is almost always at low enough stress a linear elastic regime of behavior, and we do typically run the calculations to a time late enough to get at least the last material activated in that regime. Why isn't such a final calculated field sufficient to define initial conditions, or input, for a calculation of far-field radiation?

MR. TOKSOZ: First of all, when you are computing the reduced potential as a function of time, what is the longest time (maximum t) you compute these things for?

MR. TRULIO: That will change with the medium. We run them typically until the strains that are taking place in the materials are purely elastic.

MR. HARKRIDER: The potential then just decreases at 1/R.

MR. TRULIO: Yes, if you are talking about a spherical calculation now....

MR. COOPER: How far did you calculate Piledriver, for example?

MR. TRULIO: We were really looking to compare with just two gage stations, so that is probably not a good case. But we carried Diamond Dust, for example, out to a time of several seconds, scaled to a kiloton. That takes the field of motion a long way into the elastic regime.

MR. HARKRIDER: In other words, it was propagation as an elastic wave.

MR. TRULIO: Yes. As a matter of fact, to save computing time, we don't extend the region of calculation much beyond the eventual elastic-plastic boundary, that is, the ultimate range at which inelastic deformation takes place for a given material model. What we do is compute for a distance a little bit greater than that range, and use the fact that the source is in the interior of the region of calculation to avoid calculating all of the exterior mesh points that you would otherwise put in a finite-difference calculation.

MR. TOKSOZ: I think this is important.

MR. TRULIO: You mentioned something about not being sure of the attenuation factors until you get to almost infinitesimal stresses. Is that a limitation? You could have linear behavior taking place, and not be sure of the linear dissipation that the medium would produce. How low do we have to go in stress? Maybe that is a way to raise the question. How low do you have to go in stress before you feel you understand the earth as a medium for wave propagation?

MR. TOKSOZ: Strains of 10^{-4} to 10^{-5}?

MR. TRULIO: Even if the material is behaving linearly elastically, at least under static conditions at strains of 10^{-2}?

MR. ARCHAMBEAU: Yes, I believe so.

MR. CHERRY: The thing that sort of worries me now is not so much the elastic assumption, but the adiabatic assumption that is in all of the codes, the fact that there is no heat transfer taking place.

MR. ARCHAMBEAU: How much basically do you trust the results coming out of the code?

MR. TRULIO: I think we have the same problem you do. It is modeling the medium.

MR. ARCHAMBEAU: Things like cracking and so on.

MR. TRULIO: Yes, the main problem is to model the mechanical properties of the medium.

MR. ARCHAMBEAU: What if the medium is prestressed, say?

MR. TRULIO: Then I would say the best results are obtained for soft rock or soil.

MR. ARCHAMBEAU: Sure.

MR. TRULIO: That is probably where most attention should be put right now.

MR. RODEAN: But if you talk about calculations of explosions in prestressed media, then you are probably talking about three-dimensional calculations.

MR. ARCHAMBEAU: Or, say, two, two-dimensional ones.

MR. RODEAN: But how real would they be?

MR. ARCHAMBEAU: I am sure the shear fields in Nevada have two dimensional symmetry and have an axis.

MR. HARKRIDER: Could you just extrapolate it using wave theory?

MR. CHERRY: Yes, you could extrapolate from the source region on out.

MR. HARKRIDER: There is no sense then going any further, because they have reached that point.

MR. COOPER: If you assume it is elastic at some point, then you can solve for elastic response in a straightforward manner.

MR. TRULIO: I think a useful case might be one in which, within the farthest range of inelastic behavior, you don't get return signals so early that the material inside that range is still deforming inelastically. What I mean is that there may be some interesting cases where interactions with interfaces and the ground surface can all be calculated as linear wave processes, because after a short time, all of the stresses have decayed to the point where nothing is deforming inelastically any more. Some material may have flowed plastically early, but not everything is behaving elastically again.

MR. ROTENBERG: You are using a different material.

MR. TRULIO: Yes, the material is changed by its deformation history. It has flowed plastically and so on. It finally becomes a material that behaves elastically, and you know its state from a fairly early time on.

MR. FRASIER: Could we see some time histories of some of these maybe tomorrow?

MR. TRULIO: Yes.

MR. FRASIER: At a previous meeting in April people showed that one code would often not be consistent with another code calculation. If we could just see a couple of time histories of pressure or velocity it would give us more of a feeling for the solutions going into the elastic zone.

MR. TRULIO: But you have given us some feeling. 10^{-4} or 10^{-5} is where you want to go in strain before you trust the models you have.

MR. ARCHAMBEAU: In reality things are stressed and quite heterogeneous, and if you have strains any bigger, you get a lot of movements along joints, and so on. Essentially the wave does work on the medium, so you have another mechanism. This would not be explicitly considered in your codes. That is, you have other mechanisms that are fully outside the scope of your code for dissipating the energy of that wave as well as being outside the scope of our calculations, so we want to get off where the strains are small enough that these effects are not going to be important.

MR. TRULIO: If you describe it that way, that you have slippage along faults and so on, it may turn out, as Howie says, three dimensional. Of course to calculate such motion is not practical at present.

MR. ALEXANDER: As I recall, 2 or 3 yr ago, and I have not kept up with your code calculations that carefully, the real place where the uncertainty lay was in being able to predict accurately the fracture zone and the extent of the fracture zone. How well can you in fact do that from code calculations in terms of results of post-shot drilling? How well do these codes work to estimate the extent of fracturing and crushing and that sort of deformation?

MR. CHERRY: I can tell you that we missed the Gasbuggy chimney by 3 ft or something like that. I don't know.

MR. PERRET: Ted, did you miss that by 3 ft from my calculation for it or where it was measured?

MR. CHERRY: No, it was by the drilling.

MR. RINEY: Let me ask one question. Our reduced displacement potential this morning in comparison to Bill's measurements, there was the peak that could not be explained by the calculation ....

MR. CHERRY: Yes, that is really a puzzle.

MR. RINEY: That is a puzzle, and I would think that you would agree that those are probably some of the better calculations being made by code, so it must be that the codes have difficulty modeling what was actually physically there as proved by the measurements.

MR. CHERRY: As far as the fracture radius is concerned, I am not so very much worried about that, but right now I am more concerned about the details in the pulse shape, like that initial overshoot. I guess I am forced to believe that I just don't do that so well. I think that may be more a function of how we start the problems off, rather than a lack of an equation of state.

MR. ALEXANDER: But in determining the far field there, I doubt that bump is going to make an awful lot of difference.

MR. CHERRY: For your problems it may not.

MR. TOKSOZ: I think it does.

MR. ALEXANDER: That is sort of the direction you are coming to.

MR. TOKSOZ: That is true. Our results are converging closer to some of the large explosions that Bill Perret presented than to some of the code calculation results.

MR. RODEAN: One other thing, too: A paper by Ben Tsai which he gave at Woods Hole (there is a preprint of it around by Tsai and Aki). He used two different reduced displacement potentials: one of them sort of oozed up to a steady-state value and was probably based on a tuff measurement, and the other one had an overshoot in it like the Hardhat measurement. It was the one with the overshoot that did show a yield-scaling effect as far as surface-wave spectra was concerned. Not much, but that was the only one that seemed to do it.

MR. CHERRY: Is that good or bad?

MR. RODEAN: I don't know. I am just saying that is what this guy got.

MR. TRULIO: The models are far from complete, too. There just isn't anybody that I know of now who has a dispersive model for hard rocks, jointed and cracked; dispersion from cracks and joints is left out entirely. Whatever wave shape changes take place because of elastic reflections from boundaries does not appear in the calculations, and neither do the effects of simpler kinds of inhomogeneities, like a large inclusion of a kind that would diffract waves.

MR. RODEAN: Furthermore, in the codes we are also affected by the zone size, and then the artificial viscosity starts getting in there, too.

MR. TRULIO: Yes, those things are controllable, but I think there really are some gaps still in the models. Maybe the best way to go about plugging them is to make sure you can model materials like tuff, that are not as complex as cracked rock. I think that to start with NTS granite is probably to start with the most difficult phenomena exhibited by the spectrum of geological materials.

MR. TOKSOZ: Once you get to what you call the elastic zone, you no longer have energy loss in the medium.

MR. TRULIO: That is right. With present material models you can compute far enough from the burst point and far enough in time, so that material that is being disturbed for the first time does not get stressed enough to make it behave inelastically. You can build the models in such a way that there is no level at which material will ever behave elastically, but that is not the way we build them. An example of a not-so-simple kind of dissipation would be hysteretic behavior at any level of stress. You might load with one hydrostatic stress-strain slope and always unload with another--but we don't model materials that way.

MR. TOKSOZ: What Bill showed, if I remember correctly, for Salmon, (even at the distances of 620 and 740 m), there was still a sizeable loss of energy going from one distance to the other. If we assume that the material is lossy (attenuating), this means the pulse shape changes unless this attenuation is very, very small. If the calculation is not carried to the zone where the attenuation is very small, this means one would get a change in the pulse shape.

MR. ARCHAMBEAU: That is what he is saying, that you take it out beyond that.

MR. PERRET: I don't think we can measure with our kind of instrumentation motions that will be in the region where the strains are of the order of 10^{-4} or 10^{-5}. I don't think we can bring our kinds of instruments down that low.

MR. ARCHAMBEAU: This is the insensitivity of the instrument?

MR. PERRET: I think so. These are instruments that are built to respond to something a lot bigger than that. We have operated surface stations on Jorum and Handley using logarithmic amplifiers, and got down in the neighborhood of 10^{-3} g's, which is two orders of magnitude below signals from our linear amplifier systems. But we have never put them in free-field instrumentation.

MR. BROWN: We have the same problem with just laboratory measurements on rock properties. They are not made down in these stress levels where you are talking about 10^{-5} strain. If you consider the modulus of a million psi, 10^{-5} strain occurs at only 10 psi. We don't make measurements there, and I don't think Handin makes any.

SEISMOLOGISTS REQUIREMENTS IN TERMS OF BOTH OBSERVATIONS AND THEORETICAL CODES

Charles B. Archambeau
California Institute of Technology

Let me start by summarizing what we think we need, and then I am going to talk briefly about some of the spectral properties of the seismic field that we observe, and then mention some of the discriminants. I am going to try to keep this discussion fairly short, and depending on what kind of questions you have, I or some of the other seismologists can elaborate.

I think we said a couple of times that what we want, or what we need, is merely the displacement field in potential form, for example, in the elastic zone. It has to be, of course, something quite realistic. What we mean by the elastic zone is some elastic radius beyond which the strains are of the order of something like 10^{-4} or 10^{-5}. This will insure that if the medium is jointed and stressed, which is probable, then this level of strain will not cause any large scale, nonlinear effects associated with movements along joints and faults.

MR. HARKRIDER: Where did you get those numbers, Arch?

MR. ARCHAMBEAU: These are from strain observations close to the source. We find that when we have strains of this level, then there is no appreciable movement on faults or joints.

MR. CHERRY: Wouldn't you say that it may very well happen that the elastic radius, as you people get it, might be wavelength sensitive, depending on, say, the size of the joints?

MR. ARCHAMBEAU: Yes. This is always in the context of what bandwidth we are looking at seismically. It is the seismic bandwidth that we are interested in, and I think we ought to specify that. At the high-frequency end we put it at 5 cycles, and at the low frequency end we are looking at energy around 100 sec or even greater, but let us just say for purposes of detection, 100 sec is about as far as we are going.

MR. CHERRY: The thing I meant was it might be that the elastic radius would be different for the body waves than for the Rayleigh waves.

MR. ARCHAMBEAU: Yes. I think that is probably true, but let us take the greatest radius appropriate for all surface and body waves in the range of frequencies of interest. We are saying that this is probably the best practical way of specifying it, in terms of the distance at which the strain is at most 10^{-4} for any frequency in the 5 to 0.01 cps range.

Now we have observed, of course, from explosions, that we have any number of anomalous effects as well, stress relaxation in the zone outside the fracture zone, and in general there is some movement along cracks and fractures in this zone. This closer-in zone is what one might call a zone of cracking. You have been working out to within the fracture zone someplace, from what I can gather, and this is the end of your nonlinear zone. In other words, at this point, you talk about infinitesimal strains because they are perhaps 10^{-2} or something of that order.

MR. PERRET: On reexamination of our data, it seems that our measurements yield st.ains of 10^{-3} and 10^{-4} calculated from peak particle velocities or displacement differences.

MR. ARCHAMBEAU: Okay. So we are getting close. It is some place near the outer radius of the fracture zone.

MR. PERRET: Gasbuggy measurements, for instance, give peak strains of about 5×10^{-3}.

MR. ARCHAMBEAU: That's good. What we might need are measurements out even farther to make sure that things are behaving elastically.

MR. PERRET: For instance, on Discus Thrower, we do not have data out in the neighborhood of 10^{-4} or 10^{-5} strain. The real problem is that at times beyond 1 sec our records indicate from particle velocity divided by the propagation velocity we get strains below 10^{-6}, but here we are down in the record noise. I looked up some of these data to see what strains the noise represented. What I am saying is that in free-field data from distances like a few hundred to a few thousand feet we see no frequencies of the order of one cycle which are out of the noise. Frequencies are all higher than one cycle and noise is of the order of 10^{-6} calculated strain.

MR. SMITH: The energy is all there.

MR. PERRET: It must be.

MR. SMITH: The earth is acting like a filter, so you are not going to see lower frequencies at greater distance if it is not there close in.

MR. ARCHAMBEAU: That is right. What you are saying is that the noise level is up above the energies in that frequency band.

MR. PERRET: That is right, and the strains I am talking about are less than 10^{-6}, i.e., our noise level is equivalent to strains of this magnitude.

MR. SMITH: I think we are losing perspective here. There is a tremendous amount of data out in this intermediate range, the entire Coast Survey strong-motion program, but by the time you get so far away that the strains are that small, the signal is totally determined by reverberations in the crustal layers.

MR. PERRET: But what you are losing sight of is the fact that we are talking about free-field measurements and Coast Survey records only surface motion.

MR. SMITH: That is my point. By the time you get far enough out that the strains are this small, you might as well be at the surface, because there has already been time for reverberations in crustal layers and surface reflections.

MR. PERRET: The kind of thing they are seeing that represents strains this big are probably surface waves like the Rayleigh waves; in free-field data we observe only the body wave and there the dilatational wave dominates by an order of magnitude.

MR. SMITH: That is right.

MR. PERRET: The P-wave strains are away down by the time you get out to a few kilometers.

MR. ARCHAMBEAU: In any case, I wanted to state at least roughly at this point more or less what we had in mind and what we needed in terms of both observations and the theoretical codes. Actually it would be very useful for us to have spectral data. That would be the most useful form for us to look at, either the data or the theoretical results.

MR. SMITH: My point is that that data exists at those distances, and we don't know how to use it.

MR. ARCHAMBEAU: You mean the surface measurements?

MR. SMITH: Yes, at a distance of 5 or 10 km, it does not make any difference whether it is surface or a thousand feet down. The signal is still really badly distorted.

MR. ARCHAMBEAU: Nevertheless, that is our effective source function. If we are going to get anything from what they do, it is going to have to be here. It can't be closer in because it is nonlinear, so we can't use that. What you are saying is we can't use anything they have.

MR. SMITH: I am just saying we don't know how to use it yet.

MR. ARCHAMBEAU: Perhaps that is true. Perhaps we will never be able to use what they are giving us, but at least we can define what it is we think we need.

MR. COOPER: The codes have calculated out to strain levels on the order that you require.

MR. TRULIO: I didn't recall that correctly yesterday. The strains are typically 10^{-4} and 10^{-5} when we stop a problem, even in two-dimensional calculations, because we need to determine ground motion at stress levels of a few hundred psi.

MR. ARCHAMBEAU: Yes, that is easy to do, of course. It depends on how sophisticated your model is. What we are saying is we need a fairly sophisticated theoretical model.

MR. COOPER: The amount of data you have must determine how sophisticated you are justified in making the model.

MR. ARCHAMBEAU: Yes, that is true, but I say from an assessment of our observations we need something fairly sophisticated. After all, we are seeing tectonic release from these things, and so on. So that means we are going to have to see something a little more elaborate, I think.

MR. CHERRY: Do you think your knowledge of the structure warrants any ...?

MR. ARCHAMBEAU: You mean close-in structure?

MR. CHERRY: Yes.

MR. ARCHAMBEAU: I am just thinking in terms of teleseismic distances, what we are measuring, and what that represents in terms of the source character. We are seeing anomalous effects, and we are seeing a fair degree of detail. In other words, our observations, in total, are fairly sensitive to the character of the source function, the equivalent source function, if you like to think about it that way, although we are observing in general in a limited-frequency band. The thing Nafi showed, for example, yesterday, was an equivalent source function which was derived from very limited bandwidth data, not unique in any sense, but we observe in other frequency bands. In the body-wave frequency band we are observing actually up to five cycles in some situations, and down to 10 sec. So that we have various bands that we are looking in, and we are getting a fair degree of detail concerning the source. The question is, is it at all justified to make a very elaborate material and geometrical model? I can't precisely answer that question. You might want to put in a certain amount of jointing, fault zones, and things like that under stress conditions, which I think you are going to do anyway.

MR. TRULIO: Overburden stresses are the only ones that we have included so far in initial conditions. I don't know if other people try to do more than that, but we don't.

MR. ARCHAMBEAU: What about layering?

MR. SMITH: Discus Thrower has been calculated.

MR. TRULIO: Yes, Discus Thrower, and layered basalt media for another example.

MR. ARCHAMBEAU: How do your results compare to observations, for example.

MR. TRULIO: On Discus Thrower, they were closer than we had a right to expect from our limited knowledge of the mechanical properties of the pertinent materials, that is, the difference between calculation and observation lay within the variation in ground motion one would predict by varying constitutive parameters within their likely limits of uncertainty. There were no ground motion data for the layered basalt medium we calculated.

MR. ARCHAMBEAU: Okay, then it seems to me at this stage what we ought to do is use those results and try to predict the seismic field from them to see how that compares with our observations in the far field. It seems to be a logical and obvious thing to try to do at this point.

MR. SMITH: My point is I keep remembering the data I have seen on an 8-km radius from Jorum and Handley and things like this, and the tremendous variation over 20-deg azimuth in the character of the signal at that distance. A symmetric calculation such as you are describing cannot possibly explain what one sees at that distance.

MR. TRULIO: The medium surely exhibits asymmetries over the distances spanned by our calculations, but it is still modelled as perfectly homogeneous and isotropic.

MR. COOPER: I think asymmetries exist close in also. For even a contained burst in a "homogeneous" medium (homogeneous in the sense that it is one real material), you will find that it is not really symmetric. In fact, the data would scatter in one given direction. So calculations that assume symmetry at best can be assumed to represent a prediction of some sort of a norm or mean of what you are measuring.

MR. SMITH: Then if we are going to work with some numbers near the source and relate them to the far field, then the measurements and the calculations we need are practically for the down going from the source, so we need some measurements underneath.

MR. TRULIO: I think the place to start is not with bursts in NTS granite. It is with small yields in soft rocks, or dirt, or salt.

MR. ALEXANDER: Is there any situation where you sampled the actual displacement, say a small shot where you got a real good azimuthal variation and also some depth measurements? That would be very interesting to see.

MR. TRULIO: One in tuff.

MR. COOPER: That experiment would not have useful variations. We discussed this last night. The measurements were all in one direction.

MR. TRULIO: That is almost right. The measurements were made not just in one direction, but covered a small solid angle. The yield was small, and the experimental and theoretical pulses were quite similar.

MR. COOPER: Frankly, I don't see how that really perturbates what was said a minute ago about using what is coming out of the codes as input to the other calculation. You are assuming symmetry with what you are using now, your point source, are you not?

MR. ARCHAMBEAU: No, we have rather complete flexibility.

MR. TRULIO: What is wrong with dealing with a source as spherically symmetric if it really is?

MR. COOPER: That is what I am trying to get at now. I don't understand. I thought the initial condition was a pressure pulse for a point source.

MR. ARCHAMBEAU: Yes, it was. (But we aren't limited to that kind of representation.)

MR. COOPER: That was the input, and you are looking at surface movement. That is a symmetric problem.

MR. ARCHAMBEAU: Let me show you the next sequence of figures, which should clarify this point.

MR. CHERRY: Before you do that, you said you had complete flexibility. What does that mean? Did you have a three-dimensional Rayleigh-wave model?

MR. ARCHAMBEAU: The source field need not have high symmetry. In that sense we have flexibility, but the Rayleigh-wave calculations require cylindrical or spherical symmetry in the earth model used.

MR. HARKRIDER: You make a different calculation for each direction.

MR. ARCHAMBEAU: We can expand the source in multipoles for any kind of source we wish to consider and calculate the (free-field) radiation from it.

MR. HARKRIDER: But the material is azimuthally symmetrical, right?

MR. ARCHAMBEAU: Yes. We do it in a spherical layered earth, and we can do some other things for cases in which things are not quite that nice. We can do asymptotic rate theory in media with less idealized properties, for example. We have some considerable computational capability in terms of body waves and surface waves. Dave's programs, for example, are for layered half-space problems, so when the propagation distances are not too great, and you don't have to worry about curvature then we can predict surface waves very nicely. You can predict the source-radiation field for any kind of equivalent point source. After all, any volume source can be mathematically represented by a point source, an equivalent point source which is just a multiple expansion. So that this program then is capable of modeling any kind of source.

MR. HARKRIDER: Yes, and if it is under 100 sec, you don't have to worry about the curvature effect on phase and group velocity, and I can correct the difference in spherical and cylindrical spreading.

MR. ARCHAMBEAU: Besides that, we have free-oscillation programs to which we are adding source functions to produce surface waves which are equivalent to the earth's free oscillations. By adding up all of the free-oscillation modes, you can represent the total seismogram.

MR. HARKRIDER: Those are also azimuthally symmetric.

MR. CHERRY: There should be a few sources now where the calculations are complete and they really are spherically symmetric down to strains of 10^{-5}. Now asymmetries that grow will, hopefully, just be linear wave propagation dominated.

MR. ALEXANDER: He is probably going to show some slides that will have that effect.

MR. ARCHAMBEAU: Let me show you two figures and then I will talk some more about this question. Figure 46 is based on Bilby data, and these are radiation patterns. These numbers are observed amplitudes at the different stations. We don't have a lot of coverage on this particular event, but this is typical of what we see. These data represent amplitudes of a compressional wave, a body wave, and the ray paths are down through the upper mantle of the earth. We compute the spectrum of the P wave, that is the compressional wave, at each one of these stations, and then we plot the radiation patterns as a function of frequency by contouring the amplitude data for a particular frequency. These results are for 1 cps spectral data.

The pattern for an idealized explosion should theoretically be perfectly symmetrical, that is, have circular symmetry around the source, and roughly speaking it does. It is, however, modified in shape by structural effects. It could be modified by stress release, but it can be shown that tectonic effects on the P or compressional phases are of second order compared to the explosion itself unless the stress is extremely high. So that tectonic release, if we adopt that

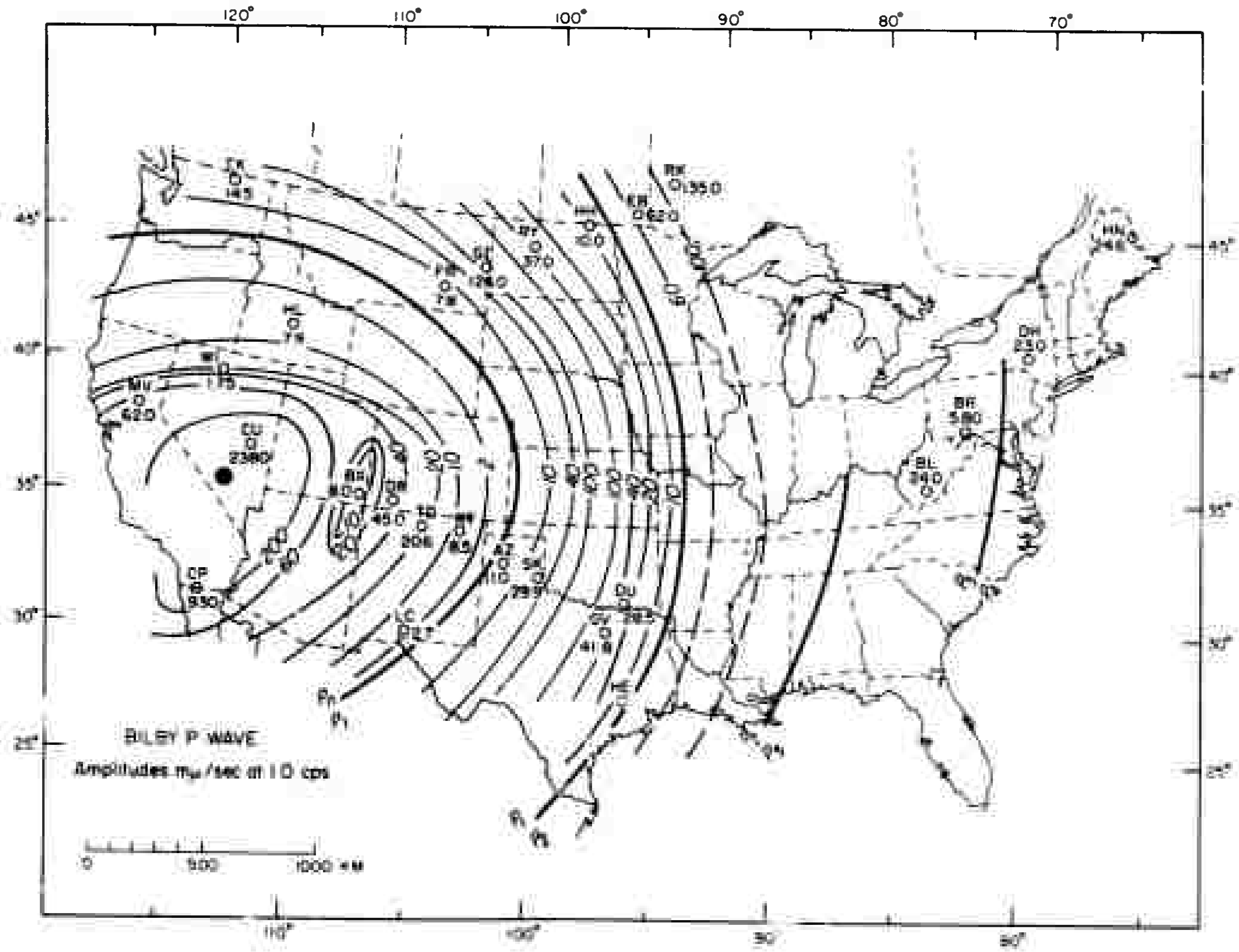

Figure 46. Bilby P-Wave Radiation Patterns.

hypothesis as an explanation of the anomalous radiation from explosions, can be shown to be totally responsible for SH-type surface waves and SH shear waves in general, and while it does rather strongly perturb the Rayleigh-wave radiation pattern and spectra, it does very little to the compressional waves, so that we get the circular symmetry shown here. This is a discriminate, because even while we have complications due to tectonic release they do not change the P-wave radiation very much, and as I will show a little later, the radiation patterns from earthquakes for compressional waves are of a very different nature. They are quadripole in form, or a superposition of multipoles excluding the monopole term. So earthquakes correspond to a high order of expansions in multipoles. They are a higher order than the explosive P-wave field, which is a monopole field, so that we don't have the simple circular symmetry of an explosion. So this is one way of distinguishing between earthquakes and explosions. It works reasonably well, and it is one of the ways used.

There are a lot of complications that arise in the detailed explanation of the P-wave amplitudes shown on this Figure 46 and I don't think I will go into them in detail. But, briefly, there are various compressional phases traveling along different paths in the earth's mantle that come in at different distances as the first arrival and we have to take that into account. We therefore have to take into account what knowledge we have of the mantle structure of the earth. We have ignored in this interpretation the lateral variations of the earth's structure and have interpreted these amplitudes in terms of one standard continental structure. You can do pretty well using such a first-order approach.

MR. CHERRY: In order for that to be a discriminate, don't you need fairly dense coverage?

MR. ARCHAMBEAU: Yes, you do. That is why it is less effective, perhaps, than others. You need a good azimuth coverage.

MR. ALEXANDER: You don't need it close in, though.

MR. ARCHAMBEAU: You don't need observations close in, but you need fairly dense azimuthal coverage, that is right. You need a lot of stations and you need to cover a fair azimuth. Figure 47 shows surface waves. Those on the left (a and b) are Love waves at two periods, 15 and 20 sec, and those on the right (c and d) are Rayleigh waves. This is the Bilby explosion. The insets show what would be predicted theoretically if one assumes a prestressed field for the medium which is consistent with the tectonic activity in the area in the first place.

MR. ROTENBERG: What does that mean in this problem, though? What is that prestressed condition?

MR. ARCHAMBEAU: It can be visualized in terms of an equivalent shearing couple. It is a shear couple with its axis in the northwest

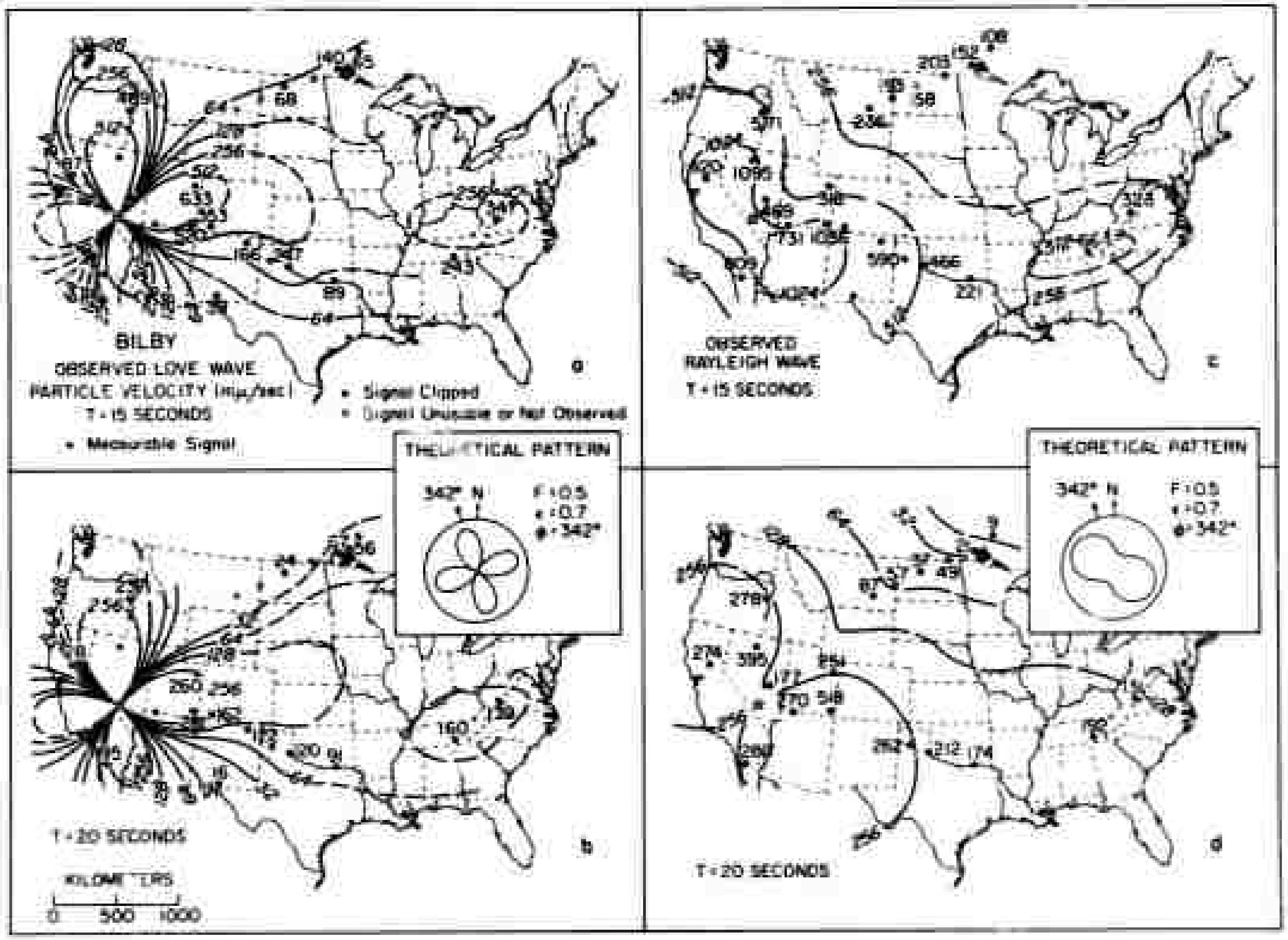

Figure 47. Bilby Surface-Wave Radiation Patterns.

direction. If you have an explosion in such a shear field, you get a pattern that looks like that shown in the insets. Again, structure actually has a fairly profound effect on the radiation patterns. As these waves propagate out into the eastern part of the continent, you can see that the patterns are getting broken up, of course, and you see anomalies in the pattern. What actually happens is that once the waves get beyond the Rocky Mountains, for example, the mantle has a higher velocity than in the west and has some nice properties for the propagation of surface waves. They are very efficiently propagated throughout the rest of the continent. They have a tough time getting across the Rockies, and particularly, as Shelton has pointed out before, the waves are generally highly attenuated in the western part of the continent because of the presence of partial melt in the upper mantle, and so the west is a high-attenuation zone. It absorbs energy fairly efficiently, and the mountains themselves scatter the surface waves pretty efficiently. In any case, you can see that the pattern maintains a shape as a function of period, although this is not much of a bandwidth. We actually looked at periods from about a couple of seconds out to something of the order of 80 to 100 sec with these instruments, and the patterns are maintained, although the power of course goes down very rapidly at the longer periods. The data shown are for the maximum power observed.

Incidentally, if we had an earthquake at this point with this same shear field, we would get a Love-wave pattern that looked like this, except that it would be frequency dependent, and a little later I will present some slides that show the effect for an earthquake in this region. The Rayleigh-wave pattern for the earthquake would be different than the one shown here however.

These patterns on the right are the Rayleigh waves. If one had an explosion that was ideal, with no anomalous effects, then you would expect a circular pattern just like I showed you for the compressional waves. The departure from circular symmetry shown here however is a perturbation in the pattern explained by the same tectonic stress-field orientation and magnitudes as was used to explain these Love waves. So that what we are trying to do is explain both kinds of surface waves with the same source, with the same orientation of stress field and so on. The inset shows the theoretically predicted shape on this basis. It is in rough agreement with what we see. Here again the structure in the eastern part of the United States is distorting the pattern, and we don't really have a lot of stations there. But in any case, it looks like it is consistent with these observations.

MR. ROTENBERG: Can you explain easily qualitatively why there is a 90-deg degeneracy in the Love-wave case? Why do you have this quadripole kind of pattern?

MR. ARCHAMBEAU: Well, this usually takes a little time.

MR. ROTENBERG: Would you rather leave that question for later?

MR. ARCHAMBEAU: Let us just do it this way. I will get these vectors turned around, but it does not matter--the quadripole is equivalent to what we call a double couple, that is, a couple of forces in the same structure with another couple pair oriented at 90 deg to it. This will give rise to a quadripole radiation field.

MR. ROTENBERG: Oh, I see. You did not assume a simple couple.

MR. ARCHAMBEAU: No, I did that so you can visualize the stress field. Of course, there is a Poisson effect, so you always get this pairing effect.

MR. SMITH: It is more important than that. If you do have shear release on a surface like this, it is equivalent to a double coupling, and not a single couple.

MR. ARCHAMBEAU: You know that to begin with, because you know that the couples have to be balanced to conserve angular momentum and so there can be no unbalanced couples.

MR. ALEXANDER: This has been looked into by a lot of different people, and if you have got to pick one kind of point source to represent an earthquake, it would be a double couple with some arbitrary orientation. In this case it would be vertical.

MR. ARCHAMBEAU: Yes, this is vertical.

MR. HARKRIDER: But it is a simple shear field.

MR. ARCHAMBEAU: It is pure shear. Figure 48 is the Shoal explosion, and again these are the Love and Rayleigh waves at the 15- and 20-sec periods. We again fit the observations reasonably well with theoretical predictions. This is important, because we want to understand the long-period surface-wave radiation. Since it affords us a fairly sensitive discriminant we want to understand where these anomalies are coming from. The concept of tectonic release is at least a tentative explanation. There are some details concerning which we would like a little more information. This concept seems to work pretty well however.

