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THE SHOCK AND VIBRATION BULLETIN. PART 2. OPENING SESSION, PANEL SESSIONS, SEISMIC, SPECIAL PROBLEMS

Naval Research Laboratory
Washington, D. C.

June 1975

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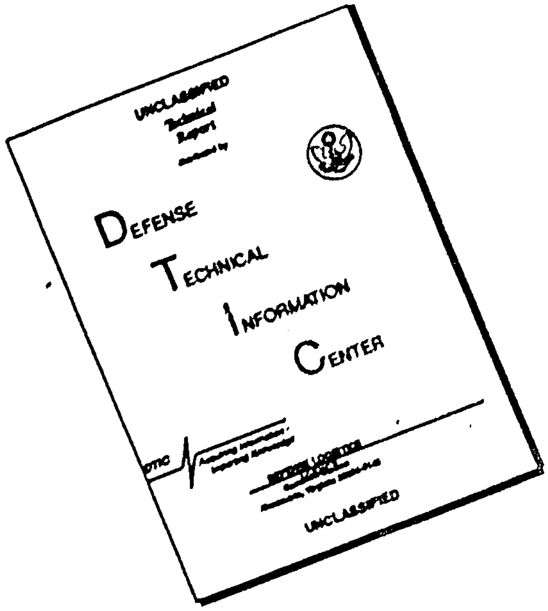

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Bulletin 45
(Part 2 of 5 Parts)

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THE SHOCK AND VIBRATION BULLETIN

Part 2
Opening Session, Panel Sessions, Seismic,
Special Problems

JUNE 1975

A Publication of
THE SHOCK AND VIBRATION
INFORMATION CENTER
Naval Research Laboratory, Washington, D.C.

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THE SHOCK AND VIBRATION BULLETIN

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The 45th Symposium on Shock and Vibration was held at the Dayton Convention Center, Dayton, Ohio on October 22-25, 1974. The Air Force Flight Dynamics Laboratory, Air Force Materials Laboratory and Aeronautical Systems Division Air Force Systems Command, Wright-Patterson AFB, Ohio were the hosts.

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WELCOMING SPEECH

Dr. D. Zonars
Chief Scientist
Air Force Flight Dynamics Laboratory
Wright-Patterson Air Force Base, Ohio

The Air Force Flight Dynamics Laboratory has for a very long time, given support to shock and vibration technology as well as to the Shock and Vibration Information Center. If my memory serves me correctly, the Flight Dynamics Lab people were the hosts of your first symposium that was given outside of the Washington Area. I believe it was the 13th symposium.

We have all seen great changes in the aeronautical technology since that time. Perhaps the advances in aviation and aerospace have been more dramatic than in any other area of our National Technology. The shock and vibration people have always been an essential part of these advances and believe me, these advances in aerospace vehicle configuration, size, performance, and combat effectiveness have taxed everyone's ingenuity.

The Flight Dynamics Laboratory's activity in shock and vibration involves two Divisions of the Laboratory: The Vehicle Dynamics Division, of which your Chairman is the Division Chief, and the Vehicle Equipment Division under Dutch Hildebrandt.

The Vehicle Dynamics Division assails such problems as flutter, vibration, dynamic loads - which is the airframe engineer's name for shock -- the problems of noise and its effects on the crew and on structures, which as we all know, have become quite an important part of the dynamics technology. An important function of the Dynamics Division is the practice as well as the development of dynamic measurements technology. Much of the data on vibration in airplanes comes from this Division of the Air Force Flight Dynamics Laboratory.

The Vehicle Equipment Division has advanced technology in the area of the reliability of equipments -- considering not only shock and vibration, but also the combined effects of temperature, humidity and other associated aspects of the environment in which we find our military airplanes operating. Vehicle Equipment Division also develops components of the landing gear, taking into account the shock and vibration standpoint: wheels, tires, brakes, struts, side braces -- and new concepts in flotation and landing systems such as the ACLS, the air cushion landing sytem.

We look forward to continuing our support of the shock and vibration technology as a constituent part of the efforts of the Laboratory. We sense the challenging problem of keeping down the cost of ownership of military airplanes as pointed out by Mr. Peterson. I can think of no other technology that has so much promise for doing just that. With the skills and the technology that you have developed, with the far reaching implication of the reliability problem, you are uniquely qualified to play a major role in this endeavor.

The Flight Dynamics Laboratory sincerely welcomes you to Dayton and wishes you well in this 45th Shock and Vibration Symposium.

