SUMMARY DISCUSSION

Robb Thomson, Chairman (National Bureau of Standards): What I thought we would do this afternoon is to pull together, to the extent that we can, what we have heard earlier in the day in the plenary talks and the poster presentations that we have just observed. The plenary talks addressed methodology for reliability and two general categories of materials: ceramics and polymers. In the poster sessions there were quite a variety of topics covered, mostly having to do with specific instrumentation addressed to the question of NDT—not so much on the methodology side, but more at the NDE interface. We would like to explore the convergence of these two elements.

To begin, I would suggest that methodology be considered as the first category of concern (together with materials), and that attention be drawn primarily to the gaps in the methodology that we have relative to particular materials categories, even though primary attention in materials has been placed upon ceramics today. We will include, of course, metals and composites in our discussion. Now does anybody want to add to or subtract from these categories before we go ahead?

Otto Buck (Science Center): What are the elements of the methodology?

Robb Thomson, Chairman: I include under methodology three general subcategories. They are defects and distributions, measurements, and stress history.

Robert L. Crane (AFML): My only suggestion is in the area of ceramics and metals. You might wish to consider materials with limited ductility, i.e., not a classic brittle solid like ceramics and not a material as ductile as a metal. A material with limited ductility or a ductility which is very much a function of temperature would be of interest.

Robb Thomson, Chairman: Good. Anything else?

Since we apparently agree on our format, let’s begin the discussion. Dr. Rogorsky of Drexel University has requested three minutes for comment.

Alexander Rogorsky (Drexel University): Thank you very much.

Gentlemen, I have listened to the speakers in the morning session with great interest. Actually, the problem of reliability and the determination of accept/reject criteria are most important elements of NDE. I would like, briefly, to present one idea which has been recently started at Drexel University. In many cases of ultrasonic inspections, the parameters of the part to be inspected and the associated testing conditions are unstable. For example, ultrasonic attenuation, surface conditions, quality of acoustical coupling, etc., can affect the amplitude of ultrasonic signal. To avoid these variables, ultrasonic signal can be presented as a function of random variable characteristics related to the defect, to the part tested, to the testing conditions, and signal distributions. Another approach consists of measurement of an experimental reference histogram for ultrasonic signals and their combination with scattering considerations and probabilistic distributions of other parameters. This experimental histogram can take into account real distributions of such variable parameters as attenuation, scattering condition, etc. The first step is to measure reference ultrasonic signals from different spots of the component being inspected, and the second step is to predict the most probable amplitude level which corresponds to the sensitivity of inspection. This second step uses results of preliminary statistical analysis. A simple algorithm using this approach can then be prepared. Results to date show that the accuracy of flat bottom hole size determinations using this approach can be improved by 3 to 4 times.

The main advantage of this procedure, we think, results from the elimination of standards and a consideration of real testing conditions.

Thank you.

Robb Thomson, Chairman: Thank you.

Gordon Kino (Stanford): It seems to me that the procedure that was just suggested is very closely related to the procedure that Mucciardi of Adaptronics uses. In fact, his first examples were on flat bottom holes in which he calibrated on a series of flat bottom holes in different samples of metal. He used an adaptive routine and then came back and sized the hole very accurately.

Alexander Rogorsky: I think this procedure is not repetitious of the Mucciardi method simply because I used it 8 years before for another problem of testing adhesion. Now we are using it for steel castings. We didn’t use adaptive learning networks for this procedure, but simply probabilistic approaches.

Robb Thomson, Chairman: I’d like to suggest that we come to grips with a couple of general questions on the methodology.
Let us assume for now, anyway, that reliability can be achieved by putting the three elements referred to earlier together in the right combination.

John Brinkman (Science Center): I am bothered by a couple of things. First of all, requiring an a priori knowledge of a defect distribution tells me that somebody is assuming we are going to use sampling techniques as opposed to a hundred percent inspection. I believe that many industries are moving more and more toward the requirement of a one hundred percent on-production-line inspection. I know that the Army has committed itself to do this for the inspection of the metal parts for its large caliber projectiles. I believe I know that much of the automobile industry would like to do the same thing with respect to the inspection of many of the discrete parts that go into automotive production. If you assume that, then another question arises. That question is, what is the reliability, not of measuring the defect properly, but of detecting it first of all?

