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Abstract
Frequency analysis of acoustic emission spectra has been done by our group and others for several years now. Lloyd Graham presented some of the results in a previous paper. One would like, of course, to extract as much information as possible from these spectra. We hope, for example, that at least some fracture or failure processes, microscopic failure processes, will have distinctive frequency signatures: perhaps certain kinds of phase transformations or, as has been discussed, microcrack initiation by brittle fracture of intermetallic particles.

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THEORETICAL ANALYSIS OF ACOUSTIC EMISSION SPECTRA

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Frequency analysis of acoustic emission spectra has been done by our group and others for several years now. Lloyd Graham presented some of the results in a previous paper. One would like, of course, to extract as much information as possible from these spectra. We hope, for example, that at least some fracture or failure processes, microscopic failure processes, will have distinctive frequency signatures: perhaps certain kinds of phase transformations or, as has been discussed, microcrack initiation by brittle fracture of intermetallic particles.

We recently began a theoretical effort to produce models for such sources of acoustic emission, and quickly realized that it doesn't do any good to have a model for the source unless you understand what happens to that emission on the way to the transducer. I'm going to discuss two simulated acoustic emissions, two examples, which are processes that are simple enough that we can make models for the source with reasonable confidence, and which permit us to see if we do understand the medium response. They have some intrinsic practical significance, but the main purpose is to test our understanding of the medium and transducer response.

The first of these is the white noise experiment that Lloyd Graham mentioned. It's a fracture of small, 20 to 40 μm silicon carbide grains on a steel slab. Because of the absence of intrinsic structure below 25 MHz in this source emission, it proved to be a useful practical device for the characterization of medium and transducer response. The second process I want to discuss is the acoustic emission of a small steel sphere being bounced gently off a steel slab. This produces qualitatively a quite different spectrum, which is one reason I considered it. In fact, there's a certain philosophical contrast.

The silicon carbide fracture, although used in practice for the characterization of medium and transducer response, is, in fact, a rather good analogy to a real acoustic emission process where one at least hopes from an understanding of the medium to learn something about the source of the acoustic emission. The sphere impact problem is probably more plausibly viewed as a situation where one understands the source and uses it to measure some property of the medium. In fact, there's an interesting, although probably remote, possibility of using it to make a real non-destructive testing acoustic emission device.

Let me describe my idealization of Lloyd Graham's--I guess he's not the only one that's done it--white noise experiment. I consider a steel
slab, think of it as about 1/2 inch thick, with a small silicon carbide grain on top of it with a force on it which fractures the grain. That sets up a wave bouncing back and forth between these two planes, which causes the bottom surface to wiggle (see Fig. 1). Now, in the simplest form of this experiment, there is a capacitor microphone. In the figure, the bottom surface is one side of a capacitor, so that the voltage signal across the resistor is a direct measure of the displacement, or at least the displacement averaged over the capacitor at the lower surface of the slab.

Now, in fact, there are a lot of modifications of this experiment in a real laboratory. The slab doesn't extend infinitely. Although there are reasonable experimental approximations to that, they haven't been done. In fact, it's more convenient to put the microphone on top of the slab, usually, although there have been some studies at the theoretically convenient spot at the bottom.

I'm going to consider solely the frequency spectrum of the displacement, not its time dependence. However, in order to make a model for the source of this acoustic emission, let's think about the time dependence of the force, which is the source of it.

Now, Lloyd Graham has a slab in the laboratory about 4 inches square. It has a little hollow in the top. He puts some silicon carbide grains in it, and he takes a pestle and he grinds by hand. So, the loading frequency is something like 1 Hz, very, very slow loading. The release of that stress is determined by the crack propagation time through a silicon carbide grain, 20 to 40 \( \mu \text{m} \) in diameter. That time is about \( 1/25 \) \( \mu \text{sec} \). That's an instantaneous release on the frequency scale which we expect to observe, which is no more than 5 MHz at present. So, this is very well approximated by a step function. Now, of course, there's lots of detailed structure here, but the point is you can't observe that unless you can go to very high frequencies. So, we don't need to know about that. The Fourier transform of a step function is \( 1/\omega^2 \). Empirically, Lloyd finds the output voltage, that is, the displacement of the lower surface of the slab, decays roughly as \( 1/\omega^2 \).