There are situations in which the simple model that we have used here, which is really just tectonic release due to the roughly spherical shatter zone, does not appear to be totally capable of explaining the observations. Tectonic effects again seem to be involved, but the kind of tectonic release is somewhat different in character from the one that we would predict from a spherical symmetry breaking. That is, it appears that faulting is induced in the medium or breakage along a long fracture. There would be a difference in the radiation from those two different geometries. The results I've shown so far basically assume just the spherical-shatter zone, and this appears to be what is most often involved.

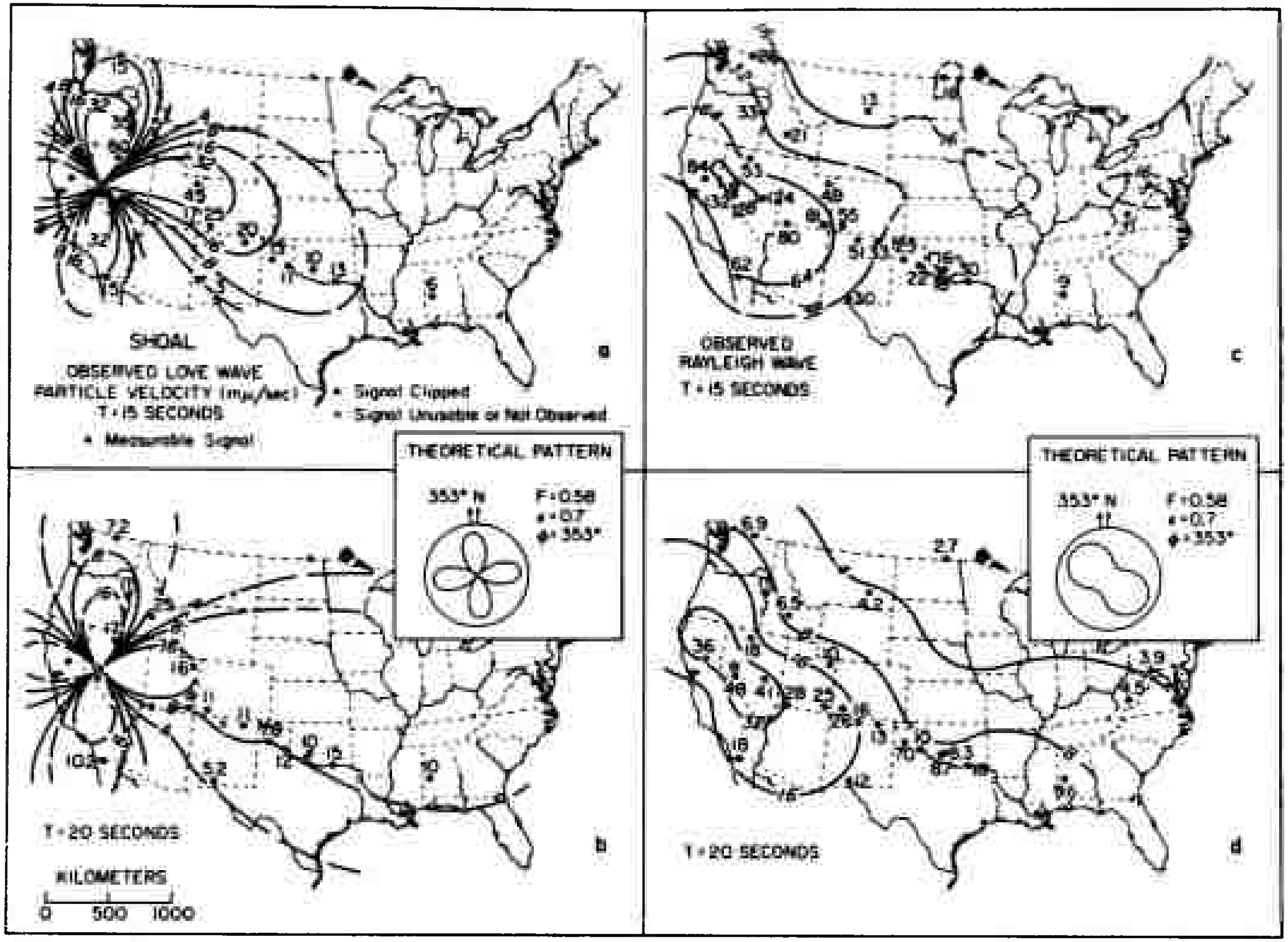

Figure 48. Shoal Surface-Wave Radiation Patterns.

MR. HARKRIDER: You might mention also the fact that Love waves are due to the release of the shear field alone and the Rayleigh waves are due to the release of the shear field and the explosive source itself.

MR. ARCHAMBEAU: One has a monopole and a quadripole superposed in this situation.

MR. HARKRIDER: And only the quadripole on that.

MR. ARCHAMBEAU: There is quadripole radiation of Love waves because the Love waves are totally anomalous, so they involve just the tectonic effects.

MR. ALEXANDER: Before you leave this whole thing, you notice that the orientation of this field would be equivalent to a fault oriented at 353 deg in this case and vertical. This is the equivalent-force system. That geometry tends to remain pretty much the same for all of the different NTS shots that have been looked at in this way. The strikes were within plus or minus 20 deg of North. So you could put in a shear field in that area, and keep it fairly uniform over the whole area. In other words, you don't have to keep fooling around with different parameters.

MR. ARCHAMBEAU: Yes, we don't have to select different parameters for this fit for every explosion. There is an obvious physical interpretation, and a shear field in this region at least seems to be regional in character, that is, it does not vary a lot over quite a large area.

MR. ROTENBERG: Excuse me, before you leave Figure 48, somebody mentioned something about the Rayleigh source being the superposition of a monopole and quadripole. That still has a four-fold symmetry. I don't see that there.

MR. ALEXANDER: It depends on the orientation of the quadripole. You can get just a two-lobe pattern from a double couple.

MR. ARCHAMBEAU: By the way, this is a projection of the pattern on the surface.

MR. HARKRIDER: When we say it is quadripole, we don't mean that all of the lobes are equal. In this case, the lobes for the Rayleigh wave were very small in one direction, and very large in the other direction, depending on the orientation of the field.

MR. BROWN: Don't the couples have to be equal from a statics point of view to keep the thing in balance?

MR. HARKRIDER: Yes, that is right. Of course, the double coupling guarantees that.

MR. CHERRY: I am glad somebody mentioned equilibrium because that sort of bothers me. What sort of equilibrium state does this represent in the rock pre-shot?

MR. ARCHAMBEAU: I can show you that. I have done this study from the point of view of determining what the stress is in the rock. The theory that this is based on is essentially that of an initial value problem. As you can see, this is susceptible to that kind of attack. It insures that the medium will go from one equilibrium state to another. The difference in stress or displacement between the initial state and the final state is an initial value. You just crank that into a Green's-function solution, and out it comes.

MR. HANDIN: But at the test site you do in fact get slip by the fault.

MR. ARCHAMBEAU: Thas has been observed at the surface, indeed, but that is almost certainly just a surface effect.

MR. HANDIN: Certainly the alignments are right on the line in the afte shock.

MR. ARCHAMBEAU: There is no radiation from them, so that is almost certainly just a very near surface effect.

MR. SMITH: Well, be careful, now. Let us not give the impression that this is totally without controversy in the seismological community There is some probability that part of this effect is structurally induced, and it is really an enigma that the observed faulting at the source, which does have something to do with the after shock, does not show up in the radiation of long-period waves. I think it is somewhat of a puzzle.

MR. ARCHAMBEAU: We can state the evidence as it exists now. There are observations of fault movement on the surface, and these movements are in the same sense or direction that all other fault movement has been in the region. Just the same as normal tectonic movement has been in the past. In other words, it is not just the shock wave pushing things along, or something like that. It is actually tectonic release in the sense that other faults in the area have moved. That seems to indicate that there is a certain amount of stress release on these faults.

MR. CHERRY: Isn't that region under a state of tension, though?

MR. SMITH: That is a northwest tension that he has.

MR. HARKRIDER: On these patterns also, it is just the amplitude. He does not show the phase. So if you were adding it to a circle, one of these would be positive and one of these lobes would be negative, and that is why you get it subtracting from the circle and adding out to the circle, even with the lobes if they are the same size. The phase is different on both of them.

MR. TRULIO: On the principal axes there is a compression in one direction and a tension in the other.

MR. RODEAN: What connection does this have with the statement that Carl Kisslinger made at Woods Hole about the difference between two of the big shots, Benham and Jorum, or another one? One had a lot of post-shot tectonic release or after shocks, and the other relatively little. He mentioned that one was inside a caldera, and another one was not. The caldera was like a hole punched into the crust locally, and therefore there was relatively little stress inside compared to outside.

MR. ARCHAMBEAU: I think what he meant was that there was a stress concentration in that area for the one explosion and not the other. His argument was that there is a regional stress field, but that that field can be highly concentrated by the presence of inhomogeneities of one kind or another in the medium and he showed geologic evidence that such an inhomogeneity existed. He then pointed out that the one shot that showed a lot of tectonic release was near this inhomogeneity. The supposition was that the stress field there was concentrated and much higher, so he got more tectonic release.

MR. ALEXANDER: That is secondary, though, to the thing that you actually observe from that event. It is sort of completely after the fact, and indeed, if you look at the signals of Jorum compared with Boxcar and some other ones there, at a fixed distance, fixed receiver, there are virtually overlays in the long period portions at least.

MR. RODEAN: So this was just sort of a superposition or a local variation of the overall stress field.

MR. ALEXANDER: In other words, that did not contribute significantly to the primary radiation field of that event, but it was important in the aftershock activity.

MR. ARCHAMBEAU: Let me go back to Stewart's comment here. There are quite a few other possibilities for explaining this kind of thing. I am just showing you one. I am showing you a theory that works and that uses reasonable kinds of parameters that can be put into the theory and agrees with what you see.

In addition, there is other evidence which is consistent with this kind of explanation, but this still is not the only one that might be operative. It is clear that anisotropy of the medium and other things are probably contributing. It is also quite possible that the strain gradients are high enough for essentially finite strain effects to have an appreciable effect. So that this might be or could very well be a superposition of a lot of things. I have assigned the anomalous observations to one cause. It may be more than one. I tend to believe that this is the predominant one, partly because I get reasonable answers, but it clearly is not the only one.

MR. SMITH: You might point out that one of the stronger bits of evidence here is that this same experiment repeated on a shot in salt gives essentially symmetric radiation.

MR. ALEXANDER: And no Love waves.

MR. ARCHAMBEAU: Well, this has been shown, yes.

MR. SMITH: That is one of the stronger arguments for believing your hypothesis.

MR. HARKRIDER: But it was in a different part of the country.

MR. ALEXANDER: But the geological structure there was still pretty complicated.

MR. HARKRIDER: It was east of the Rockies.

MR. SMITH: It does not matter. You still have a complicated structure.

MR. ARCHAMBEAU: If you had high-stress gradients or strain gradients, you would still expect anomalous effects for salt if the explanation for this is finite-strain theory in its nonlinear form. Then you can get all sorts of conversions since you are dealing with a nonlinear process and all you need are high-strain gradients on the edge of the salt dome.

MR. CHERRY: The interesting thing about salt is that it is very homogeneous, and there should not be any conversion from P to S around the source at least anyway. How about alluvium at the test site?

MR. ARCHAMBEAU: Alluvium shows small anomalous SH waves.

MR. CHERRY: Do you still find an effect in that?

MR. ARCHAMBEAU: There are some problems. Carl Kisslinger once ran an experiment in silt or similar material, something that just could not conceivably have any prestress, and he got shear waves coming out of that.

MR. ALEXANDER: SH waves?

MR. ARCHAMBEAU: SH waves. He had SH waves from an explosion. At the moment I don't remember quite how large they were relative to the explosion itself. I know they were small, but clearly observable. These are pretty big effects that we are talking about here.

MR. HARKRIDER: That was a model study he did?

MR. ALEXANDER: No, he actually did the shot.

MR. ARCHAMBEAU: In the field.

MR. HARKRIDER: Did he ever look at the cavity to see if there was a different burning rate in different directions?

MR. ARCHAMBEAU: That was a long time ago, and I don't remember the details. He could have had all sorts of explanations which could apply in that situation to try and explain that observation. I just bring that up to point out that there may be other things entering into this that might be considered.

MR. ALEXANDER: Again on account of the collapse data, one example of which I showed you, where the explosion produces Love waves and SH waves, and the collapse at the same point does not, it seems to me you cannot appeal to local scattering and inhomogeneities to explain the Love waves.

MR. ARCHAMBEAU: In that case, not even high-strain gradients.

MR. ALEXANDER: Why not?

MR. ARCHAMBEAU: Because the one source is not producing the effect and the other is. What one would appeal to in a case of finite strain theory is the high gradients, and they should still be there for the collapse as well as for the explosion. Therefore finite strain gradients don't work.

MR. SMITH: But long periods are quite comparable sources. They are within a factor of two.

MR. ARCHAMBEAU: Let us get on. We can discuss this particular aspect in more detail if you want to later.

MR. TRULIO: What shear-strain amplitudes are you talking about here?

MR. ARCHAMBEAU: For Bilby, the one event I have investigated in detail, they are quite large, but that was in tuff. Tuff has ridiculous elastic properties. But in any case the strains were like 2×10^{-3}, and such a strain in tuff corresponds to a stress of 70 bars. The USGS went out and measured a stress of 70 bars by overcoring methods, so at least two experiments gave the same answer.

Figure 49 is based on earthquake data, and these are the radiation patterns you see for earthquakes. This is the Fallon earthquake, which occurred essentially at the site of the Shoal explosion, so this pattern should be compared with previous ones. The theoretical pattern in the inset is something that Dave Harkrider's program computes, and this is the radiation pattern you should see for a particular choice of parameters of the fault. It was concocted in order to give an approximate fit to these observed patterns. This is the Love wave. The four observed patterns are at different periods.

What I want to especially point out is that the theoretical model is a quadripole point source in layered-earth model. Real earthquakes are more complicated than that. However, at long periods they look like quadripoles, because the higher order multipoles become less important at the longer periods. The higher order multipoles are more important at the higher frequencies. So what happens is that at higher frequencies the conglomeration of multipoles that is equivalent to an earthquake add up to give you a nonsymmetric field, so the patterns lose some of their symmetric properties. In particular, energy tends to be thrown in the direction of faulting or rupture propagation.

Energy is preferentially radiated along the axis of fracture. At high frequencies, that is short periods, one expects to see larger amplitudes in the direction of rupture, and as the period of the radiation field gets longer, you expect to lose that effect. It becomes more quadripole in nature. The theoretical pattern shown therefore only applies to the longer-period radiation. We can predict the shorter-period stuff, too, in terms of a more sophisticated approach.

MR. COOPER: Is there some way to use the theory to predict the direction by some independent input parameter? Or are you bound to seeing what fits the experiment, or the earthquake in this case?

MR. ARCHAMBEAU: You mean prior to the earthquake look at the stress field?

MR. COOPER: Are there independent parameters that one can define independent of the observed event to predict the direction of the orientation?

MR. ARCHAMBEAU: Well, you can look at the fracture.

MR. SMITH: The pre-existing fracture.

MR. COOPER: Yes, pre-existing fracture and whatever else. Does this correlate?

MR. ARCHAMBEAU: Yes.

MR. COOPER: It is a predictable direction?

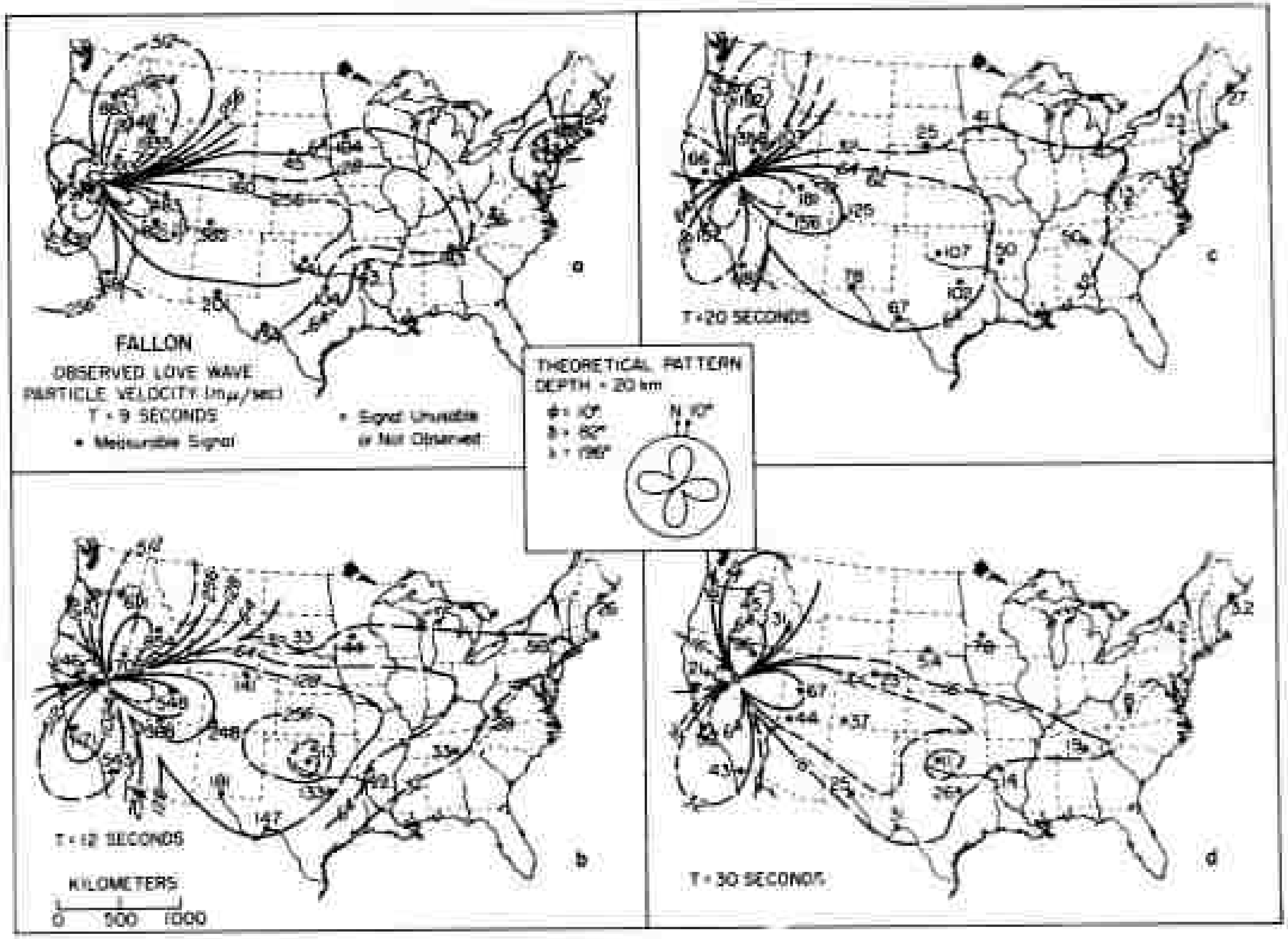

Figure 49. Fallon Love-Wave Radiation Patterns.

MR. ARCHAMBEAU: Yes, it is a more or less predictable direction.

MR. ALEXANDER: The length depends upon the size of the earthquake.

MR. PERRET: This is not in Fairview Valley. This is over farther toward Fallon?

MR. ARCHAMBEAU: Yes, I think so. I don't remember that, though.

MR. PERRET: They are about 20 miles apart, and I thought the fault up there in the Fairview Peak area leaned more toward the west.

MR. ARCHAMBEAU: Yes, perhaps that's true.

MR. HARKRIDER: About 5 deg to the west.

MR. PERRET: I just wondered what the difference was between the direction of this pattern and the direction of the Shoal analysis?

MR. ARCHAMBEAU: They are different by from 10 to 15 deg. Of course, my opinion of what happens at the surface is that it does not necessarily very closely relate to what happens at depth, so you can be misled. At least if it is within the ball park, 20 deg or so, that is good, particularly if you are observing at the surface, which is an anomalous place to observe anything, especially with respect to tectonic effects.

MR. HARKRIDER: And it was a 20-km depth earthquake anyway.

MR. ARCHAMBEAU: So it was quite deep relative to events in California or in this western region. I want to point out also that there are strong structural effects here. We are plagued in all of these pattern studies by the fact that the crust and upper mantle of the earth are highly variable laterally. This will lead to strong amplitude variations. For example, if you look over here due west of the event, this station to the west is always anomalously low in amplitude, and really should not be, although one can't be sure that it is not on one of these node lines here, and that the pattern is twisted around somehow. But in any event, the structure has a whopping effect on the radiation patterns. One might argue, for example, that this pattern does not really show this higher amplitude up to the north, but that all you are basing this argument on is the relative amplitude compared to one station to the south, and you might say, well, that is just a structural effect and doesn't have anything much to do with the source character. What we need in this kind of study is more stations, and better azimuth control to be sure whether structure or source properties are controlling the radiation pattern. But theoretically we get a prediction of asymmetry in the pattern such that more energy is being radiated in this direction to the north and this effect changes and dies out as a function of the period. If you look at these patterns observationally, this prediction is not inconsistent with the observations; on the other hand, it cannot be definitely confirmed either. It is only at the short periods where this station to the south has a lower amplitude than these up to the north, and this is consistent with the theory.

MR. ROTENBERG: The black dots are the stations?

MR. ARCHAMBEAU: Yes.

MR. ROTENBERG: That is a lot of structure you put in there on the basis of a few points.

MR. ARCHAMBEAU: Really one should hesitate in doing this kind of thing at all with this much data, except to demonstrate consistency with the theory. However, the gross features I've discussed are standard observations.

MR. HARKRIDER: It shows it is consistent with the theoretical radiation pattern. The dashed lines are his guess essentially.

MR. ARCHAMBEAU: There were a lot of reasons for doing this as well as to show consistency with the theory. People were taking long-distance observations of radiation from earthquakes and making a lot of predictions based on these long-distant observations. I wanted to show that the structural effects were strong enough to cause all sorts of anomalies, and these patterns are an effective means of showing up such anomalies.

MR. SMITH: This would be a good point to bring out what kind of close-in data would be required in order to confirm this hypothesis.

MR. ARCHAMBEAU: What we need is perhaps two rings of stations fairly close in, hopefully within a structural province, if we could, that is, where lateral variations were not so strong. Ideally what we would like would be an underground explosion in the middle of a shield, surrounded by a couple of rings of fairly close-in stations within the confines of that structural province, so that you have a nice very predictable structure with small lateral variation, and then we could look at the pattern as a function of frequency, and really nail this down in terms of whether it agrees with all our theoretical predictions or not.

MR. ALEXANDER: There is another way you can take, too, and that is to simply use other distant events to get the transfer function independent of where the event of interest is located and use that empirical transfer function to adjust it.

MR. ARCHAMBEAU: Right. That is the kind of thing that Shelton has been doing, I think.

MR. ALEXANDER: Yes, so you can get rid of the structure.

MR. ARCHAMBEAU: However it would be nice to do one experiment like this that is really controlled. Of course, I think it would require a little more thinking, but in any case that kind of experiment would be very nice since we'd have close-in control and azimuthal coverage.

MR. SMITH: That is essentially what McKevely has tried to do in the last two large explosions.

MR. ARCHAMBEAU: He is very close in.

MR. SMITH: Why don't you use his numbers rather than these?

MR. ARCHAMBEAU: He is like 10 km away. I am talking about a couple of hundred.

MR. SMITH: But if you are at a 10-km ring, you can get a whole lot more azimuthal coverage with a fixed number of stations.

MR. ARCHAMBEAU: We are talking about surface waves, now, long-period surface waves. We want to get one wavelength away from the source in the appropriate period range.

MR. HARKRIDER: That is about 50 for these.

MR. ARCHAMBEAU: For a 100-sec wave, you want to be a couple of hundred kilometers away, so that you have a surface wave that means something.

MR. SMITH: But this is why I brought this up, because the code calculations are always providing the near field. We really ought to be able to relate that calculation to what you are trying to do here. It seems to me we have a better chance of doing that if you can work closer in. I don't really see why it is necessary to be a wavelength away.

MR. ALEXANDER: If you have what is going on there, then you can calculate it, right?

MR. ARCHAMBEAU: We have a theoretical model that we want to test. What you want to ask is: all right, does this compare with the theory or not, and if it does then you have a far-field theory. That is, it is applicable at a wavelength away and beyond for the surface waves.

MR. SMITH: Then you are never going to be able to use the reduced displacement potential if you cannot figure a way to convert that near field to the far field.

MR. ALEXANDER: To do the Love waves, you have to have the SH displacement potential.

MR. RODEAN: But your reduced displacement potential by definition does not give you that.

MR. HARKRIDER: We need a reduced vector displacement.

MR. ARCHAMBEAU: That is right, a three-dimensional code. We need a vector potential.

MR. BROWN: Do you see much local discrimination by these radiation patterns between nuclear events?

MR. ARCHAMBEAU: Well, yes, we do. I think that basically what we have been using however are spectral differences rather than differences in the pattern shapes.

MR. ALEXANDER: It is not the symmetry, but it is the distribution of energy between the compressional waves and the surface waves. That is basically the discriminant. Explosions tend to distribute relatively more energy into the P waves than do these shear-type sources, which contribute more energy to the surface waves.

MR. BROWN: This in effect changes that quadripole to a monopole. The monopole becomes more pronounced.

MR. ALEXANDER: Yes. In other words, the monopole is being generated, even if there is this tectonic component. The monopole is still superimposed on that, and it has a big effect.

MR. ARCHAMBEAU: I have some spectra to show you in this regard.

MR. BROWN: So there should be some discrimination other than just in the shape of these.

MR. ARCHAMBEAU: Yes, these shapes could be used also, but the structure is what kind of kills you on this, the lateral variations. For example, these are all Love waves on Figure 49. The only difference between this pattern and that for the explosion you saw before was this frequency-dependent effect, so that is pretty hopeless to try to use this difference to discriminate. It has not been used. The Rayleigh-wave and compressional-wave patterns do have quite different shapes, but the azimuth coverage required for discrimination reduces the usefulness of these differences.

MR. ALEXANDER: There is another way to get around it, and that is to use a reference event. If you can document for one single event very well what is going on, then you use that as a reference, and you then eliminate the propagation effect entirely just by normalizing.

MR. ARCHAMBEAU: Even so, it has not been shown using that technique either that there is this frequency effect, the frequency-dependent radiation patterns for earthquakes, whereas the shape of the radiation pattern is not frequency dependent for explosions. That has not become a viable discriminant.

MR. ALEXANDER: In effect I have done that for Longshot and an earthquake in the same area and proved that there is indeed a frequency-dependent radiation pattern for the earthquake.

MR. ARCHAMBEAU: That is good. Theory predicts that but we have never really gotten down to getting a great deal of hard data to show that it in fact happens for a specific event. That is hard to do because of structural effects as these past slides I've shown suggest. But Shelton cancels out the structural effects by using a reference event and dividing out the structural and propagational effects.

These are Rayleigh-wave patterns in Figure 50 and these patterns are frequency dependent in theory. The inset is calculated from a simple double-couple model. The calculations are from Dave Harkrider's program again. Because of the depth and orientation of this quadripole we get an additional frequency dependence in the pattern. If you remember, the explosion had no frequency dependence in its radiation pattern, and it had a different shape than this. We haven't really used this, either, as a discriminant, although this difference does exist.

MR. ROTENBERG: Why is it not symmetric now as the other one was? What is different about it?

MR. ARCHAMBEAU: It is because of the orientation of the fault with respect to the horizontal layering.

MR. HARKRIDER: A better way of saying it is that strike slip and dip slip give different amounts of Rayleigh and Love, and this is a combination of dip slip and strike slip on this one, since he has a slip angle of 196 deg, which means that it has a component of about 16 deg of dip slip, if you want to call it that.

MR. ARCHAMBEAU: In other words, the idea is that this pattern changes as you twist the orientation of this double couple around in a layered medium, and since the surface waves come from the interference effects in the layering when the source is not oriented with a symmetry axis along the perpendicular to the layers, then the energy interferes constructively in different directions for different frequencies. That is just the waveguide phenomenon.

MR. COOPER: Could you do a 3-D problem?

MR. ARCHAMBEAU: In a limited sense, yes. The patterns of P waves and S waves from these sources are spatially dependent and the source pattern does not have the symmetry of the layered model.

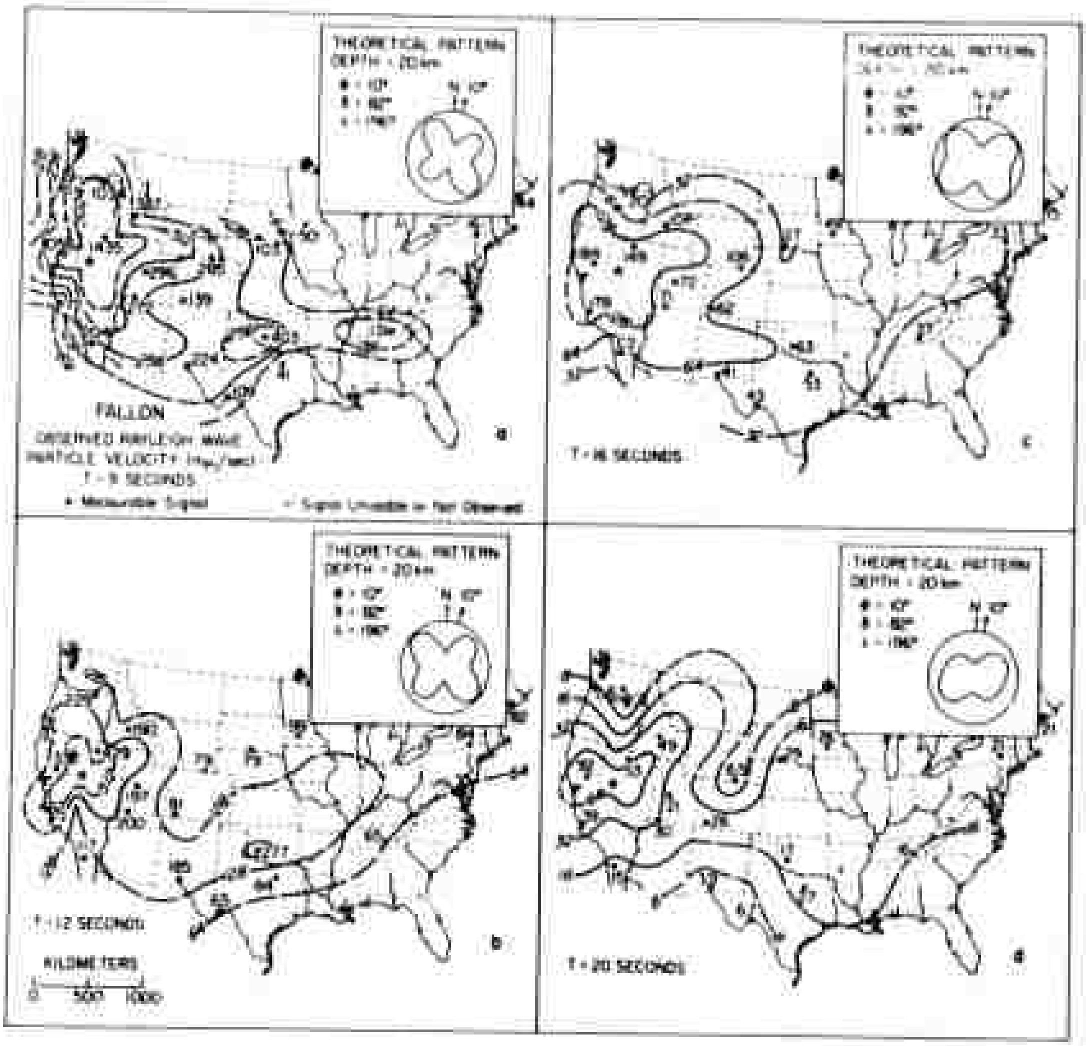

Figure 50. Fallon Rayleigh-Wave Radiation Patterns.

MR. HARKRIDER: No, it is just two dimensional.

MR. COOPER: It is a three-dimensional pattern. That is what I am trying to bring out.

MR. ARCHAMBEAU: Oh, the propagation effects I should say are obtained from a two-dimensional layered-earth model and the source is three dimensional since it can be taken to have any orientation in a layer. So these patterns are frequency dependent.

I would not say that this data is terribly good. Once you get out a little way you can see that the radiation pattern becomes very confused, and it is very difficult to unwind just what kind of source you are looking at. In other words, the only place you have real definition is fairly close in.

MR. COOPER: I still am a little confused about the question asked a while ago. The dots are the data points here?

MR. ARCHAMBEAU: Yes.

MR. COOPER: How do you distinguish between this shape from the data, and the shape you had a moment ago? How much interpretation is in drawing the warped view as opposed to the symmetric view that you had on the previous figure?

MR. HARKRIDER: He just contours those numbers.

MR. ARCHAMBEAU: You contour them in, that's all. Of course you really mean how much bias is put into the contouring in order to get that picture. After all, we have the data here, and we can quickly see that there are a number of possibilities. What we are showing here, more than anything else, is consistency with the theory. If we had more data and more azimuth coverage, then it would be tighter.

MR. CHERRY: You have not changed the orientation of the source function, have you, for each of these?

MR. ARCHAMBEAU: No.

MR. CHERRY: And you have not changed its strength?

MR. ARCHAMBEAU: You mean for the earthquake compared to the explosion?

MR. CHERRY: No, for each frequency.

MR. ARCHAMBEAU: For each frequency, no.

MR. CHERRY: It apparently just comes out of your analysis that the surface-wave radiation patterns are frequency sensitive.

MR. HARKRIDER: That is right, and it is again this combination of the amount of dip slip and strike slip. At certain depths the dip slip is more efficient than a strike slip, and at other depths it is not. When you get near the surface, the dip slip is very inefficient, and the strike slip is very efficient for Rayleigh waves, and it depends different for Love waves.

MR. ALEXANDER: The partitioning of energy depends on the orientation, so if you have a strike-slip fault near the surface, that is going to be an efficient generator of Love waves, SH, and a relatively poor generator of Rayleigh waves. So that is why the perturbation into the primary field of Rayleigh waves for explosions due to this tectonic release is pretty small, and still you get the large Love waves which I showed you yesterday that are as big as the Rayleigh waves. It is certainly an apparent enigma the fact that from source to source the Rayleigh waves look alike at a given range, and a given receiver, and yet you get variations in the Love waves over that same set by factors of three or four. But on the basis of this theory, it is reasonable that that should happen, because the Love waves should be about five to twenty times bigger over this frequency band for a strike-slip fault. So the Rayleigh waves indeed are there, but the perturbation to the Rayleigh-wave pattern is relatively small as compared to the Love waves.

MR. SMITH: I tend to think we are getting confused here in most of the discussion centering on Love waves. I would just like to come back to the point that the P-wave radiation patterns that he showed are roughly symmetric, and that means that the reduced displacement potential calculated and confirmed experimentally the way you people do close to the source should be able to predict this far field without any of these other considerations about tectonic release and SH waves and so forth. My understanding is that they do not predict the far field. It seems to me we ought to focus a little bit on that problem.

MR. RODEAN: The far field of what kind of waves?

MR. SMITH: Compressional waves, the waves from which the body wave magnitude m_b is calculated.

MR. RODEAN: What about the calculations starting with the reduced displacement potential that first Werth and Herbst, later on Eric Carpenter, and then Kogeus (Sweden) have done? What about that? They have been concerned only with the compressional waves.

MR. CHERRY: It was P_n, I think.

MR. RODEAN: Yes.

MR. SMITH: My understanding, and I may not be correct here, is that they got a very rough agreement, but that what we are really talking about now is something more refined, so we are not worried about a half a magnitude difference, for example. We would really like to get things down to a couple of tenths.

MR. RODEAN: Okay, and then this probably means that we just have to know more about the structure of the earth.

MR. ALEXANDER: But in those examples that we showed the very first day, even though the structure was nearly in common, you still get big variations in P_n from various events. This is the excitation of P_n that seems to be quite variable.

MR. SMITH: We would claim, I think, that these variations are taking place very close to the source, perhaps within a kilometer of the source, where the real difference is.

