GENERAL SESSION VI

PANEL DISCUSSION AND SUMMARY OF THE CONFERENCE

Chairman : P. M. Belcher

PANEL DISCUSSION — Moderator : P. W. Smith, Jr.

Panel Members : I. Dyer
G. L. Getline
A. A. Regier
E. J. Richards
T. J. Schultz
J. Wagner

SUMMARY — Alan Powell

Mr. Regier, NASA: I think that Prof. Richards very elegantly summarized the whole field of endeavor in the opening address in a slide which outlined all of these various steps and the inputs in this problem of fatigue. I think that they are all essential aspects of the problem. However, I think that there is a question of practicality in pursuing these various steps in the course of deriving a set of factory designs. As my division chief often reminds us, we have to attack problems on many fronts. One front that has not been mentioned this morning is the systemization of our test results so that they are presented in some nondimensional form that can be used in the preliminary design and not for retro-fits only.

I would like to draw an example from our experience in flutter. In flutter work our experiments have been numerous; and we have found it quite useful to put the data from the model test for a particular configuration into nondimensional form by using the Strouhal number. We plot results against Mach number. We find that if we multiply this nondimensional parameter by another nondimensional parameter, the mass ratio $\underline{\bar{u}}$ (which is the ratio of the mass of the wing to the mass of the air surrounding the wing) that the product is a significant quantity and that the combination represents the balance of the air forces against the structural forces. I shall qualify this example by saying that any criterion or any kind of significant parameter is only significant in certain ranges of values. There are other kinds of flutter, in other ranges of the variables, where the damping is the prime consideration. There are still others where a frequency ratio is important. Any time that parameters or criteria are used the restrictions on them must be kept in mind.

How is this method of plotting data used? Suppose that theories and our many experiments tend to define a curve for the onset of flutter, dividing the range of values for the parameters into safe and unsafe regions. The structure determines the Strouhal parameter; the flight envelope determines the density ratio and the Mach number. Suppose a designer has a wing in preliminary design and comes to us for advice. If we have a curve of this kind that fits the geometry and range of parameters applicable to his aircraft, we are able to give him some sort of answer. In many cases we can either reassure him that he does not have a flutter problem or advise him to start redesigning immediately; the point definitely lies in either the safe or the unsafe region of our graph. In some cases the design will be too close to the criterion to trust the conclusion. In any case, this kind of presentation of available data gives him a preliminary design estimate of whether or not he is likely to have trouble and of what action to take.

The point I wish to make is that, with the help of theoretical analyses and various parametric studies, we should try to find the significant parameters and use them in plotting our data so that future designers may have some basis for preliminary design.

(Written question submitted by Prof. H. T. Corten, University of Illinois, "for the acousticians"): The problem has been stated: how do we prevent fatigue fracture of structures; how do we lengthen the life of a member to the point where it can perform the intended mission. There is a long-range goal, and many discussions were intended to extend our understanding of the general problem, including the sources, the load, and the response of the structure. It is evident that these are complex problems and are currently not well in hand. With a few exceptions these topics have been treated without consideration of the factors that are important in fatigue. Can one of the acousticians on the panel state the important fatique parameters? How shall we marry the loading and response to the fatigue problem? It has not been accomplished extensively and I do not sense any extensive interest in doing it. Has this conference shaken your confidence in rms stresses? They do not have any meaning for fatigue failure.

Moderator: In the last statement I think he means ordinary fatigue testing. There are a large number of questions in this one. Does any acoustician on the panel want to start on a part of it?

Prof. Richards, University of Southampton: May I try to answer one or two of the questions that have been put to us. First of all, I personally think that the fatigue side of the problem is more amenable to treatment in acoustic fatigue than it is in the gust or other structural fatigue cases, because the questions of initial loading and of major variations of stresses with time, and so forth, do not really enter. I think that the likelihood of the sound-induced stresses being random throughout the whole of the life, with not very large ones in any one stage, is such that the problem is quite different. What is needed is an indication of whether the cumulative damage laws work for low stress levels, the sort of stress levels that are pertinent. I think that there is a much better chance of satisfaction in this regard than in any of the other uncertain items, particularly as we are never going to know the stress levels to better than about thirty or forty percent by any means at all, other than by testing the structure after it has broken or after it has been built and you have finished with it. Therefore I do not think that the fatigue aspect is the biggest problem.