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ADDRESS OF WELCOME

George Peterson
Director
Air Force Materials Laboratory
Wright-Patterson Air Force Base, Ohio

I would like, on behalf of the Materials Laboratory, to welcome you to Dayton. I noted that this is the 45th Shock and Vibration Symposium. This is an indication that the shock and vibration problem is truly one that is significant, and I know in looking at the program, at the scope of it, the diversity of it, that there is no doubt that it takes an interdisciplinary team such as is represented by the Military Services, NASA and Industry to attack the problems involved. Certainly today, in the environment we're in, it's not only the technical problems but the resource aspects that are driving us closer and closer together, not only within the Air Force, but also among inter-government agencies -- the Army, the Navy and NASA. I think you will see in the future more interdependency in program activities and certainly it is the resources picture that is driving us in this direction, more and more every day.

I would say that the Materials Laboratory is emphasizing research and development that will prolong the life of existing materials. This emphasis is to make them withstand the kind of environments that our weapons systems are faced with, make them last longer and cost less, both in the cost of acquisition and the cost of ownership. I think that's the name of the game also in shock and vibration, and I certainly would like to indicate to you the continuing, strong interest and activity the Materials Laboratory has in working with you, whether you are in government, industry or in the academic community, to help solve these problems. We are dedicated to them, and I think that we've got to solve them both from a defense standpoint and from the standpoint of the National interest.

With that, I hope you have a successful meeting. I think that the papers, at least those I looked at, are extremely interesting. I think that besides measuring the progress you have made over the past year since your last meeting, you might also look at this meeting as one of not just measuring progress, but of trying to identify the technical gaps that remain and prioritizing them from the standpoint of making sure we are putting our energies in the truly important and significant directions.

Thank you very much and I hope you have a good meeting.

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KEYNOTE ADDRESS

Lt General James T. Stewart
Commander, Aeronautical Systems Division
Wright-Patterson Air Force Base, Ohio 45433

Good Morning, Ladies and Gentlemen:

On behalf of the Aeronautical Systems Division, let me add my personal welcome to those already expressed by Mr. Peterson and Dr. Zonars.

I really mean it when I say that the Aeronautical Systems Division is privileged to co-host the 45th Shock and Vibration Symposium with the Air Force Materials and Flight Dynamics Laboratories. Of course, I would be less than candid if I didn't admit there is a certain amount of selfishness in that expression of pleasure . . . We always need help . . . . and it's not too often that we can muster locally this much national expertise in any one area at a given time.

In collecting my thoughts for this morning, I first went to my handy reference textbook -- the Keynote Speakers Guide to Instant Success -- and it advised opening one's remarks with a lighthearted story about the subject at hand . . . Well, unfortunately the Stories Appendix contained nothing about Vibration or Shock, but it did have several under the category of Shocking . . . Let's try one.

Picture, if you will, a dove, a lark, and a duck on a split rail fence. Overhead, there circled a great American eagle.

Suddenly, the eagle swooped down, grabbed up the dove and disappeared over the horizon. He returned in about ten minutes, deposited the dove gently on the fence, and resumed his majesjestic circling in the sky . . . The little dove shook herself, preened, and proclaimed for all the world to hear, "I'm a dove, and I'm in love!"

Suddenly, the eagle swooped down again, grabbed up the lark and disappeared over the horizon. He returned in about ten minutes, deposited the lark gently on the fence, and resumed his circling in the sky . . . The little lark shook herself, preened, and proclaimed for all the world to hear, "I'm a lark, and I've been sparked!"

And then, the eagle swooped down a third time, grabbed up the duck and disappeared over the horizon. He returned in about ten minutes, deposited the duck on the fence, and resumed his circling in the sky. The duck shook vigorously and proclaimed for all the world to hear, "I'm a drake, and there's been a terrible mistake!"

Well now, some of you may think your Keynote Speaker Selection Sub-Committee made a terrible mistake in selecting me for that task. But, I must say . . . in all modesty, I think they did quite well. After all, there only are two criteria for a Keynote Speaker:

First, that he know very little about the subject; and
Second, that he be from out of town.

Now, there's little doubt about my completely meeting the first criteria. And by selecting a local Keynote Speaker, your Committee has saved the cost of travel and keep of an out-of-towner.

Not only your Symposium Committee, but almost everyone else is concerned about money thes days. It's a subject of importance to each of us, personally as well as professionally. Around home, it's the cost of bread . . . and meat . . . and milk.

Here, this morning, it's the cost of attending technical symposia. Rooms, meals, airplane tickets . . . all are going out of sight.

And, you well know, the same economic pressures are squeezing defense projects. In

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some instances, even more severely than our personal ones.