MR. CHERRY: That is not consistent with what Clint snowed yesterday. The variations were taking place within very small distances.

MR. ALEXANDER: Oh, yes, but what he tried to do was get rid of that source of variation. It has a big effect, but since the path and receiver are in common, then the variations can only be attributed to the source. That is, suppose you detonate two shots exactly the same size at approximately the same location, and you receive them at the same receiver so that the path to the receiver is common to the two events. Commonly the two seem to vary from one another quite a bit. In other words, the P_n magnitude you would get for those two at a single station in practice can vary by a half magnitude. This variation tends to average out when you compute the typical P magnitude by averaging a whole set of different observations at various azimuths.

MR. CHERRY: What you are almost saying is that the experiments are not reproducible.

MR. ALEXANDER: To the extent that you have a real life situation like that, where you have two sources essentially equivalent, nearby to one another, and you observe them with a given receiver, that is really the test, to see whether or not they are the same. That is why it would be nice to see what the theoretical predictions are for some of these, if there are particular ones for which these data are available, also the field observations.

MR. ARCHAMBEAU: My personal opinion now is that we ought to take some of these reduced potentials and see what happens again, all over again, taking into account what we know about the structure and perhaps using something better than simple ray theory.

MR. ALEXANDER: I do not think that is too unreasonable from what we showed yesterday in the actual observed reduced displacement potentials, because as you go from one side of one layer to the next these potentials changed in shape rather significantly. So what is going to be received at some distance depends on where in that structure you set off the explosion. That P_n arrival represents a little pencil of energy that is going out from the source.

MR. CHERRY: It may very well be that P_n is not a good thing to use. It does take into account such a small part of the source region.

MR. ALEXANDER: This is what we would like to get a feel for in terms of the actual calculations and observations.

MR. CHERRY: It has always been my feeling that the Rayleigh wave is a much better thing to use for yield, because it samples so much more of the structural environment.

MR. ALEXANDER: Yes, I think that is a fair statement.

MR. ARCHAMBEAU: Figure 51 shows theoretical displacements, velocity, and energy-density spectra with arbitrary scaling for a relatively simple model of an earthquake. The model used is what is called a stress-relaxation model, and the idea is to compute the spectrum close in to the source, the "free"-field spectrum. The assumption is that the medium is stressed and that a roughly spherical zone of melting occurs, which grows at some rate which is less than the shear velocity. You can consider a more elaborate geometry, that is, long narrow ellipsoids, planes, and so on, but this is adequate. By the way, the calculations for more complicated geometries have not been done yet, and I am in the process of doing them. But this figure illustrates the essential structure of the spectra.

First of all, the spectrum falls off with increasing frequency asymptotically, at least, like $1/\omega^2$ or $1/\omega^3$, in that range. In addition, it peaks, and the peak occurs at a frequency which is associated primarily with the size of the event, the size meaning the long dimension of the failure zone. Asymptotically at long periods, that is low frequencies, it falls off like ω, roughly, although this plot does not indicate that too well. In any case, it falls off like ω, so what we have to do is contrast this kind of spectrum with that expected from an explosion to determine, at least in theory, what discriminants might be available to us. We have done this observationally and we are still in the process of defining the most sensitive simple measure of the spectral differences. The differences in earthquake and explosion spectra which are found, however, are principally at the long periods. The predominant differences are there in the long periods, so that is one of the means we have of discriminating.

Figure 52 shows spectra obtained by Smith and Sammis of Cal Tech. They conducted an experiment in which they looked at nearby micro earthquakes. One of the problems we have which is similar to the problems you are having measuring things close in, is in measuring

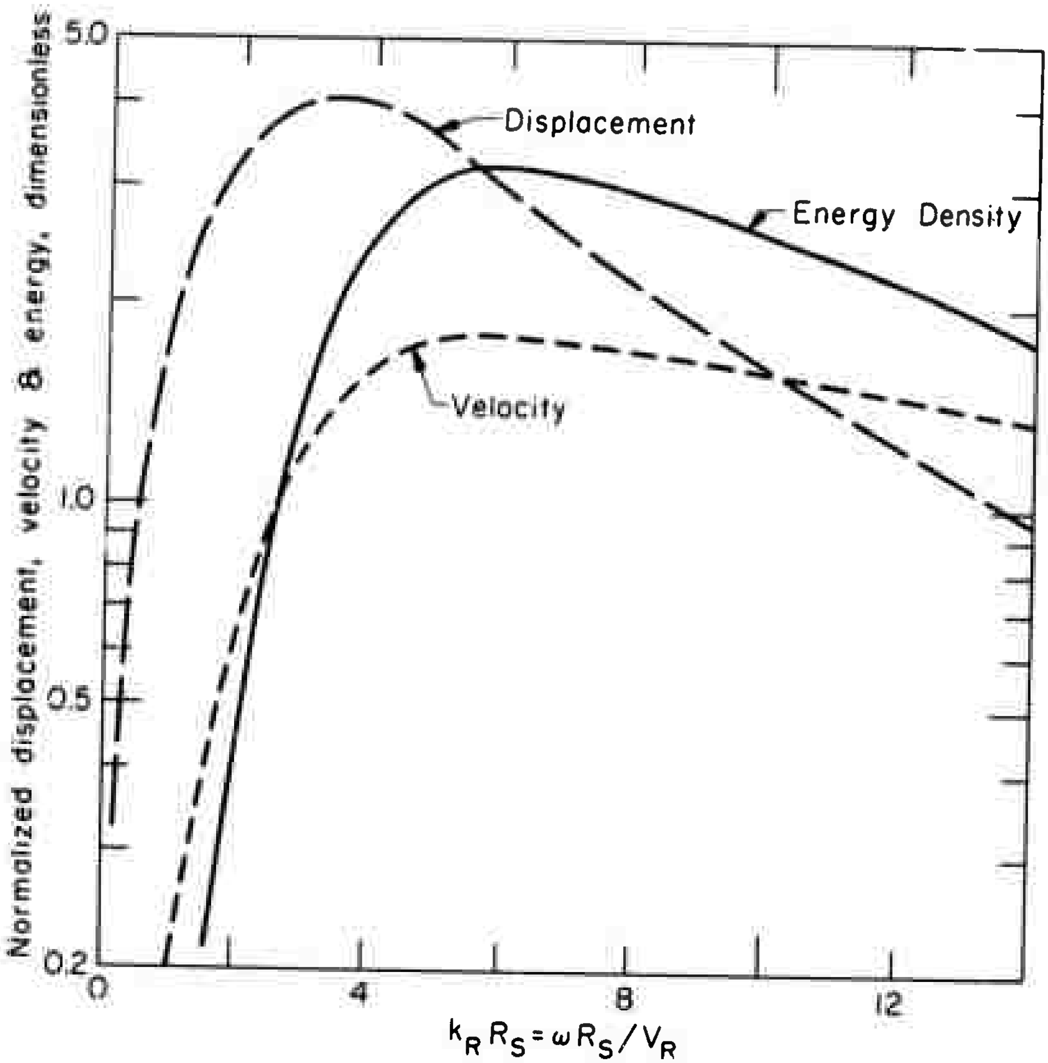

Figure 51. Theoretical Earthquake Spectra Structure.

Event	m_b	f_{max}
5	0.6	24 cps
9	0.6	25 cps
11	0.8	23 cps

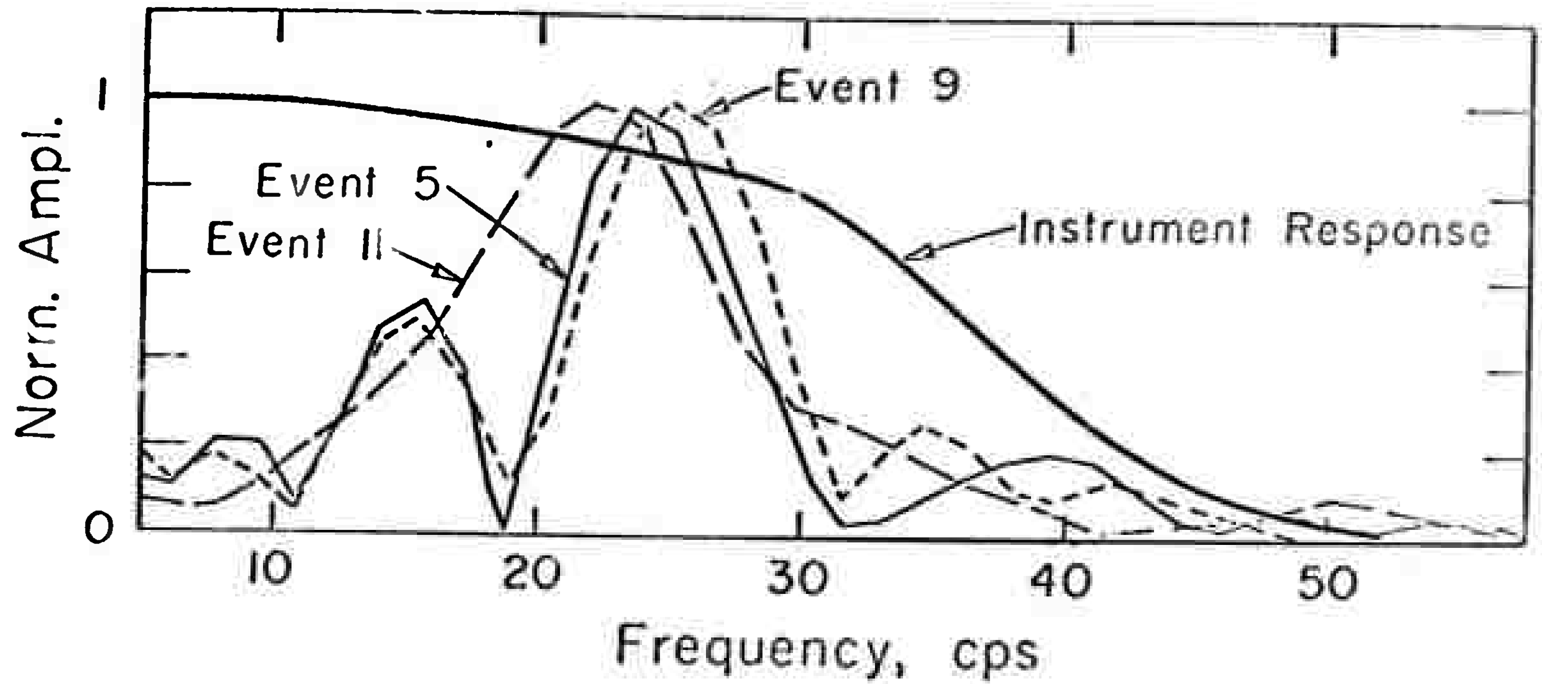

Figure 52. Three California Micro-Earthquake Wave Spectra (*Sammis & Smith*, 1967).

the spectrum of compressional waves at any distance. It is not an easy problem by any means. Smith's observations here show spectra that are shaped roughly like those theoretical spectra that I showed before. The body-wave magnitudes are very low, as you can see. The frequency at which the spectral peak occurs, f_{max} there, is around 25 cycles for all three events. As I said before, the peak frequency is a function of the source dimension, that is the length of the rupture. It also depends on the rupture velocity but is predominantly dependent on the dimension of the rupture. From that peak frequency then one can calculate how extensive the fracture zone was.

MR. HARKRIDER: That is not related to window length. That is pseudo sine X over X, is it not?

MR. ARCHAMBEAU: The effect of truncation is present, yes.

MR. HARKRIDER: You are sure of that? Because it looks like a modified Boxcar.

MR. ALEXANDER: The important thing is the difference in the period of the peaks, not the fact that it looks like Boxcar.

MR. ARCHAMBEAU: The instrument cutoff is one thing that limits the high-frequency definition most certainly.

MR. HARKRIDER: The peak was important, not the holes.

MR. ARCHAMBEAU: The holes are spurious probably.

MR. SMITH: The finite source is not sine X over X.

MR. HARKRIDER: I know it, but is it the window length or is it the source?

MR. ALEXANDER: Were those velocity spectra you were looking at?

MR. HARKRIDER: Those were velocity spectra.

MR. ARCHAMBEAU: The displacement isn't flat at long periods, if that is what you mean. It also has that shape, but a little less pronounced, and the theoretical one was that way, too.

Figure 53 shows observations from a deep earthquake. One of the problems in computing the spectra of the direct compressional

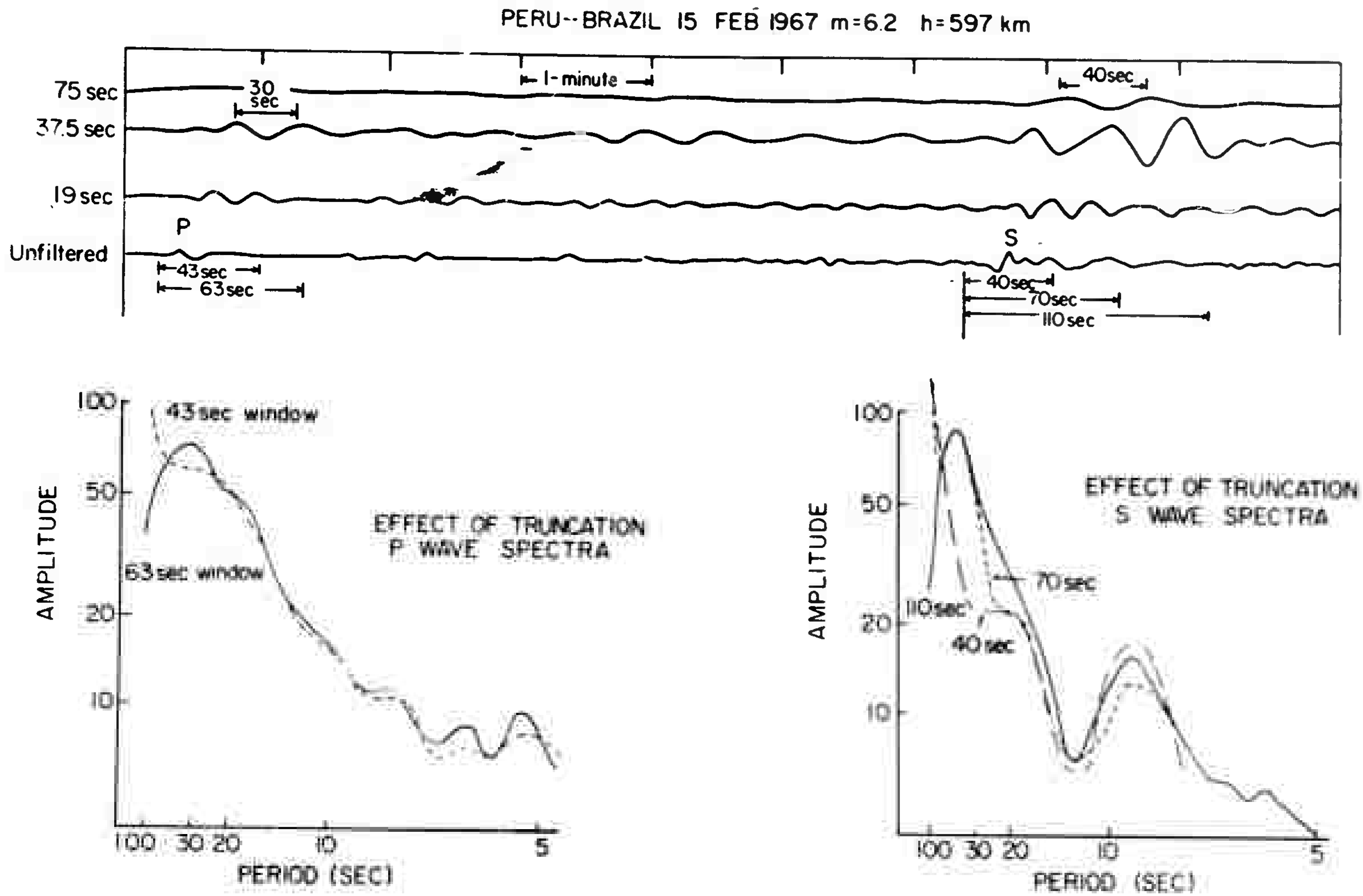

Figure 53. Observations from a Deep Earthquake.

wave from shallow earthquakes is that there are reflections or reverberations following the main or direct arrival, and it is very difficult to truncate the time series at a point such that you know that you only include the direct arrival from the source and not the direct arrival plus a lot of reflected energy. If you include the reverberations, then you get a very complex spectra that is exceedingly difficult to interpret. But what we endeavor to do is compute just the direct wave and look at its spectrum, and then we can correct that back to the source by taking into account propagative effects and absorption in a fairly simple way. For deep earthquakes, however, the separation between reflections is very great in time, so in this case we have a good opportunity of computing accurate spectra which can be interpreted with some degree of confidence. As you can see here, this is the unfiltered seismogram. This recording is from an earthquake in Peru which was at a depth of 597 km in a trench and had a body-wave magnitude of 6.2.

This Figure 53 is from Alan Linde of the Carnegie Institute. He is applying some of my theory to an interpretation of this spectrum which is why I happen to have this figure. It illustrates a number of things. First of all, when you truncate the time series, and the way this is plotted you can't see it very well, but there is long-period motion at the end of the little pulse-like affair, and if you truncate that, then what happens to the spectra is shown by the dotted line. It goes flat for a bit, and then it blows up at low frequencies. That is because of the truncation effects. If you make the window longer in time, then the spectrum comes up, peaks, and then starts down at longer periods, and this is what the theory predicts.

What we are doing is computing the source dimension by using the frequency at which this spectrum peaks. We estimate the stress from the magnitude of the peak amplitude. In short, we are fitting this spectra with the theory, but we are doing it in a parametric way. In any case, we can compute an estimate of the initial stress and the source dimension.

The second spectra is the S wave, shown on the lower right of the figure. The time series shown above are filtered seismograms at various periods so that you can see what the energy is at, for example, 37.5 sec in this S wave. Again as you take different window lengths for the S wave, you get different looking spectra, of course, and the effect is essentially to cause the spectrum to flatten and then blow up when you take too short a window. You get the true spectrum if you take an adequate length of window.

MR. SMITH: Wait a minute. You get a different spectrum, not necessarily a true spectrum.

MR. ARCHAMBEAU: All right, a different spectrum, but one more representative of the true spectrum.

MR. ALEXANDER: In fact, I do not think any of them give the true spectrum for the indicated frequency. Judging by examples, none of them is the correct spectrum.

MR. SMITH: Isn't that the conclusion you reach from here, that you can't measure the spectrum of long-period waves without long samples?

MR. ARCHAMBEAU: That is right.

MR. SMITH: The base line has to be treated differently, or you could not get a result like that. It must be a trend or something, or a mean taken out of those signals before processing.

MR. ARCHAMBEAU: This was a very low drift instrument, so I don't think there is any particular problem with that.

MR. SMITH: Isn't there a mean or something like that taken out that would be different with different window lengths?

MR. ARCHAMBEAU: I am not positive what he did. I'd have to check in his paper to be absolutely sure.

MR. SMITH: Because if everything else is equal, the effect of truncation is simply a convolution of sine X over X. That won't make it go below frequency. It has to be some other thing.

MR. ARCHAMBEAU: No, even if you don't take the trend out you get this effect.

MR. SMITH: But there has to be a trend taken out of there. That is what is giving you the phony frequencies.

MR. ARCHAMBEAU: Yes, that is right, if you detrend you get spurious results also, but you don't know how to do that anyway, and I don't think it was done here. Your point is that you don't really know how to detrend these things in any case, no matter what length window you take, so you get a spectrum that is shaped like that and is bad at the long periods if you try to detrend with the different window lengths. But you get bad spectra with truncation whether you detrend or not in reality.

MR. FRASIER: Do you do that at another station?

MR. ARCHAMBEAU: Yes, it has been done at other stations. You get the same thing.

In any case, these are spectra from earthquakes which we can compare to explosions, and there are differences, and such differences are used as a basis for discrimination. The next slide (Figure 54) shows Love-wave and Rayleigh-wave spectra from the Shoal event compared to the surface-wave spectra from the Fallon earthquake, which occurred at essentially the same place. The lower plot shows the difference in excitation of the Love waves between the explosion and the earthquake as a function of period. The difference displayed here can be used as a discriminant. At the short periods, excitation from the explosion is relatively higher than it is for the earthquake. These are comparable magnitudes, body-wave magnitudes, from these two events. In fact, Shoal was a little greater magnitude. So the ratio of amplitudes falls off with increasing period. This occurs primarily because of a difference in source dimension between the explosion and the earthquake. The ratioing used here should cancel out the structural effects, because the observations are the same station. However, there is a dip in the Love amplitude at around 20 sec that is not represented in both of the observed spectra in exactly the same way. You expect such effects, but generally the trend of the ratio is as the solid line shows it. Therefore, if you calibrate an area, and by that I mean if you look at an event in an area and you observe that it is an earthquake, then any subsequent event can be compared to that earthquake, where this kind of a plot would be made using the Love waves. If the Love-wave ratio shows this 1/T dependence on period, then the event is an explosion. If the ratio is flat, then it is an earthquake. This is probably not a foolproof discriminant by itself.

Figure 55 shows the same kind of comparison using the Bilby explosion, which had a higher magnitude than Shoal and so had better power at the longer periods. This event is used to get better definition for the longer period ratio to show in fact that when you have good power at long periods in both events, then you clearly get the 1/T dependence. Bilby had a magnitude of 5.8 and Fallon was 3.8, so there is quite a difference in energy. Yet you still get fall-off towards longer periods in the manner shown. I think one can conclude that this is a useful discriminant.

The next way that one has of discriminating using long-period surface-wave information is to compute the spectral ratios of Love waves to Rayleigh waves, and I have shown you the radiation patterns, so you know something about that. If you are at one station, and you want a one-station discriminant, then one thing you can do is to look at the ratio of Love to Rayleigh waves. Explosions are generally more efficient in their generation of Rayleigh waves relative to Love waves, even if you have tectonic release, whereas earthquakes are very efficient in terms of their Love-wave production relative to Rayleigh waves, although because of the different radiation patterns for Love and Rayleigh waves, the ratio is in fact station dependent. You have to have a few stations for positive identification. But in any case, this is a way of discriminating.

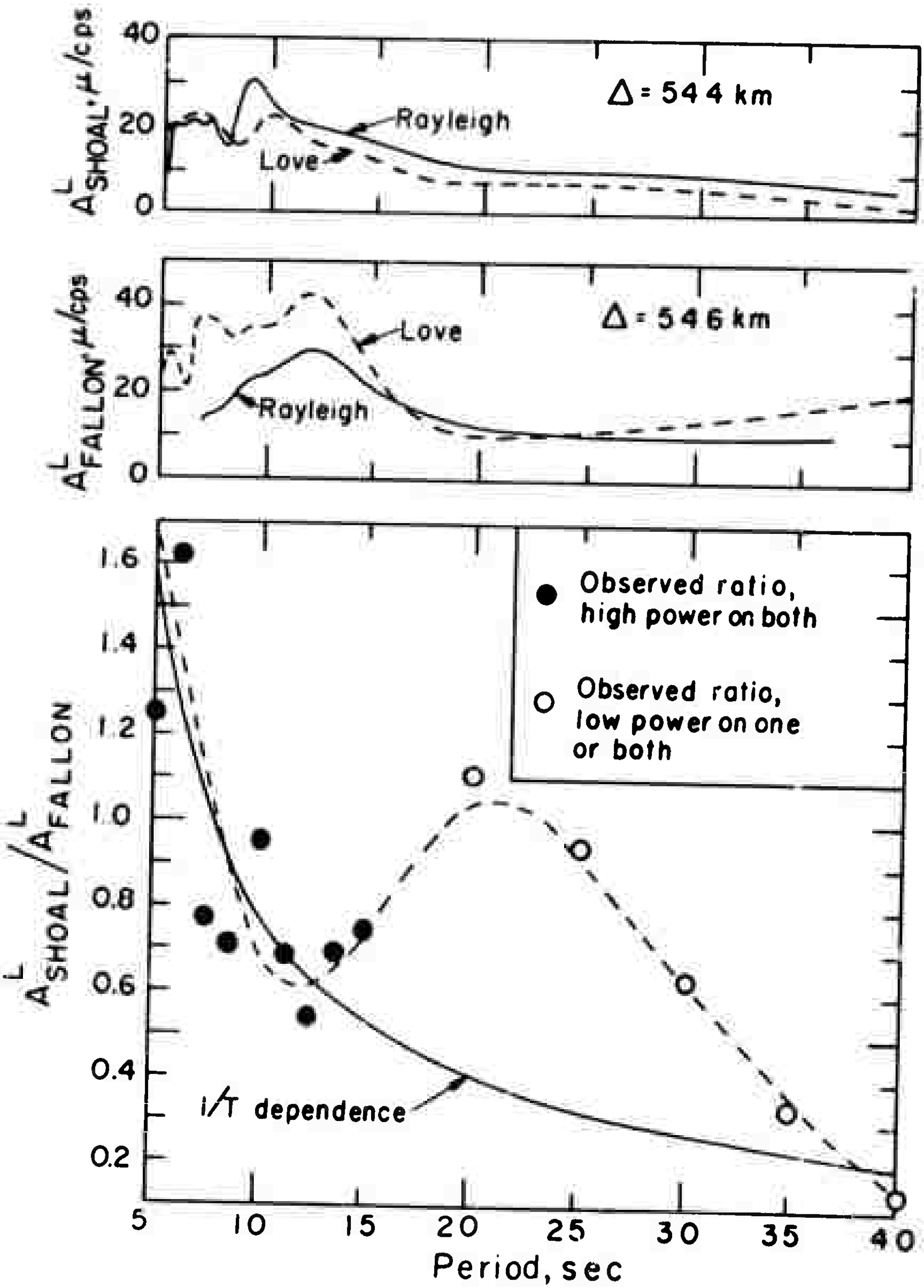

Figure 54. Comparison of Wave Spectra from an Explosion (Shoal) and an Earthquake (Fallon).

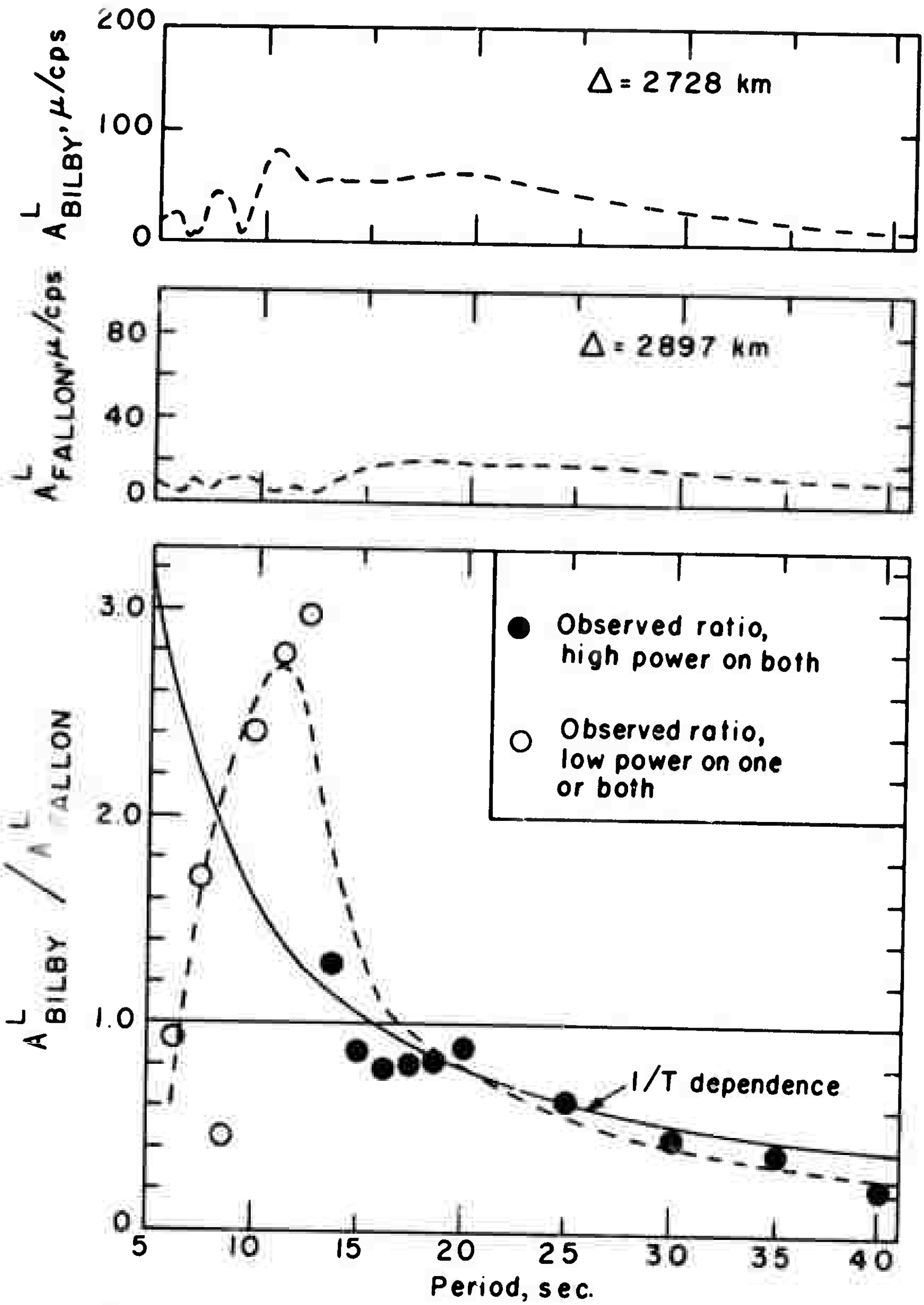

Figure 55. Comparison of Wave Spectra from the Bilby Explosion and the Fallon Earthquake.

In Figure 56 we have L/R for two explosions, Bilby and Shoal again, and this solid line is L/R for the Fallon earthquake. Generally speaking, you expect this ratio to be around one as a function of period for an explosion, and to increase in the manner shown for an earthquake. That is, the long-period excitation of Love waves is fairly efficient for an earthquake but not for an explosion. So there should be this considerable difference. You might draw a horizontal line across this graph some place, say at L/R = 2.0 and if the observed L/R falls below that value consistently over a wide period range then the event would be an explosion and anything consistently above it should be an earthquake.

MR. ROTENBERG: Are these amplitude ratios, or energy ratios?

MR. ARCHAMBEAU: These are amplitude ratios. This is going to be very much affected by structure, so in order to see what that effect is, we can look at this ratio for these sources at different distances, and see whether it holds up at greater distances. Figure 57 is this observation made at around 1700 km from these sources. Things are starting to degenerate here at the shorter periods, so if you are comparing in the range from around 10 to 15 sec at greater distances, you are going to be in trouble with this kind of a discriminant. However, if you work out to the longer periods, the discrimination still holds. At yet a greater distance, things are starting to break down somewhat, as you see in Figure 58. You are looking at very low power now. But nevertheless, at 3,000 km, except for this peak at 30 sec, for the ratio appropriate to the Shoal explosion we still have a reasonably strong difference. The probable origin of the peak in L/R for shoal is due to structure, since, while both of these should be roughly affected by structure in the same way so that the ratio might remain more or less constant, in point of fact this is not precisely true because Love waves are affected somewhat differently by structure than are Rayleigh waves, so that you can have a minimum in the Love-wave spectra without that occurring for the Rayleigh waves. Therefore you can get a ratio with these kinds of narrow spikes, but the ratio should come back down, and in fact it does. So generally speaking, even at these great distances, explosions have low L/R ratios as functions of period, and the earthquake has a high ratio, and in fact on this figure it runs off the graph here up to about 10, I believe, out at around 40 or 50 sec.

I should mention at this point that Shelton Alexander talked about m_b versus M_s, which is the most popular technique for discrimination based on spectral differences. There are other ways of discriminating that are variations of that approach, and some which I believe are superior but have not been utilized particularly much. The three that I've just described all utilize the difference in excitation of long periods for earthquakes relative to explosions.

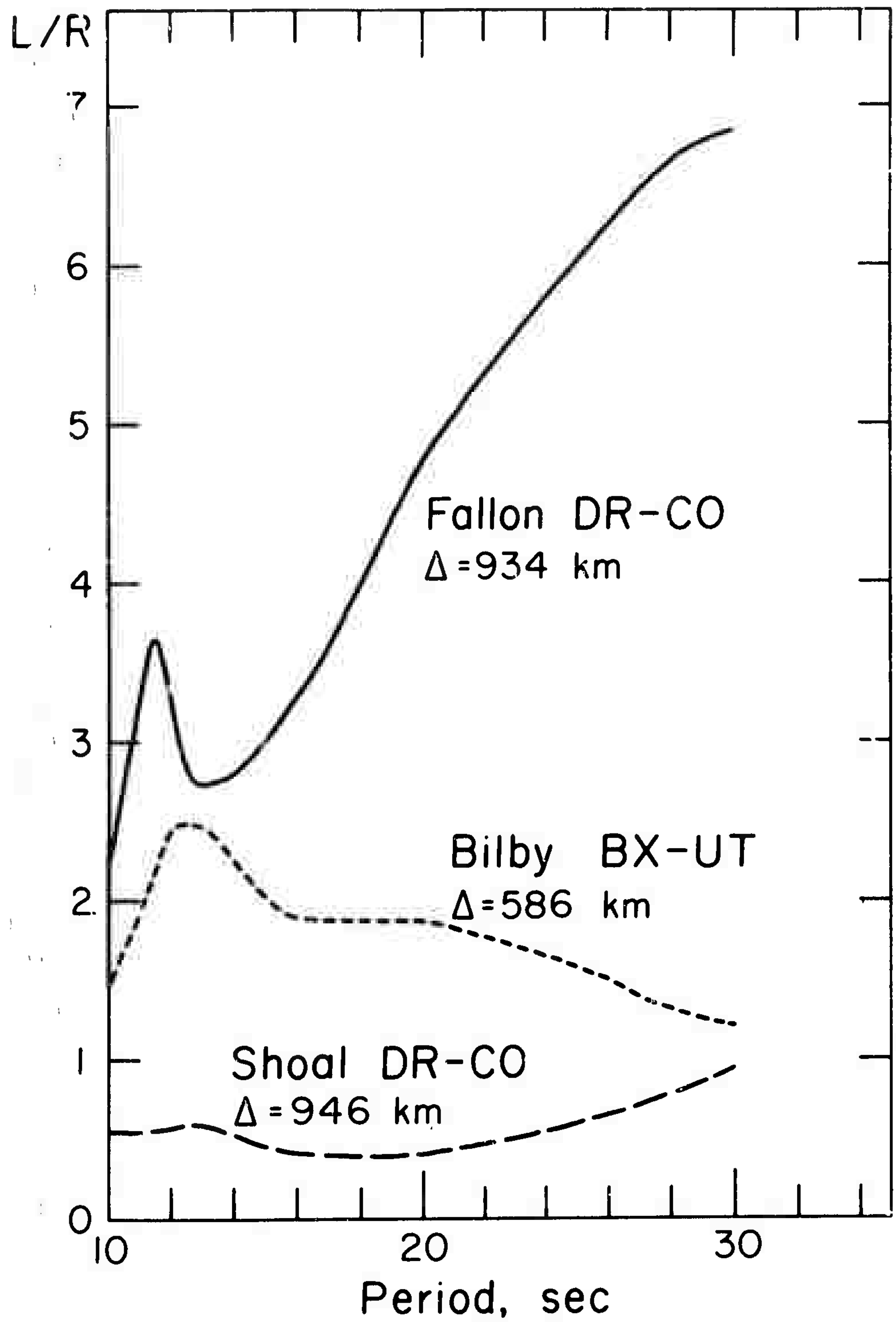

Figure 56. Ratio of Love to Rayleigh Waves for Two Explosions (Bilby and Shoal) and an Earthquake (Fallon).