I think the really big question that we have been talking about is whether we understand what is happening. I would suggest that we are not even trying to understand what is happening on our structures. I think that this is the biggest snag of all. There is an enormous opportunity to gain understanding of what is happening on the structures now that you have built them, now that you have your airplanes.

When I was working with Boeing two and a half years ago, I was very fascinated with the things they were claiming they could do, because after all these are the important things in airplane selling and designing. They were saying: We have a know-how in aerodynamics; we have a know-how in the shape of aircraft; we are the only firm that has, in fact, really understood the aerodynamics of the swept-back, high aspect ratio aircraft. Douglas, in turn, was saying: We have a know-how in airliners; we know what the operator wants; we have been building almost all of the airliners, and this is our know-how. Now I think that in some years to come, and not very many, the know-how that will be important is understanding of the dynamics of an airplane, to the point where one can be proud of this understanding of an aircraft as a dynamic body, as something which is alive and not just static.

I think that one of the outcomes of this conference has been to emphasize the possibility of doing just this. For instance, instead of only doing fatigue tests to see how many bits fall off of your airplane in five hours, you should also do some strain-gaging, trying to understand what is happening to that structure, where it is vibrating, what are the modes. (There is a fair amount of evidence that no vast numbers of modes are important on various parts of the airplanes.) I think that out of this understanding would come a theoretical approach of the type that we have been asking for. This is the big step that is to be taken.

It is true to say that at the moment there is no linkage between the various steps of analysis. So what? There was no linkage on the flutter problem when Regier started his existence. Look at it now; he has criteria and all sorts of things. I was most impressed by the Lockheed work that was described last night.* I was not sure of the details, but I did feel that it was a good step. Let's hope that when they write this up, they don't just write a dreary report which hides all of the important things. Why don't they call us all for a one-day conference to describe their results when they are half-way through their experiment, when we can really talk about it and see what can be got out of this type of test?

In answer to Prof. Corten, I think that the important problem re-

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lates to damage laws, whether Minor's or some similar law will work for low stress levels. The other fundamental factor is understanding of the structures.

Dr. Dyer, Bolt, Beranek and Newman, Inc.: I think that one of the fortunate things about acoustical fatigue is that we almost always have a feeling for the amplitude distribution in a signal. Almost always we can say that it does make practical sense to think in terms of the rms level. On the other hand, the researcher trying to understand what is going on in a material will, of course, want to be more flexible and he may very well have to know the actual stress levels. Acousticians find that it is convenient, and by habit easy, to measure rms stress. So far, I have not been convinced that it is not appropriate.

Dr. Smith: I would like to expand on that. It is not merely a matter of the ease of measurement of rms stress; we can actually justify its use, I think. If the shape of the curve giving probability distribution of stress peaks is more or less the same whatever the amplitude of the exciting forces, then it makes sense to correlate fatigue life with the rms stress induced by some random excitation. (One would have to use the stress and not the pressure.) For a given probability distribution of peak amplitudes you would get some one curve; for a different distribution, there would be another curve. If our assumptions in acoustical fatigue are valid, we stay near one of these curves. However, the structural fatigue man usually has an rms stress which is nearly constant but has one or two extra big loads which very significantly change the shape of the distribution curve and thereby the life. Therefore, with the probability distribution he assumes, the problem is quite different.

Mr. Getline, Convair: I would like to take the other side. The last two speakers do not have to provide numbers for design groups; I think this is the major line of demarcation between research and industry. The acoustician, like a mathematician, will assume a mathematical model which is very convenient for him to use; I think we have seen this repeatedly in the past few days. He is happy if he can make his arithmetic simple and come out with very nice relationships in terms of power spectral density, if you will, or any other number that he cares to choose. Somewhere along the line, he loses sight of the fact that he is not just playing with numbers or symbols, he has to deliver these numbers to people who are going to translate them into hardware. For this reason I object to the use of rms stresses, particularly when people assume to start off with that they know the distribution of the peak stresses in your structure, and go off in all directions at once and come up with design numbers.

Frankly, if structures groups in various aircraft firms are anything like ours, I would not give them an rms stress to work to. In general the world ends with $s = \frac{mc}{I}$. We have a structures research group who have a suspicion that there may be something beyond that. Until we got into this commercial jet program, the firm itself really had no extensive experience with fatigue, even though we had some fatigue people. They had a few cracks on the 340 series airplanes. There was nothing particularly astounding about them; they beefed the airplanes up and they disappeared. There were no problems in the fighter program where the noise came out the back end. Many people were completely unfamiliar with the problem. When we got into the commercial program, a big selling job was required to convince management that you could not scale up the 340 or the 440 to make the 880 airplane.