If we can solve that one, and I would like some comments on that, then it seems to me that if we do a hundred percent inspection and observe a hundred percent of all defects which are at or near critical size, we don't have to say we need to know the distribution of defects beforehand. We are going to measure it. Why shouldn't we strive for that goal?

Robb Thomson, Chairman: Are there any others? I suggest that we let several questions come up and then we will try to pull things together. Are there any further questions that you want to put up like that?

Fred Morris (Science Center): There is another related problem. There are classes of materials and loading conditions for which the parameters that control failure are not treatable with conventional fracture mechanics. I have in mind a material failure which is principally governed by crack initiation processes. I do not mean to say that cracking is not appropriately a part of what is commonly considered as initiation, but only that the laws that govern the growth of those cracks are not presently amenable to description by conventional fracture mechanics. This includes situations where the crack overloads are very large or where microcrack coalescence is an important aspect of failure. For a given alloy and loading conditions one is faced with the prospect that 95 percent of the fatigue life is in the initiation stage. Therefore, the window for failure detection is only five percent of the fatigue life, in terms of our present day NDE techniques. Thus, the core of the scatter in time and reliability prediction has to ultimately look at initiation, either dealing with microcrack development in certain circumstances or more complex precursors to the microcrack formation. As Don Thompson, for instance, has pointed out, there are techniques for looking at this regime of the failure that are currently being looked at only cursorily, that have some promise in regard to looking at these types of situations and I hope we could talk about some of those in a little detail as well.

Robb Thomson, Chairman: Any further questions or concerns to put on the table?

M. Srinivasan (Caborundum): One of the things that concerns me in the methodology aspect relates to the problems we have in identifying and classifying defects. The actual parts are quite complex, and I wonder how much the design community appreciates the NDE people's problem. The philosophy of design could make the NDE people's life a little bit easier and yield a better product. If the designers can appreciate the problem of the NDE people, they can design the parts which will be amenable to an easier inspection. That would be a very welcome thing to the NDE community.

Robb Thomson, Chairman: Anything else? Yes?

Shirley McDonald (Lockheed): I think more attention needs to be placed upon the refinement of NDE instrumentation to be sure that we are really using it right. Not all people use a given instrument in a given way. The instrument may not even be optimized for some applications.

Robb Thomson, Chairman: Any further questions or concerns?

Cecil Teller (Southwest Research Institute): I am concerned that the importance of initial residual stresses might be overlooked in the probabilistic approach to fracture mechanics. I'd like to hear some comments or thoughts on this.

Robb Thomson, Chairman: Very good. Anything else?

Robert L. Crane (AFML): There is an area which you might consider as a problem area. That is related to the rejuvenation of alloys. Dipping or hot pressing an alloy may be used to close up a crack. The structure so treated will have to be requalified. The same thing is true for composites and bonded structures. The structure has to be requalified after the repair procedure.

Robb Thomson, Chairman: I have one specific comment in regard to methodology. If you are going to develop a fully probabilistic approach as was shown this morning, I don't understand the use or omission of safety factors. It seems to me that if you are doing a full scale treatment of reliability problems, I don't understand where a safety factor comes in.
Yes, Don?

Don Thompson (Science Center): I would like to understand how various materials with various material properties can be placed in the context of an unified methodological approach. More specifically, can ways be found to account for ductility differences, as was suggested earlier, and other features such as time dependent stress-strain characteristics?

Robb Thomson, Chairman: We now have a number of comments to address. Why don't we let Tony Evans take a crack at what we have got on the floor so far. Maybe that will generate some more.

Anthony G. Evans (Science Center): Well, I think it appropriate, perhaps, that I address the question that John Brinkman raised first and then the others as I remember them.

I think you are under a misapprehension, John. I think it is fairly straightforward as to what these probability expressions really mean. Everything that was described this morning did apply to full inspection, hundred percent inspection. It wasn't sampling. The reason that we need the a priori distribution comes when you realize that, upon making a measurement, you only have an interpreted defect size and you are trying to estimate what the actual defect size is from the errors associated with the width of that band. To do that you need the a priori distribution.