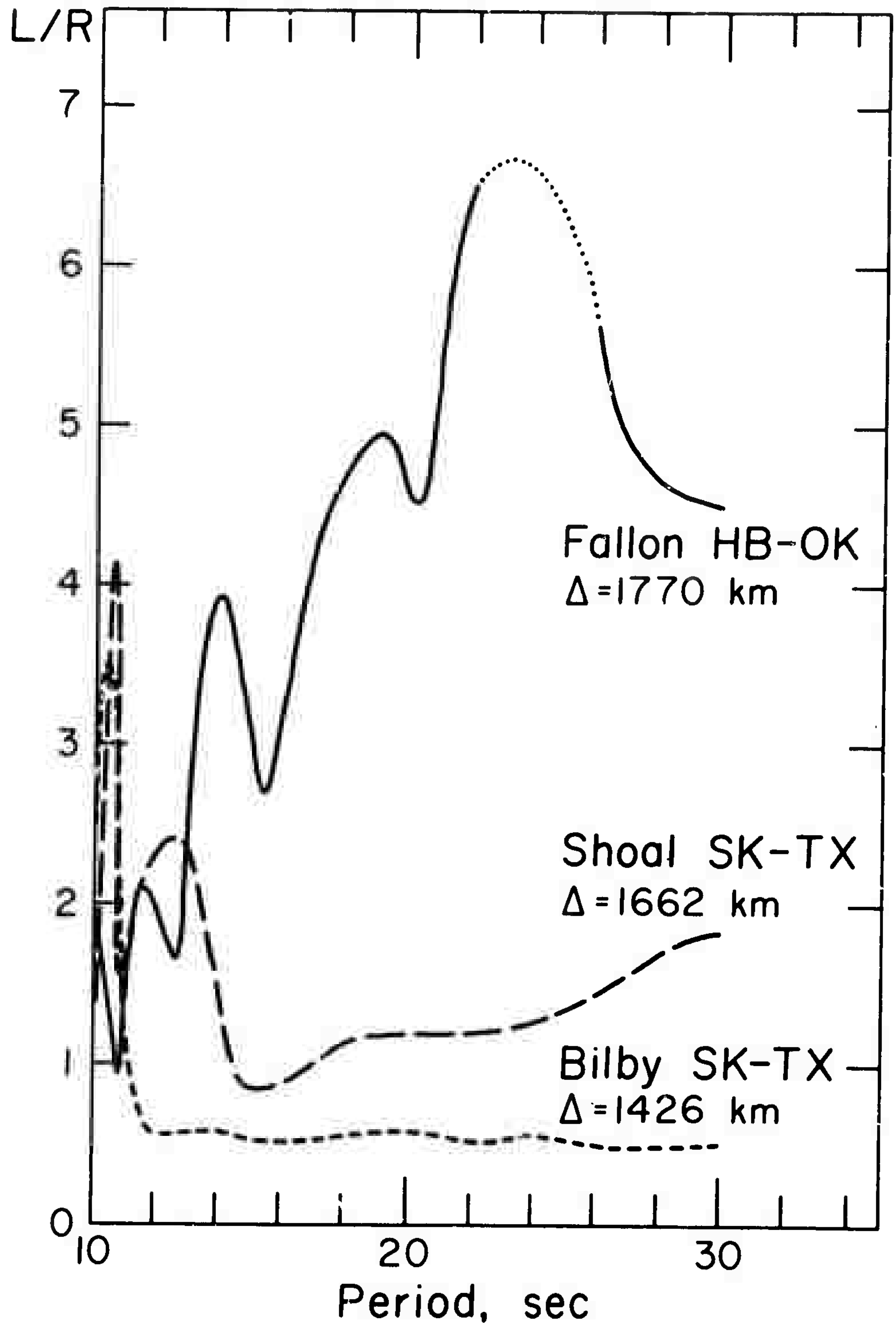

Figure 57. Ratio of Love to Rayleigh Waves for Two Explosions (Bilby and Shoal) and an Earthquake (Fallon).

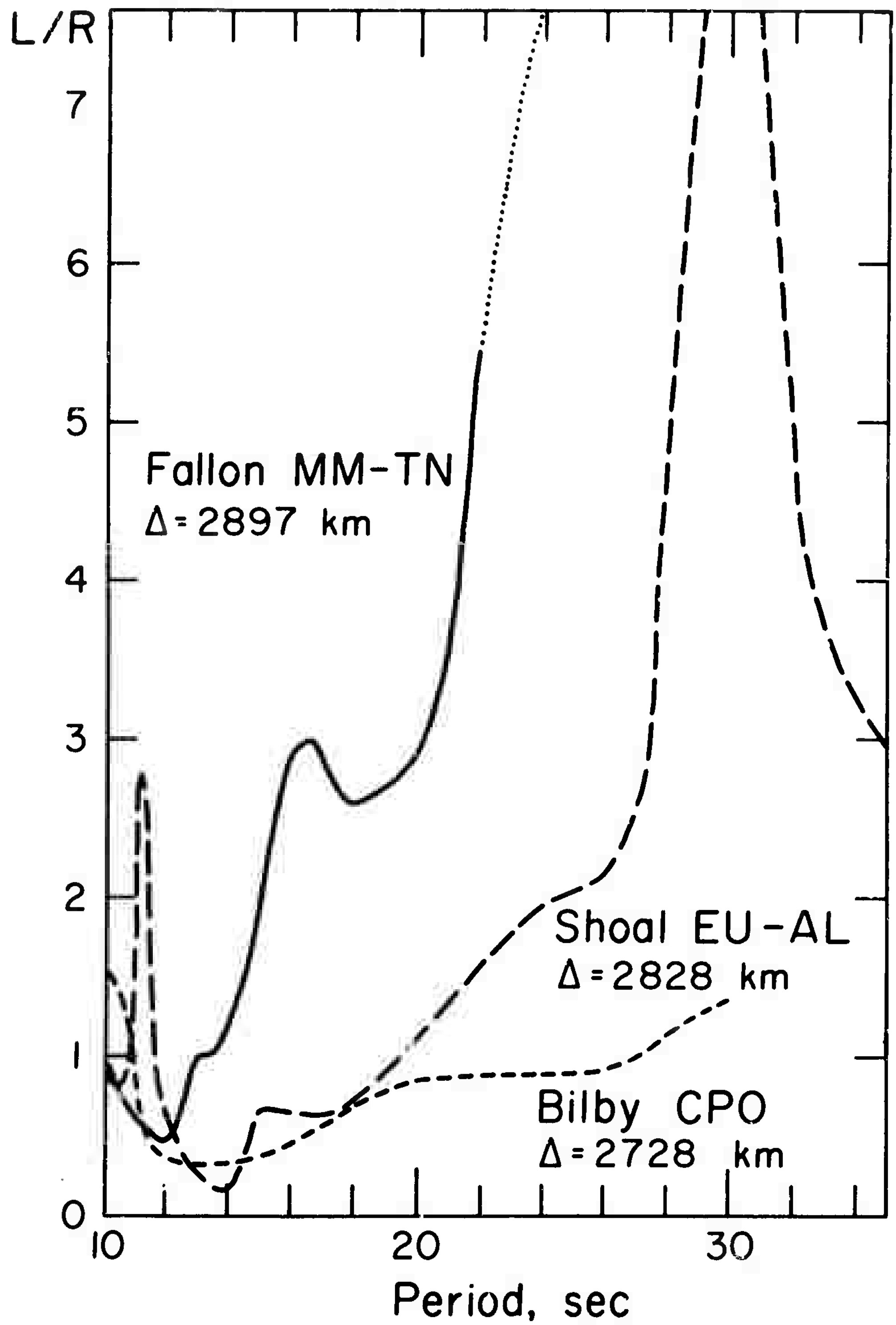

Figure 58. Ratio of Love to Rayleigh Waves for Two Explosions (Bilby and Shoal) and an Earthquake (Fallon).

MR. ALEXANDER: I have looked at relatively close-in measurements for short periods for the Shoal and Fallon events that Archie was discussing. He was discussing primarily the Love and Rayleigh waves in the long period.

This Figure 59 is seen just to the north of Haley, Idaho, and the top half is the Shoal event. The top trace is vertical, the second one is the radial, and the bottom one is the transverse for the Shoal event. Below are similar traces for the Fallon earthquake. These are both comparable body-wave magnitude events.

Just looking at the seismograms, those ticks in the bottom there are at 10-sec intervals. You can see right away there is a big difference in distribution of energy with time between Shoal and Fallon. I have looked at lots of other azimuths in the same fashion. Figure 60 shows just the verticals from the previous figure, and a running spectrum or "seismoprint" so you can get an idea of the spectral distribution of energy with time down the record. The record is at the top here, and you just move a window along and compute the power in a running window. I am sorry you cannot see the contours, but the maximum is here in this case (Fallon). These plots are all normalized to the maximum power value. By and large most of the energy is concentrated late in the record. These are the shear waves or higher modes; in effect, they are surface waves.

In the case of Shoal, the energy is highly concentrated at the beginning, and more or less shifted to higher frequencies. In other words, the energy distribution in both time and frequency are different between the two. I really do not have enough earthquakes to establish the generality of this, but looking at various azimuths I see the same sort of thing for Shoal and Fallon.

MR. ROTENBERG: Is there a standardized window that seismologists use to do this kind of analysis?

MR. ALEXANDER: No, not really. In this particular case, though, the window is 3 sec. Tests were made to decide what was a reasonable window to use. The effect of not using the correct window, of course, is to smear the energy out, but still it is obvious from just looking at the signal where the energy is located. In any case this will give you a feeling for the kinds of differences observed between earthquakes and explosions of comparable magnitudes. This is the typical kind of thing that we have to resolve. There is really a striking difference in the energy distribution, and it shows up in the long period surface waves also.

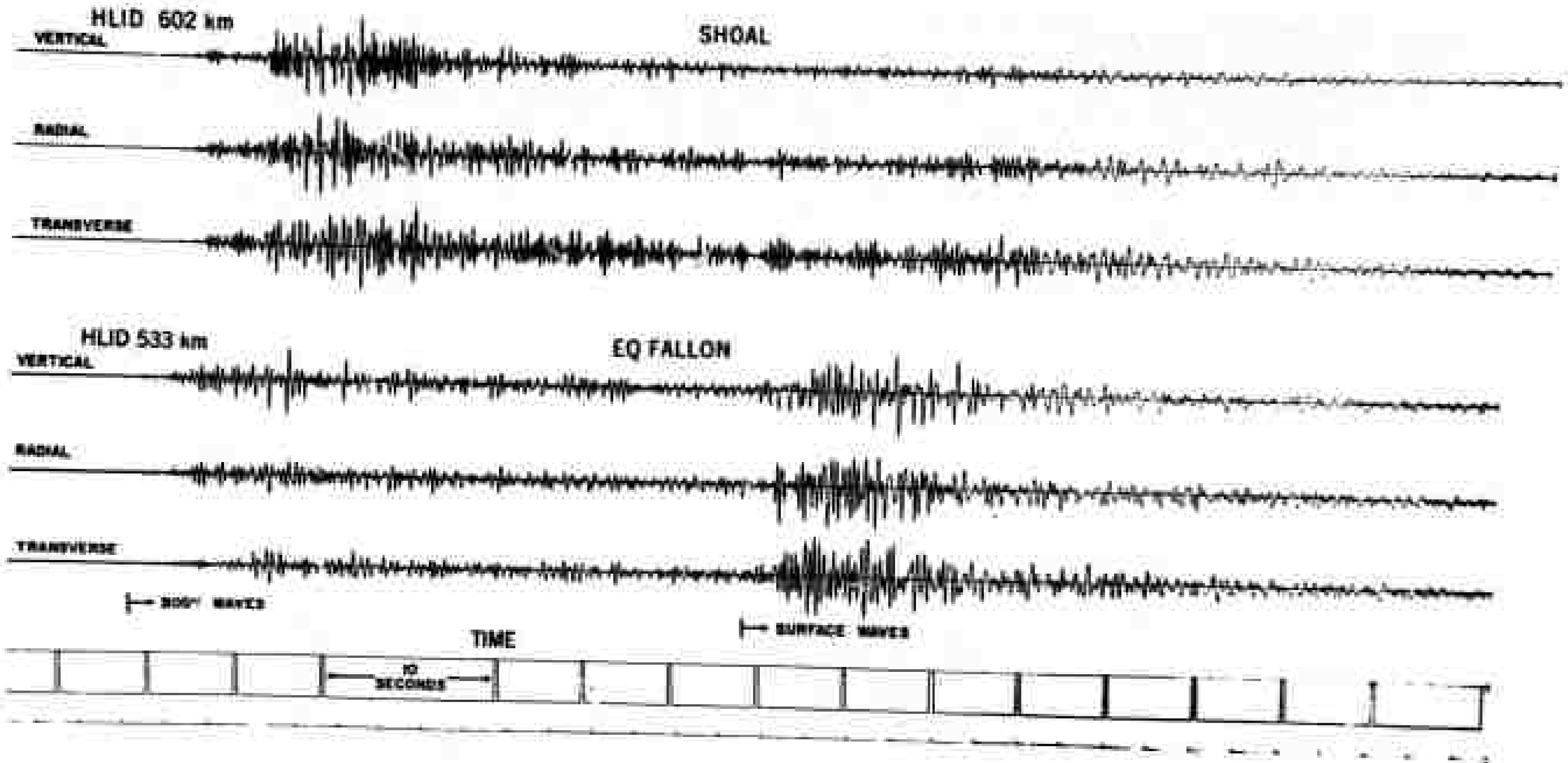

Figure 59. Wave Spectra at Station HL-ID.

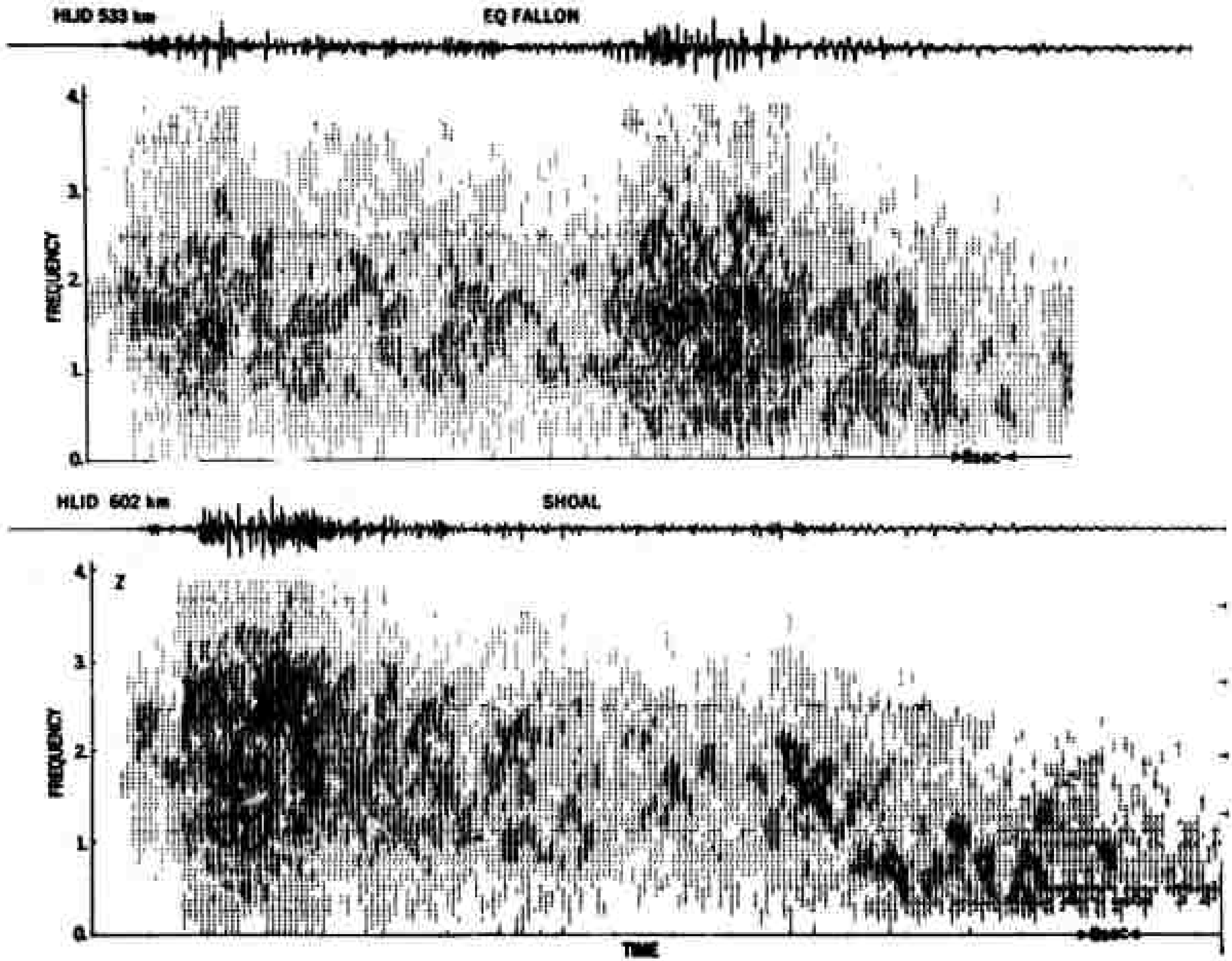

Figure 60. Vertical Traces from Figure 59.

CODE CALCULATIONS: STRESS-WAVE PROPAGATION IN A PRESTRESSED ENVIRONMENT

J. Ted Cherry
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A simple mechanism is available for amplifying and changing the velocity field associated with a particular type of elastic stress wave. The suggested mechanism is the interaction of an elastic shear wave with a localized gradient in the ambient stress field. The ambient stress field couples into the equations of motion due to the rotation of the stress field caused by the incident shear wave. This coupling causes a velocity field to develop which sends the prestressed region into either a state of compression or extension, which in turn depends on the sign of the rotation and the sign of the gradient of the ambient stress field.

Here we report the results of a TENSOR calculation that illustrates some of the features of this type of coupling. Some questions are raised that I have not been able to answer except in a qualitative way.

Statement of the Problem

An ambient stress field T^A_{xy} was initially placed in static equilibrium by using an appropriate body force Y, where

$$\frac{1}{\rho}\frac{\partial T^A_{xy}}{\partial x} + Y = 0 \tag{1}$$

In the TENSOR problem T^A_{xy} was specified as

$$\begin{aligned} T^A_{xy} &= A_1\left[1 + \cos 2\pi\left(\frac{x^0}{A_2}\right)\right] & \left|x^0\right| &\le \frac{A_2}{2} \\ &= 0 & \left|x^0\right| &> \frac{A_2}{2} \end{aligned} \tag{2}$$

where x^0 is the center of the zone at $t = 0$. In the calculation we set $A_1 = 1$ kb and $A_2 = 0.01$ m and 0.05 m.

The body force Y was evaluated such that Equation 1 was satisfied. The zone center was calculated on each cycle to find the body force appropriate to the zone position.

Figure 61 shows the variation of T^A_{xy} with distance. Figure 62 shows a sketch of the problem run on TENSOR. The initial line marked "velocity input" was given a velocity u^i in the x direction that varied with time.

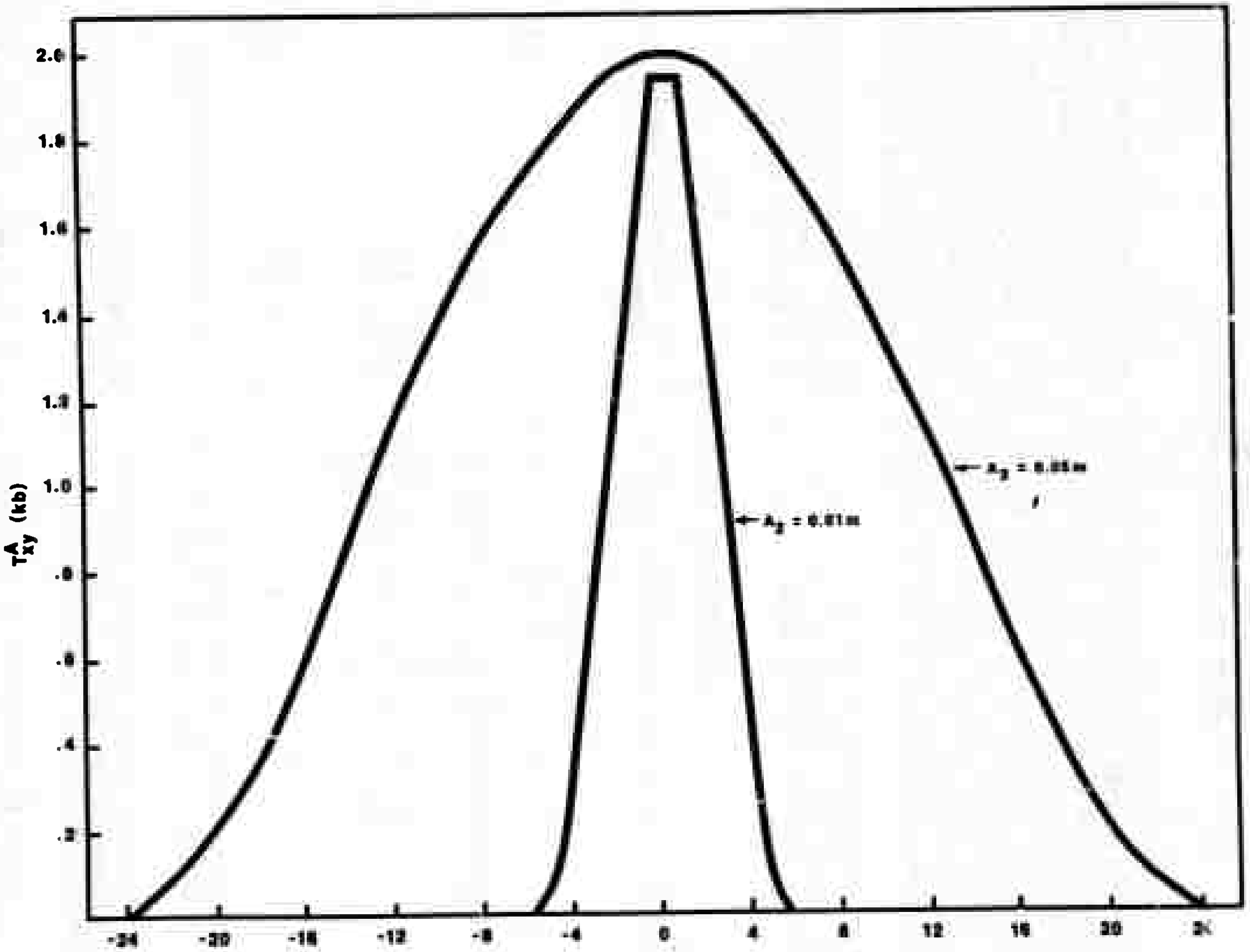


Figure 61. T^A_{xy} vs Distance (x).

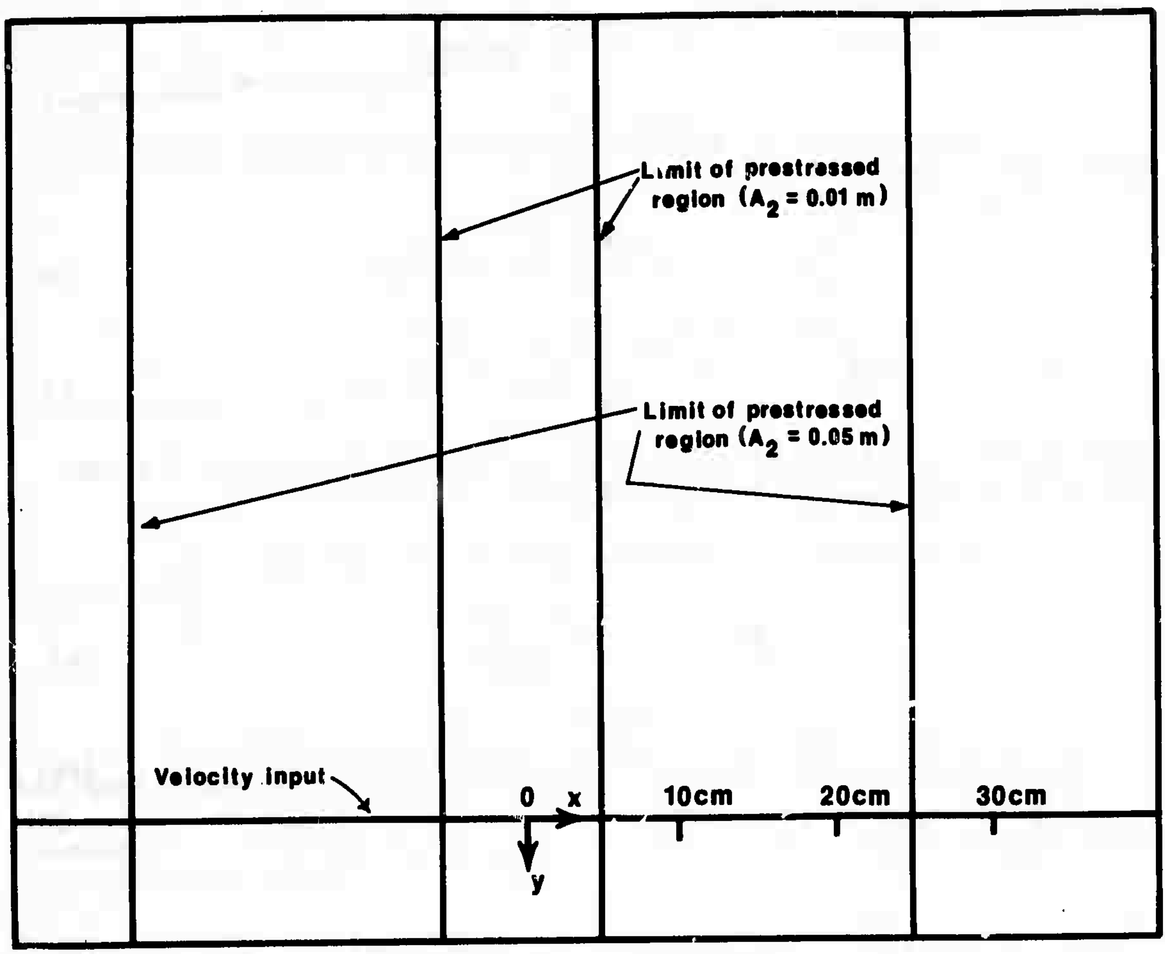


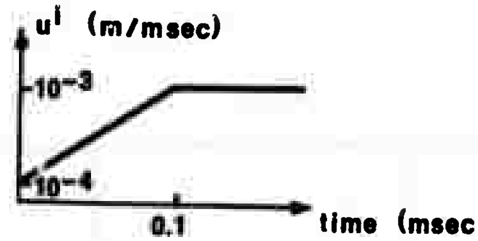
Figure 62. Sketch of TENSOR Problems.

$$u^i = B_1 + B_2 t \qquad 0 \le t \le T$$
$$= B_1 + B_2 T \qquad t > T \tag{3}$$

where $B_1 = 10^{-4}$ m/msec

$B_2 = 9 \times 10^{-3}$ m/msec^2

$T = 0.1$ msec



The equations of motion numerically integrated by TENSOR (in plane strain) are

$$\frac{du}{dt} = -\frac{1}{\rho}\frac{\partial(P - T_x)}{\partial x} + \frac{1}{\rho}\frac{\partial T_{xy}}{\partial y} \tag{4}$$

$$\frac{dv}{dt} = -\frac{1}{\rho}\frac{\partial(P - T_y)}{\partial y} + \frac{1}{\rho}\frac{\partial T_{xy}}{\partial x} + Y \tag{5}$$

where u and v are particle velocity in the x and y directions, P is the mean stress, and T_x, T_y, and T_{xy} are stress deviators.

The stress-strain relations used in the code are

$$P^{n+1} = k\left(\frac{V^0 - V^{n+1}}{V^{n+1}}\right) \tag{6a}$$

$$T_{xy}^{n+1} = T_{xy}^{n} + 2\mu\Delta t^{n+1/2}\,\dot{e}_{xy} + (T_x^n - T_y^n)\dot{\phi}\Delta t^{n+1/2} \tag{6b}$$

where $T_{xy}^0 = T_{xy}^A$.

$$T_x^{n+1} = T_x^n + 2\mu\Delta t^{n+1/2}\,\dot{e}_x + 2T_{xy}^n\dot{\phi}\Delta t^{n+1/2} \tag{6c}$$

$$T_y^{n+1} = T_y^n + 2\mu\Delta t^{n+1/2}\,\dot{e}_y - 2T_{xy}^n\dot{\phi}\Delta t^{n+1/2} \tag{6d}$$

The last terms on the right in Equations 6b, 6c, and 6d allow the Eulerian components of the stress tensor to change during a rigid body rotation even though no strain is occurring.

In these equations k and μ are the bulk and rigidity moduli, n is the cycle number, $\Delta t^{n+1/2}$ is the current time step and

$$\dot{e}_{xy} = \frac{1}{2}\left(\frac{\partial u}{\partial y} + \frac{\partial v}{\partial x}\right) \tag{7a}$$

$$\dot{e}_{x} = \frac{1}{3}\left(2\frac{\partial u}{\partial x} - \frac{\partial v}{\partial y}\right) \tag{7b}$$

$$\dot{e}_{y} = \frac{1}{3}\left(2\frac{\partial v}{\partial y} - \frac{\partial u}{\partial x}\right) \tag{7c}$$

$$\dot{\phi} = \frac{1}{2}\left(\frac{\partial u}{\partial y} - \frac{\partial v}{\partial x}\right) \tag{7d}$$

= the angular velocity of the zone.

Since

$$\frac{\dot{V}}{V} = \frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} \quad \text{(conservation of mass)} \tag{8}$$

Equation 6a can be written

$$P^{n+1} = P^{n} - k\left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y}\right)\Delta t^{n+1/2} \tag{9}$$

assuming $\dfrac{V^{n+1/2}}{V^0} \approx 1$.

What to Expect

We can get an idea about how the problem should behave if we make some simplifying assumptions about the stress-velocity field coupling. To do this we first obtain the analytic solution to the problem assuming no prestressed region $T^A_{xy} = 0$. We then apply this solution to the prestressed region.

If $T^A_{xy} = 0$, then, for the velocity source function specified by Equation 3, we have the standard solution for a plane shear wave propagating in the y direction. These solutions are

$$S_x(y,t) = \left[B_1(t - y/V_s) + \frac{B_2}{2}(t - y/V_s)^2\right] H(t - y/V_s) \quad y > 0 \tag{10}$$

$$u(y,t) = \left[B_1 + B_2(t - y/V_s)\right] H(t - y/V_s) \quad y > 0 \tag{11}$$

where S_x is the displacement in the x direction

$$V_s = \sqrt{\frac{\mu}{\rho}} \qquad \text{(the shear-wave velocity)}$$

$$H(t - y/V_s) = 1 \qquad t - y/V_s > 0$$
$$= 0 \qquad t - y/V_s < 0$$

We can use Equations 6b and 7a to find $\dot{T}_{xy}$ and 7d to find $\dot{\phi}$

$$\dot{T}_{xy} \approx \mu \frac{\partial u}{\partial y} = \frac{\mu}{V_s}\left[B_1\delta(t - y/V_s) + B_2H(t - y/V_s)\right] \tag{12}$$

$$T_{xy} = -\frac{\mu}{V_s}\left[B_1 + B_2(t - y/V_s)\right] H(t - y/V_s) \tag{13}$$

$$\dot{\phi} = \frac{1}{2}\frac{\partial u}{\partial y} = \frac{\dot{T}_{xy}}{2\mu} \tag{14}$$

$$\phi = \frac{T_{xy}}{2\mu} \tag{15}$$

In the region where $T^A_{xy} \neq 0$, then Equations 6c and 6d give

$$T_x \approx 2T^A_{xy}\phi \tag{16}$$

$$T_y \approx -2T^A_{xy}\phi \tag{17}$$

where we have assumed $\dot{e}_x = \dot{e}_y = 0$ and $T_{xy} = T^A_{xy}$.

With these assumptions the only significant contribution in acceleration will be the x direction. Equation 4 gives

$$\frac{du}{dt} \approx \frac{\partial u}{\partial t} \approx \frac{1}{\rho}\frac{\partial T_x}{\partial x} = \frac{2\phi}{\rho}\frac{\partial T^A_{xy}}{\partial x} \tag{18}$$

Since ϕ is negative (Equation 15) then accelerations should develop in the prestressed region that cause the region to expand.

$$\phi \frac{\partial T^A_{xy}}{\partial x} > 0 \qquad x > 0$$

$$\phi \frac{\partial T^A_{xy}}{\partial x} < 0 \qquad x < 0$$

Assuming $$\frac{\partial T_{xy}}{\partial x} = \frac{\partial T^A_{xy}}{\partial x}$$

in the prestressed region, then

$$u = \frac{2}{\rho} \frac{\partial T^A_{xy}}{\partial x} \int \phi dt$$

$$\frac{\partial u}{\partial x} = \frac{2}{\rho} \int \phi dt \, \frac{\partial^2 T^A_{xy}}{\partial x^2}$$

Therefore Equation 9 gives

$$\dot{P} = -\frac{k}{\rho} \frac{\partial^2 T^A_{xy}}{\partial x^2} \int 2\phi dt \qquad (19)$$

Since $$\int 2\phi dt = -\frac{1}{V_s}\left[B_1 t + \frac{B_2}{2}(t - y/V_s)^2\right] H(t - y/V_s) \qquad (20)$$

and $$\frac{\partial^2 T^A_{xy}}{\partial x^2} = -\left(\frac{2\pi}{A_2}\right)^2 A_1 \cos 2\pi\left(\frac{x^0}{A_2}\right) \qquad (21)$$

then the rate of expansion $\dot{P}$ should increase with time and should decrease as the wavelength A_2 of the prestressed region increases. Equation 12 shows that $\dot{T}_{xy}$ should be constant for $t > y/V_s$.

The Numerical Solution

The input constants for the TENSOR problem were

$$\left.\begin{aligned} A_1 &= 1 \text{ kb} \\ A_2 &= 0.01 \text{ m, } 0.05 \text{ m} \end{aligned}\right\} \text{Equation 2}$$

$$\left.\begin{aligned} B_1 &= 10^{-4} \text{ m/msec} \\ B_2 &= 9 \times 10^{-3} \text{ m/msec}^2 \\ T &= 0.1 \text{ msec} \end{aligned}\right\} \text{Equation 3}$$

$$\left.\begin{aligned} k &= 10 \text{ kb} \\ \mu &= 10 \text{ kb} \end{aligned}\right\} \text{Equation 6}$$

$$\rho^0 = 1 \text{ gm/cc}$$

Figure 63 compares the analytic (Equation 11) and TENSOR solutions at 0.0462 msec outside the prestressed region. The code seems to be adequately treating the propagation of the shear wave in the stress-free region.

Figure 64 shows T_{xy} vs time outside the prestressed region and P (mean stress) vs time at the center of the prestressed region. Both T_{xy} and P behave as expected. The prestressed region does expand, with the rate of expansion varying with time and the wavelength of the ambient stress field.

The significance of the coupling shown in Figure 64 is that the material would become weaker due to the expansion, thereby lowering its ability to support its prestressed state.

Summary and Questions

This problem is extraordinarily artificial both in the assumed elastic constants and "fault" representation. It was the simplest problem that I could think of to run for an illustration of the shear wave-ambient stress field coupling. It would be much neater to develop a crack in the TENSOR grid by allowing a slip surface to open under the action of tensile forces applied at the grid boundaries. This prestressed equilibrium state could then be subjected to a transient disturbance (either compressional or shear).

In terms of "earthquake triggering" the importance of the prestressed state of the rock and how to determine it preshot still are unanswered questions. Since large stress gradients should give a "noisy" static equilibrium state, we might consider monitoring the seismic noise in the shot hole for some time prior to the shot. Data from this effort would be qualitative. However, after enough sites are listened to, we should develop the experience to isolate those sites with high stress gradients, i.e., high, unexplained seismic noise.

If shear-wave generation is a worry, then the impedance contrasts of the layers at the site (including the free surface) along with the asymmetry of the source become important. It should be possible to evaluate a site in terms of the total shear energy developed by the source and mode conversion at the important interfaces.

MR. RINEY: Did this have a free surface?

MR. CHERRY: No, there was no free surface.

MR. SMITH: Could you say once again what the physical rationale for this is?