Our design philosophy, if you can call it a philosophy, is the philosophy of stretch engineering. That is, let our structures group design the structures to the load to which they are customarily designed. Then we will demonstrate to them that the structures are not adequate on the basis of criteria which we have agreed, among ourselves, will be used. Once we have demonstrated that the structures are not much good under these acoustic loadings then we "stretch" them up to the point where we feel they will be adequate. Once we have gone through the stress group and they have approved the initial structures, they are no longer concerned and we merely work with the design people. (We make sure we do nothing to the structures which will raise the stress levels.) Our criteria make use of the peak stresses and the frequency of occurrences which we expect over a lifetime chosen by our service engineering people.

Dr. Dyer: I think you are absolutely right. I want to bring out the point that when the acoustician says he would like to talk of rms stress he must also have, with that, knowledge of the amplitude distribution. I think we are in agreement that one eventually has to think in terms of the peak levels or of the amplitude distribution and the rms level; the question is how best to present the information. I think your point is that your people are most likely to accept it in terms of peak levels, and perhaps we are most likely to use rms values. I think that the total information would be the same in the two cases.

Comment. Dr. Morrow, Space Technology Laboratories: I also wanted to make a few comments to indicate that I think these points of view are compatible and, I hope, to clarify them a little bit. In the first place, the acoustician is interested in measuring certain things, in the way of sound pressures on the outside of the airframe, from which some significant characteristics of the response of the airframe can be computed and, perhaps, related to fatigue life and other things. There are some good reasons to believe that the sound pressures on the outside of the airframe, when the sound level is high, will not have exactly a Gaussian distribution. However, if the vibration of the airframe itself deviates from the Gaussian distribution it does not necessarily deviate in the same way. There are filtering actions and some nonlinear effects that take place in transit, so that there is a good chance that the distribution of levels in the response of the airframe will tend to be somewhat independent of the original distribution. Furthermore, it is not feasible at the present time, and probably won't be for some time, to calculate the distribution of stresses in the airframe in a very direct manner from the knowledge of the airplane dynamics and any information you can get by measurements on the outside. This is just too difficult a statistical problem. If we are going to start from acoustical pressure measurements, we probably have to content ourselves with rms responses. Admittedly, once you get to the place where the failure occurs, the distribution will not be Gaussian, if the systems are nonlinear; particularly if one is vibrating far into the fatigue region, there will be definite and important deviations from the Gaussian distribution. I think one can probably get a little feeling for this by suitable measurements at these points; there will probably be some correlation between the ratio of the rms level to certain characteristics of the material. One could probably make some estimates of the deviation from the Gaussian distribution, after a bit of experience. The point is that you cannot get these things by any further refinement of measurements and analysis of the acoustic process outside the structure.

Question. Dr. Kerwin, Bolt, Beranek and Newman, Inc.: Dr. Von Gierke showed us that the environment in which he placed his random siren affected the distribution curve of the signal and that the peak-to-rms ratio was modified by the reverberant nature of the room. Isn't it possible that the structure can also do this to the distribution of an excitation? Shouldn't one therefore be quite careful, in line with what Prof. Richards said, about trying to observe the effect of the structure on the distribution as well as the distribution before the structure is excited?

Prof. Richards: In a reverberant room the sound is coming from all directions and consequently a certain rms value of the pressure won't always excite the structure in the same way; the pressures won't be correlated in anything like the same manner as if it were a free field or a sound field very near to a structure. So I think that testing in reverberant rooms--putting the structure there and just measuring the amplitude of the pressure and considering that to be representative--is really quite wrong. I think it can yield optimistic results; the response will not be the same. To some degree, placing the panel on its own without any reflecting plates around it as there would be in the airplane, is also wrong. But I do feel that the important thing is to really find out what the structure is doing.

Comment. Dr. Schelderup, National Engineering Science Co.: I would like to address my comments to both Prof. Richards and Mr. Getline. Apparently the Convair Company has a poor structures department who do not understand rms. At the time I was at Douglas we were not afraid of rms in our structures department; in fact, we in structures handled the fatigue problem rather than some other department. From that standpoint, I think the audience should understand you were talking for Convair.