John Brinkman: No. What you need then is to characterize the particular instrument or technique you are using as to the manner in which its errors are distributed over the spectrum of sizes (or whatever other parameter it is that you are measuring). But that doesn't mean that you need the a priori knowledge of the actual defect size distribution. That is a lot less demanding a requirement.

Anthony G. Evans: Maybe John Richardson can help me on this. Let's consider just the probability of estimating the actual size from a measured size. You need the a priori distribution as well as other information.

John Brinkman: No.

Anthony G. Evans: We are obviously not communicating too well.

John Brinkman: What you have said we need is sufficient. But it is more than sufficient. It is not necessary. All that is necessary is a knowledge of what is measured and what is the probability distribution of defects that will give that measurement. That's right. That is a far less demanding requirement. If you have those two you have all the information you need.

John Richardson (Science Center): John, to get that you need the a priori distribution.

John Brinkman: No, you don't. This is what you use calibration standards for. You can get the latter set of functions from a series of calibration blocks.

John Richardson: No. Characterizing the instrument is not going to give you the defect size distribution, given the measurements, completely. You have to have more elements than that. That is a part of it. That is not all of it.

John Brinkman: If I have characterized my instrument so that I know how it errs, how frequently it errs, and in which direction and how far it errs, I have everything I need.

John Richardson: It is part of it.

Anthony G. Evans: It is clearly part of further debate. It would be better settled privately, I guess.

Robb Thomson, Chairman: I think so.

John Brinkman: May I ask a question?

Anthony G. Evans: I see about 6 hands up.

Charles Rau (Failure Analysis Associates): Is anybody else bothered by the same point? Because if they are, I think we should pursue it.

Robb Thomson, Chairman: I think we are.

Harris Marcus (University of Texas): Could I extend the question a step further?

Robb Thomson, Chairman: Yes.
Harris Marcus: The thing that bothered me about most of the discussion this morning is that you are looking for flaw sizes that will eventually become critical and that are detectable with some degree of reliability. Yet, the treatments go back to initiation as part of the statistical analysis. I don't know of any way to use NDT to characterize initiation state. So what you are doing is running the distribution out beyond the detection limit of any of the devices that are being discussed here; yet, NDT is being used as a baseline for this whole statistical analysis that you are talking about. I am confused by this and suggest that attention be given to measurement techniques for the microcrack regime.

Charles Rau: Different question, but we will address that one. I think it is important to get straight. It is key that we understand the need for both characterization of the inspection equipment, that is, its capability to detect a defect given that the defect of a certain size is there, and the a priori distribution.

John Brinkman: And the probability it has detected it wrong, quantitatively, by a given amount.

Charles Rau: Well, the key thing is, what does the instrument do given the fact that there is a flaw there of various sizes, what is the probability that instrument will detect that flaw.

John Brinkman: Not detect, but detect and measure.

Charles Rau: Detect and size, okay?

John Brinkman: Okay.

Charles Rau: That is only half the question though. If you want to know what distribution of defects really gets into service, you also have to know what preinspection defect distribution you started with which you then operated upon with your inspection tool, and the rejection that took place. Multiplication of those two functions gives you what gets into service. You do have to have the preinspection flaw distribution as well as the capabilities of your instrument. That is the point we are arguing. There are two distributions that you must have. This, in fact, is a key point which many inspection people over the years have failed to recognize. That is one of the reasons why we have had difficulties in getting to a quantitative NDE.

John Brinkman: There is a philosophical problem. The philosophical problem is that if we have to have an a priori knowledge before we can measure any distribution, we can never get there to begin with because we never have it. We have to measure one first.

Anthony G. Evans: No, we are talking about accumulating a knowledge of what a typical a priori distribution is, and then on a specific component, we are looking for whether that distribution is liable to cause failure. You need to characterize a batch -- a typical batch of material or a class of material in terms of typical a priori distributions. You can do that on sectioning a component or even inspecting components and inverting.

Robb Thomson, Chairman: I think there is a confusion between making the initial standardizing measurement and the measurement that the inspector has made. I think maybe that is the confusion.

Anthony G. Evans: That's where it lies.

Don Thompson: Tony, would you amplify upon the problem of obtaining the a priori distribution? These distributions must be obtained by a measurement of some kind, either destructive or non-destructive.