MR. CHERRY: We have written the equation of motion in terms of the stresses referred to the fixed x-y coordinate system. When a volume element with stresses in it sees a rigid-body rotation, you have to bring the stresses

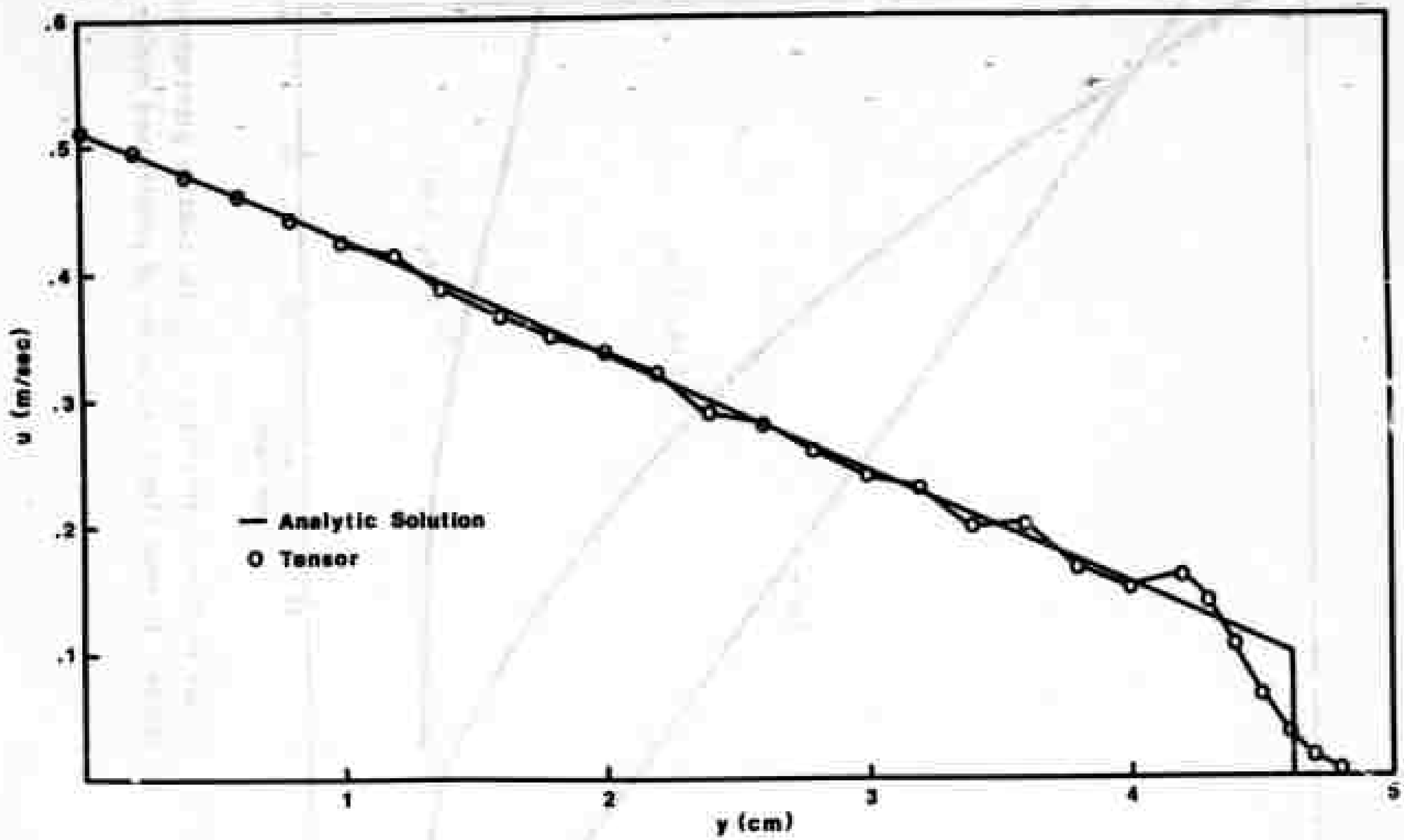


Figure 63. Horizontal Velocity (u) vs Depth from Interface (y) Outside Prestressed Region
t = 46.2 μsec.

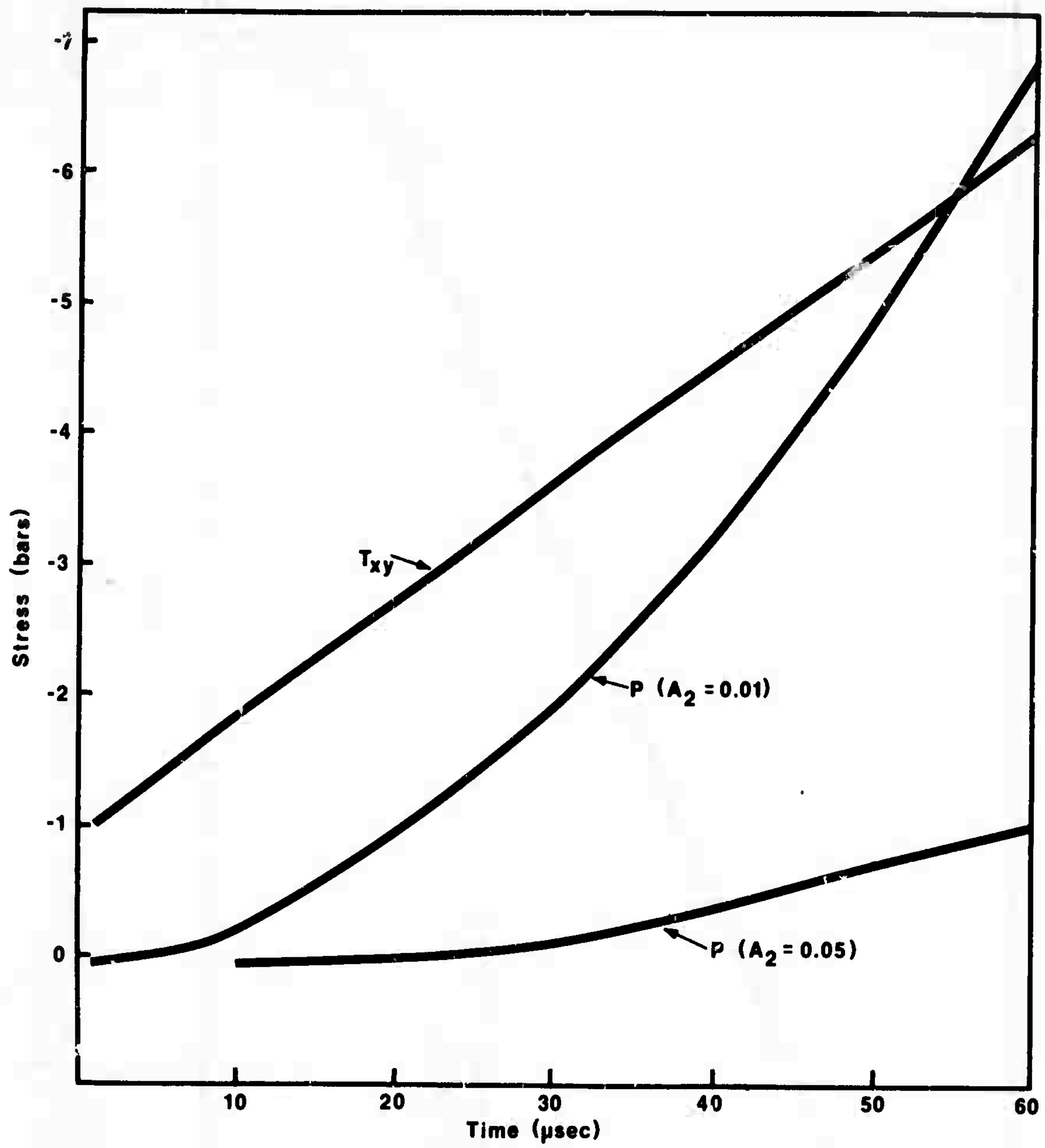


Figure 64. Stress vs Time at Interface. T_{xy} far outside prestressed region. P, mean stress, at center of prestressed region.

back to the x-y coordinate system, since that coordinate system is what the equations of motion are written for. That is all that is really involved. It is simply a rotation back to the equation of motion coordinate system.

MR. SMITH: So you are permitting rigid-body rotations. You are not applying some other force system.

MR. CHERRY: This shear wave is producing a rigid-body rotation along with shear strains.

MR. SMITH: So you are letting it happen.

MR. CHERRY: Yes, it is happening because of the source function that I have used. In other words, what sort of wave is now propagating into this prestressed region. The original displacement field is at $y = 0$. So all along the x axis I am propagating a plain SH wave into the region.

MR. ARCHAMBEAU: All right, for a compressional wave ....

MR. CHERRY: For a compressional wave you would not see this.

MR. ARCHAMBEAU: So that your nonlinear effects then are effectively zero.

MR. CHERRY: Right. The thing you need in order for this to work is the rigid-body rotation.

MR. ARCHAMBEAU: Which involves the curl.

MR. CHERRY: Yes, $(1/2)\Delta x\vec{S}$.

MR. ARCHAMBEAU: Right, so you don't get P to SH wave conversion in that.

MR. CHERRY: That is right.

MR. ARCHAMBEAU: This is a way of converting SV to SH, however. In other words, you could change the polarization of the S wave.

MR. CHERRY: Correct. In fact I have taken an SH wave and converted it into a compressional wave. It seems kind of funny, but it indeed happens.

MR. ARCHAMBEAU: That is all right, but the inverse is not going to work.

MR. CHERRY: No, the inverse is not going to work; that is right. In order for this term to be operative, you simply need the curl of the displacement.

MR. ARCHAMBEAU: A rotation in a stressed medium.

MR. CHERRY: Yes, rigid-body rotation in a prestressed medium, and the only way I could see to get that was to first develop a shear source just to see what would happen to the ....

MR. TRULIO: Are you doing a problem where the elements actually do rotate a lot?

MR. CHERRY: No, not a lot.

MR. ARCHAMBEAU: This effect will always be of second order unless the gradient in the strain is large.

MR. CHERRY: Unless the stress gradient in the prestressed region is large.

MR. SMITH: Ted, where did you apply the boundary condition again? Was it at x = 0 ?

MR. CHERRY: It was at y = 0, all along the x axis.

MR. SMITH: I am a little confused as to whether you are modeling stress release in an earthquake or whether you are modeling the effects of an explosion in a prestressed medium, in a medium with gradients of prestress.

MR. CHERRY: The latter.

MR. FRASIER: But in a real problem, you generally have an existing fault or joint surface which may be thought of as being locked by static friction, so we can turn this problem around and talk about any of the waves, the P wave even, unloading the normal stress across the surface.

MR. CHERRY: That is another possible mechanism. I am really not happy with the stress distributions that I assumed. It was just the simplest thing to do. I don't like the body force. What it does is give you a force that is changing as the sine function.

MR. ALEXANDER: But that is what you expect to maintain equilibrium.

MR. CHERRY: That is what I have had to use.

MR. ARCHAMBEAU: Well, you don't necessarily have to use a body force. You can balance out the equation at equilibrium in other ways.

MR. CHERRY: My point is I just could not do it easily.

MR. ARCHAMBEAU: You get complicated stress systems.

MR. CHERRY: That is what I would like to look at next.

MR. ARCHAMBEAU: One thing you can do is the classical notch problem, wherein you specify that the stress be uniform at infinity but nonzero, so that it corresponds to the prestress. With an inclusion in the medium you get stress concentration around it.

MR. CHERRY: What it really would be neat to do is to actually open up a crack in a grid by pinning two points. You could apply tensile stresses to the grid. It is possible to actually open up this region and to see what sort of stress concentration you get at the ends, and now subject these ends to various kinds of impulsive stresses.

MR. ARCHAMBEAU: And then send a wave through it.

MR. SMITH: But you already know.

MR. FRASIER: That is the point of doing it. It is a classical elasticity problem for an elliptical crack.

MR. CHERRY: That is right. Then you could say, here is my source, and I have a point source of some kind--either compression or shear--run it into these regions, have an appropriate failure criterion, and see if it would not be possible to trigger the region.

MR. ARCHAMBEAU: You ought to put in some rheological descriptions.

MR. SMITH: Yes. I claim that is farther from reality than we need to get, though, because it is very seldom you ever get fracture of virgin material in the tectonic release problem.

MR. ARCHAMBEAU: No, he has a pre-existing crack.

MR. CHERRY: It has a pre-existing crack. I was going to open it up now by applying tensile forces to the end of the grid. So that this region now has a hole in it.

MR. ARCHAMBEAU: Now you run a wave through it.

MR. CHERRY: Yes.

MR. ARCHAMBEAU: You specify the rheology based on rock mechanics or whatever, and see what happens. It is a good problem for you to do.

REDUCED DISPLACEMENT POTENTIAL

Howard C. Rodean
Lawrence Radiation Laboratory

There have been a lot of comments made about the reduced displacement potential yesterday and today. I would like to talk about what it really is, what it can do, and what it really can't do.

Implicitly, a reduced displacement potential means that we have a spherically symmetric system. For our explosion problem it is a good first order approximation to the phenomenon, but it is probably more useful for some things that we are interested in than for others. So let us for the moment say that we have the spherical source in this half-space with a free surface. This is our elastic boundary or elastic radius, and let us say that it is big enough so that it meets your maximum strain criteria of 10^{-5} or whatever.

In calculating explosions and also accounting for observations, let us talk about either the head wave that gets refracted at the Moho, or the teleseismic wave which penetrates deeper into the earth. If we talk about the first arrivals, whether it be the head wave that Werth and Herbst calculated, or the teleseismic case that Kogeus from Sweden and Eric Carpenter of England calculated (all using the reduced displacement potential) the spherical explosion is probably a pretty good approximation. Based on our observations of drill-back and other exploration at the Nevada Test Site, it appears that the lower half of the region around an explosion is reasonably spherical, forgetting about layering effects. Let us just assume that this is in uniform rock material.

Okay, the reduced displacement potential is probably a pretty good thing to use for that. But in Werth and Herbst's papers I recall they also had to introduce a surface reflection to make their calculated seismograms look like the recorded head waves. Instead of using a reflection coefficient of about two per ideal elastic theory, they had to change the two to three, implying perhaps that there was some nonlinear process. This concerns the wave that goes up to the surface, then down, and arrives at the receiver shortly after the direct wave (Figure 65a).

Now, the head wave rays, I believe, go out more toward the horizontal than do the teleseismic ones. Perhaps a reflected path like this (Figure 65b) is what Werth and Herbst were trying to match.

Dai Davies mentioned at Woods Hole that when he looks at teleseismic data of what are presumed to be Russian shots in the Soviet Union, he sees very little evidence of surface reflections. He

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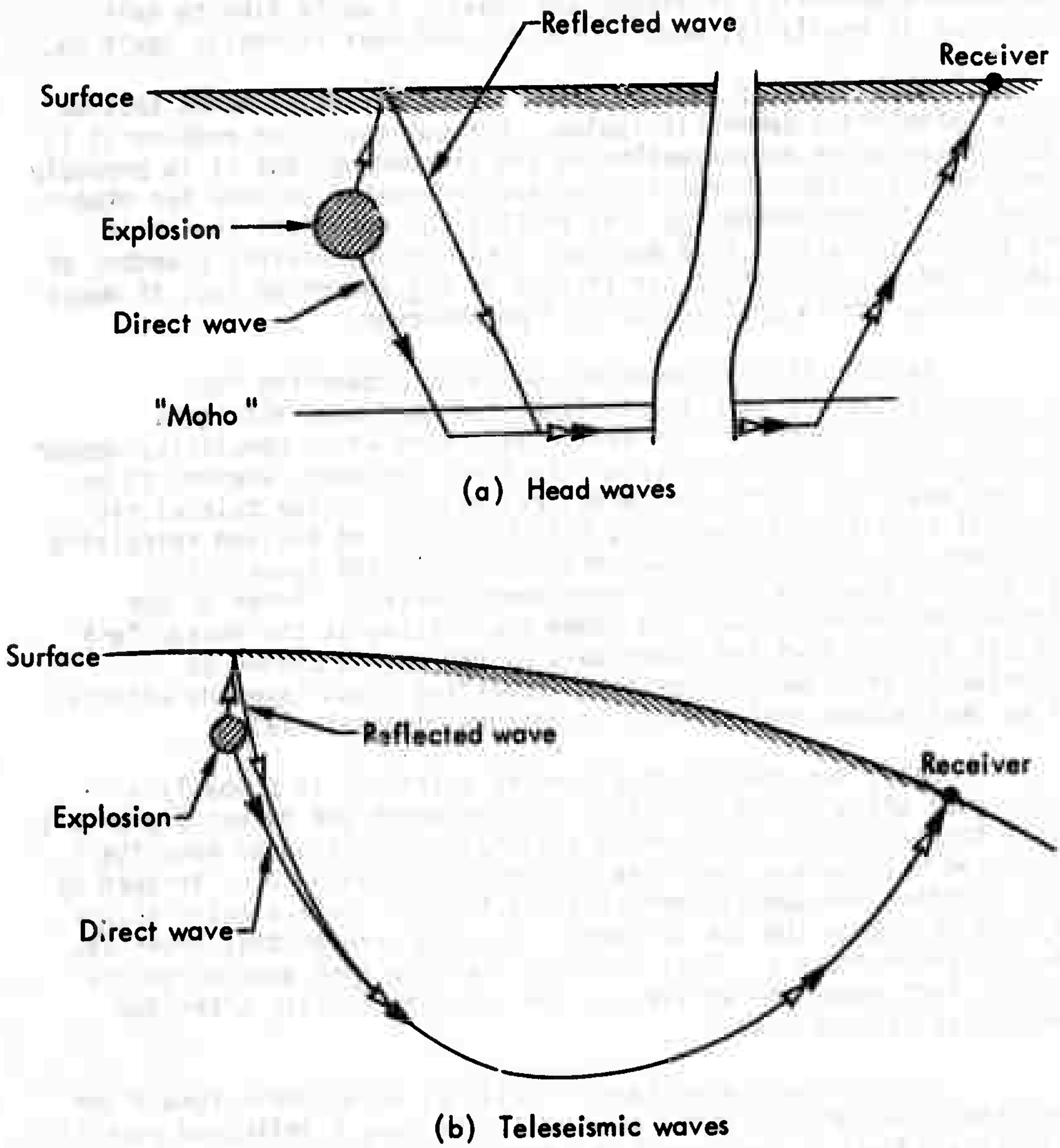


Figure 65. Direct and Reflected Head and Teleseismic Waves.

is wondering if the surface reflection is so attenuated by trying to get past the inelastically deformed zone that the surface reflection isn't very important in this particular case.

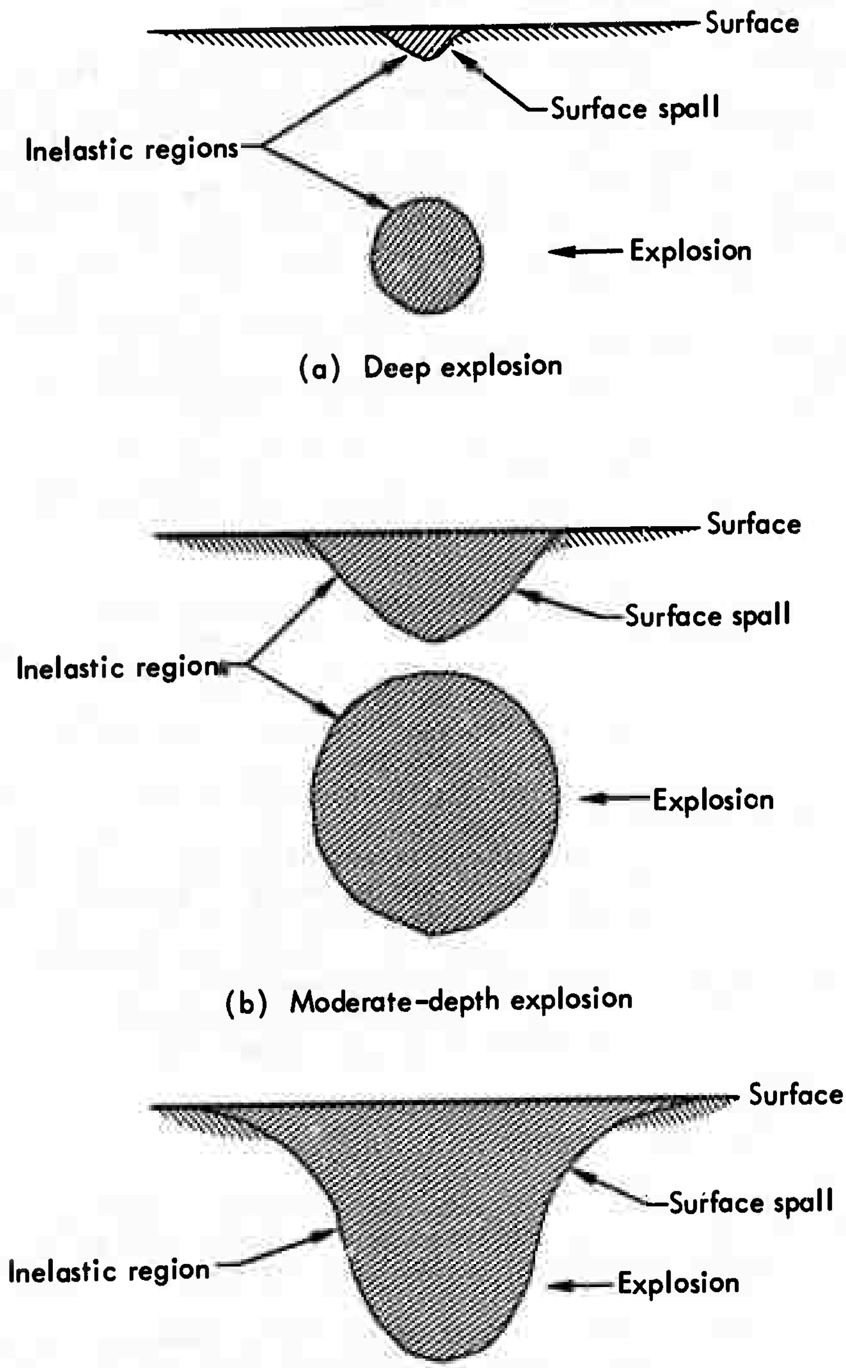
Obviously, a reduced displacement potential, by itself, will never give you Love waves. I also wonder if it will really give you the picture you need for Rayleigh waves for the following reasons. Again let us forget about the complicating effects of earth layering, and let us say we have a real deep explosion, such that we don't have any surface spall at all. I am not sure about Rulison, but I am quite sure that for every other explosion we have ever fired, including Salmon, there always has been some region of surface spall. So even in deeply buried shots, we have an inelastic region of some shape, say roughly spherical around the explosion, and we also have another inelastic region at and below the free surface (Figure 66a). We have evidence at the test site that sometimes there are distinct geological layers at which the spall occurs, so here is a layer of earth that gets thrown up and falls back down and then--wham! Bill Perret talked about some of the high g's recorded when the ground comes down and hits bottom.

As we go to shallower depths (again I am just drawing pictures because no one really knows what these regions look like), the surface spall region will grow (Figure 66b). Finally we have the limiting case of a cratering shot in which we have the initial inelastic deformation merge into the spall region, so we have millions of tons of rock which are thrown up into the air and then come down again. To take something that Ted Cherry said earlier this morning: when we talk about generating surface waves, we deal with the radiation in all directions, that is, with the interaction of these rays with the free surface and with layering, so on and so forth. Therefore, I am asking the question of the seismologists, don't you think it is about time for the code calculators to break out of their one-dimensional world with the reduced displacement potential and to run some problems in two dimensions with failure mechanisms to find out really what is the true shape of the elastic boundaries both around the explosion and in the spall regions?

MR. ALEXANDER: The answer is yes.

MR. SMITH: I am a little confused about this unloading near the surface. The high g's that you referred to, they were with reference to a cavity collapse I understood, rather than the spall?

MR. RODEAN: Yes, but I think the same thing would happen with respect to contained explosions--even though we would not form a crater or have cavity collapse. I am talking about the phenomena associated with the reflection of the outgoing explosion-produced shock waves from the free surface and the resultant spall. The spalled region will go upward in a ballistic trajectory and then fall down. When it hits bottom again, there will be a sharp ....



(a) Deep explosion

(b) Moderate-depth explosion

(c) Shallow cratering explosion (before venting)

Figure 66. Effect of Explosion Depth on Inelastic Regions.

MR. SMITH: This is the nonlinear magnification Carpenter was waving his arms about.

MR. RODEAN: Perhaps.

MR. SMITH: And you can do that, you think?

MR. RODEAN: I think with perhaps the right failure models, this could be done with two-dimensional codes.

MR. TRULIO: More difficult phenomena than that have already been included in cratering calculations.

MR. RODEAN: Really what I am saying is that instead of calculating craters, we should use the same two-dimensional codes and calculate some contained explosions to see what happens in the near field, including the surface.

MR. CHERRY: One thing it would be very easy to save in the codes is the curl of the displacement. I don't think anybody is. I tried it for a while and nobody seemed to be interested.

MR. RODEAN: So you could have CRT printout of the compressional wave going out, and then with the term that Ted talks about you could see where your shear waves are converted, and then you could also introduce problems where you would have different kinds of geological layering, too.

MR. PERRET: Howard, one thing in connection with these spalls is that a part of the energy is trapped in the spall, so that the reflected wave that is coming back down below the spall does not include all of the initial energy.

MR. RODEAN: Yes, there is some irreversible dissipation.

MR. PERRET: When it comes back down that is put back in the ground except for what you have lost.

MR. TRULIO: I guess I just don't understand why there are any limitations on the code at all with respect to strain.

MR. CHERRY: I did not say there were. I said we have been using the technique for 7 yr to rotate the stresses back to the coordinate system of the equation of motion.

MR. ARCHAMBEAU: But you have not been putting prestress in.

MR. CHERRY: No.

MR. ARCHAMBEAU: That is the only time that is important.

MR. CHERRY: It could be important to put the shear-wave reflection back into a region that was stressed by the compressional wave, or something. I don't know.

MR. ALEXANDER: There are in fact clearly observable SV waves, P to SV, associated with particularly larger explosions. You can see them at teleseismic distances, 10 to 20-sec SV waves from nuclear explosions, and they, too, are practically overlays of one another. They come in at the right time to have been generated by P to SV conversion at the free surface.

MR. CHERRY: Of course, but that is in there.

MR. ALEXANDER: Yes. In other words, I am saying that you do have that included , and the kind of interaction you are talking about might convert those.

MR. ARCHAMBEAU: You have P to SV anyway.

MR. TRULIO: Something we have not done is to prestress the medium in shear in a way that might correspond to the types of deformation you showed.

MR. ARCHAMBEAU: Sure, your codes normally do that.

MR. RODEAN: I think the question I posed we have answered.

MAJOR CIRCEO: On the deep shot, is the spall really that significant? I know that some of these surface accelerations are high in the velocities, but on Gasbuggy, say, was the spall really significant?

MR. RODEAN: I don't think as far as this is concerned--I am just making a judgment--when you go real deep like Gasbuggy or Rulison it is probably not too important any more, but for a lot of the shallower NTS events, it may well be important.

MR. PERRET: It was significant on Milrow. I don't think it was very significant on Gasbuggy. I don't really remember for sure, but it seems to me it was not.

MR. RODEAN: Those shots were at comparable depths, but had a factor of about 50 or so difference in yield.

MR. COOPER: There have been calculations made of the kind that you are mentioning here. I am not sure they really looked at spall. ATI recently completed a calculation on Discus Thrower, for example, and data were taken above the burst point. We are calculating Salmon, and we intend after having completed that one to move toward the surface to consider cratering. So there have been and are relevant calculations going on.

MR. PERRET: I would be interested to know how you match the Salmon surface records that had -2.5 g's.

MR. FRASIER: Keep one thing in mind. If you ever do get the exact reduced displacement potential, you will really put the seismologists on the spot, because we do not know the heterogeneities of the layering exactly enough to predict the fine detail on short-period data. If we know exactly what is coming out of some of these shots, this will give us a real tool for estimating these things, which we have not had before, simply because we don't know what the source is. All you have to do is look at short-period data, and you will convince yourself that you can never explain every wave because we don't know what the source is.

MR. COOPER: That was to be a part of the second part of my comment. I think we can do the calculation you want. What will you do with it once we give it to you?

MR. FRASIER: You will give us many, many more years of work. There are Carpenter pulses, there are all of these things you can approximate and say this is for a spherical cavity, but you just look at the number of wiggles on a seismic trace--like the stuff Shelton was showing just a little while ago--take a typical station and its typical short-period data and the way it wiggles, you can't possible explain those wiggles.

MR. RODEAN: A lot of these codes were originally designed, and their print-outs and the like, for completely different purposes than what you guys are interested in. Inherently, however, I think all of the stuff is there.

MR. FRASIER: One thing that can be done, if you can actually specify what is going on, we can make absolute measurements of, say, gross attenuations through the earth by just recording this thing some place else on wide-band instruments. We can't really do that now, I don't think.

MR. RODEAN: Of course, the one thing we can't do with two-dimensional codes is put in realistic strata that dip in one direction. Be content with two dimensions for a while.

MR. ROTENBERG: I just wonder whether codes are the efficient way of doing it, that is, whether one should do it digitally, or take some plaster of Paris, foam rubber, and one thing and another, and I think somebody could build an analog computer.

MR. RODEAN: I think that, too, but I am talking about getting out to the boundary of the inelastic region. Yes, you can do some things like that with plaster of Paris, but how well could you mock up tuff or granite, or what have you?

MR. ROTENBERG: I might ask the same question digitally.

MR. COOPER: You can't do the laboratory experiments alone, and you can't do calculations alone. You have to have experimental data. That keeps everybody honest.

MR. RODEAN: Now, for example, we can start out and check our codes against some existing experiments and analytic solutions, things that have been done in the laboratory. One example is in a planar system: some years ago Sherwood did both analytic calculations and experimental measurements showing the effects of a small charge on the surface of a plate. He solved for the outgoing P wave, the S wave, and the head wave in between, as well as the Rayleigh wave going on out along the surface. Then Tsai and Kolsky did some experiments at Brown University. They measured only the surface waves from an explosive charge on the surface, and compared them with theoretical results based on the Miller ....

MR. COOPER: Was this with glass?

MR. RODEAN: They did it first with glass and then with polyethylene, so that they had an "elastic" as well as a viscoelastic substance. Then they used the Miller and Pursey solutions to try to get an analytic solution. You can start out and check the codes against some of these other cases where we have solutions obtained by other means as well as experimental results. You match those so that you know that you have confidence that, yes, you are doing these things correctly. Then you go on putting in the best rock-failure models that we know of.

MR. RINEY: I wonder if there is not the possibility of this being Brock-Coffin factors. Two-D code calculations with the zoning required for the accuracy that we have been trying to do in one-D until the ILLIAC IV gets on the air are going to be an order of magnitude more expensive, and yet we have seen yesterday, I think, on the experiments and the one-D calculations close in that we could not actually calculate the RDP accurately without looking inside the cavity. I think that was the conclusion yesterday.

MR. RODEAN: Without looking inside of what?

MR. RINEY: Without looking inside the original numbers you did close in, the representation of the energy source, the finger was pointed at it as a possible source of ....

MR. RODEAN: Oh, you mean the peak in experiment versus calculation.

MR. RINEY: Yes. It seems to me that in one-D we should do a better job there before we go into the two-D. I think it might be worthwhile to really understand and be able to verify that first.

MR. RODEAN: Except let us put it in perspective. As far as VELA Uniform is concerned, ARPA wants many of these answers in a few years, and who knows how long it will take to work up this other thing? I think it is legitimate to use available tools to do a few representative problems now because at present the pictures I drew on the board are just hand waving.

MR. COOPER: It is not as if nothing was being done or has been done in this area.

MR. RODEAN: Let us recognize that what you say is true. Let us also realize that we have been concentrating on a seismic source based upon an assumed spherical explosion for 6 or 7 yr. Really, the two-dimensional seismic source, which is much closer to reality, has never been addressed from the standpoint of our explosion-calculation point of view, so let us at least make a start. That is what I am saying. I am not proposing a big parametric study or anything like that. These problems should be, at least in the beginning, few in number but well thought out.

MR. COOPER: Well, I would vote for parametric studies because I wonder if you know the details of the things that really count to define just a few problems. I think this may be the thing that Dave is worried about. Once you say you are going to start into parametric studies, the alternatives, or parameters that may be varied are so numerous that you really have to do a lot of calculating.

MR. TRULIO: And if you do it in one dimension, it is hard to see why you won't have to in two.

MR. HARKRIDER: I have one question. When you do the two dimensional, do you save the free-surface displacements if you do a free-surface problem?

MR. TRULIO: Yes.

MR. HARKRIDER: Because speaking not only for seismologists, some of us are interested in the generation of acoustic gravity waves in the atmosphere due to, say, the surface displacement, and use that as our driving force. We would like to have those if you have any.

MR. TRULIO: We save the tapes for a certain length of time, like a year.

MR. SMITH: It seems like the discussion is incomplete unless we also consider the fracturing in the prestress medium as well as that. I wonder is that totally out of the question at the moment?

MR. RINEY: It seems to me that you could actually solve this, or do a calculation--I don't know whether "solve" is the correct word--you could do a calculation neglecting the fault being there, and then consider a plane in which you could actually do a very cheap parameter study if you wish to, and orient it in various ways, and see what this would do on defining the pressure across that. Then apply, using the calculation as your criterion for initiating such a failure, a slip-stick process, and evaluate it relative to the direction that it might be, or the prestressing.

MR. SMITH: In that case you would then be ignoring the effect that movement had on the radiated field.

MR. RINEY: That is true. That would be the initiation of it. It might be in a combination where you would do one type of problem and then do another, but I think you could learn a lot as far as initiation of such a failure.

MR. TRULIO: I don't think at this point that, even in a spherical approximation, we can give you a source for an explosion in a crystalline rock that makes any sense, or that we believe. There is a certain amount of experience with that kind of cracked medium, in which theoretical calculations were compared with experiments, and even experiments with one another, that says we don't know how to model a hard cracked rock.

MR. COOPER: It is a thing of degree, isn't it?

MR. TRULIO: I'm thinking of NTS granite again, and the fact that Hardhat and Piledriver are not the same event scaled.

MR. COOPER: When you say that, you are talking about two data points in Piledriver. Bill is better qualified to comment on scatter in the data along different directions, etc.

MR. PERRET: This was probably far enough below the tunnel so that the tunnel produced only a mild perturbation. The tunnel diameter was small compared to the wavelengths, and only about one-fifth the separation of instrument stations from the tunnel floor. So the perturbations were small although probably present.

MR. FRASIER: I saw some accelerator records from shots--Shelton will probably remember--a couple of shots at exactly the same point produced identical records, wiggle for wiggle, all the way down here. Have you noticed this correspondence?

MR. COOPER: Are you talking about very close in?

MR. FRASIER: Very close-in stuff, yes, including initial wave, and all of the reverberations, all of the details afterwards for a long time. I can't remember what events they were.

MR. PERRET: It sounds to me like the Sterling records versus Miracle Play data. Those were the records that included compression wave and shear waves. They did fit together very nicely but were from quite different kinds of shots. One was a nuclear shot in a cavity and the other was a gas mixture in the same cavity.

MR. FRASIER: But that should be telling us something about the similarity of shot environment from shot to shot. I wonder if the two codes for these two different type events would converge to give you similar records?

MR. PERRET: They should theoretically, anyhow. If those are the ones we are talking about.

MR. TRULIO: I don't think there are good accounts of Salmon yet.

MR. COOPER: The data?

MR. TRULIO: The theoretical calculations.

MR. RINEY: There were some that were done.

MR. TRULIO: Yes, the way in which agreement was obtained with experiment was to ignore the lab data on strength, and again repeat the game that was played with Piledriver. It is sort of a meaningless and circular procedure if you want to develop a prediction capability.

MR. COOPER: But it is not entirely meaningless, I don't think, either. Again I refer to the experiments that we were involved in at Cedar City, Utah. All of the experiments were very near the surface. One thing that came through very clearly in terms of close-in phenomena near the surface was that the joints and the fractures in the rock controlled the late-time phenomena. I think that this late-time phenomena is what you are really interested in, because that is where you get your low frequency input.

MR. TRULIO: What I am saying is that we should stay away from jointed rocks right now.

MR. COOPER: That is a great idea, but is it realistic?

MR. TRULIO: Alluvium and tuff and salt are the kinds of media in which we ought to try to understand signal propagation before we try to understand them in rocks that are cracked and faulted.