With reference to testing in the free field or in test cells, last year in Chicago I gave a paper on proof-testing. I followed up the earlier test with some additional studies. One thing that we did notice was this: Even though there was absolutely no correlation between the structural response and the measured noise level, the characteristics of the structural response were very similar in spite of the differences in the noise levels. From my brute-force peak-distribution analysis, we could not observe a deviation from the Rayleigh distribution of peaks in our comparisons of the response in the test cell and on the airplane.

Prof. Richards: I think that is true. The structure does respond similarly in the two cases in the sense that it responds to sounds with the various natural frequencies of the structure. However, we found that, as you move the panels forward and aft, there is a tendency for them to pick up the different harmonics rather differently. That is really the point.

I think that the structure will in fact vibrate in a relatively simple manner, but in many instances the structure is not going in the way one likes to think, as simple panels or anything of that sort. The frames may be moving in unusual ways, and so forth. Once you know how it is going, once you get that know-how, I think that designing is then a matter of good sense.

I was talking to Gordon Getline last night and he said that he doesn't give a stress figure. What does he give? The drawings come to him and he then takes a good look at them; he says: That's not a very good detail; you should get rid of the stress concentration here, and put in a doubler there. Now this is the point. If you knew how your structure was going, how it was responding, I still don't think you could calculate the rms or peak levels or anything of that sort; but you jolly well would know the things which are important to strengthen. Moreover you would know something else which I would guess very few people here know about: how to put your damping on. One of the main reasons why damping has been poorly accepted is that you just slap the stuff on, not knowing anything about how the structure is moving; eighty percent of it is just wasted.

Dr. Smith, Bolt, Beranek and Newman, Inc.: Prof. Richards has just made reference to damping. A number of written questions were submitted inquiring about the efficacy of damping treatments in relation to the acoustical fatigue problem. Will you, Dr. Schultz, introduce our discussion of damping.

Dr. Schultz, Douglas Aircraft: What I had in mind doing was forming a sort of reconciliation between the view that Dr. Plunkett advanced yesterday afternoon and that which our moderator, Mr. Belcher, was putting forward. The question was: to dampen a structure or to beef it up.

I would like to approach this question with a simple discussion of what happens, or might happen, for a simple oscillator. Let us say that we have a structure which resonates at a certain frequency, and discuss what happens when you add thickness to the skin. Generally speaking you can say that the stress per unit force would be decreased, and the resonance frequency would be increased. If the damping remained unchanged, the velocity of motion of the panel would be unchanged; however, since the frequency has gone up, the amplitude has decreased, thereby leading to the decrease in the stress. To a man who is interested in acoustic fatigue, this might be a very reasonable approach. However, Dr. Plunkett is trying to avoid radiation to the far field; he must consider, not the amplitude, but either the velocity or, I believe, the acceleration in the case of a finite body. In the previous example, since the frequency is increased, the acceleration of course increases too; this would not be the way for him to proceed.

On the other hand, if you approach the question by adding damping to the original structure, the addition of the damping treatment may either lower or raise the frequency. I shall assume first that it lowers the frequency. The velocity in response to a given forcing function decreases because of the damping. However, there is a compensating effect if you are interested in amplitude: Lowering the resonant frequency tends, for the same velocity, to raise the amplitude so that the net reduction in amplitude due to the damping treatment is less than the reduction in velocity. If, on the other hand, as Dr. Mead has found, the addition of damping stiffens the structure and raises the frequency, then both effects add in the same direction and the reduction in amplitude will be larger than the reduction in velocity.

Now in this discussion we assumed a simple oscillator, and I am sure that the question is much more subtle than that. I invite Dr. Mead to comment.

Comment. Mr. Mead, University of Southampton: I look at this from the structural engineer's point of view and I will just reiterate what I said yesterday afternoon when I was talking about the inertia forces that act on the structure around the panel. These inertia forces are probably the forces producing the stresses which cause the fatigue, so that we can look upon the fatigue stresses as being proportional to the inertia forces on the panel. Here it is important to recognize that, in a random system, the amplitudes of the inertia force or of the displacement are not dependent only on the damping of the system; the stiffness of the system is important as well. It is the product of damping and stiffness which determines the rms displacement of the system. In the case of the inertia force it is a bit more complicated still; it is the ratio of the natural frequency of the system to the damping ratio that is important. An increase in the stiffness raises the natural frequency, with the possibility of raising the inertia forces. An increase in the damping decreases the inertia forces, so you have a battle between the two tendencies.