Anthony G. Evans: There are a couple of ways. Charlie mentioned one this morning and I mentioned another. The way we have done it is destructive. You take big billets of material and section through them metallographically. You can characterize the flaw size and the size distribution. We know from statistics that it is liable to fit an extreme value distribution and that helps us enormously because that gives us only three choices of distribution to fit it to. Then we can extrapolate it out to larger defect sizes to get into the range we need to for our prediction.

Don Thompson: How do you carry this procedure across to that product that comes off the line in which specific materials and especially specific procedures are used?

Anthony G. Evans: You have to take samples out of that product and characterize the a priori distribution on samples taken at random out of that product, probably on a continuing basis to make sure that the distribution isn't changing with time as the manufacturing process might change. So that's a rather laborious task and something that has to be done. You might have some other thoughts, Charlie.

Charles Rau: Here, in fact, is where the alternative technique of having identified what your instrumentation capabilities are may be used. You can then use just the nondestructive inspection results
Charles Rau: Well, even when you operate with the combined analysis, you are still using the NDE to make predictions about propagation on the sample you are going to operate on. That doesn't come across all that clearly; it is only with the NDE to give you any information on that? That's the only question I really asked.

Harris Marcus: How do you get the NDE to give you any information on that? That's the only question I really asked.

Charles Rau: Somewhere along the line you must have an independent measure of what you are dealing with, or else you have to have some statistical way of massaging your data iteratively and tying it back into the actual failure rates. Somehow you must get an independent measure of what you have, otherwise you just go round and round in circles.

John Brinkman: You calibrate your instruments.

Charles Rau: No, somewhere you must also have a back-in to what the preinspection material distribution was. You have to have it.

Let me try to address a couple of the other quick questions that were brought up, if it is appropriate now.

With regard to the safety factor, Robb, we are not talking about a safety factor in the conventional safety factor sense in which you take the maximum stress and divide it by the stress base safety factor. This is a life base safety factor which means you calculate a probable life. You say it is going to last 1020 hours. Now what fraction of that do you actually use as an inspection interval? That's the safety factor I'm talking about. It's not the conventional safety factor.

Robb Thomson, Chairman: It seems to me you should be able to calculate the best time for inspection without addressing a safety factor.

Charles Rau: You can. It is just semantics.

With regard to Don's question about the integration of a wide range of different materials into a single methodology, I don't really think that is a problem with the methodology. What will happen, of course, if you go to vastly different materials, you will find that various parameters in your total formulation will be dominant in one set of alloys or materials and others will be dominant in different materials. Obviously, in dealing with ceramics you are dealing with the inspection process as dominant. In certain metals it may be fatigue initiation that is important and the prior flaw distributions are relatively innocuous. The methodology doesn't change -- just the sensitivity to the specific parameters. If you get big scatter on certain parameters, they will be dominant in the total reliability of the product. I think the basic parameters are the same no matter what engineering system you are dealing with.

Let me answer Harris' question about the initiation.

If you use the conventional probabilistic fracture mechanics leading to a retirement for cause, that is strictly a propagation based analysis. In fact, initiation doesn't really enter into the process at all. Initiation enters into it when you start to utilize the combined analysis approach in which you are trying to make use of the success and failure data which you measure with your NDE in the parts which have already experienced some service. There, of course, you have to subtract out the initiation; otherwise you get a distribution of crack sizes at one lifetime in your whole fleet and if you sit there and assume that was all propagation, you'd be markedly in error because many of those sat there for 90 percent of that time before they initiated. So that has to be in the model. You can, if you like, take advantage of the total observation to make predictions about propagation on the sample you are going to operate on. That doesn't come across all that clearly; it is only when you go to taking advantage of the field data that you have to incorporate the initiation. If you are strictly going with probabilistic fracture mechanics you are not taking advantage of field data. You are right; it's all propagation.
Anthony G. Evans: I might say something about that. I think we have confined ourselves mostly to propagation controlled phenomena because the defects are usually large unless they are distinguishable separately using ultrasonic measurement. That's a regime in which most people in the audience certainly are interested. But you are quite right, of course. One has to extend one's nondestructive method into a region in which individual defects are no longer discernable and you have to measure some other property of the material which can then be related to initiation times. As you well know, ultrasonic attenuation and a host of other measurements have that potential and they need to be explored. We certainly shouldn't ignore it.