MR. COOPER: But I did not finish my point. What we found was a lot of data scatter. It may be that the best you can ever hope to do with the calculations is to fit the norm of the data scatter. The experiments were conducted with high explosives in planar arrays that presumably generated a plane wave over some region. However, we found that the motions were not planar. This is evident not only in the transient records, but also from pre- and post-test surveys that clearly show that displacements took place along paths of least resistance down the jointing planes.

MR. CHERRY: That may be true, but as far as Hardhat is concerned, I think we had three measurements there, two stress-history measurements, and then some displacement records.

MR. PERRET: You had five or six stations.

MR. CHERRY: I don't know whether you can see this or not. (Showing Figures 28-30 from Cherry, J. T., and F. L. Petersen, Numerical simulation of stress wave propagation from underground nuclear explosions, *Proceedings of the Symposium on Engineering with Nuclear Explosives*, 1970, Las Vegas, American Nuclear Society) The puzzle on Hardhat, 5 kt in granite, was that here the dotted line is the measured radial stress versus time, and the solid line is the calculation at 62 m, 4 kb. At 120 m the dotted line is the measured value; the solid line is the calculated. It is not bad agreement. To go to the reduced displacement potential you find that the initial peak is missing, and I don't know what happened to it. It is going to be very difficult, I think, to match the stress-history measurements at 62 and 120 m on Hardhat, and then still reproduce that peak on the reduced displacement potential. I just don't see how to do it. You are going to have to throw something out.

MR. PERRET: Are those the stress-gage measurements or are those the stresses calculated from the velocity?

MR. CHERRY: No, these are stress-gage measurements.

MR. HARKRIDER: By the way, Hardhat was one of the most anomalous explosions we have ever observed in the sense that the Love waves were enormous. We apparently had a great deal of tectonic release.

MR. COOPER: Let me finish. I don't entirely disagree with Jack. I think the problem of determining rock properties for use in code calculations is a problem of translating what you can measure in the laboratory with respect to material properties to a condition that relates to the in situ state of the rock. In situ rock property testing is expensive, if the attempt is made to determine the general response of rock under all sorts of stress and strain.

MR. PERRET: I think one thing you have to remember about laboratory tests on cores is that they give upper bounds for the strength of materials, because if you can get a core out of a rock, you have some of the better rock from that formation. If there are any close-joint systems or weak bedding planes you don't get much core.

MR. TRULIO: What they do in the lab to get around that problem, and nobody knows whether they succeed or not, is to crack the sample.

MR. COOPER: I agree that is the right direction.

MR. ARCHAMBEAU: But what if the medium you are dealing with is stressed to a fairly high level? When you do the test in the lab, you are not stressing it at that level, and you are then measuring failure properties without prestress are you not?

MR. PERRET: When you take the core out, you unload it right away.

MR. ARCHAMBEAU: Yes, I know; so it may not be very relevant to the site.

MR. TRULIO: In much of the work that has been done, the experimenters have attempted to get around that problem by prestressing their lab samples.

MR. ARCHAMBEAU: Yes, if you know what the prestress in the earth is and if you deal with fractured rock, that's fine.

MR. COOPER: Are you familiar with the work that Wayne Brown has been doing?

MR. BROWN: We have done about every kind of prestressing you can do in a triaxial-test apparatus, which means σ_2 is equal to σ_3. We have done this with various stress ratios and with constant confining pressure. We have also performed such tests in uniaxial strain and hydrostatically up at stress levels on the order of 10 kb and with a variety of stress and strain conditions. The crack conditions we have been doing to date have been in specimens where we precracked the rock. We put a soft jacket on the specimen and end load it until it fails. Then we carefully place this in a pressure vessel and apply triaxial loads. The initial crack pattern is random, and varies from specimen to specimen, but the triaxial results are reasonably consistent from specimen to specimen.

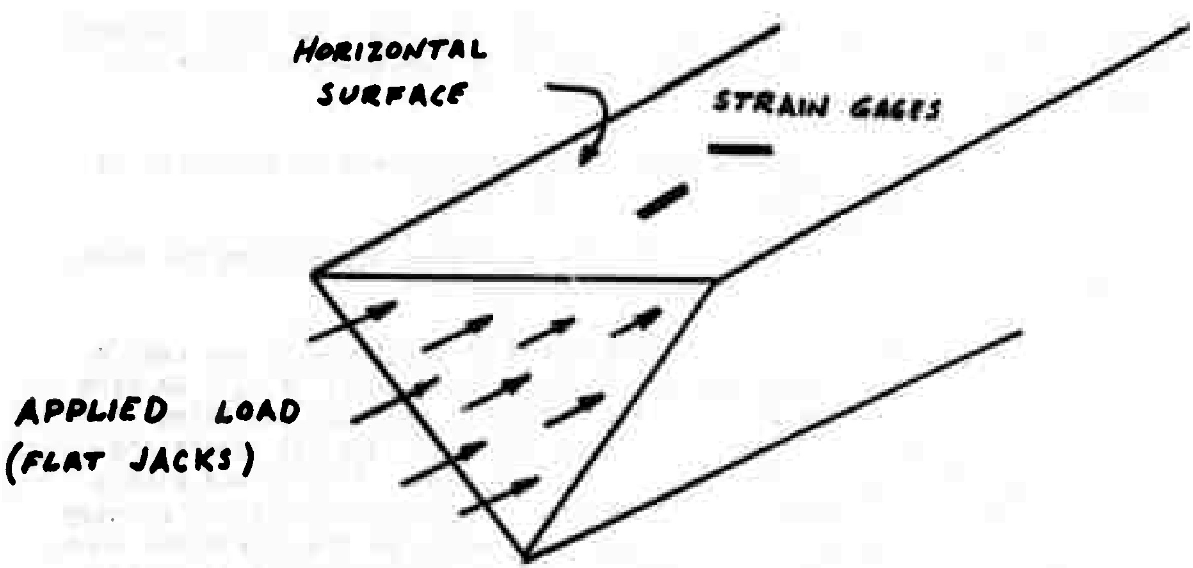
MR. ARCHAMBEAU: The joint systems maybe have a great deal of regularity to them.

MR. BROWN: Right. One of the things we are hoping to do next is to look at specimens where we have well known joint systems.

MR. ARCHAMBEAU: What about edge conditions?

MR. BROWN: Edge conditions are something we can control in a triaxial test vessel to the extent of keeping σ_2 equal to σ_3, or in a cube-type test, where all three principal stresses may differ. However, the present method of performing the cube test is not very satisfactory for precracked specimens.

The other problem that Hank is referring to, though, is the gross problem of the in situ rock. We have done some work on the Cedar City granite in situ. Let me sketch the configuration of our in situ specimen.



This work involves a specimen which is an end-loaded cantilever prism. The specimen is formed by cutting two parallel slots inclined at 60 deg to the horizontal surface of the rock such that they intersect. A third, vertical, slot is then cut normal to the other slots, forming the end of the specimen. In an end view the specimen is an equilateral triangle. The load is applied at the free end with flat jacks. Strain gages and displacement transducers attached to the surface record the rock deformation.

This test configuration has the advantage of being well defined in cross section. We apply a uniform stress to the end so the stress field in the specimen is well defined. In the tests conducted to date we have found that breakage occurs in the rock at stresses on the order of 1500 psi. This is from two tests, one with a specimen dimension 2-ft across and another with a width of 4 ft. A series of tests is being performed and other specimen sizes will be tested.

Now, if we take samples of this rock into the laboratory and run unconfined compression stresses, we find compression-failure stresses of the order of 9,000 to 10,000 psi, considerably higher than the strength observed in field tests. Another interesting thing we find is a size effect on the modulus. We don't have the data fully reduced, but it appears that we are getting a size effect as we plot modulus versus specimen width, with the modulus decreasing with size. It is possible that the modulus is dependent on flaws. As the specimen becomes larger we take in a greater number of flaws and eventually the effect ought to level off as the size increases.

MR. SMITH: At the small specimen-size end of the curve, do you go anywhere near the single-crystal measurements?

MR. BROWN: The laboratory samples are typically 1 in. in diameter and 2-in. long, and the larger grain dimensions are of the order of 1/8 in.

MR. SMITH: Does it look like it is heading up to what one expects?

MR. BROWN: As far as the specimen dimensions, they are still on the low side. The modulus for lab tests will run typically anywhere from 900,000 psi up to 3 million psi.

MR. HANDIN: This rock has a porosity of about 5 percent.

MR. BROWN: If you look at the data we have so far, it indicates that modulus is decreasing with size. For the smaller specimen it is on the order of 450,000 psi and for the larger one on the order of 700,000 psi. We seem to be getting a size effect, and when this phase of the experimental program is complete, we should have a good idea of the size effects for Cedar City granite. Two things are apparent at this point. First of all, the strength is drastically lowered from nine or ten thousand to 1500 psi, and secondly, we find that the modulus is decreasing by a factor of two. The in situ measurements are showing a significant decrease with size in both strength and in modulus.

Whether or not this happens in a dynamic test we don't know. All of the tests I have described were quasi-static, so no inertial effects show up. I presume these will be apparent in the code calculations.

MR. TRULIO: Yes, and some others that we don't model, such as dispersive effects. So it seems to me if now or in the near future, anyway, output from code calculations is to serve as a realistic source for seismic-wave propagation, it is going to have to come from calculations in soft rock or soil.

MR. ARCHAMBEAU: For now, yes.

MR. SMITH: I am really puzzled by your comments about the modulus there, because you don't see that size effect when you infer the modulus from acoustic velocities. You get fairly good correspondence from a small sample to the in situ measurements.

MR. BROWN: Yes, but I think that is quite a different situation. Here you have stresses high enough to start closing cracks. The low-stress levels with acoustic signals don't do anything to the rock structure.

MR. COOPER: Maybe I am not understanding you. If you do laboratory sonic tests, and you compare the compressional-wave speeds that you get there with what you would get from seismic measurements in the field, the seismic measurements are lower.

MR. SMITH: No, I think in the laboratory if you reproduce the pressures you are looking for you get quite good correspondence.

MR. COOPER: No, they are lower. The field-value seismic values are lower than that.

MR. HANDIN: You guys are talking on the basis of different experience. He is talking as a seismologist and you are talking about using seismic velocity for engineering purposes. Very shallow conditions are what you are talking about. He says if you put the sample under a kilobar and change the velocity, it is the same as you measure in nature, and that is not true.

MR. SMITH: I guess the inference from that is the reason we don't see the size effect there is that all of these cracks are all closed up.

MR. BROWN: Yes, in our test there is no confining pressure. This is a surface-type test.

MR. CHERRY: How important is the viscosity of the rock in affecting both the attenuation and the transmission?

MR. ARCHAMBEAU: It has a very strong effect on the high-frequency waves. The effective Q for transmission of body waves is like 1000 in the crust and decreases to something of the order of 100 in the upper mantle.

MR. CHERRY: How would that Q go into a stress-strain rate formulation?

MR. ARCHAMBEAU: I was referring to a measured or an observed Q. You can interpret the observed Q in several ways. One of the ways we treat it analytically is to specify an operator equation relating stress and strain such that an operator P acting on the stress is equal to some operator Q operating on a strain, and that both operators are linear combinations of differential and integral operators in time. So if you use that in the equations of motion, what comes out are complex moduli, which are in general functions of frequency.

MR. CHERRY: Is it the Voigt solid?

MR. RODEAN: You are talking about things like the Futterman theory of attenuation, essentially.

MR. ARCHAMBEAU: Yes, basically the same sort of thing. You get a generalized solid which has elastic moduli that are frequency dependent in a particular functional way, since you assume linear operators which are either of convolution-type or time-derivative operators, and if you take the Fourier transform of the stress-strain relationship, then all you get out of that are complex moduli, and they are frequency dependent in some complicated way.

MR. CHERRY: Maybe we could make it simpler.

MR. ARCHAMBEAU: It is a generalized viscoelastic medium.

MR. CHERRY: If you stay in the time domain, it is just like aerodynamics. A strain rate appears in the stress in the same way as in aerodynamics.

MR. ARCHAMBEAU: You mean the linear viscosity?

MR. CHERRY: Yes.

MR. ARCHAMBEAU: No.

MR. CHERRY: Could you write a formulation down?

MR. ARCHAMBEAU: What you want to construct is a formulation which is a generalization of Hooke's law, so you define some operator P, where P is an integral-differential operator with the general form:

$$P\{f\} = \sum_{n=o}^{N} a_n \frac{\partial^n}{\partial t^n} f(t) + \sum_{m=o}^{M} \int_o^t b_m(t - \tau)\ f(\tau)\ d\tau$$

where f is any function of time and the spatial coordinates. Now this is the "stress-operator" form, and Q, the "strain operator", has this form as well with different coefficients, so we have

$$Q\{g\} = \sum_{\ell=o}^{L} c_\ell \frac{\partial^\ell}{\partial t^\ell} g(t) + \sum_{k=o}^{K} \int_o^t d_k(t - \tau)\ g(\tau)\ d\tau$$

MR. CHERRY: Does the convolution give you a solid with memory?

MR. ARCHAMBEAU: Yes.

MR. HARKRIDER: It is rate and memory.

MR. ARCHAMBEAU: Now the stress-strain relation is:

$$P\{\sigma_{ij}\} = Q\left\langle c_{ij}^{k\ell}\ e_{k\ell}\right\rangle$$

where σ_{ij} is the stress, $e_{k\ell}$ is the strain, and $c_{ij}^{k\ell}$ is the normal matrix of elastic moduli. I've employed the summation convention in writing this, so there is an implied summation over both k and ℓ. The coefficients in the operator Q, such as the ce, would normally depend on the indices k and ℓ, but I'll suppress that here for brevity and clarity.

Now, you can take a Fourier transform of this. Then what you get if you operate on this with the Fourier-transform operator on both sides, since P and Q have the properties of being differential- and convolution-type operators, is:

$$P(\omega)\ \sigma_{ij}(\omega) = Q(\omega)\ \left[c_{ij}^{k\ell}\ e_{k\ell}\right]$$

These are the simple algebraic products, and the P's and Q's are:

$$P(\omega) \doteq \sum_{n=o}^{N} a_n \omega^n + \sum_{m=o}^{M} b_m(\omega)$$

with a similar form for $Q(\omega)$.

If you want to write down the effective elastic constants, then you just get:

$$\sigma_{ij} = \left[c_{ij}^{k\ell} \left(\frac{Q(\omega)}{P(\omega)} \right) \right] e_{k\ell}$$

The constants $[Q(\omega)/P(\omega)]\ c_{ij}^{k\ell}$ are your effective elastic constants. You just insert this constitutive relation into the equations of motion and what you arrive at is the ordinary equations of motion in the frequency domain wherein the "elastic" constants that appear in the equations are:

$$\left[\frac{Q(\omega)}{P(\omega)}\ c_{ij}^{k\ell} \right]$$

and are frequency dependent.

It is unfortunate I used Q as the notation for the operator in these equations, but we observe that the anelastic property or Q for the earth is a very slowly varying function of frequency over quite a large bandwidth, at least as we can so far determine. So whatever the ratio Q/P is, it is very slowly varying as a function of frequency.

In any case, from this rough formal specification, you can calculate what attenuation and phase shift can be expected.

MR. CHERRY: When you people talk about the velocity at the core-mantle interface, how much of this velocity is influenced by the viscosity?

MR. ARCHAMBEAU: Very little. It is not influenced particularly by the viscous properties.

MR. RINEY: Do you believe the possible frequency dependency of the earth properties is due to viscosity or is it some other mechanism?

MR. ARCHAMBEAU: No, at least not in the usual sense. The construction I just went through is purely formal. What you want to consider are the solid-state mechanisms that are appropriate. They are predominantly relaxation mechanisms associated with dislocations and defects in crystalline structure. Seismic waves have associated low stresses, so what you want to look at as candidates for absorption in the earth are effects activated at very low stress levels. There are a large number of such possibilities, including movement of interstitial atoms in the lattice and the diffusion of dislocations.

MR. RINEY: How about water in the pores?

MR. ARCHAMBEAU: Yes, partial melt, in the upper mantle for example, would be a possibility, along with pore water in the crust. All of the ones I previously mentioned can be described, if you like solid-state terminology, in terms of defect structure and dislocation phenomena.

MR. FRASIER: One thing you see is you see waves going all the way through the earth, through the core to the other side, that are not dispersed at all, body waves of one cycle, say. They are very, very sharp wave forms, so they can't have been dispersed.

MR. ARCHAMBEAU: The dispersive effect of this attenuation is very, very small.

MR. RINEY: On this wavelength.

MR. ARCHAMBEAU: In the seismic bandwidth. At very high frequencies, of course, dispersion is going to be important, but we don't work in that high-frequency range, simply because the attenuation associated with these mechanisms is very strong for high frequencies and we just don't see them of course.

MR. RINEY: Could that make some difference in our calculated reduced displacement potentials?

MR. ARCHAMBEAU: In the near-source zone? No, you are talking about effects that are much stronger than what we are talking about here and involve different physical mechanisms.

MR. COOPER: If you look at the data from the underground tests and compare the propagation velocity of the peak stress or the peak particle velocity with seismic velocity, you will find that the wave spreads as it propagates. Most of us do code calculations that involve stress-strain curves that are concave upward. This would lead to a shocking of the wave front as opposed to the observed spreading. Thus, we are missing something that the effect under discussion would provide.

MR. ARCHAMBEAU: What do you do about viscosity? You are putting artificial viscosities into your program, but there are real physical processes which would give an effective viscosity.

MR. COOPER: It is generally smaller than the Q that is used.

MR. ARCHAMBEAU: Smaller than the Q magnitudes that I am talking about here?

MR. COOPER: Than the Q that is used for the artificial viscosity.

MR. ARCHAMBEAU: Yes, but that is an artificial viscosity you are putting in just to keep your program from having a fit.

MR. RINEY: I think you are agreeing.

MR. ARCHAMBEAU: Yes, but there are real, nonlinear dissipative effects that you have to put in also. They are different from those I mentioned and probably can't even be reasonably approximated as a viscous effect. The mechanisms are different, because you are dealing with a high-stress mechanism, if you like, dislocation climb, grain rotation, and so on.

MR. COOPER: I don't know what the reasons are. The dispersing thing that Jack was discussing would give you pulse gradients.

MR. TRULIO: Yes, and that does not have anything to do with dissipation at all. All of the energy is either kinetic or potential.

MR. COOPER: The in situ stress-strain curves could be concave downward rather than upward. If that were so, it would cause the pulse to spread. We are just looking for reasons for it to happen. All I am saying is the observation is that the wave spreads. Don't you have some fix on that by the energy that you calculate?

MR. ALEXANDER: It is the energy that is propagated away and accounted for by theory. It was on the order of 2 percent at one range in some cases and 0.5 percent in others.

COL. RUSSELL: Why don't we break for lunch, gentlemen, and then after lunch I hope in about an hour or hour and a half we can attempt to summarize the code-calculational side of the house and the seismology side of the house, and hopefully we can have some direction to march to in the months and dollars and years ahead.

CODE CALCULATIONS: REVIEW OF CURRENT OUTPUT CAPABILITY

John G. Trulio
Applied Theory, Inc.

We can now solve routinely problems of one- and two-dimensional ground motion. For the purposes of this meeting, the one-dimensional problems of interest are spherically symmetric and determine the field of motion on regions whose breadth is very small on the teleseismic scale of distance. Perhaps an hour of UNIVAC 1108 time is required to calculate spherically symmetric ground motion from an explosive source to a range somewhat greater than the largest range at which shear failure occurs, if the calculation is carried to the point where all material within that range has come to rest.

The output from such a calculation can consist of just about anything you want to see. If you want displacement as a function of time at a point in the field just outside the ultimate range of shear failure, that can be obtained; so can the reduced displacement potential. In fact, those quantities are presently part of our standard code output. Radial stress, or almost any other continuum-motion variable can be exhibited in a table or graph as a function of time.

For two-dimensional continuum-motion fields I would say that present capabilities are about like this: plane-symmetric fields (which are probably not of much interest here) and axi-symmetric fields can be calculated by expending about 10 or 20 hr of UNIVAC 1108 time. In such a calculation, the number of zones might be 20,000, that is, an array of 400 x 50 mesh points might be employed. You can see that even for 10 or 20 hr of 1108 time the fineness of definition of the field of motion is not as great as that obtained in the spherical case at a cost of 1 hr. On the positive side, the amount of information generated for the field calculated is simply enormous. The output again can consist of almost anything, including, if you want, the scalar and vector displacement potentials all over the region of linear motion. At the moment our codes do not contain routines for generating those particular quantities, and I don't know whether other people compute them; what is put out now are the standard variables of stress, strain, and velocity.

In both the one- and two-dimensional cases, we should be helped a great deal by what I have heard here. Your interest as the motion progresses, centers on longer and longer wavelengths, and if we put in the right kind of dispersive and dissipative mechanisms, we are helped by that fact. We start with fine definition of the spatial region of disturbed material. Since our continuum equations and their discrete analogs are written to accommodate quite general mesh-point motion, the points of a mesh can be made to spread in such a way as always to cover the field of motion even though the number of points remains constant. Then, although the points move farther apart, accuracy is retained for your purposes because as time goes on and frequency

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conversion diminishes in importance, the wavelengths of disturbances that you are interested in get longer and longer. Increasing the zone size may not really place much of a restriction on providing the kind of input you need for what we would call far-field calculations.

In the two-dimensional case we have also carried calculations to the point where the curve bounding the spatial region of shear failure has stopped growing, and the particles within that contour are stationary as well (apart from numerical noise that one can identify readily in any of these calculations).

With respect to the accuracy of numerical solutions to the mathematical problems posed (not necessarily the real physical problems) I think that any major parameter of motion can be computed to within a few percent in the one-dimensional case. The discretization error does not vary much with strain amplitude, for example, or, as a field of motion expands, with particle speed. In two dimensions, I don't think I would want to claim numerical solution errors less than 20 percent; even in a very finely zoned problem, errors less than 10 percent would be uncommon.

All of the codes are probably now set up so that round-off error has no significant effect on the numerical solutions; wherever necessary, the codes work with quantities that are incremented timestep by timestep. For example, in computing strain the displacement field is updated, and not just the particle position field.

I think that for us and for you the main problem really arises in defining the medium in which propagation takes place. It does not matter to the codes whether the medium is prestressed or not. It is literally just about as easy to solve a problem in which an initial tectonic stress distribution is prescribed as it is to solve the same problem of motion in a homogeneous medium--but you have to know those stresses, and go to the trouble of putting the detail of their distribution into the initial conditions of the problem. You therefore want to have some assurance that you are representing some material or some site realistically. Otherwise, all we normally put into these calculations is an overburden stress, making sure that initially the whole earth is in a geostatic equilibrium state. Up to now we have generally put in overburden stresses as uniform hydrostatic pressures at any depth. The point is that present practice does not reflect any basic limitation on the codes or the methods that they implement; more elaborate initial stress distributions could be assumed, and the fields of motion calculated to the stage where maximum strain amplitudes of about 10^{-5} are found, and the maximum stresses are not much more than 10 or 20 percent of the overburden.

The problem then remains of representing the mechanical properties of the various media whose motion we try to compute. I would say that to date we have been most successful in the case of a recent shot in tuff. In our calculations and in others as well, the objective has been to reproduce the whole pulse at a given range--not just curves of peak velocity or displacement versus range, but the whole time history of the motion measured at various points with accelerometers or velocity gages. For the recent NTS shot I mentioned (a small-yield shot), we made two prediction calculations which can probably be considered representative of the state of the art for this kind of medium. The calculations employed descriptions of the medium that we thought (based mainly on laboratory data) might express extremes of its mechanical behavior; otherwise the calculation was the same in each case. The predicted velocity-time pulses were roughly a factor of two apart over most of the interesting range of calculation, and the pulses calculated at a given range exhibited a positive phase and a negative phase followed by some minor wiggles. As it turned out, the pulses that were measured fell between the two calculated extremes at all the ranges we looked at. We were also fortunate in that the measured pulses too were consistent with one another; that is not always the case. I think, in this kind of medium and for a small yield, there is some reason to hope for local homogeneity over a big enough distance so that no serious asymmetries are observed in the measured motion. It is also worth noting that the medium is not thoroughly cracked, and is therefore nearly free of the built-in directional biases introduced by joint systems.

As I remarked earlier today, I think the inclusion in the material models of the effects of cracks and joints--some healed, some partially healed, some open--is going to limit the accuracy of the representations of the medium in ground motion calculations for quite a while. I don't see any easy solution to that problem. What makes the problem of modelling cracked hard rocks especially forbidding is that field measurements of quantities like particle velocity at a given range, and sometimes at almost the same azimuth, often disagree among themselves; evidence of inconsistent measurements of ground motion, as well as of strong anisotropy, is common in plots of measured peak velocity vs range, while individual pulses that one would expect to be nearly identical show large differences in shape as well as amplitude. But for the softer geologic materials, and for small yields, I think it is possible to predict the major parameters of explosively induced ground motion to within 50 percent. For example, in a reasonably homogeneous soft-rock medium, I believe that the entire curve of peak particle velocity versus range down to seismic particle velocities can be predicted with that accuracy--and I would define a medium as "reasonably homogeneous" if its most important properties (compactibility, strength, elastic moduli) varied less than a factor of two from sample to sample. Actually, one might have trouble determining a predicted ground motion field accurately because of the great scatter in our gage records. Similar accuracy is also possible for the amplitude of the negative phase usually found in the pulses of interest here, and for the durations of both phases. For crystalline rocks good accuracies are seldom obtained; salt may be an exception, but so far early motion in the Salmon event has not had any convincing theoretical explanation. The accuracies quoted are based mainly on experience in performing calculations of spherically symmetric motion, but it seems to me that comparable accuracy is possible even for layered media provided that the individual layers are reasonably homogeneous and at worst orthotropic.

I think that is where the state of the art stands--although this has been the briefest of summaries, and there are probably important aspects that I have not touched. I think the best way to get at them is just to answer questions.

MR. BLACK: Let me ask you a question, Jack. The event that you are referring to, the two curves or the two sets of predictions that you made for that event were based on a very, very extensive set of experimental measurements of the physical properties, Hugoniots, isothermal compression, the whole works?

MR. TRULIO: Yes.

MR. BLACK: What would you say you would get, or how much difference would it have made if you had not had that vast amount of experimental data, and suppose you had to make a guess as to what the kind of tuff would have been?

MR. TRULIO: I think the best way to answer is to say that some of that data is just essential. Without it, you don't make a calculation that is worth doing.

MR. CHERRY: I agree. As far as I am concerned, without some of it, and the "some" I include is the logging data, the compressional velocity, and the in situ density, plus the isothermal compressibility. I feel these are absolutely essential.

MR. TRULIO: You must know the water content, and the compactibility of the material, and it is hardly possible to proceed without loading and unloading hydrostatic data of the kind Ted just mentioned.

MR. CHERRY: Yes. Once you have compressed the rock in the laboratory test, you can just release it. That is no problem.

MR. TRULIO: I would say you don't need much data at mean stress levels as great as 40 kbar, but you certainly need all the data you can get up to a kilobar or two.

MR. BLACK: I guess I am asking the people who have worked with this, certainly LRL, and you, Jack (for tuffs that you are all familiar with) could you hazard a guess as to what kind of range in factor you might expect to get if you just use average numbers out of the tables? Is it very large?

MR. CHERRY: I ran into a tuff at the test site on Schooner that was the hardest rock I have ever encountered. One hundred fifty ft below it was cotton candy, and that was tuff also. It was just like a layer of steel over a layer of cotton candy, and it was all tuff. One was a welded tuff and the other was an ash flow tuff.

MR. ROTENBERG: Which one did you use in your code?

MR. CHERRY: Both of them.

MR. BLACK: How much difference did it make?

MR. CHERRY: It made a large difference.

MR. BLACK: For predictive purposes where one is interested in being able to predict the effects of an explosion in a given geologic material assuming the source to be in some foreign country where we don't have data on equation of state, Hugoniot, etc) how well are we going to do?

MR. TRULIO: I am saying that that is what has been done. I think it can be improved further for soft geologic media. I think we can get our predictions accurate to within the error of field measurement.

There are two things missing from the present model, but you're concerned with what is possible right this minute; in fact, your specific question presupposes that all you have is a geological description of a material like tuff.

MR. BLACK: No, a little more: the density of the material and the seismic wave speeds.

MR. TRULIO: Although you don't know "the wave speed"; you don't have curves of stress and strain for arbitrary deformations, or even uniaxial or hydrostatic data.

MR. BLACK: I think it is very unlikely.

MR. TRULIO: And you don't know how much porosity the material actually has.

MR. CHERRY: That is possible.

MR. BLACK: You may know the porosity.

MR. TRULIO: That is absolutely necessary.

MR. BLACK: These are numbers that are in the common geologic literature for description of geologic materials worldwide. You do have this kind of data. It is not complete, but there is something to guide you.

MR. TRULIO: If you give the porosity alone, and a general descriptive category like "tuff", then you won't know the water content, so ....

MR. BLACK: Maybe you can't answer the question. I am just asking could you hazard a guess as to what kind of range you might expect?

MR. TRULIO: I am talking about reproducing the velocity-time pulse at an interesting distance, for instance where the peak stress is half a kilobar.

MR. CHERRY: We tried to model the French data with just the name "granite", and some of their description of the kind of rock. I just used the Hardhat granite model.

MR. BLACK: As you pointed out, it didn't fit.

MR. CHERRY: It did, but I had to assume that the rock was dry.

MR. RODEAN: And consolidated.

MR. BLACK: By assuming it was Hardhat granite, you mean what, then, Ted?

MR. CHERRY: Assuming it was Hardhat granite unfractured and ....

MR. BLACK: You mean in terms of density and that sort of thing?

MR. CHERRY: Yes. It was really the same equation of state as for Hardhat granite, except for the wet crack strength curve, I used the dry consolidated curve for the material.

MR. TRULIO: Let us draw a sharp line here between pre-shot and post-shot predictions. They are much different beasts. To account by choice of appropriate parameters for an event that has already taken place and whose results are known is one thing. To go out ahead of time and say "This is what the burst will do in that medium", is something else again.

MR. ALEXANDER: Which were you referring to here now?

MR. TRULIO: Pre-shot.

MR. ROTENBERG: Do you think this tremendous amount of detail that the rock-mechanics people are able to present is really necessary to give a source function to seismologists of the same order of accuracy as they are able to cope with? They don't need six-decimal accuracy, do they?

MR. TRULIO: Well, the kinds of errors I was quoting were not in that ballpark.

MR. SMITH: No, but the critical thing is that we don't need 20-cycle information, either. You are suggesting that a magnitude-yield relationship would be off by one magnitude unit here, and we don't think it is that bad at longer periods.

MR. TRULIO: I am glad to hear that.

MR. ARCHAMBEAU: How are you judging that you are off by an order of magnitude, in the shape, in the amplitude? What quantitative way do you have to describe your fit?

MR. TRULIO: The only real check we have after a calculation is made is a subsequent measurement of actual motion in the shot that is supposed to have been calculated. First, we examine gage records, comparing them with one another. Are they consistent? How big a spread is there in the experimental data? Then we compare the most probable measured velocity-time histories with those calculated.

MR. ARCHAMBEAU: Is the shape of the curve off, or is the amplitude off?

MR. TRULIO: We are talking about both shape and amplitude now. What I am saying is that for some media--the relatively homogeneous ones--you won't be off by as much. That leaves out NTS granite, even though Hardhat was predicted well enough by Ted (and by us), but then, why was Piledriver motion different from Hardhat motion?

MR. SMITH: If you low-pass filter this stuff, you would not find nearly that much variation.

MR. ARCHAMBEAU: That is what I am suggesting by my question. He should look at it in a frequency band of interest, and then talk about how badly you are doing, or how good you are doing.

MR. BLACK: How can they, though? The measurements that they get up close are not in the band of interest at the teleseismic distances.

MR. SMITH: Well, you have to low-pass filter those, too.

MR. ALEXANDER: Just run these through a low-pass filter, cutting out anything above a few cycles.

MR. RODEAN: Some of the stuff that you are talking about is in the nonlinear inelastic region, and how do you apply that?

MR. ARCHAMBEAU: We are talking about looking at what you predict in the elastic region.

MR. TRULIO: In the case of the tuff burst we calculated, measurements were taken only in the inelastic region; the gages were deliberately placed close enough to the burst point to provide information on ground motion in the inelastic source region. However, it seems to me that the calculated wave forms will be no less accurate in the elastic regime than they are closer in. But you have mentioned some things that we do not now include in our calculations. I suppose dissipation does not involve mode conversion if it is linear.

MR. ARCHAMBEAU: But your kind of dissipation is nonlinear.

MR. TRULIO: Well, yes, in shear failure or in compaction.

MR. ARCHAMBEAU: So you do get mode conversion.

MR. TRULIO: Yes, there, but does any such thing take place in what you call the elastic region?

MR. ARCHAMBEAU: No.

MR. TRULIO: No mode conversion?

MR. ARCHAMBEAU: No mode conversion.

MR. TRULIO: Then we can analyze the signal that emerges from our calculated region of inelastic behavior.

MR. ARCHAMBEAU: That is right.

MR. TRULIO: Still, in the best case you will have the signal only for a spherical field of motion. I think that data of comparable accuracy can be computed for axisymmetric motion in a layered medium, but I would not rely on the accuracy unless the separate materials are reasonably homogeneous.

MR. ALEXANDER: I would claim that at the long wavelengths they would be nearly homogeneous.

MR. ARCHAMBEAU: Yes, your media variability is not going to affect you so much at long periods, either. That is another point.

MR. TRULIO: I see we are both aiming for the same seismic source description, but between the device and the linear regime of earth motion, processes much more complicated than linear wave propagation take place. A little while ago I emphasized the desirability of performing small-yield shots, just because you don't have to go to very far from the burst point to reach the elastic domain of material behavior, and we want such bursts to take place in relatively homogeneous material so that symmetry is preserved. A few explosive events have been carried out that meet those constraints; Salmon should be one of them, and I think it is important to find out why it isn't. For the few satisfactory shots around, we should be able to provide you with accurate source functions in your definition of the term, i.e., at distances from the burst point where the strains never exceed 10^{-4} or 10^{-5}, and times when inelastic deformation has permanently ceased to occur all over the field of motion. The field of motion will have evolved to a point where even material that deformed inelastically experiences only elastic changes thereafter.

I also understand you to say that over big distances the media of concern to you are pretty homogeneous. Still, I would like to see how you would make use of the seismic source data we can provide. For example, how would you process the displacement-time history we would compute at distances say 50 or 100 ft from the center of a burst of 10 tons yield, where stress-wave propagation at still greater ranges would be linear? As I understand the point of view that you expressed, the problem is then defined for you in the sense that the source of seismic disturbances is adequately specified. However, I noticed that you did not talk about distances from the burst point in terms of wavelengths--maybe wavelengths are not relevant units of distance for purposes of seismic-wave propagation. Is the frequency content on a contour, where only linear deformation takes place, all you have to worry about?

MR. ALEXANDER: That is all we need.

MR. TRULIO: And you throw away all contributions that might be made to a seismic signal by local inhomogeneities?

MR. ARCHAMBEAU: Yes.

MR. TRULIO: Maybe that really does define the problem. You don't worry about complex motion that might be generated in a real geologic medium outside the domain that we calculate, but small compared to hundreds of kilometers ....

MR. ALEXANDER: What we are saying is we don't worry about the high frequency energy at the very short wavelengths.

MR. TRULIO: Yes, you would see those in close-in motion, but they would damp out at greater distances.

MR. ALEXANDER: Those would get progressively less important.

MR. TRULIO: I think it would be very interesting to analyze our source functions with your requirements in mind.

MR. ROTENBERG: We could do fairly rough calculations, rough with respect to our own standards, and still yield one-cycle information that

MR. TRULIO: Yes.

MR. ARCHAMBEAU: The only thing that bothers me is that you are really dealing with a messy physics problem here, a nonlinear problem, in your zone, so you might not be able to get too sloppy about it, because you have frequency transfer, that is, energy transfer between different frequencies.

MR. TRULIO: For the sources that can reasonably be considered spherical (and the experimental data are the ultimate test of that) there is no worry about the cost of doing the calculations. They are not expensive. You can put in all the zones you need, especially if (as I suspect) the calculation of wave propagation from the seismic source we supply is a much more elaborate and expensive job than getting the source itself; the calculation of the seismic source might just as well be done accurately.

MR. RINEY: Jack, in your calculations for tuff, how did you represent the source: Accurately, or did you use the adiabatic expansion?