Before I describe how I approach a fairly careful calculation of this, let me give you an heuristic argument which makes all sorts of omissions but, which most people prefer to start with, that is, to hear before they hear how you calculate it in detail. I just argued that the input stress decay is \( 1/\omega^2 \) over the frequency because of the step function. Now, if we think of this exciting a stress wave which propagates through the medium and neglect boundary conditions, attenuation, dispersion, the tensor character of the force, all those things you can't really neglect, then, we expect something like \( \sin \left( \frac{i \omega r}{c} \right) \) times the input stress function divided by \( r \), the distance from the source. Don't worry about whether it's a sine or a cosine or \( e \) to the \( \frac{i \omega r}{c} \), the error is much greater than that. Then the force on the lower surface,
Fig. 1. Schematic diagram of the silicon carbide fracture experiment.

Fig. 2. The oscillating curve is the theoretical calculation for a 1/2 inch thick slab and a 1/2 inch diameter microphone. The straight line is exactly $1/f^2$. 
the thing that goes into the wave equation, is the divergence of the stress
tensor. I just looked at one component of that, and the leading term in
that, because $\omega^2$ is large, brings down a factor of $\omega$. It cancels the $\frac{1}{\omega}$
from the input. That means that the force, except for a possible
oscillation, is independent of frequency, and the amplitude of it doesn't
decay. Now, I put that into $F = ma$. The acceleration gives me a factor of
$\omega^2$, and I calculate the displacement $u$ of $\omega$ by dividing the force, which
I just argued is frequency independent, roughly, by $\omega^2$, or I get the $\frac{1}{\omega^2}$
answer. If you don't like that, forget it.

To approach a more serious calculation—I shouldn't call that a cal-
culation—a more serious attempt to determine the frequency spectrum, let me
set up a more general classification. Let me separate forces, that is, the
sources of acoustic emission, the forces which give rise to the acoustic
waves, into two kinds: those that are on the surface of the slab and those
that are internal. I will give a couple possible examples of a surface in-
elastic source, and these are forces which aren't described by the wave
equation, for example, crack nucleation by the brittle fracture of inter-
metallic particles, which we have just heard is probably very common in
aluminum, and perhaps corrosion pitting. That isn't established, to my
knowledge, but there seems to be a correlation. And there are internal
inelastic forces. There are lots of these, for example, a martensitic
phase transformation.

Now I'm going to suppose that the source is localized compared to a
wave length. This is not an essential restriction, but it's a great con-
venience, and there are so many interesting physical sources which are much
smaller than 1 mm that it isn't a serious restriction. What I need is a
stress transfer function, which I call $H$, leaving aside for the moment the
question of where one gets that. Then, the stress any place, inside or up
to the surface, is given by a matrix multiplication of these two—well, any
given event has one or the other of these zero. So, it's a matrix multipli-
cation of some transfer function times the source.