Fred Morris: It is part of it because we're picking the problems that current technology can deal with. It's just that initiation problems aren't being tackled as yet.

Charles Rau: I didn't mean to imply that that's the only way to go. I would like to make another point which I didn't have time to really make in my presentation this morning. I don't look at using the field data as a last resort because we can't do it any other way. In fact, quite the converse is true. I look at the field data at the present time in a more appropriate way to go if and when it's available, because that's the horse's mouth. It includes everything, if you like, within it. Let me give you an example. I went through an example this morning where I had a fatigue model which included both initiation and propagation. It contained the sixth power of stress dependence on initiation. Let's turn it around for propagation. We had a fourth power of dependence on the cyclic stress range and, say, a first power on the mean stress. We also ran that same analysis, assuming that the analyst was stupid and he confused fatigue with creep. Instead of having a fourth power in delta sigma and a first power in stress, he had it the other way around, a fourth power in mean stress and a first power in delta sigma. This yields a completely irrelevant engineering model, and via a conventional probabilistic fracture mechanics, you get all the wrong answers and make all the wrong decisions.

But with the combined analysis approach of using the field data to calibrate your system, we found we still got, believe it or not, a very substantial cost benefit from the retirement for cause approach, approaching a 30 percent cost savings. It was, of course, an 80 percent cost savings if you had had the right model. The point is, however, it wasn't zero and the reason, of course, is that by using the field data to pin your point even if you employ a bad engineering model, you can't be too far off. On the other hand, start from scratch with your lab data and you've missed out on the mechanism or something, you are extrapolating over much longer distances and have, therefore, a much higher chance for large errors.

Robb Thomson, Chairman: We have spent quite a long time with this first category. I think it was appropriate, but let me just ask the two people on their feet for a brief summary of where you feel the major road blocks are for the ultimate application of this approach to reliability for use in materials. Could you do that? Could each of you do that in a couple of minutes?

Anthony G. Evans: Give us a chance to think about it.

Robb Thomson, Chairman: Yes.

Jerry Tiemann (General Electric): While you're thinking, maybe I could ask about what seems to me to be a road block--I call it the chicken and the egg problem. Tony, this morning, you wanted to use the low frequency data to determine the defect volume. However it's the defect volume times the acoustic impedance mismatch that's actually determined at the low frequencies. On the other hand, the high frequency data can give you the acoustic impedance only if you know what the volume is. So there you have the problem of the chicken and the egg.

Anthony G. Evans: Well, that I can answer. I think it turns out that in the high frequency method I described, you don't need to know the volume. You just need the details of the impulse response time.

Jerry Tiemann: The amplitude of that depends on how much of it is there.

Anthony G. Evans: It is the sequence of the respective peaks and the impulse response function that is
Jerry Tiemann: Only if they are spheres. You don't know that when you come upon an arbitrary defect.

Anthony G. Evans: I showed some data, and I said we still have to prove some of these features for real shaped defects. It turned out, however, that the two or three we looked at so far, even though the shapes are quite irregular and by no means spherical, they still exhibit the same features you calculate for the sphere.

Jerry Tiemann: But only at a risk of increased uncertainty as to what the acoustical impedance is.

Anthony G. Evans: Sure, right.

Jerry Tiemann: So therefore you then don't know the acoustic impedance unless you know the shape.

Anthony G. Evans: It's not quite as black and white as you make out in the sense that one has a limited set of possibilities one knows can exist in the material. If there is an infinite set of possible defects in there, then I think the question you raise is a very serious problem. You know from a low of experience in working with materials, however, that there's a limited set of possibilities -- say six types of defects -- that you are liable to expect from the fabrication. When you have that limited set of possibilities, it appears, even though the defects are irregular in shape, the impulse response function retains enough of that shape, if you calculate for the sphere, that you can distinguish those six possibilities without even knowing its shape. Believe it or not, that's the way it seems to be working out. Then, of course, when you do have that in answer to the first part of the question, you do know the impedance. Then the volume comes out from a low frequency measurement.

Robb Thomson, Chairman: Charlie, since you didn't have to answer that question do you want to start with this other? Let's make it short. I would like to go on to the other topics up there.