MR. TRULIO: The burst to which computation I keep referring happened to take place in a cavity. We put in only a little device detail because the cavity was large compared to the device, but we made sure that we included the detail that prior experience showed to be necessary.

MR. RINEY: Okay, I realize there was not too much detail. It was not merely the adiabatic-expansion type of thing.

MR. TRULIO: Oh, no.

MR. RINEY: I think that is important.

MR. TRULIO: Well, for a cavity whose volume is only double that of the device, for example, you calculate the explosion; it's not a difficult thing to do.

MR. RINEY: But it is not done quite often, I think.

MAJOR CIRCEO: Say, Jack, your first calculation of this shot was done with the information from the tuff in Area 12, isn't that right?

MR. TRULIO: That is not quite right. Doug Stephens put out a report on work done (I think) at least a year before the shot we were calculating, and maybe earlier, in which he defined an "NTS tuff" supposedly representative of the many tuffs found at the whole site. Data for Area 12 tuff influenced Stephens' "NTS tuff" somewhat.

MAJOR CIRCEO: If you get average physical properties in the area, you could probably come pretty close.

MR. TRULIO: That might be fortuitous. I think that the important properties of Stephens' average material really do bound the corresponding properties of the material we happened to be modelling; otherwise the fact that the experimental data were bracketed by our "bounds" would have little or no meaning. We computed two fields of motion. I think one really presents a lower bound to the principal parameters of ground motion and the other an upper bound. Now that I think of it, I believe (a) the only important feature of the material's behavior that was not included in the material model is the strain-rate dependence of the stress, and (b) if strain-rate effects were incorporated in the model, the resulting curve of peak velocity vs range would lie between the two that we presented as pre-shot bounds.

MR. CHERRY: I think it might be interesting to go through how an equation of state of a site is developed for a calculation, and have you people criticize it in terms of the physics that you think may be missing for the kinds of wavelengths you are interested in.

MR. TRULIO: Or any other things that might be missing from the calculation.

MR. CHERRY: Of course.

MR. TRULIO: For example, and remembering that the calculations are carried to a point where material experiences maximum stress excursions that are maybe 10 or 20 percent of the overburden, we may need to define better initial conditions on the field of motion.

MR. CHERRY: But I don't see the problem, because once the reduced displacement potential that the code calculates does not change as you move to the next point that you call for, then you are by definition in the elastic region as far as the code is concerned. There is nothing else you are going to get out of this calculation by running it further.

MR. TRULIO: Not only is that a true statement, but we have used the principle that underlies it to shorten the time required to calculate seismic source functions. From the equations of linear elasticity we constructed a boundary condition for spherically symmetric motion that would permit computation to be confined to a finite range without any errors other than those of discretization. I have not finished formulating an analogous boundary condition for two-dimensional motion, which is much more complicated than the case of spherical symmetry; the displacement fields are rotational and require the evaluation of shear-wave source strengths as well as compressional wave sources. For spherical motion we tested the boundary equations by applying them at the boundary of the region of inelastic deformation, and comparing the resulting field with that obtained when the entire region of disturbed material was included in an ordinary continuum motion calculation. The differences between the fields generated in the two calculations amounted to no more than the usual discretization error.

MR. ARCHAMBEAU: You treat the medium as an elastic plastic medium, but in real earth, of course, you get fracture phenomena, radial fracture phenomena.

MR. TRULIO: Yes, but brittle and ductile failure are represented in the same way in our present material model--on some surface in stress space the material fails in shear. Inelastic hydrostatic deformation is treated in another way, although it is also possible to define failure surfaces which yield inelastic volume changes.

MR. ARCHAMBEAU: In one case it is microfracture. In the other case, that is not the situation. I mean plasticity is really just microfracture, if you want to look at it that way. When you get farther out, you get radial fractures, and that is important.

MR. CHERRY: That is the same failure surface, except you are in a different stress state.

MR. ARCHAMBEAU: Okay.

MR. TRULIO: All types of shear failure are given one and the same kind of mathematical representation.

MR. ARCHAMBEAU: A prestress would modify that.

MR. CHERRY: You would start out at a different stress state. The strength may be correspondingly better.

MR. ARCHAMBEAU: Then that may be an important feature to have the prestress in just for that?

MR. CHERRY: I agree.

MR. TRULIO: If the initial field contains material in various states of shear stress, then such details should be included in the specification of initial conditions, because the initial shear stresses will affect the amount and kind of burst-induced deformation required to cause shear failure in the material.

MR. ALEXANDER: But you don't make any attempt to program in a certain pre-existing crack distribution?

MR. CHERRY: No.

MR. TRULIO: No, and I want to reiterate that if the ground medium contains such a crack system, then it is not a good idea to use as a seismic source the results of a current state-of-the-art calculation of ground motion.

MR. ARCHAMBEAU: Can't you model that by some anisotropy factor, because a joint system mainly controls fracture and all of these things in the outer zone? Can't you modify your yield criteria to put in an anisotropy so things fail easier along certain planes?

MR. CHERRY: Yes, but remember, with the present limitations of the code, that joint system has to be either horizontal or vertical.

MR. TRULIO: I have not said anything about calculations of three-dimensional motion. There are a few very special three-dimensional problems that might be solved numerically to study the effects of anisotropy on computed flow.

MR. ARCHAMBEAU: It might be nice to do one of those very simple cases. You said it has to be horizontal or vertical.

MR. TRULIO: You can have concentric cylindrical surfaces, if you like.

MR. ARCHAMBEAU: Yes, it might be interesting to see what the effect is, even if you did it in that very crude way. The physics is there at least.

MR. CHERRY: Yes, of course. You could make it a slip line, so that you just get slipping along that boundary after the stresses have exceeded some value.

MR. TRULIO: I believe we have already done that for one or two bursts on layered media. Interior coordinate lines representing interfaces were treated either as surfaces along which frictionless sliding could take place, or as surfaces on which the material could also be considered bonded--or anything in between, for example, a certain amount of friction might be required to induce the sliding of material at an interface.

MR. CHERRY: That is clever, but a simpler approach might be just to have a small layer representing that joint pattern, and have that layer be very weak in terms of its strength properties.

MR. TRULIO: You can enforce a condition between perfect slip and tight bonding.

MR. ALEXANDER: What is not clear is really the importance even of that on the low-frequency radiation? It is still going to perturb only that high frequency, and we don't really care one way or the other. It is a question of what effect is that going to have on the long wavelength radiation?

MR. TRULIO: Those are the really interesting and difficult problems.

MR. ARCHAMBEAU: You have to address yourself to those problems eventually.

MR. TRULIO: Yes, but I want to point out again that there are some fairly simple (but not trivial) problems that now appear to be tractable from the point of view of defining source fields.

MR. ARCHAMBEAU: You clearly don't want to do anything really elaborate until you try out some of these simpler cases.

MR. CHERRY: My feeling is, before we go to the field we ought to go to the laboratory and do model studies, and normalize the codes from the results of the model studies. I think that is the first step. Apparently ARPA has people conducting experiments in prestressed pieces of plexiglass with notches ....

MR. RUBY: That was not supported by ARPA; I think that is DASA.

MR. CHERRY: There is absolutely no reason why the codes could not be used in conjunction with those small model studies to look at the details of the stress-wave properties. First of all, see if the codes are handling the effects that you want to predict properly in the laboratory. At LRL we are looking at the effects of material properties on model studies trying to use the codes to reproduce the results of small explosions in grout, sand, ice, and water, as well as other things.

MR. CHERRY: We are going through the normal standard equation of state procedures to get the material properties to throw into the codes. Then we do the experiment in the laboratory with the proper pressure transducers, and see if the results match.

MR. SMITH: There is one other important thing that we are leaving out here, and that is the interface between the distant seismic observations in the codes, that interface with the close-in measurements, and what I heard at this meeting. My impression is that those measurements are not adequate for the kinds of problems we are talking about. First of all, the dynamic range of the close-in accelerometers is not adequate that one can recover the low frequencies in that initial pulse.

MR. CHERRY: At some point you age going to have to believe somebody, and whether you believe the codes or whether you believe the experimental people, I don't know. The thing I am saying right now is that the first step is to go to the laboratory with both the codes and the kinds of model studies that you want to do, and see how they compare. You may not be able to build up your confidence in the gages that way, but you sure will be able to build up your confidence in the codes, and the prestress conditions that you can apply in the laboratory.

MR. TRULIO: I agree with that. I do think though, that the best tests are still well-instrumented field tests. Ultimately, you have to get to those. Not many have been done in the past, but there are some.

MR. CHERRY: No one has ever done a well-instrumented field test. Not enough data has ever been taken so that a confidence limit on a particular measurement could be obtained.

MR. TRULIO: I think DASA and ARPA have done at least one.

MR. CHERRY: Look at Piledriver. What was known about Piledriver pre-shot? There was no exploratory logging program pre-shot, for instance. There was no azimuthal coverage for stress-history measurements either. Also, the effect of the drift on these measurements has not been determined.

MR. TRULIO: Fortunately, I didn't have to make any pre-shot predictions for Piledriver.

MR. RODEAN: I think it might be pertinent to point out to the seismologists that, with respect to these computer codes and these close-in measurements, from the standpoint of history, the initial intent was to find out what are the phenomena close-in in the inelastic region. This was because it is the inelastic effects that are of interest and use to Plowshare, for example, in breaking up millions of tons of rock at one crack. DASA is interested in the response of silos to a warhead hitting a quarter of a mile away. So it is a relatively late development from the standpoint of explosion codes to go on and run the problems longer to get to the beginnings of the phenomena that you are interested in.

MR. ARCHAMBEAU: Of course, anything you do in the problem we are discussing will be helpful in those other problems as well, because presumably you would be modeling the media in some better way.

MR. RODEAN: Yes, although real close in, it almost does not make any difference what your material model is.

MR. ARCHAMBEAU: You are overdriving the thing to such an extent that you could have cheese there and it would not matter.

MR. RODEAN: Yes. That is why real close in, the Taylor-Sedov similarity solution is a good first approximation, even to explosions in rock.

MR. TRULIO: Stewart stressed the importance of long wavelengths, and I would like to add a comment or two on that subject. Happily, short waves are the hardest to compute, because disturbances narrower than the spacing between adjacent mesh points can't be resolved in a finite-difference calculation. In fact, it requires something like eight mesh points along a line to propagate a harmonic wave with reasonable accuracy. Accordingly, the codes do quite well for long wavelengths.

MR. ROTENBERG: You can't treat the short wavelengths too cavalierly close in.

MR. TRULIO: No, because there is mode conversion. You only know from running calculations with different finite-difference meshes whether you have converged numerically on a solution.

MR. ROTENBERG: You don't mean mode conversion. You mean frequency conversion.

MR. TRULIO: Well, all right, frequency conversion. There are no normal modes for the nonlinear problem.

I wonder, though, if we have to run the calculations so far that the disturbed region covers distances like the wavelengths of interest.

MR. SMITH: That is not necessary.

MR. TRULIO: I would have guessed not. The low-frequency content of a much shorter signal is probably the item of greatest interest.

MR. ALEXANDER: Yes, if you can give us the low-frequency contribution to it, that is all we care about.

MR. TRULIO: I think that gives us a really good place to start. There is a point of contact already. I think that at least one or two reasonable sources have been tested experimentally in the field; the sources were calculated and checked against data from those fields.

MR. ALEXANDER: The other thing of concern is the fact that at large distances we are really looking at the bottom, or looking essentially right under the event, as opposed to these measurements that are made out to the sides. If we have some feeling for what you expect the variations to be, say in a layered system directly below the shot as opposed to the side, it would be very helpful.

MR. CHERRY: LRL can talk about that.

MR. RODEAN: Yes, but not at this meeting, though. The report is still classified.

MR. TRULIO: For the source I would suggest, the flow field really is well approximated as spherically symmetric. There may be layers of material below the source, but any such layers lie so deep that they do not interfere with the symmetry of the motion on the region of calculation.

MR. CHERRY: What happened in the shot they were talking about is that the elastic radius below the shot ran into the water table horizontally.

MR. TRULIO: Yes, and we have also done layered calculations of surface bursts in which the top layer of earth was blown away out to an appreciable radius, but I think we should try to define seismic sources for simpler media.

MR. ALEXANDER: In the magnitude-yield relation, what we are seeing as far as the teleseismic P-wave magnitude is concerned is the energy that is going out on a small cone about the vertical axis.

MR. ARCHAMBEAU: That and the reflected wave which involved any spalling would be of some importance.

MR. ALEXANDER: The surface waves that are generated by the P waves that propagate to the sides would be the most relevant to the near-in observations.

MR. CHERRY: It probably is the whole thing. You are sampling the entire source region for the Rayleigh waves, some angles more than others.

MR. BLACK: I would like to ask you a question Ted. Suppose that we wanted to predict the seismic signals for a nuclear explosion from some specific place in the world. Suppose geologists tell us that the geologic map shows that there is tuff at the specific site we have chosen. Let us suppose, further, that the geologic literature provides us with a geologic description of the tuff, possibly including data on density and seismic velocity. Could you calculate the seismic source function as a function of yield by using ranges of physical property values for tuff based on your knowledge of the variation of the seismic source function with variation in source rock parameters?

MR. CHERRY: The best thing to do is to go to the equation-of-state efforts that we have done and just see if you can't find a tuff that fits the density or whatever description you have.

MR. RINEY: I might mention that we have taken all of the data on tuff that you have generated, and some data that came from experimental stations, and we have been able to model a consistent picture out of all of that, given the crystal density, porosity, and this is dry material. We are working on water as the next thing. These were really considered as two phases, pores and dry, and now we are working on water as the third phase. I think, within that context, we might be able to make a pretty good guess based on the inputs that you have given us and data that you have generated.

MR. BLACK: Assuming that you had all of that information, would you agree with Jack that depending on the actual properties, you might be off by an order of magnitude in the seismic signal?

MR. TRULIO: No, a factor of three is what I said for a medium like tuff.

MR. BLACK: You said a factor of two if you had real good data.

MR. TRULIO: Even better--50 percent if we had really good data. We found that we could place bounds on the motion such that the average of the bounds did not differ from the bounds themselves by more than 50 percent. A factor of two, perhaps, but I think that with really good material properties data, 50 percent is feasible. With not-so-good data for a tuff, the bounds might differ from their mean by a factor of three. But for a cracked granite, I don't think a factor of ....

MR. BLACK: Do you agree with a factor of three?

MR. CHERRY: I think as long as you have a handle on the density and the elastic velocity, you can probably get within a factor of two of say the reduced displacement potential.

MR. TRULIO: The biggest worry I would have is this: You need to know the mean stress as a function of excess compression (which is equivalent to the cubical dilatation). However, a loading curve will look like this for one material, and like that for another material of the same kind, but at different locations. Both samples may even unload from high stress in the same way, but at low levels of loading stress it matters a great deal whether the residual strain on unloading (compaction) is 2 percent or 3 percent. The irreversible work done in compacting material has an almost overriding effect on stress attenuation as you go away from the source. Measurements of compactibility are really important; lacking such measurements, one might guess at the compactibility, and I think most of the factor of three would originate with that guess.

MR. BLACK: As I understand it, you are telling me that with a reasonably good geologic description for a material like tuff, you could estimate the source function to within a factor of say 0.3 of a magnitude. The process of converting the source function to a seismic signal at teleseismic distances produces no further scatter and the seismic signal can be predicted to within a factor of roughly 0.3 of a magnitude?

An empirically derived curve for tuff, from our limited source area at NTS, shows a similar scatter of about 0.3 of a magnitude for a given yield. Do I need these calculations, therefore, for the purpose I have discussed, namely, prediction of seismic signals from unknown areas--unknown except for the geologic literature? What will the calculations provide that cannot already be obtained from the empirical magnitude-yield data derived from NTS tests?

MR. RODEAN: Rudy, you could say it in another way that sort of ties in with my plot (Figure 8, p. 32). I found that if we exclude unsaturated alluvium shots, then 80 percent of all of the data points I could find are within plus or minus 0.2 of a magnitude unit.

MR. BLACK: ARPA and DASA have a number of reasons for developing a computational capability to predict close-in ground motions that have nothing to do with the problem of predicting yield-magnitude relationship for teleseismic distances. For that long-range prediction of seismic amplitudes, as functions of source media and yield, problem, is it likely that the theoretical calculations are going to improve our estimates from empirical measurements?

This is the problem. I want to predict seismic signal amplitudes for a given size explosion in a given country to which I do not have access and from which I cannot obtain samples for Hugoniots, isothermal compression, or other physical property information. I am forced to look in the literature for a description of the source material. For any country in the world you can find some kind of geologic description, and let us assume I have a geologic description of a particular source medium. From that description, I would like to be able to compute the source function for say 10 kt fired in that particular material. I would then ask the seismologists to take the computed source function, propagate it to some hypothetical or some real network of seismic sensors, and determine the seismic detection threshold for that system. That is what I want to know.

I can get that information now, and we do, by using empirical yield-magnitude curves of the type that we showed here yesterday. With the empirical data I think that we can predict seismic magnitudes within 0.3 or 0.4 of a magnitude for a given yield in a given source.

The question is this, is it likely that we will be able to do very much better with code calculations (assuming imperfect knowledge of the physical property input parameters) in predicting teleseismic signal amplitude than we can currently do using empirical data?

MR. CHERRY: If you have the experience, scale it. My code is not set up for guesses. It is set up to use material properties data that the rock-mechanics people furnish me, to get answers in an environment that may be totally different from any experience that we have.

MR. BLACK: Right. The point is that the rock-mechanics people may not be able to do better than quote ranges for a medium which they know only from a geologic description in the literature.

MR. TRULIO: You already have a yield-magnitude curve.

MR. BLACK: One of the major reasons for ARPA support of the work in developing computer codes for prediction of ground motion was the anticipation that they could be used for prediction of the amplitude of seismic signals at teleseismic distances under circumstances when we could get a good geologic description, and that detailed work would have been done on geologic materials for which detailed properties were available, that it would be possible to make some reasonable extrapolations to the untested material, and therefore come up with a better answer than we could estimate from empirical yield-magnitude data. That was the argument. What do you think of the argument now? Was it right or wrong?

MR. CHERRY: We can do better extrapolations than we could have a year ago, but can we do better than 0.2 of a magnitude unit? Maybe 0.15?

MR. TRULIO: Yes, but the only materials you really have good curves for, empirical curves, are tuff and alluvium. What about other kinds of material?

MR. CHERRY: Look at it a little differently, Rudy. There are two neat things about the codes. First they take the mathematics out of the wave-propagation problem, so you don't have to spend your time keeping track of poles and branch points, and still make assumptions about the distance you are away from the source. That is the first one. Mathematics is gone from the wave-propagation problem.

The next thing that is neat about them is that you can change the physics in the constitutive model almost at will. Right?

MR. TRULIO: Yes, there is no problem in doing that.

MR. CHERRY: It takes you like half a day to throw in a different failure criterion, if you like.

MR. SMITH: At the risk of being really redundant here, I think that your question can't be answered without coming back and saying, look, these code calculations at high frequencies are irrelevant to the problem, and that the variations are going to be a whole lot less than a factor of three when one looks only at the low-frequency limit. I just don't think it makes any sense at all for us to be talking about one cycle at one end, and I don't know, 20 or above on the other, and talk about comparing them. It doesn't make any sense. That has been said several times.

MR. TRULIO: You say you can look at a yield-magnitude curve and you don't have to have a detailed source.

MR. BLACK: You don't have to know source details if you are going to use the empirical yield-magnitude data for prediction. I'm sure we all recognize the limitations of such a procedure, namely, that the yield-magnitude data is all from one source region, or almost all from one source region, and that region is not necessarily like the rest of the world; secondly, the empirical yield-magnitude data base does not represent the total variety of possible source media. It is very restricted. For example, we only have two shots to my knowledge, or maybe three, that are off NTS in new materials. We have Gasbuggy, Rulison, and Salmon. So what do you know about what happens if you shoot in limestone somewhere else, or suppose you shoot in salt somewhere else, or in thick shale or something like that?

It was hoped that, when the codes had been developed and tested sufficiently so that we could have confidence in predicted seismic source functions, it would be possible to use ranges of properties that people know about for shales, for example, and derive source functions corresponding to the range of shale parameters for a particular yield in shale by pumping in real parameters.

MR. TRULIO: That you can do, if you want bounds. I believe those bounds would be much better than to a factor of two.

MR. BLACK: Okay, but that is an approach to this problem.

MR. TRULIO: But you posed another question, namely, how well can we predict a shot of a certain yield at a certain place where we had only fragmentary data about the behavior of the material? I would still say a factor of three for soft rock or earth. But for bounding the possible range of responses, I think those bounds would be good ones. They might be a factor of three apart.

MR. SMITH: What about the cavity size? How close can you get in that? That is not a factor of three off if you don't know the materials.

MR. TRULIO: Some important aspects of ground motion are very sensitive to specific material properties. Even the answer to the question, what can be done with fragmentary data at a specific site for a specific yield, really depends very much on what those fragments are. If we had some loading and unloading data, or just the loading curve since it is not a bad approximation to unload with nearly infinite bulk modulus if you have no other data ....

MR. BLACK: Jack, suppose we had never fired a shot in thick shale. Let us also suppose that you have developed confidence in the procedures that you are using in making code predictions, based on physical properties of the source media, by testing predictions against field measurements for tests in a given rock type. For the shale, in which we have had no experience, you could vary the code input parameters to take into account the normal physical property variations that geologists know exist in shale and determine an upper and lower bound for the resulting ground motion in a medium for which there is no experimental data. That is useful.

MR. TRULIO: There is no question about that. Those bounds will be correctly set.

MAJOR CIRCEO: As a matter of fact, if we look at the comparison between the granite charts at the test site and the French charts, the geology itself was completely different, and yet for some unexplained reason we get similar magnitudes with yields, if we can believe that curve.

MR. TRULIO: I don't know why the two gave similar magnitudes. Was the Sahara granite event predictable? For a competent granite, meaning one that does not have lots of cracks and faults in it, probably the response to a given explosion is predictable. Maybe the French test was predictable, but that would not imply predictability for another medium that is highly cracked and jointed. You might conclude that the two bursts would produce nearly identical seismic sources, but I would not place much reliability on a prediction of that sort.

MR. RODEAN: When I heard this, I was very startled at the difference in cavity volume, and then I almost immediately remembered a figure in the SIPRI report that said seismologically they are the same, and I said "What gives here?"

MR. ROTENBERG: It could be just a conversion from French units to English units.

MR. CHERRY: It would be very difficult to obtain a factor of five difference in cavity volume, chimney volume, and cracking radius, and then to expect the same body-wave magnitude out of the two shots.

MR. TRULIO: It is.

MR. ARCHAMBEAU: That may not be so peculiar after all. For a body-wave magnitude, people are looking at 1 sec. For the spectrum at 1 sec the cavity size is not going to be that important.

MR. ALEXANDER: The radius of the cavity is small compared to the wavelength.

MR. SMITH: The strength of the source is the pressure times the volume in some way.

MR. ARCHAMBEAU: The spectrum in the two cases may be like this. (Figure 67). This is one cavity size. But they are measuring body-wave magnitude out here.

MR. CHERRY: That is the point that Howard made, I think.

MR. RODEAN: That is what I was saying. Maybe it is the distance.

MR. CHERRY: If that is all there is to it, then I guess that is it.

MR. ALEXANDER: I still come back to the question, where in all of the code calculations do you think the mistake is when you make a mistake in predicting? Is it the values that you get out of these laboratory measurements are not truly representative of the in situ properties? Is it because of the geometry of the calculations versus the geometry of the actual shot point?

MR. CHERRY: If you are asking me what things need to be improved in the calculations, there are two. The way we handle the nuclear source, I don't think that is right. The other thing that needs to be done is to include the stress-strain measurements that the rock-mechanics people develop in the laboratory. As the codes stand now, we are only using their final stress or failure data. We only take their final stress data, and say that is representative of failure, where in fact they are now starting to produce stress-strain curves as they load the sample up in triaxial compression. These data have to be matched by the codes. They have to be taken into account by the codes. The stress-strain path that they take to get up to failure as far as I know is not being used in the codes right now.

I guess what I am saying is that we are not using all of the data that they can furnish us, or that they are now starting to furnish us. We still have to learn how to use their strain measurements.

MR. ALEXANDER: Then there is another step, and that is how does that information relate to the in situ behavior?

MR. TRULIO: I would place the emphasis there myself. For materials like tuff and soil, we probably lack important strain rate data; with such data we may be able to predict motions in those media to within the accuracy that they can be measured. There seems to be little point in going beyond that. I think the biggest problems lie in representing materials in situ, and the problems are most formidable when the medium is cracked and jointed.

MR. RUBY: If I understand you here, I have an interest. What you are saying is we still have a reason to do a lot of rock mechanics, to sharpen your model.

MR. TRULIO: Yes, I think so.

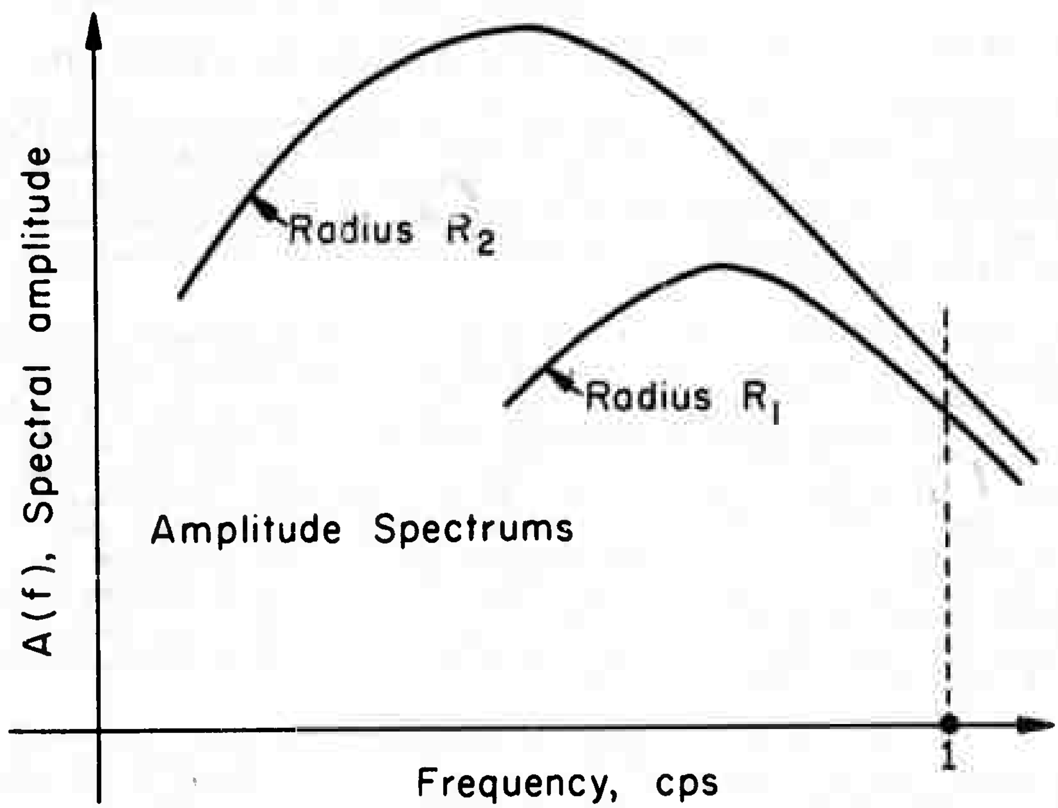

Figure 67. Hypothetical Spectral Amplitudes for Two Explosions With Different Effective Cavity Radii, R_1 and R_2. Here $R_2 > R_1$ and the peak in the spectra are controlled to a large extent by the source dimension, the peak moving to lower frequencies for larger source dimensions. Since M_b is measured from the amplitude near 1 cps this shows that the two explosions could have nearly the same M_b value, yet have quite different source dimensions so long as both R_1 and R_2 are large enough to give a spectral peak at a frequency significantly lower than 1 cps.

MR. RUBY: We never have any hope of getting such data for somewhere else, and it is worth getting for us to sharpen your models.

MR. TRULIO: I can see the possibility, for example, that a badly cracked material can be simulated in the labs, but that has to be proven. Maybe you can crack a laboratory sample on a small scale and relate the properties of that aggregate to the properties of the cracked medium in the field. But that has not been done; neither has anybody included the dispersive effects of the whole crack system into his material model.

MR. RUBY: DASA is starting a program this year of field tests with explosions that may shed some light there. But you see, the thing we are looking into is, are we spinning our wheels trying to get data? If I understand what you are saying, no, because it hopefully will end up in making your models better.

MR. TRULIO: I don't see how it can fail to do so, but to extend laboratory data to the field is a big step for cracked granite. It will take work and time to learn to make that extrapolation.

MR. BLACK: Why don't we close the gap now between what you are doing with the codes (coming up with source functions) and what the seismologists need. You mentioned the frequency. Is there anything else? Dr. Archambeau?

SEISMIC CALCULATIONS: REVIEW OF INPUTS NEEDED

Charles B. Archambeau
California Institute of Technology

I will put up a couple of things, and undoubtedly the other seismologists will have something to say as well.

MR. HARKRIDER: Are you going to put just the things that we need to satisfy ARPA or the things that we are interested in?

MR. ARCHAMBEAU: We hope they will be both. We will put up everything, and then we can put little asterisks on some of them to indicate things most important to the discrimination problem.

We need something equivalent to a pressure time function, and it must be given at a distance such that the strains are about 10^{-4} or 10^{-5}. The specification must cover the frequency band from 0.01 or 0.02 cps up to about 2 cps. That is probably the most important thing. Next, in the context of what was already said, you want the source function for more and more sophisticated representations of the medium, more and more sophisticated codes.

To start out with, I think we could utilize what you have already done in your one-dimensional codes, and look at that, and as time progresses hopefully there will be other results coming out which take into account cracking, prestress, and all of these other things that we have talked about.

There is something else that would be nice. I will put down the things I think of, and then other people may have some other things to suggest. We would like to know the fracture-zone radius. We want to know the radii or the dimensions of the various nonlinear zones. One of the reasons we would like the zone radii would be for our calculations involving stress relaxation.

A third item would be a description of surface spall effects in terms of energy propagating back down into the medium. That would be useful to us because we do get, of course, teleseismic signals corresponding to a surface reflection. It clearly is not a linear phenomenon. Some more precise knowledge of this signal or pressure wave would be very useful indeed, particularly because that gives us some information on source depth, which is a discriminant. If the phenomenon is nonlinear and gives us a bigger signal with a different waveform than we might expect from an ordinary elastic reflection, then that is very important. This then would be usable information, particularly if we could obtain a good estimate of the waveform.

All of these things would be given as functions of source parameters, of course, for a variety of materials. I think that is

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about all I can think of right now. This is really all we need. We can go from there.

MR. ALEXANDER: If you could just add on one more thing that would be ideal, although it is very difficult I realize to get it, and that is the pressure-time function over some reasonable volume enclosing the sources.

MR. ARCHAMBEAU: Yes. It need not even be specified on a spherical surface. The elastic zone is not necessarily going to be spherical, especially in more complicated situations. This is important, too, in the stress relaxation phenomenon that we have been talking about. This interior region around the explosion is a zone of highly disturbed material, and it clearly would have different properties than the material farther from the explosion, and you might want to worry about interactions of a large zone which has elastic properties that are very different from the medium around it, just because it has been shocked. I don't know how different one might expect the shocked zone to be, but it might have an effective rigidity that is small. Some of the problems we have had in the past have been: what the shape of this zone would be under different conditions, and how it might affect surface-wave generation and the static field in the surrounding elastic medium. For stress relaxation calculations, if we had this information, we would have one less unknown parameter to solve for when we are trying to reconstruct things.

MR. CHERRY: Can you take velocity instead of pressure?

MR. ARCHAMBEAU: Oh, sure, you can give us medium velocity, displacement, or stress, any one of which is essentially equivalent. We don't care, because we work in the frequency domain.

MR. CHERRY: What are your input functions? The thing I have not been clear about yet is what sort of input functions have you been using, and how different are they from the ones that Bill Perret measures, or we think ought to be given?

MR. ARCHAMBEAU: For the explosions, we use a pressure function specified on a surface in the elastic zone. The overpressure has the character of a step but with a finite rise time, followed by a slow decay to zero. This can be modeled by a simple functional representation. In the frequency domain we have used spectral representations of the pressure that look like the Sharpe solution with an exponentially damped step function. You see, we are interested again in the seismic band so very high frequency behavior is not important.

MR. CHERRY: And the reduced displacement potential?

MR. ARCHAMBEAU: Yes, that is adequate also.

MR. HARKRIDER: I have also used the Haskell time functions for the reduced displacement potentials. How good are they? That is one of the things I would like to know.

MR. RODEAN: That is really based on four measurements.

MR. HARKRIDER: Yes, but how good are they? That is what I have had to use because that is all the data I had.

MR. CHERRY: How good are you doing?

MR. ARCHAMBEAU: People adjust the available parameters in order to get a fit, and the question is, do the parameter choices they make, make any physical sense? They don't know whether they do or not.

MR. COOPER: How do we affect those parameters here?

MR. RODEAN: To me it is very interesting that from the long-period waves, surface waves, you infer an impulse function for this cavity pressure that decays down to zero. Yet our code calculations and Bill Perret's measurements reflect the permanent set with definite finite.

MR. ARCHAMBEAU: You are referring mainly to data in the nonlinear zone, the zone near the explosion?

MR. RODEAN: No, we are talking in the elastic zone.

MR. ROTENBERG: You are looking at 20 cycles.

MR. RODEAN: What I am saying is some of the low frequencies that you are talking about, your 20-sec periods, are in that which you throw away, because that pulse there lasts only about 1 sec.

MR. HARKRIDER: I had better clarify this. I don't know that any of us has synthesized Rayleigh wave using that. I used Haskell's results and I have not compared it to the data yet because I also wanted to calculate the difference between M_S for explosions and M_S for earthquakes. I calculated these things maybe a year and a half ago, and I just drew a curve to see what the yield versus magnitude was, M_S versus yield was, for just granite, I think it was, but I went on to something else and haven't gotten back to actually comparing it to observed data yet.

MR. RODEAN: By Haskell's you are talking about his analytic approximations for measured curves?

MR. HARKRIDER: Right, his parametrics, and that is all I had. What I want to know here is, if you have better ones, I would sure like to use them.

MR. TRULIO: Here again the distinction between pre-shot and post-shot predictions is probably important. He worked back from the signal to get a close-in wave shape that would give him the signal he worked bac from.

MR. SMITH: Howie, I think you didn't have the scale right on this pulse. It was a very long pulse, perhaps as long as a minute. It might well be a step function, because it seems to have a band-pass filter that does not include zero frequency.

MR. ARCHAMBEAU: Actually, people have used step functions for the pressure-time function and they work almost as well.

MR. RODEAN: Maybe I was misreading the horizontal axis, but I thought it was a much shorter pulse.

MR. ALEXANDER: Whatever that function is, it is slowly varying from shot point to shot point.

MR. COOPER: Yes, but how well do you need to know it?

MR. CHERRY: Hold it a second. The point is how much is it influenced by things that we don't have in the code right now, like condensation of the cavity gas from just heat transfer?

MR. ALEXANDER: That is what we do not know. How well can you predict what we observe is really the question. What we can say from observations is that whatever that source-time function is, it does not change too much from event to event, as far as these low frequencies are concerned.

MR. COOPER: Suppose you were to take a calculation of an event that has been done, is existing right now, and take information that you can get in the low-strain region?

MR. ARCHAMBEAU: Yes, that is what we ought to do.

MR. COOPER: Why don't you do that?

MR. CHERRY: There are some things that we have confidence in. I think Gasbuggy is one of them. There is some question about Hardhat.

MR. RODEAN: I think Salmon and Gasbuggy are about the best.

MR. COOPER: Salmon won't give you the Love waves.

MR. RODEAN: Well, the reduced displacement potential won't.

MR. COOPER: One of the things he does not have on the list up here is that.

MR. TRULIO: I would like to ask if you have a transfer function for a grid that covers the whole earth at regularly spaced points?

MR. HARKRIDER: You mean the real earth?

MR. TRULIO: The real earth. An event occurs somewhere, and you make measurements at many other places.

MR. HARKRIDER: You mean an observed one, not a theoretical one?

MR. TRULIO: An observed one. Is there such a thing now?