I should note at this time that this is the acoustic emission analog
to the integral equation approach to scattering theory, which Prof. Krumhansl
described previously, and it shares many of its attractions, such as an
additional simplification here that because, by taking the source to be
localized, I don't have any integrals left. I just have matrix multipli-
cation, and no convolution integrals in time, which is, I think, a consider-
able attraction for the analysis of data if one hopes to extract this source
function. Now, I do as I did in the heuristic arguments: the quantity of
interest today is the displacement field. I take the divergence of this stress
tensor—that just involves differentiating this matrix—and divide by $\omega^2$. The
quantity of physical interest, the displacement as a function of frequency
on the lower surface, is given by a transfer function which has all the
properties of the medium including attenuation, dispersion, and the boundary
conditions multiplied by the source function which, in general, has all of
the things which are really of interest.
Now, one turns to what is a computationally efficient approach to getting an approximation for this. Because we're talking about slabs which are at least 1/2 inch thick and frequencies which go up to at least 5 MHz, an expansion in normal modes is unattractive. The other extreme is to ignore the boundaries all together and use a free space Green's function. That turns out to give answers which are in gross disagreement with the experiment. So, I have a third possibility. What I do is take this free space Green's function. Now, remember that this is the Green's function for the stress tensor. That's a fourth rank tensor, not the usual acoustic Green's function, although they're related by taking a few derivatives. It is a 9 x 9 matrix, and I add to it three similar terms which do not change the singularity structure. They still satisfy the differential equation and help to satisfy the boundary conditions. In particular, the boundary conditions on the components which contribute to the quantity we're calculating, that is, the z component of the displacement on the lower surface, these boundary conditions are exactly satisfied. There are some other boundary conditions which are not.

Now, without going into detail on what this rather ugly Green's function looks like, I should like to make clear that this is not an image approximation. It does involve an infinite number of reflections, but it just doesn't include everything that could happen. In particular, some boundary conditions are oversatisfied in the sense that $\alpha_{xx}$, in which we're not interested, always vanishes at the surface even though physically it need not, and in fact, does not in general.

Figure 2 shows a quick spectrum for silicon carbide and silicon carbide fracture. The ugly curve is the theoretical calculation. I was disappointed that it wasn't a little smoother, but the straight line is exactly so that you see it does roughly represent the trend of the data. However, $\omega^2$ as a by-product of this calculation, we learned something about transducer response. I analytically averaged the results over the transducer area, and I find that the results are sensitive to transducer area, which is not surprising, very short wavelength signals tend to cancel over the transducer area. So, high frequency contributions are suppressed.

The detailed behavior, the detailed falloff with frequency changes significantly. This calculation is for a 1/2 inch diameter capacitor on a 1/2 inch thick steel slab and I chose Q to be about 3000, which is, perhaps excessive for the steels of interest. These oscillations are thickness resonances of a slab, and although it's a little ugly, when you hit a high Q material you have to expect it to ring. There are, in most experimental situations, things which tend to smooth this. For example, the reflections from the end, which I've neglected, will tend to smooth, although this is not too bad a qualitative agreement with what's observed.

Before I discuss another couple of spectra, let me describe what happens in the qualitatively different situation of the sphere striking the slab. Again we make a model for the force (see Fig. 3). This is a classical problem in elasticity which you can look up in many, many places. The name
Fig. 3. Contact force of a steel sphere on a steel slab obtained from the Hertz contact theory. The dotted curve is a qualitative description of the contact force in the presence of plastic deformation.

Fig. 4. Theoretical calculations of the displacement of the lower surface of the slab for the two types of input stress.
Hertz is normally associated with it, and I haven't done anything new. I've just taken a simple approximation to standard formulas. The important point is that I calculate the time dependence. What happens, when the sphere strikes the slab is that the force starts out at 0, increases to some maximum negative value, and as the sphere recoils, the force decreases again to 0, and they separate. Now, for a 2 mm hardened tool steel sphere dropped 1 mm on a steel slab of the same material, the contact time is 10 μsec. If there is plastic deformation, the force will not reach as large a negative value and it will fall off more quickly.

The time scale is very important because that determines the frequency scale where we will have appreciable contributions. This is very long compared to the 1/25 μsec for the silicon carbide fracture. That means that the frequency spectrum will be concentrated at much lower frequency. It's also rather interesting that if there's plastic deformation, it will fall off more quickly, and the frequency spectrum will be extended to higher frequencies. I didn't do any calculations with the plastic deformation because I didn't have any reasonable model for it. That's just the qualitative effect.