Charles Rau: I have three things I jotted down. There is probably more, but three things which I think are limiting. First of all, overall, I don't think there are any insurmountable roadblocks. I think that we have the technology now and I am very excited about the combined analysis approach because it's gotten over some of what I felt were the major roadblocks with the conventional approach. However, there are two problems associated in applying it. One of the problems is that we really don't have the preinspection flaw distributions for most of the cases. In most cases I think, quite frankly, we still don't have the probability of detection, given the fact that a flaw of a given size A is there. We now appreciate that we need it under realistic field conditions. The last thing is just with the unknowns which crop up in the prediction of failure probability knowing that a crack of a given size "A" exists. These are related to such things as the loading, the guy doing something to it that he never should have done, something ridiculous tied in along with three or four other design problems. I still see a difficulty in quantifying those sorts of things from a limited amount of data.

Robb Thomson, Chairman: Thank you.

Anthony G. Evans: To some extent my concerns are the same. Certainly, I am concerned about the a priori distribution and we all agree that is a concern to get that with sufficient accuracy at low probabilities. Perhaps an overriding problem is to get all the accept/reject decisions very material specific, not just because of the a priori distribution but, perhaps, also because of the inspection and that means that it takes a lot of money to get the information you need for each particular system. Someone must recognize, I suppose, that if he wants to use inspection methods it is going to cost a lot of money at the outset to get the information that one needs at the statistical levels of confidence that are required. That is a concern that people will recognize. They must make the money available for that purpose.

Robb Thomson, Chairman: Okay. Thank you very much.

I suggest then that now we switch completely. Let's skip ceramics and metals unless there is an objection to that. It is 4:20 and I think we have at least had some implicit discussion of ceramics and metals in the discussion we have had so far. I suggest we jump down to bonds and to polymer materials. Unless there is an objection in particular, I would like to start that discussion by asking Dave Kaelble if he can relate the terms we have been hearing for the reliability description for ceramics and metals to the polymeric material. As I understood you this morning, I had difficulties bringing these two approaches together. I had difficulties understanding what were the primary parameters that we had to come to grips with in the polymeric situation. Maybe that would be a good place to start.

Dave Kaelble (Science Center): I believe the polymeric materials generically require somewhat more detailed descriptions in terms of major mechanism of failure. I think in running through the
statements for a mathematical criteria of Weible failure distributions, a generalized Weible distribution function is required which has in it a two parameter model for stress dominated failure. In other words, it says that failure in the range of very short time or low temperature will be a brittle criterion similar to that for ceramic failure. But in the condition where you go into ductile response, very often you have a strain limitation. Accessibility becomes a critical parameter as a failure criterion. One has then the state of strain over the mean strain characteristic of the Weible failure process. As we go further, there is a third condition. If one, in fact, has moved out in time under conditions of decreasing stress, one has a constant strain state. Including stress relaxation effects, one has failure by a time dominated flow process. The third criteria operates. These three effects are essentially independent of each other.

Robb Thomson, Chairman: I would like to focus on the question of overall reliability in terms that we have had it presented, i.e., defect distributions, measurements, and stress history.

Don Thompson: I'm not sure I know quite how to ask the question either, but let me ask Charlie (Rau) what happens if you put time dependent failure phenomena into the reliability formulas which you have developed.

Charles Rau: You get more complicated reliability formulas, but they work.

Don Thompson: Could you pinpoint in your methodology, Charlie, where that happens?

Charles Rau: It is in the engineering model. I suppose that if the defects extend without load, it would also tie into the preinspection defect distribution which may change with time. I haven't thought about it in detail however.

Dave Kaelble: I think I agree with Charlie. Most of these complexities I brought up this morning will be detailed in the engineering model. To relate the particular physical responses and degradations of the systems, of course, is a material science problem which I think needs to be worked in a sort of new interface between material science and design engineering.

Anthony G. Evans: I think I lost something, but I think in Dave's presentation he didn't specifically talk about individual defects and their detection non-destructively. In many cases, adhesive bonds and composites don't fail that way. They fail either due to a loss of load or compliance, or a linking of smaller defects related to Fred Morris' comment. We are therefore talking about completely different formulas and a completely different methodology in which we are not interested particularly in the a priori defect distribution. However, I think the generation of the reliability accept/reject model is relatively straightforward, but it is different than the ones Charlie and I described. I think if you worked it out, one stage says this is the way the thing breaks.