MR. SMITH: It turns out that there is just a tremendous variation from one place to another that is not very far away, because there are lateral variations in the earth.

MR. TRULIO: Okay, but that has to do with how fine a grid you need. For a start, grid points might be spaced every hundred miles along longitude lines. We would have conducted an explosion, or an earthquake would have occurred near the hypothetical burst, and you would record the signal transmitted from the burst point to every other point.

MR. SMITH: That does not exist. The distribution of sources is not that adequate. None of the earthquakes is in a very narrow band.

MR. TRULIO: You might use explosions to do that.

MR. COOPER: If you attempt to do this sort of thing, can you expect reciprocity? It would seem to be essential to that kind of approach.

MR. TRULIO: It is a linear field.

MR. COOPER: Right, but the concept is based on an a priori assumption.

MR. SMITH: Not if there are lateral inhomogeneities you don't have it.

MR. ARCHAMBEAU: Yes, reciprocity works out.

MR. SMITH: Not in the sense he is talking about here.

MR. TRULIO: Another thing you have going for you, as large a job as it seems, is that there is only one earth.

MR. ALEXANDER: In this situation you can finesse the whole problem by considering several events from a very localized area, and if you can tell us the expected differences between those, we can measure the actual differences very well, and without the influence of propagation distortion.

MR. TRULIO: You normalize it by having the experiment.

MR. ALEXANDER: You don't have to know anything about the transmission path at all.

MR. SMITH: No, but you have to have the data points.

MR. TRULIO: No, it is a black box, and you don't even know the path. You know for a given input here what comes out over there.

MR. ROTENBERG: Let me ask you a question which will show how naive I am about this. Let us say you have an earthquake which you measure at some distant point. Now you have another earthquake and you measure it again. Does the first earthquake, by and large now, and I am talking about something at magnitude six or something like that, does that change the local environment enough so that your transfer-function idea is no good any more?

MR. ALEXANDER: At most it would change it in the very immediate vicinity of the source, because you have all of the rest of the path unchanged. By far the most important factor in shaping the signals we get is the transmission through the earth, not what goes on at the source. So we have to get rid of this enormous effect in order to see what is going on at the source.

MR. CHERRY: But that is not consistent with everything else you are saying. What you have just said is that the phase of the signal is determined by the transmission characteristics, and the amplitude of the signal is determined by what is going on at the source.

MR. SMITH: No, the amplitude too. Both are affected by the source.

MR. CHERRY: Yes, but they ought to be invariant.

MR. ALEXANDER: The part due to the propagation. So is the phase part due to propagation. You can get rid of that.

MR. CHERRY: But the source phase ....

MR. ALEXANDER: That is still there.

MR. CHERRY: But it is a minor perturbation.

MR. ALEXANDER: I am claiming we can get it, though.

MR. ARCHAMBEAU: We can do well enough that we can get back to the source, but of course there will be an uncertainty in the result.

MR. ALEXANDER: If we have a reference event, we can tell the difference in phase between two different events.

MR. ARCHAMBEAU: If we study the earth carefully enough in some areas, we can infer the source spectrum without the use of multiple events.

Phase velocities we know pretty well. Of course, you don't need that information if you have more than one event. That's Shelton's point. If you are only faced with comparing events, then we can tell differences in the event spectra when they're in the same area and have common paths by dividing out the common-path effects.

MR. ALEXANDER: If you give us one displacement potential and another one from another nearby event, we should be able at teleseismic distances also to perceive the difference in these two, not what either one is absolutely, but we will give you the function that transforms one into the other.

MR. COOPER: That may be a break for the codes, in a calculational sense, because we have a lot more confidence in our ability to compute relative effects than absolute numbers.

MR. CHERRY: Yes. If you are only worried about relative amplitudes of the source, I think we stand a good chance of helping, but if you are asking what are the detailed characteristics of the failure associated with the source, then I don't know.

MR. ARCHAMBEAU: I think relative spectral differences will be useful, surely.

MR. SMITH: The original problem about the m_b thing is an absolute.

MR. ARCHAMBEAU: That is absolute, that is right.

MR. SMITH: That is amplitude.

MR. ARCHAMBEAU: That is amplitude spectra, really.

MR. SMITH: Frequency amplitude. That ought to work. I don't see any reason why ultimately that should not work.

MR. TRULIO: Could I ask you also if there is some recording or detecting instrument that you consider standard, and for which you could give both the real and imaginary parts of the frequency response curve?

MR. BLACK: The bulk of the teleseismic data used for magnitude-yield determinations was recorded by the LRSM stations, which have been operated for ARPA by AFTAC. The LRSM stations use standardized instrumentation for which response curves are available.

MR. TRULIO: Do you have a reference in which I could just look them up?

MR. ARCHAMBEAU: They are given in the SDL Shot Reports, that is, the Seismic Data Lab reports from Teledyne in Alexandria, Va.

MR. TRULIO: One of the things we should do as standard practice in calculating earth motion for various source conditions is take our own elastic output and put it through that kind of a calculation.

MR. SMITH: If you are going to do that, you have to do the Q structure of the earth as well.

MR. ARCHAMBEAU: Are you going to do our problem?

MR. TRULIO: No, just use the response of a standard (mathematical) detector as a measure of source strength--and maybe a better measure than the crude ones we have used. It might be the best way to compare calculated sources.

MR. HARKRIDER: These shot reports don't have the phase; they just show the amplitude response. You are not going to get the real and imaginary parts.

MR. ARCHAMBEAU: There must be copies of the response curves around, maybe not in the later shot reports, but just write SDL. They should be able to dig up those.

MR. BLACK: That is the best source.

MR. ALEXANDER: They have on file a calibration for each station. The amplitude is actually field calibrated at different frequencies, so you can get an observed frequency-response curve.

MR. SMITH: He does not care about the individual stations. He just wants the response so he can have a consistent thing to measure.

MR. TRULIO: You could use that as a measure of what the source is putting out, and you could make variations in the source geometry, and you would want to know in a rough way what it does to the signals put out. The criterion used now is a little too coarse--$R^2\delta$, where δ is the displacement of some spherical elastic surface.

MR. CHERRY: How sensitive is the phase of the recorded data to the phase of the source function?

MR. HARKRIDER: For M_s measurements, for magnitude measurements, which is what ARPA wants, right? Not just shape.

MR. CHERRY: M_s is surface waves. What you are saying is for surface waves the only thing that we have to look at is the amplitude of the source function. How about for the body waves?

MR. COOPER: Did I misunderstand? Didn't you say that for the surface waves you really need the relative amplitudes?

MR. ALEXANDER: In order to compare sources, yes.

MR. CHERRY: The thing is, if we give you two reduced displacement potentials, one that has one shape, and another one that has a different shape, you would be very interested in that difference.

MR. ARCHAMBEAU: This is in the time domain?

MR. CHERRY: Yes.

MR. ALEXANDER: We can derive a function that should take one of those into the other.

MR. CHERRY: That is a separate problem though.

MR. ALEXANDER: Yes, if we have that, we can relate it.

MR. CHERRY: You would be very interested in that difference out here, is that true?

MR. ALEXANDER: The whole thing, just the way you have gone.

MR. COOPER: But you could make use of nothing more than the difference, even if you did not believe either number precisely.

MR. SMITH: There is a discrimination problem, and then there is the absolute problem of determination of yield.

MR. COOPER: I was talking about discrimination.

MR. SMITH: I don't think this is very relevant to discrimination.

MR. ARCHAMBEAU: What? The question of the difference?

MR. SMITH: No, the reduced displacement potential I don't think is relevant to the discrimination problem.

MR. ARCHAMBEAU: We would like to know the difference between the spectrum of the explosion and the spectrum of an earthquake.

MR. SMITH: Yes, but that is only out at long periods. It is just the area under that curve that makes any difference at all. In the discrimination problem, we would be much more interested if you would give us the reduced displacement potential for an earthquake.

MR. CHERRY: If you give me the stress distribution, I will try.

MR. ARCHAMBEAU: I think I can do it already.

MR. ALEXANDER: But you have to do an instrumented earthquake for us.

COL. RUSSELL: Several people have planes to catch. We want to thank everyone for participation. I think there has been an excellent interchange of information. Everyone, I think, to use President Nixon's term, clearly understands the other one's problems. Thank you very much.

(Thereupon at 3:30 p.m., the meeting was concluded.)

A SYNTHESIS OF THE PROBLEMS IN SEISMIC COUPLING

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Introduction

These two conferences (June 8-9, 1970: reported in ARPA-TIO-71-13-1, and August 18-19, 1970: reported in ARPA-TIO-71-13-2) established communications between the diverse disciplines required to predict the shock effects from nuclear explosions out to teleseismic distances. These disciplines involve the use of rock mechanics, geology, nuclear physics, computer hardware and codes, seismology, and field instrumentation. Results from the conferences included (a) improvement in the communication links between the engineers and scientists engaged in research relevant to the seismic coupling problems, and (b) identification of open circuits at some points along the communication lines. This paper focuses attention on those open circuits.

In the prototype experiment a nuclear device is embedded in a hole (cavity)* at some specified depth beneath the ground surface. The device is exploded (triggered). The energy produced is partitioned into electromagnetic and radioactive radiation, thermal and mechanical (kinetic) energies. The radiation and thermal energies attenuate rapidly; therefore, their possible appearance at teleseismic distances is ignored. However, the kinetic energy stimulates intense motion of the earth media surrounding the explosion; the resulting body (m_b) and surface (M_s) waves can be identified and measured at distances ranging upwards of thousands of kilometers from the explosion (seismic) source.

This simplified perspective is presented to show why several different scientific disciplines are required to interpret the effects at the measurement point. First, there must be an accurate evaluation of the partition of nuclear energy during and subsequent to the explosion; this quantifies the amount of kinetic energy available to stimulate ground motion. Next, an understanding of how different characteristics of the earth media can affect the propagation of this kinetic energy is required. It is necessary to install instruments that can measure the resulting motions close in to the seismic source. These characteristics and measurements then can be introduced into computer codes designed to describe the orientation and amount of the stresses produced by the ground motion from close in out to teleseismic distances. These stresses can be resolved into the ground displacements that can be expected at teleseismic distances. Measurements are also

*There appear to be differences in the use of the word "cavity". Dependent upon the individual user, the word may refer to the hole produced immediately after the explosion, to the hole that develops after the ground in the explosion area reaches stability, or merely to the shape and size of the hole in which the nuclear device is placed.

made at teleseismic distances. These are compared with predicted measurements to establish the criteria required to reveal the location and the yield of seismic sources that are inaccessible for U.S. measurements (U.S.S.R. and Communist China).

What Do We Know?

A prominent scientist once said, when discussing the effects of shock waves on hardened installations, that a conference discussing what we know about such effects should be completed within a few hours; however, a conference that discusses what we do not know, would require many days. This philosophy guided the preparation of this report. Part of the conference time was a discussion of what we now can do to predict effects from nuclear devices, particularly at teleseismic distances. The objective was to explain how such effects can be extrapolated to define the yield of explosions that occur in inaccessible areas and also to discriminate between explosions and earthquakes. Our current capabilities in the latter cases had to be qualified by numerous questions relating to the gaps in our prediction ability. This paper summarizes these questions, describes the weak links in the communication lines between the different disciplines involved in the prediction problem, and directs attention to the research required to close the communication gaps.

Role of Geology and Rock Mechanics

If frequent reiteration of a communication problem is any key to its importance, the most significant problem is the lack of numerical methods that will describe the effects of geologic defects, anomalies, discontinuities, etc upon the seismic signal. Time and again the following questions were raised:

"What effect do fractures have upon the energy dispersal and the wave shapes?"
"How can a computer code consider movements along joints?"
"What effect will prestress (also termed 'residual', 'ambient' or 'tectonic' stress) have upon the wave propagation?"
"Can a dispersive model be constructed for jointed and cracked hard rocks?"
"What is the effect of anisotropy in rock properties?"

Ancillary questions were related to the inherent integral properties of a rock element. For example, identification is required of those parameters that can significantly affect either laboratory or in situ tests. Attention has been directed at the changes in wave characteristics produced at various levels of compaction of the rock but there has been little attention to how tensile stresses might affect such characteristics, and, because most waves have a rarefaction phase, it is possible that the behavior of rock in a tensile mode would be of significance.

In Situ Vs Laboratory Properties of Rock

One question that perhaps was most frequently asked was whether the in situ properties of the earth media can be accurately portrayed by laboratory testing. The answers to this question disclosed a divergence of opinion: one group believed it feasible to impose special boundary conditions on the laboratory test specimens to the degree necessary to simulate the prototype performance reliably. However, some conferees felt that reliable answers could be obtained only by in situ tests. A major foundation for these diverse opinions was that because of natural fractures, the in situ media is not a continuum, whereas most laboratory techniques and concommitant analyses are based upon the assumption that the test specimen is a continuum.

Laboratories have used artificially fractured material in an attempt to simulate the effect of joints or fractures. These tests have developed coefficients of friction for such fracture interfaces, but there remains the question of whether such coefficients are valid for natural fractures. Resolution of this problem will require large-scale laboratory or in situ tests. A subsidiary problem is to identify the physical factors that can affect the coefficients of friction on such surfaces.

There also is a need to know the pressures or frequencies or amplitudes that will cause fractures to close and perhaps become transparent to shock waves. Or will discontinuities of this type produce wave refraction and reflection? Most rock systems (and intact rock elements) exhibit some degree of anisotropy in their velocity characteristics, strength, and moduli. There is some evidence that the degree of anisotropy decreases with increasing loads, but further study is required to determine the influence of rock fabric and other natural constituents.

As input to the code calculations it is necessary to have the true in situ compressional velocity, density, isothermal compressibility, water content, compactibility, and the loading and unloading hydrostatic data. At present these values generally have to be obtained or extrapolated from laboratory tests, but their comparison to in situ properties has not been quantified. For example, how does the density determined from an intact laboratory specimen compare with the density of the discontinuous rock system through which the shock-wave propagates? To evaluate the degree of accuracy necessary for such comparisons it will be necessary to conduct parametric studies to define the variation permissible in such values when used in code calculations. A related information gap is the current lack of data on the aforenoted rock properties at pressures up to about 2 kb. There appears to be adequate laboratory data above that pressure level.

A recent step has been taken towards correlation of laboratory and field properties. These studies have found a definite size effect on the Young's modulus of elasticity: the modulus (and the strength) of rock appears to decrease with increasing size of the test specimen. These conclusions are derived from laboratory and in situ tests upon comparable rock elements.

Regardless of the feasibility of achieving a laboratory-in situ test comparability, it was suggested that there would be considerable use for dimensionless rock-property combinations. The latter might provide a more rational method to identify combinations of shot-point rock properties. Also, if such dimensionless values could be established, then instead of using rock names (such as granite, tuff, and alluvium) a dimensionless rock description could be inserted in the magnitude vs yield vs rock-property type of plot. Such dimensionless numbers are difficult to establish because of the wide scatter in the velocity and displacement data that appears to be caused by local cracks, joints, faults, folds, and inhomogeneities. The present analytical approach is to assume a mean value that hopefully will give proper weight to these scatter-inducing properties. The very strong influence of the inherent properties of a rock element has been indicated by the field measurements of such quantities as particle velocity where, at a specific range, such measurements often disagree among themselves by factors of two or three.

Pore Pressure, Porosity, and Water

What are the effects of pore pressure and/or porosity? Does the porosity of a laboratory specimen have a definable relationship to the porosity of the in situ rock system (with its open joints, fissures, etc)? Secondly, how much range or variation in porosity can be tolerated in the code calculation without significant effects on the output? A subsidiary effect of porosity is that an increase in pores may permit an increased water saturation of the material and also a possible increase in pore pressure when the media is subjected to load. The latter occurrence could be of considerable significance in calculations that include media strength because an increase in pore pressure generally means a decrease in effective strength--depending upon whether the pore pressure is sufficient to disrupt molecular bonds between crystals or between grains and the matrix. Another point to be explored in this regard is that in rock (unlike soil) there may be no continuity or connections between pores; therefore, do we have an adequate understanding of the porosity vs water-saturation effects when such rock is subjected to a dynamic load?

The effects of water, including the pore-pressure problem, require considerably more study. There appears to have been insufficient dynamic testing of both intact and cracked material in both the wet and the dry state. Such research is important because it has been established that the change in mass density caused by presence of a water table has an effect upon the wave propagation. A further question stems from the present assumption that once the depth to the water table is established, all media below that depth must be saturated. Observations in deep tunnels, however, have disclosed tunnel walls that are relatively dry (or, at the most containing only a few percent moisture) even when there are perched water tables above the tunnel elevation. Thus, it is possible that a perched water table might introduce a spurious layering effect in the seismic signatures. There are other possible effects from the presence of water in the media. Relatively close in to the explosion the water may be converted to steam that has an as yet undefined effect on the stress distribution and wave propagation. Also, the effect of water on coefficients of sliding friction between rock elements has not been entirely clarified.

Viscosity

Another factor that appears to have been given too little attention in laboratory and field tests is the influence of the rock viscosity. Theoretically, viscosity should have a strong influence on the high-frequency waves; this has been learned during studies of the transmission of m_b waves in the earth's crust. The effective Q for transmission of m_b waves is on the order of 1000 in the crust but decreases to an order of 100 in the upper mantle. Related factors that may have to be considered in evaluating wave propagation through the crust and upper mantle are the possible movement of interstitial atoms in the lattice, and diffusion of dislocations, partial melt, and pore water.

Failure Criteria

Perhaps the most significant gap in our knowledge of the fundamental properties and behavior of rock is the lack of a reproducible failure criterion. We require a criterion that can provide a mathematical description of the state of the media when failure occurs, including the stress distribution that develops at the failure point. The comparatively recent development of the "stiff" testing machine has made it possible to obtain complete stress-strain curves for many rock materials. For very brittle rock, however, the failure is too rapid to permit delineation of the entire failure path. Therefore, there is a need for a complete stress-strain curve for all rock materials that might house a seismic source.

Reduced Displacement Potential (RDP)

The seismologist measuring effects at teleseismic distances has found that the properties of the earth media definitely influence the reduced displacement potential, but quantification of these effects has not been too successful. The lack of success is attributed to the difficulties in developing a numerical description of geologic defects such as faults, fractures, joints, structure, and stratification.

Formulation of a theoretical method that will accurately translate a shock wave from an inaccessible seismic source to a measuring point thousands of kilometers distant presently encounters two major gaps in the transmission sequence: (1) the inability to translate the influence of geologic anomalies into numbers that can be used in code calculations, and (2) the lack of detailed knowledge of the rock properties at the source and between the source and the measurement point. Present opinion is that if we have a geologic description of the earth media at the source we can extrapolate the value of the yield to within 20 to 30 percent of its real value. Also we probably can get within a factor of two of the actual reduced displacement potential if we are provided the density and the seismic velocity of the source material. Our prediction accuracies could be improved if we could establish that the source material had geologic and physicomechanical properties that closely resembled some of the materials intensively studied in field and laboratory tests (such as granite, tuff, and alluvium). However, it was stated that the present dynamic codes might produce a yield prediction that could be in error by a factor of three up to an order of magnitude for such material as tuff! Also, we will require better correlation between the conduct and analyses of nuclear tests and the pre-explosion laboratory and field tests. For example, it was suggested that an objective appraisal be made of the comparisons that have been made between code prediction of nuclear test effects and the actual effects.

Instrumentation and Measurements

Many of our current problems stem from technical deficiencies in our instruments and our procedures. We now lack data on stress conditions at the hypocenters of earthquakes. Therefore we cannot accurately define the resulting seismic-source configuration and establish specific differences between it and a nuclear source. We are severely limited in the depth to which we can make in situ stress measurements. There has been limited success in stress measurements at depths of as much as 4000 ft; however, hypocentral depths are beyond our instrument (and possibly even our drilling) capabilities.

In the laboratory tests, present techniques permit us to measure only the average stress. Thus we must consider the specimen in its entirety; our measurement techniques have not developed to the degree where we can pinpoint the effect of microscopic and, in some cases, macroscopic defects on the stress distribution in the specimen.

One of the most significant gaps in our measurement techniques occurs when we attempt to relate laboratory to in situ measurements. Regardless of whether we are using static or dynamic loading techniques, as discussed previously in this paper, an acceptable correlation between laboratory and field measurements seems to occur as an exception rather than as a rule. Until this gap is closed, we will have to place increasing reliance on field measurements. However this requires us to develop more reliable and relatively inexpensive methods of making in situ measurements. Also, as was pointed out by one of the conferees, we appear to have no way to make direct use of laboratory-determined material properties to estimate the late-time response of an in situ rock system to an intense shock wave.

Available accelerometers and velocity gages are sufficiently rugged and sensitive to acquire usable information relatively close to the seismic source. However, we do not have a good displacement gage for such close-in effects, particularly one that is capable of measuring displacements on the order of feet in a small-diameter bore hole. At the other end of this spectrum is that because our close-in instruments primarily were designed to measure relatively high motion, they cannot measure strains down to the order of 10^{-5} to 10^{-4}; consequently, in the purely elastic response region such instruments are not effective. We can make reliable measurements at teleseismic distances, but we need a parametric study of instrument capabilities. This may enable the design of instruments having degrees of sensitivity that change with relation to their distance from the seismic source.

Another problem occurs in the establishment of the instrument arrays at teleseismic measurement points. At present, extensive extrapolations of their data are required because only a relatively few instruments are placed at these distances. If we had more stations and azimuth control it could be ascertained whether the geologic structure at the measurement point or the properties of the media at the source control the radiation (of the shock effects) pattern. For example, it would be desirable to have two rings of stations fairly close in to the source and all located within one (geological) structural province where lateral variations in properties were known to be insignificant. Such arrays would permit a study of the radiation patterns as a function of frequency and thus determine whether the theoretical assumptions were correct. The design of such instrumentation, however, necessarily will depend upon a decision as to what parameters should be measured. There are some code specialists who believe that the Rayleigh wave would provide much better information for extrapolation of yield because it samples much more of the structural environment, whereas the P_n wave would not be too good because it considers only a small part of the source region.

One suggested aid to the measurements is to monitor microseismic noises in the vicinity of the seismic source prior to the shot. This might provide a clue to the prestressed state of the rock because large stress gradients probably would give a relatively high frequency of noise. At the very least, it would enable a comparison to be made of the ambient stress situations at different shot environments. (Instrumentation for such measurements does exist, and it has been used frequently to monitor potential rock-fall areas in tunnels. Therefore, it merely is a question of adapting this instrumentation for the purpose suggested.)

The foregoing questions point toward the need for in situ measurement techniques that (a) have a greater reliability than the present ones, (b) can evaluate the changes in properties under dynamic loading, (c) can test several cubic meters of a rock system, and (d) can accomplish the aforenoted measurements without introducing new defects into the rock system. The latter accomplishment would make it possible to test the same rock system under different boundary conditions.

Prediction Code Accuracy

A definitive study of the different codes now used to calculate stress distributions close in to the source indicated that the primary differences between these codes are the manner in which they conserve energy and mass. Some conserve total energy by definition whereas others compute changes in both the kinetic and internal energy analogs and then check each time step to be certain that total energy is conserved to within one part in a very large number (such as 10^6). Other codes use kinetic and internal energy analogs defined so that the finite-difference equations explicitly conserve total energy.

Teleseismic Prediction

The present codes were designed to study effects close to the source, and they have not been expanded to predict ground-motion effects at teleseismic distances. However, it appears to be within our capabilities to expand these codes so they will produce the latter effects because most, if not all, of the codes now can describe the stress behavior from the source to within the elastic zone. Their expansion to describe effects at teleseismic distances should be relatively simple because the earth media between the present prediction limit and the teleseismic point would be responding as an elastic body.

The first step would be to check the codes for the sensitivity of their calculations. We then could learn what parameters should be measured and just how precise these measurements should be. On the one hand, this will require the seismologists to input the degrees of sensitivity that they require and are able to measure; on the other hand, the rock mechanicist will have to state not only the available sensitivity of laboratory tests but, more importantly, the current capabilities of field instrumentation. For example, is it useful for laboratory measurements to be carried out to one or more decimal places when such precision is not feasible in the in situ measurements? Also codes are structured on the basis that the material being modeled is homogeneous, isotropic, and originally elastic, but, the true media may exhibit none of these properties.

Equations of State

At present we do not know the degree of accuracy required for the Hugoniot data to serve as input for theoretical calculations of the decoupling situation. Our codes also currently presume that we have a complete and accurate equation of state for the earth media subjected to the energy forces. This implies that the equation relates stress, strain, and some of the thermodynamic variables; however, we do not have equations of state for all the types of earth media that might house the seismic source. And, it is not yet clear which rock properties and wave effects are significant in strain-rate dependent behavior. The absence of the latter information makes it impossible to specify the shock-stress levels where purely hydrodynamic rheological effects will occur.

Effects of Heterogeneities and Defects

Existing one-D spherical codes can be used to describe the early stages of ground motion only in a homogeneous media. The introduction of inhomogeneities or defects in the media forces consideration of at least two-D and possibly three-D effects--but such two-D and three-D codes still are in their infancy for such calculations. It was suggested that the calculation difficulty might be partially alleviated if cracks were introduced as an isotropic phenomenon, i.e., they would be assumed to be distributed in such a random manner that there would be no preferential influence on the physicomechanical effects they would produce. However, this introduces the earlier discussed difficulty of defining the wave characteristics at the interface between two cracks. This factor needs resolution, particularly at teleseismic distances where the wave energy is too weak to close the cracks. Thus, in summary, the problem is to determine the degree of wave dispersion close in to the seismic source where the cracks could be closed by the shock energy and the effects at teleseismic distances where dispersed waves would be disrupted further when they encounter fractures that do not close.

Essential to the input of a prediction code are the geologic and rock mechanics data. At present code calculations force-fit preconceived theoretical models for geologic media to the laboratory data, even when there is only a relatively small number of applicable stress states. The requirement is for numerous parametric studies that interface controlled laboratory and field experiments with the code calculations. Such studies would improve the quantitative understanding of the in situ response to dynamic effects.

One empirical finding that has not been predicted successfully by our codes is that the yield vs magnitude curves for different rock types appear to be indistinguishable. For example, unsaturated tuff, granite, and salt all lie approximately on the same curve. Theoretically, the inherent strength of the media elements should exert an influence on the energy dispersal and thus the inherent physico-mechanical properties of the media should be significant. Resolution of this apparent anomaly would indicate the direction for future research on the rock-mechanics problems associated with nuclear effects. It may be that very close to the source, the rock type is relatively unimportant, but at what critical distance does it become influential, i.e., at what pressure and strain ranges does the rock type become significant? Also it would be of interest to find if defects and inhomogeneities in the rock system exert more influence on wave propagation than do the properties of the intact rock element.

Miscellaneous Considerations

An undecided factor in the calculation of energy dispersion is whether the codes should consider that open fractures may accept large volumes of gas from the explosion. That is, if there are existing fractures or if the explosion opens large fractures, will the latter accept sufficient volumes of gas to attenuate some of the energy relatively close to the source? Our only clue is deductive in that if radiation does not leak to the surface, it is presumed there were no fractures. Part of the answer could be acquired by determining what percentage of the volume of the rock system is occupied by such fractures subsequent to the explosion. Another possible factor in energy attenuation is adiabatic loss. Most of the codes used for ground motion prediction give little or no consideration to such losses because their primary concern is with kinetic energy.

Could codes be made more accurate by decreasing the zoning size, that is, use very fine zoning? It was pointed out that in many cases you would get less accurate answers if this were done, and that for two-D problems, it would not be practical to zone down to a very fine degree. The possibility, however, is that the ILLIAC IV computer may have the capability to handle a very finely zoned problem, particularly those problems derived from two-D or three-D codes.

ILLIAC IV

A brief comment on the ILLIAC IV is appropriate at this point. Most of the conference presentation on this computer related to the hardware although there was a considerable discussion of its operational capabilities. Of special interest to future code calculation is the tremendously increased computation speed as compared with that of existing machines. For example, one of the 64 processing elements in the ILLIAC IV can fetch information from the memory to the operating register in less than one-half the time required by a CDC-6600. Full utilization of the 64 processing elements in the ILLIAC IV will enable it to produce floating point operations at a rate comparable to somewhere between 64 and 128 CDC-6600s.

This new machine should facilitate two-D code work because of the methods it would use to store a matrix and to perform finite-difference calculations. For example, if you want to do one manipulation in the interior of a mesh and a different manipulation on the boundaries, the ILLIAC IV storage capacity and arrangements make it possible to access and parallel all of the values on the top boundary and the bottom boundary because they each are stored in different processing element memories. They then could be copied to the operating registers in parallel and adjustment could be made of the boundary values. Reportedly, these types of calculations may have efficiencies in excess of 80 to 85 percent, i.e., the average number of processing elements turned on during a calculation is approximately 80 percent of 64. Matrix calculation efficiencies generally will be in excess of 50 percent.

On the other hand, accessing information in tables will not be too efficient if the table is so large it cannot be contained in the memory of a single processing element. In particle-motion problems and in nonlinear radiation transport where the particles affect the absorption properties of the media through which they are being transferred, the efficiency may degrade to as low as 25 percent. Another difficulty is that there are no parity checks in the machine at any point. The only way to determine errors is to run a confidence diagnostics program that exercises all of the branches of the logic in the processing element. In other words, you would compute 64 answers simultaneously and determine if any one result differed from all of the others. If so, this presumably would be a logic error.

(NOTE: All of the conferees' statements about the ILLIAC IV were presented prior to actual operation of the machine; presumably, therefore, its precise capabilities and efficiencies are yet to be determined.)

Back to the Codes

A basic and recognized deficiency in code operations is the frequent lack of suitable input data. This deficiency would be alleviated to a considerable extent if there was a comprehensive compendium of all of the test data that is relevant to the calculation of nuclear shock effects. Such a compendium would be particularly valuable if it included time-history details and peak-value tabulations. These would have to be listed in comparison with the more or less standard property data. Such a compendium also would identify significant gaps in the data.

Present codes presume a spherical cavity with a spherical field of motion. Either of these factors can become asymmetric with a resulting degradation in the accuracy of the computation. The amount of such degradation is unknown but it would be desirable to determine the influence of other than spherical cavity shapes and other types of wave shapes. This could be accomplished by a parametric study designed to evaluate the significance of the resulting differences.

Another useful exercise would be to perform model studies with changing boundary conditions and changing inherent properties. Code calculations then would be performed to see if the results from at least small explosions can be reproduced by codes for various types of materials. The work on just one type of material, tuff, has considered crystal density and porosity but has not introduced water. The latter work is now being initiated, and it is believed that water would introduce a third phase, the first two phases being a porous and a dry material. A related suggestion was to introduce ranges of properties about each main rock type and derive source functions that would correspond to the range of parameters for each particular rock type for a particular yield. This study at least might establish the bounds for the rock types that are studied.

Seismological Input and Output

It would be desirable to modify the codes so they can compute m_b waves and surface phenomena simultaneously with the production of the effects produced by Love waves. One difficulty is that most, if not all of the "large" explosions generate Love waves, but the Love wave does not appear in most lower-yield explosions. Therefore, for code computations using these parameters it would be necessary to define the critical points or boundary lines between yield and the type or types of waves generated vs the distance to the measurement points. And, as stated earlier, the code calculation should be extended to a radial distance sufficient to compute strains as small as 10^{-5}. This would permit a direct comparison between seismological and code calculations.

One point remaining unclarified was whether the present codes can estimate the radial extent of fracturing and crushing out from the source. This definition is required for delineation of the earth-media model that must be used to characterize wave-shape changes and dispersion.

The seismologist would find it useful if the codes could produce the displacement field in potential form within the elastic zone. This implies the definition of ground motions at stress levels of only a few hundred psi, and present codes do not have this capability. The present codes do not contain routines to generate the scalar and vector displacement potentials throughout the region of linear motion. Two-D routines are required and the resulting errors can be on the order of 20 percent or greater. A better feel for two-D problems with a failure mechanism included would permit determination of the true shape of the elastic boundaries around the explosion and in the spall regions. There still would be a need to introduce geological anomalies such as faults, but this might be approached by first doing a calculation that ignores the fault, and then consider the disruptive plane in a manner that permits an inexpensive parametric approach. For example, the plane could be oriented in various ways to determine the orientation effect on the definition of pressure across the plane. This study could be expanded by evaluating the effect from slip-stick motion and from prestress in the media.

The Teleseismic Signature

The seismologist observes a signature on his instruments at the teleseismic recording point--what does it mean? This brings us to the final step in the sequences of wave propagation.

Reduced Displacement Potential

The most important element in an accurate diagnosis of the teleseismic signature appears to be the prediction of the reduced displacement potential of the wave at teleseismic distances. A major control on the nature of RDP is the calculation of the radius at which the earth media starts to react as an elastic body under the influence of the shock. Field measurements and calculations indicate that the RDP is affected seriously by the material properties such as hysteresis and strength. This implies a need to determine late-time displacement in all possible media for all possible source configurations. Although it is known that the RDP is seriously affected by material properties, there is some doubt whether there is sufficient accuracy in the methods now being used to quantify the behavior of these properties. Thus we face the problem of accurately calculating the full range of effects from an explosion close in (where the pressure may be in millions of bars and the temperature in millions of degrees) out to teleseismic distances where the pressures will be a small fraction of a bar and ambient temperatures prevail.

Questions

One diagnostic question is raised by the fact that cavern collapse (at the source) may produce surface waves that appear almost identical to the surface waves produced by the explosion itself; yet the description of these two phenomena in a code calculation would be considerably different. Another question evolves from the situation where the crustal structure at the receiver significantly influences the wave form; therefore it would be desirable to calibrate each source region insofar as the signal level vs yield is concerned.

In general, resolution of the following would assure a better diagnosis of the teleseismic signal and extrapolation back to its source:

(1) How can correlation be achieved between the shot medium and the surface-wave magnitude?

(2) Is it possible to predict which seismic signals in the pass band 0.5 to 2.0 Hz actually propagate out into the elastic zone?

(3) Further attention should be directed to the use of spectral shape as a discriminant although it is recognized that this will not be feasible until there are several azimuths of instrument arrays.

(4) Earthquakes are more efficient in production of Love waves than Rayleigh waves, although the ratio is station dependent and the energy distribution in both time and frequency are different. However, there is not sufficient earthquake data to achieve an accurate diagnosis of the signal by comparing the spectral ratios of Love and Rayleigh waves. Although the exact mechanism of Love wave generation still is unknown, a better understanding might be acquired if theoretical calculations were made near the source. Then, it would be possible and desirable to design a shot that produced propagation effects similar to those from an earthquake.

(5) Can we quantify data distortions that are caused in the short-period data by attenuation, spherical spreading, and layering? The solution of this problem is the key to use of absolute signals as a means of determining the source parameters. There also is a requirement for a model that considers all of the crustal heterogeneities, including such factors as the variation of velocity and density with depth, the reasons for wave attenuation in different media, and the influence of surface topography, subsurface stratigraphy, and structure. And, although we know that the coupling of energy in hard rock may be an order of magnitude greater than that in soft media, can these distinctions in the source media be identified at teleseismic distances?

(6) The prediction accuracy would be enhanced by efficient operation of two-D codes, including use of a failure mechanism to describe the true shape of the elastic boundaries around the explosion and in the spall region. Surface spall effects clearly are not a linear phenomenon, therefore more precise data is needed on the description of these effects in terms of energy propagating back down into the medium; also, these factors should be expressed as functions of source parameters for a variety of materials.

(7) More accurate predictions would be possible if more precise data were available on the properties of source material that are inaccessible to U.S. investigators. [Author's Note: Such additional data might be extracted from the open Soviet literature on rock mechanics tests within the past decade. This literature rarely indicates the geographic source of the test specimens, but collation of such data may make it possible to group the rock types having similar properties. And, it may be feasible to delete data where the testing evidentially was related to civil, mining, or petroleum engineering projects. Analyses of such collations could provide us with at least a reasonable range of expectable properties in potential source materials.]

(8) There is a requirement for something equivalent to a pressure-time function at a distance where the strains are on the order of 10^{-4} to 10^{-5} and that cover the frequency band of 0.01 to about 2 Hz. Further this pressure-time function should encompass some reasonable volume that encloses the source.

(9) The present codes can predict relative amplitudes of the source, but it is questionable if the codes can provide detailed characteristics of the failure associated with the source. This problem requires knowledge of absolute amplitude and frequency spectra.