Figure 4 shows the spectrum for sphere impact. Lloyd Graham has done this experiment quite sometime ago, and found that it does fall off significantly more rapidly than the silicon carbide fracture. Analytically it falls off as 2 powers of ω faster, although there was no effort at that time to determine the quantitative behavior. In addition, there's an oscillatory behavior superimposed, as perhaps you can see from the silicon carbide fracture curve. There are two points to be noticed. One is that they excite substantially the same modes of the slab, and you can see that it does fall off significantly more rapidly, and that some of them are suppressed because of the oscillating envelope.

Let me compare (Fig. 5) the silicon carbide fracture for two different transducer diameters. The 1/8 inch transducer is a much smaller transducer which sees a spherical wave front which is much closer to being a plane wave, and therefore, its response falls off significantly less rapidly with frequency and it's less wildly oscillating. The rate it falls off with frequency can't be determined very well from this linear-linear plot, but it can be easily determined from a log-log plot. But the fact that the oscillations are less wild is clearly present, and that is a consequence of the fact that a large capacitor microphone acts a little bit like an interferometer, suppressing some modes and enhancing others.

What have we learned? We have qualitative, even semi-quantitative agreement between the acoustic emission experiments and the calculations in three areas: the silicon carbide fracture with its rough 1/ω² behavior, the fact that the sphere impact falls off much more rapidly—we haven't had a chance to test that quantitatively, but this is obviously not a very surprising prediction—and finally the capacitor response, which I think is an interesting and important practical question. I should note that the NBS transducer calibration
Fig. 5. Theoretical calculations of the displacement of the lower surface of the slab, averaged over the capacitor area, for two different capacitor diameters.
experiment described in the paper by Eitzen (Session III) due to Breckenridge, Tschiegg, and Greenspan suffers in general from the same sort of consideration. They used a very thick slab. If you use a thick slab and you don't go too high in frequencies and use a small capacitor, then you can neglect this effect.
DISCUSSION

DR. TIEN (Henry Krumb School of Mines, Columbia University): Thank you, Bill. Any comments?

DR. GREEN (John Hopkins University): I'd just like to make a brief comment about Greenspan's work at the Bureau of Standards.

DR. PARDEE: I implied no criticism whatsoever.

DR. GREEN: I think it's a beautiful experiment.

DR. PARDEE: I think it's very well done. I think it would be very nice to do a frequency analysis of it, and I didn't mean to imply that there was anything wrong with their experiment. It's just a consideration which should be used if one attempts to apply it in inappropriate circumstances. I agree, it's a beautiful experiment.

DR. TIEN: Mutual admiration society here.

DR. DEWAMES (Rockwell International Science Center): Do I understand that the peaks you're getting, then, are plate modes?

DR. PARDEE: Thickness modes of the slab.

DR. DEWAMES: Can you identify specifically the position of these modes in terms of the thickness?

DR. PARDEE: Yes.

DR. DEWAMES: Can you make a one-to-one correlation?

DR. PARDEE: Yes.

DR. DEWAMES: So, if you changed the thickness of that plate you will see a change in the spectrum?

DR. PARDEE: Yes.

DR. DEWAMES: Has that been done?

DR. PARDEE: Not yet. We plan a series of experiments to quantitatively test these calculations. So far I have been just going through and looking at Lloyd's old experiments to see how close I can come to calculating something which corresponds to them.

DR. DEWAMES: So, that would be one point. These modes are quantized according to the thickness, then. Have you found them quantized according to the length?
DR. PARDEE: According to what?

DR. DEWAMES: To the length. They are quantized in three dimensions, basically. You have an incidence--

DR. PARDEE: Well, it's really quantized in one dimension, when the reflections between the top and bottom surface superimpose coherently. That's what the quantization is for.

DR. DEWAMES: Your calculation does not take into account the other dimension?

DR. PARDEE: Well, the other dimension is present, but it just radiates--there are two kinds of things that happen. It either radiates out to infinity or it's localized in the other dimension, exponentially decaying.