Dave Kaelble: I might just go one more step toward composite system response involving polymers. This step is a very simple system response model that contains the types of statements which were previously detailed for a single subphase. If one says that one has a system of phases, one can describe them in the conventional statement for reliability of a system response of several phases. You can also describe them in terms of the modulus failure in a series combination. The product of these become the reliability modulus. The same thing can be done in parallel and combined to produce a hybridized series parallel model. Now this has been developed without the reliability argument by Halpin as a composit analog model. It is very useful in early phases, i.e., before you come to a discrete laminate theory. One can do a lot of design optimization within the scope of such a model. That's the kind of picture I have, that if one were working toward a discrete design for composites, let's say fiber reinforced composites, one would use that kind of a model as a prelude to an exact finite element model with discrete mechanisms for probabilistic failure built within it.

Steven Wallace (Union Carbide): I do experimental work in measuring adhesive bonds. One of the things that bothers me is that there are no standards. It would appear that, with many organizations being interested in this field, that the Air Force Materials Lab would want to have standards. It appears that, if you don't have these standards, that it is every man on his own. I don't know how many other people feel this is a problem, but standards have always been a real problem with us in bond work.

Robert L. Crane: NASA did some work in the area of standards. No one was particularly interested. A principal problem was that the materials change. For example if I want to buy aluminum 7075, I can call Alcoa or call any place I want and they are going to give me 7075 within a very narrow range or composition, within a very narrow range of mechanical properties. But if I want to buy an adhesive, any adhesive, I can't guarantee that what I have today is what I am going to have next week. One organization who has done more work in this area than anybody else has been McDonald-Douglas, St. Louis on the F-15 program. It was many many years before they could standardize the adhesive for the bonding of the boron epoxy composites for the vertical-horizontal stabilizer to
the metal substructure. They got into lots of legal problems when they began to take apart the epoxies and characterize exactly what they were. The supplier was very cautious because it was all protected by proprietary rights. They don't patent it, and it is all trade secrets. It changes dramatically from day to day and that's the reason why the standards didn't make any sense. Things change so rapidly that I couldn't depend upon my standard.

Dave Kaelble: The bond is a composite. You have the independent responses of polymeric subphases. One finds it if you examine it. It is being done now, lot-to-lot variations in polymeric material inputs. There is an area of large variability in that alone.

Robb Thomson, Chairman: Any further discussion of failure modes in composites?

Fred Morris: A typical failure mode of a graphite fiber epoxy component in fatigue is to have crazing of the epoxy followed by a delamination, a cracking which proceeds from laminar defects in the layup planes subsequently followed by failure of the matrix in the piles that don't have fibers in the direction of the stress. Now, if you likened the crazing to an initiation process in metals, I think it is fair to say we know very little about how that proceeds and couples into crack development from the defects. It should be pointed out that these defects are often gross compared to what we are looking at in metals. It's not uncommon to see defects of the size of a centimeter across or more. In many cases the time to initiation can be practically zero and we are strictly in a propagation based failure situation where the main challenge is to be able to find the defects amid background clutter that is always present from the non-uniformities of the composite material. Much has yet to be learned about the coupling between the crazing aspects of the initiation process, the subsequent development of a crack at a defect site and a plane of delamination, and the composite material parameters as affected by the moisture content of the material.

Robb Thomson, Chairman: May I ask you a question? Does this mean that you feel at this stage the most crucial thing is the understanding of the physical processes of failure modes or is it in some other aspects, for example, development of instrumentation for the detection of defects?

Fred Morris: I think we know very little about failure modes in composite as compared to what we know about metallic failures. I think we are easily 5 years behind in understanding the details of failure modes. Of course, I have been lobbying heavily for looking more closely at the initiation failure mode aspects in metals. That hasn't been touched in composites. Propagation requirements in composites also need further work.

Robb Thomson, Chairman: Any other opinions on this point?

David S. Dean (P.E.R.M.E.): I would like to back up what has been said. We have recently been testing carbon fiber rocket motor cases and it is extremely difficult to find anybody who can tell you exactly what is required of the testing equipment. We have subjected these to a whole series of tests: ultrasonic, x-ray and acoustic emission tests and to date, I don't think we are very near to being able to predict which cases are really going to fail in service and which are not.