Dr. Schultz: Is it fair to say that an argument based on the effectiveness of a damping treatment in a case concerned with acceleration or velocity need not necessarily influence the man whose concern is mainly with amplitude? The argument which Dr. Plunkett advanced was that they got a very good effect by application of a damping treatment. Do you feel this necessarily constitutes a strong argument to influence the acoustic fatigue people, whose concern is more with amplitude or with stress? I don't.

Prof. Richards: I was going to ask if we could have an example of what can be done with damping, because there is rather a feeling around that damping doesn't do a thing. How much reduction in the rms stress in a panel, vibrating in a panel mode, might result from an application of Aquaplas which stiffens in the optimum fashion?

Comment. Mr. Mead: I shall quote from memory some figures that I calculated for a simple panel. I think that we added Aquaplas to the extent of about one or two times the panel thickness. Taking some reasonable sort of values for initial damping, we found the actual panel stress in the center of the panel was reduced to about ten percent of its original value. The inertia forces, and consequently the loads upon the rivets on the panel's edge, would be reduced by about sixty or seventy percent.

However, may I emphasize what Prof. Richards said a moment ago about putting the damping material in the right place. I think many of the fatigue failures that occur in airplanes are due to the overall distortion of the fuselage cross section and not just to the vibration of the panels. If these overall modes of the cross section are excited, then there is no use in putting damping tape on the panel; you just get a mass-law type of transmission loss, which is the experience of many people in this country. However, if you put a damping material on the frames and stringers, which in this case are distorting considerably, considerable attenuation may result. We can quote here from practical experience the fact that Aquaplas material has been used in our country on the frame flanges and stringer flanges of a large bomber with a very marked effect on the fatigue life. This was in the nature of a fix. A remarkable increase in the fatigue life of the structure resulted from putting the stuff in the right place.

This is the point I wish to emphasize here. If you have panel modes, then it is all right to put material on the panels; but it might well be that you have the overall distortional modes of the fuselage, and then you have to put the damping in a different place.

Dr. Smith: I think that, to sharpen up Dr. Schultz's point, I will say that he was trying to point out that there are two entirely different problems, minimizing the sound radiation to a distance from a structure, and minimizing the stresses in a structure itself. Or, to put it another way, an airplane isn't a submarine. Does Dr. Plunkett want to comment on that?

Comment. Dr. Plunkett, General Electric Co.: I am afraid that the major problem here is that I have expressed myself in my usually obscure fashion. I was not at all trying to say that, because damping may be successfully used in submarines to cut down on noise transmission, therefore it will increase the fatigue life in aircraft. I was trying to say that I don't think the resistance of your manufacturing people is a good excuse for not using damping. I was trying to point out that we had exactly the same problem in submarines and we were able to overcome it. I was bothered by the attitude: We have tried damping; it is messy stuff to use; it hurts production; our production people do not know how to handle it; we don't quite understand it anyway; therefore damping is no good. In our work with submarines, automobiles, steam turbines, and other things, we have also used damping improperly and damping has also got a very bad name. My major point is that damping can be harmful if it is improperly applied and this is the point that Prof. Richards was making. It is a question of proper design. As far as I can see the only way you can use damping properly is by thoroughly understanding the dynamics of your structure. If you don't, I think you had better stay away from damping, because you are at least as apt to do harm as to do good. But if you can make a thorough analysis of a problem, investigating the amount of damping to be used, then I think it may do you some good.

There is one further point. I have a suspicion that probably the least efficient use of damping for panel work is a spray coat treatment, like a plain coating of Aquaplas. I think you have to use some kind of a septum treatment or sandwich construction, or something like that, if you are going to get full value out of it.

Dr. Schultz: I think I agree with Dr. Plunkett almost completely. However, apparently he has sufficient knowledge of the behavior of a submarine to constitute a strong weapon in his arguments for damping. I think we in the aircraft field are in the position of admitting, by our presence here, that we don't have such a weapon.

Comment. Mr. Belcher, North American Aviation: I think an obvious and correct conclusion of Dr. Schultz' comments is this: When his airplane needs damping for sound transmission, that airplane will have damping material on it. In the aircraft company there is not the resistance to using something new that you might have sensed from earlier remarks. A new treatment must be worthwhile, it must be dependable, it has to be practical, and it cannot cost too much money.