PROF TIERSTEN (Rensselaer Polytechnical Institute): Could you say something about the equations that the Green's function satisfied or what they looked like and under what conditions? In other words, just define the Green's function in terms of a differential equation and boundary conditions--but you didn't do that?

DR. PARDEE: Yes. Linear elasticity with two elastic constants, complex elastic constants, in general, but not a very sophisticated approach to attenuation. Then the differential equation is roughly what one would derive simply by taking the normal equation for the displacement field with a body force, where the body force is physically a source function, and then deriving from it an equation for the stress tensor by standard manipulation using Hook's law. Now, the boundary conditions are obtained by the following procedure. The equation that one obtains in this way is not self-adjoint. The differential equation for the Green's function is then--the proper Green's function--is written as a differential equation for the adjoint operator, and by a suitable multiplication and integration by parts, one expresses the stress tensor at an arbitrary point in terms of an integral over the source function and two surface integrals, one of which involves a displacement field, and the second of which involves the stress tensor and certain normal components of the stress tensor at the surface. Now, that's analogous to VonNeumann and Dirichlet boundary conditions, and one chooses one or the other. Of course, the physically interesting one is the stress-free boundary condition, which is the only one which I discussed.

PROF. TIERSTEN: But you used a displacement field for the representation of the Green's function. You used the stress tensor.

DR. PARDEE: The displacement field only occurred in deriving the differential equation.
DR. LARRY KESSLER (Sonoscan, Inc.): I looked at that result and looked at the input function, and, really, maybe oversimplified it, but you measured displacement as your output function and found that it varies $\frac{1}{\omega^2}$.

DR. PARDEE: I didn't measure anything, Lloyd did.

DR. KESSLER: Okay, Lloyd did. It was measured and was $\frac{1}{\omega^2}$.

DR. PARDEE: Although Lloyd does find that, if you look underneath the slab with a suitable transducer diameter, it's not exactly $\frac{1}{\omega^2}$.

DR. KESSLER: Okay. Let me stay with the oversimplification, then. If you go back to intensity, if you have a constant intensity source, and vary the frequency, the displacement would decay as $\frac{1}{\omega}$. In other words the displacement is a function of frequency and decays as $\frac{1}{\omega}$ over frequency at the constant intensity. What I'm trying to get at is the intensity of the sound source, however produced inside by the emission as a function of frequency. If you divide out that $\frac{1}{\omega^2}$ by $\frac{1}{\omega}$, you wind up with a $\frac{1}{\omega}$ frequency dependence of the acoustic emission source, wherever it is. Now, your input function was a stress which had a $\frac{1}{\omega}$ dependence, and they tracked exactly. You're saying that your output is proportional to $\frac{1}{\omega}$, but your input was proportional to $\frac{1}{\omega}$.

DR. PARDEE: No, the output was proportional--

DR. KESSLER: You said output was the intensity, the output intensity.

DR. PARDEE: Okay.

DR. TIEN: I've got a question. You had a phase transformation as one of the sources. Now, the kind of phase transformation that people talk about is usually twinning or martensitic transformation. Is the noise due to the phase transformation or the dislocations, the transformation or the dislocations that go with it? Do you have any feelings for that?

DR. PARDEE: I'm reluctant to speculate in front of the stenographer.

DR. TIEN: Can your fingers stop, please?

DR. PARDEE: I don't know the answer to that.

DR. TIEN: Thank you, Bill.

DR. CRAIG BIDDLE (Pratt/Whitney Aircraft): Would you repeat the last question? I didn't hear it.
DR. TIEN: It strikes me that most of the phase transformation noises people hear are associated with transformations that involve dislocation motion, like twinning. You have twinning pole dislocation. Martensitic transformation certainly has with it elements of dislocation motion. So, my question is: are these really two parts, or maybe you guys know. The noise is generated, but do you hear the emissions from the dislocations or from some lattice stress release due to the transformation?