Dr. Schultz: I have a comment to make to Dr. Mead's statement about vibration in the gross modes of the airplane, with which I completely agree. I would like to point out that Douglas, in the DC-6 and -7 series aircraft, mounted the partitions in slip rings, of a sort, which were filled with silicone grease. This treatment in effect does add very effective damping for these large gross modes.

Comment. Prof. Lazan, University of Minnesota: I don't think that it is necessary to choose between adding stiffness and adding damping, that one must decide between these two, based on weight penalty and so forth. The last comment would indicate that you need not make this choice. If you consider only the surface type of damping treatments--something you add to a surface later, to cure a problem--then perhaps we have the balancing of the decreased stress against increased weight and cost. But if you consider all the various means for achieving damping, then you want to add both stiffness and damping and not just stiffness. I think that in this context the price paid in weight for damping is greatly reduced.

Prof. Richards: In our own investigations we have found Aquaplas to be the most efficient treatment for the panel modes, though not when it is applied over the whole of the panel. We found it better than damping tape. If there are firms or people who have got more efficient ways of doing it, we certainly would like to hear about them and try them out on engineering applications.

Comment. Dr. Von Gierke, WADC: I would like to mention just for completeness that the choice is not only between adding some weight in stiffness or damping. The third possibility is to have the weight in the form of noise suppressors. I think that in some applications on some aircraft it has already been decided that the best use of weight is in noise suppressors that reduce the noise level.

Comment. Mr. Karnesky, Aerojet (formerly of Douglas Aircraft Co.): To answer your question, Prof. Richards, we found a very convenient and very simple way of adding damping. Most panels are very lowly damped, in the order of one-tenth percent of critical damping. If you can get to two percent or one percent you have gone as far as you can, or as far as you want. We found that simply the addition of visco-elastic materials along the boundary, where the stringer and panel meet, resulted in as much damping, for a very minimum weight, as does spreading an entire panel with gunk. We went through all sorts of experiments with Aquaplas and other such materials; if they are placed in the center of the panel, you are wasting your time. We were able to get two percent of critical damping by merely putting EC-801, and other materials like that, along the boundary where the skin and the stringer meet. The damping material works very efficiently there.

Comment. Mr. Fuller, Boeing Airplane Co.: I think that there has been too much emphasis on thinking in one degree of freedom. If you just consider the generalized equation of motion, you can't add damping of any kind without changing the other two factors, the generalized mass and the generalized stiffness. While you might fix the problem for one mode of vibration, you may shift to another mode. In other words, the application of damping tape around the edges may cause failure to occur in a different mode. I think the problem is not that of a one degree of freedom system but is more complex. In other words you may move from skin failures to stringer or frame failures.

Dr. Smith: We must at this time call an end to our discussion of damping. To summarize, I might say that we have brought out the need for a careful definition of the desired goals and a careful investigation of the nature of the vibration and the effects on that of damping treatments. We pass on to a new topic with this written question:

Question: Mr. Baskin, NAA, Columbus: Would the gentlemen of the panel discuss the relative merits of the discrete frequency siren and the random siren as means for getting predictions of the sonic fatigue life of structures?

Dr. Dyer: It is important to recognize the various kinds of tests and their different objectives. I can think of at least three types of testing. There is the research type of test where you might be interested in studying the kind of idealized panel for which Getline has no patience. Here you might want to study in detail and in a very idealized way the effect of amplitude distribution on failure, the nonlinearity of panels, the effect of having two modes excited simultaneously, and so forth. One ordinarily thinks of such a test as belonging in the university or the academic laboratory. Another type is the development test where you compare several designs. You want to find out which is going to last the longer time without saying much about its life. Finally there is the proof type of test where you must find out how long a given practical structure will last, or where the failures will occur.

It seems to me that we must be very careful to distinguish between these objectives. Whereas a pure tone source might be quite adequate for a research or a development test, it may not be at all adequate for a proof test. At least according to my present knowledge I would not tend to use a pure tone siren alone for a proof test; I do not think I know enough about the problem to do so. On the other hand I would use a pure tone siren for certain phases of either a development or a research test.

Mr. Regier: I would like to add a comment on this question. At Langley we have both a siren and a jet whose sound we hope is fairly random. Of course the jet is very much more expensive to operate than the siren. In our experience we can use the siren for exploring, say, the S-N curve of a panel or a structure if we know the dominant vibration modes that would lead to failure. However, if someone brings us a black box (we may not even know what is inside it) and we have no idea of what the failure range will be, then we would not consider the siren at all. The siren has to be run for quite awhile at a particular frequency to develop failure, so that we find the random source a necessity in those cases where we have not defined the failure mode.

Mr. Getline: Today, with what we know and don't know about the interaction of sound field and structures, about accumulated damage laws, etc., no matter what type of test procedure you use you are going to make a lot of initial assumptions which may or may not stand up in the long run.

Prof. Richards: I would like to say that none of these devices gets very near to the truth. The more you know about the structure and how it vibrates then the easier the facility you can set up in order to test it. For instance, if you know that your structure is moving in a fairly simple way, then you do not need a siren; you can just shake it mechanically and still get the same stress levels and fatigue failures. The danger lies in thinking that you have a facility that is going to give you the answer. I would rather look at all of these devices as means of developing general knowledge on how things happen. The wind tunneler does not just think in terms of giving figures for loads on airplanes; he gets the feel of the type of things that are good or bad on the airplane. Then when he becomes a designer or is involved in design he knows the type of thing to do. I would say that all of these facilities are good providing that you use them properly. I would like to emphasize the "using them properly" rather than which one you have. Any one of them can be really very useful indeed; equally it can be the most misleading thing that you ever had. So use them properly and don't worry very much about what you have.

Dr. Schultz: In line with Prof. Richards' first comment, I think it is true that in all of the design groups who are designing structure to withstand random loading, and also are doing the development by means perhaps of proof testing with single tone sirens, there is hidden in the design procedure an implicit notion of some equivalence between the two types of testing; this notion is almost never made very clear. As we learn more of the behavior of the structure in response to random loading, we can make this equivalence more explicit. Developing the explicit equivalence between two situations which lead to the same time of failure, let us say, may require theoretical analysis or simply the acquisition of a large mass of experimental data, or perhaps the use of both approaches in conjunction.

Dr. Dyer: Another point which I do not think has been adequately aired in this conference is the limitation inherent in using true sound in our present acoustical fatigue testing. As we have discussed in the last two days, there are very many environments that are not sound, for example, boundary layer noise or the environment of a missile in a silo. In such a problem we would have to face even more stringently the choice between pure tone and random signals. I do not have the answer to this but I would like to put it out as a question.

Comment. Dr. Morrow, Space Technology Laboratories: I just wanted to add one further comment to several that have been quite excellent and pertinent to this subject. If you are going to use a siren for proof testing an airplane structure, it is quite necessary to know something about the dynamics of the structure, partly because, as has been pointed out, otherwise you do not know what frequency to use, and also because the interpretation of results in terms of a random field is very much dependent on measuring or knowing the response at the point where the failure takes place. That is, if you are trying to relate the two in terms of an equivalence back in the sound field, you have a very difficult job. On the other hand, if you can estimate or measure just what the response is where the failure takes place, you narrow the problem down to the properties of the material on the immediate structure. It is more nearly possible, with a little experience, to estimate equivalence for these more limited problems.

Prof. Richards: Dr. Von Gierke said one of the most important things this morning in noting that anything that can be done at the source is worth far more than anything that is done by the structural man once the source level is very high. I think that this is something that should be said. On the Valiant, for instance, the use of a convergent-divergent nozzle pushed the noise source downstream and reduced the pressures on the fuselage (which is the part that was going) to some half. It did not reduce the noise on the ground, and this brings me to the point. Practically all the nozzle work that has been done in this country, other than the very interesting work that we heard from Sutherland of Boeing, has really been aimed at reducing the noise levels on the ground. Reducing the pressure levels in a very close field is a different problem. The pressure levels in the very close field are related to the mixing of the jet and the turbulence. If there are going to be investigations, it would be well worthwhile to try to see how to reduce the pressure levels by modifying the jet in ways that may not affect the radiated sound but can really affect the pressure fluctuations on the structure. Dr. Von Gierke said that if you used a rocket engine you would push the noise down to from twenty or thirty diameters. Well, that pushes it right out of the airplane area altogether. Maybe we will see some rocket engines fitted to the 707.

Comment. Dr. Von Gierke: I would like to comment on Prof. Richards' remark that all work on suppressors in this country was related only to reduction of the noise in the far field. This is not completely correct. Quite some effort was expended by Boeing to develop a small noise suppressor or extended tailpipe for the B-52 to reduce noise levels at the trailing edge of the wing. I think this noise suppressor is used now and was very successful. It had no effect whatsoever on the noise in the far field but it really helped the situation on the surface of the B-52.

Mr. Getline: We looked at that on the 880 aircraft. I assume that we have reasonable noise suppression in the far field. In making a survey around the near field in the region of the wing, we found rather peculiar results: that we might get a tremendous reduction in pressure in one spot but we get just the reverse at a point six inches away. The results were so spotty that we didn't dare try to take advantage of any general pressure reduction.

Dr. Schultz: I would say that on the DC-8 we found almost exactly the opposite. We had a different suppressor, of course. The near field was reduced by a few decibels more than the far field.

Comment. Dr. Kerwin: I would like to make one plea from a very personal sort of thing. I am trying desperately these days to discover the pertinent design parameters in specifying a damping treatment. It doesn't matter what kind, let us just call it viscoelastic. Since we are all heavily influenced by the remarks of those who are making contributions in studying fatigue, I would like to ask for caution in making such remarks as, for example, that we have looked at free viscoelastic layers and constrained viscoelastic layers (to recast your remarks, Prof. Richards) and have found that the free one is by far better. I would rather see you say that you have looked at what can be bought in the store today, and compared those two products in a certain way. We really don't know which one is better or how they should be used. There are a number of us trying desperately to find out, but it is damaging from our standpoint to have the pins pulled out from under the field with the result that nobody in the hardware business is interested.

Prof. Richards: I had not realized that I had implied that one was very much better than the other. If you remember, I said that I heard that there were firms in this country that were making other things; we would very much like to get hold of them because we are quite sure that we have only fiddled with the problem so far.

Dr. Smith: I would like to make a comment on this problem. I do not think you can talk about a specific material. Let's consider what the problem is. First let us make the assumption that fatigue is a function of the stress and you want to minimize it. What is the excitation? As far as the panel is concerned the excitation may even change markedly with the frequency. You may have a broad band noise, but by changing the resonant frequency of the structure you may have less generalized force exciting a certain mode of response. It becomes difficult to talk about the efficiency of a certain damping treatment. You have to talk about the efficiency of a treatment on a certain structure.

Prof. Richards: And oscillating in a certain way.

Comment. Dr. Von Gierke: I would like to come back to one question about testing panels which was discussed yesterday afternoon and which Dr. Dyer touched on a few minutes ago. Yesterday it sounded in some remarks as if it were more or less a question of philosophy whether one tests with rectangular incidence or parallel incidence. Dr. Dyer said that there are other things besides sound fields: aerodynamic fields, boundary-layer fields, etc., and that we have to be careful how we test for them. Now as I look at it, all one has to do is simulate on the panel the exact correlation of the sound pressures which one has in the real conditions; then the panel doesn't care if it is boundary layer noise, noise from a random siren, or rocket noise. I think this is what we should all strive for in our testing procedure: to simulate the real conditions, in other words to measure the correlation over the test specimen. I think we do not have to mention in our test report under what angle of incidence we tested, if we give the correlation over the test specimen. This, I think, is the only thing that counts.

Dr. Dyer: You are quite right but I think it is impossible, according to my understanding of the physical laws, to duplicate the correlation of the boundary layer pressure field (which may be convecting subsonically) by a sound field which has as its lowest speed that of sound.

Dr. Schultz: Not only that, but I think there is also the matter of the interaction of the panel back on the boundary layer, which would be hard to duplicate.

Dr. Von Gierke: I think it is only a question of trying it out to see what correlation we get. I think that if there is rectangular incidence on the plate, you actually have no propagation of the pressure field over the plane. You have a velocity of zero. If you have parallel incidence, then you have the case just mentioned of a speed equal to the sound velocity. Between these two angles you should be able to simulate a condition with the right sub-sonic wave propagation.

Dr. Dyer: The available range of speeds along the panel with a sound wave is from an infinite value to the velocity of sound.

Prof. Powell, UCLA: For inclined sound waves, you can have any velocity between the speed of sound and infinity. So forget the sub-sonic boundary layer and think of the supersonic boundary layer. This, I think, will be the big problem.

Mr. Regier: The question of damping came up, and I would like to talk about it from a flutter standpoint. Suppose you want to stop a flutter condition. If you have a choice of using mass balance or damping, even though the damping can be shown to be lighter, the choice will be made for the mass balance mainly on the basis of maintenance and reliability; you will never have to look at it again. I think that there have been some publications on the fatigue life of damping materials and their maintenance requirements. That is a point that I think some people consider.