Skyrocketing costs, coupled with the knowledge that the threat to our freedom and existence continues unabated, is putting DOD and defense industry managers and engineers in an awkward posture -- not unlike the football player being tackled high and low from opposite directions.

It is eminently clear that we're going to have to improve our products by getting equal or better performance with fewer dollars.

So, I hope today to examine that with you -- and identify some of the steps that we can take together -- in the shock and vibration arena -- to contribute to the over-all engineering accomplishments we must make.

It is a particular pleasure for me to address this group of experts, people who, for a long time, have been uniquely dedicated to the common goal of developing and improving shock and vibration technology -- a technology that has application to all kinds of systems -- airplanes, spacecraft, submarines, tanks -- you name it.

No doubt about it -- you have continued to provide the Armed Services with increasingly numerous and complex components, having longer, better failure-free service lives in our defense systems. In a sense, you have managed to build a dam which has held back a flood of problems which would have otherwise drowned us . . . . As it is, we have almost choked on some of the leaks.

An example was quite evident in a 1971 Air Force study of the number, causes, and costs of failures occurring in a defense system over a fairly long period of time -- something like 24 months. One-third of the failures that occurred due to all environmental factors were caused by shock and vibration. Costs for shock and vibration related maintenance was $7,000,000 per year for 200 aircraft or $35,000 per aircraft. That's a lot of money.

The lesson was inescapable -- we have to have more timely -- by which I mean early -- planning, analysis, and tests to get the problems out of our equipments before they are in production. It is no longer enough that we do a great job and preclude the major problems -- the economics of today and of the future are such that even sizable leaks through the dam are unacceptable.

We are making substantial efforts in the Air Force to develop approaches to reduce the costs of fixing failures of equipment in the field. Major efforts of this kind, such as the Air Force's Rivet Gyro Program, of which you will hear more shortly, have shown that major logistic cost savings can be obtained with product improvements -- often at modest cost, and often even when the improvements are relatively expensive. Unfortunately, these projects are in a sense, locking the barn door after the horse is gone.

The key to real improvement lies in preventing the problem at the beginning. Since the costs of system acquisition and particularly of system ownership are increasing at a much faster pace than our budgets, it is clear that we must improve our effectiveness -- or lose out to the threats which endanger our freedom and existence. This indicates changes in the way we do our business.

Let me make a few observations, applicable to your endeavors in the shock and vibration area, as to where we might begin on this task of improving our effectiveness while achieving lower life cycle system costs.

If we stand back and take a look, it is readily apparent that everyone has become very good at developing technologies to meet operational requirements and attendant problems -- in fact, quite elaborate and sophisticated solutions have been developed in your area and in the reliability area, for example. But along with sophistication of these technology areas -- we sometimes demonstrate those familiar human maladies like tunnel vision and isolation, and conduct elaborate programs, which are often isolated from each other . . . and not compatible, thus inducing more costs. These maladies fortunately are not terminal. I firmly believe they can be "treated!" -- and, I have some ideas on that! In fact, the technologies need a little treatment also, and this is the challenge that I offer. Let me explain!

To begin with, consider some technologies that have interaction with the Environmental Qualification Technology of which Shock and Vibration is an important part. What I am referring to are: System Safety Engineering which is hazard identification, control, and/or elimination; and a group of technologies referred to as the "ilities," Reliability, Maintainability, and Survivability/Vulnerability. And I'll toss in Quality Assurance for good measure.

Most of you are familiar with these areas. You all should be because there are many common problems -- goals -- interfaces -- methodology between and among all of them.

Shock and vibration tests are involved in the control of safety hazards and certainly are involved in the development of reliability. Environmental factors such as temperature are an important cause of failure, but they are considered by a different environmental group and by Reliability specialists. Is there enough communication, interchange, or mutual effort between these concerned parties? I don't think there is.

As to objectives of these technologies, in Environmental Qualification, you aim at validating that an item will perform to specification -- and last for life -- in its environment. Meanwhile, System Safety engineers are figuring out whether the item could be a hazard source and the consequences if the item does fail. Reliability people are busy estimating out how long the item will last without failure -- MTBF -- and what this means with respect to probability of successfully completing a mission. Maintainability experts are making the item easier to maintain with acceptable expenditures of manhours to replace/repair, etc.

Vulnerability specialists are making it unlikely that the item will be hit or shot out of the location where somebody put it, probably for a good reason . . . . such as a low vibration environment. And Quality Control people may find that the item delivered is not anything like the item everyone spent all this effort on.

This picture is a rather gross over-simplification and an exaggeration, but it does illustrate that there is a lot of mutual interest in failures of equipment, in design to preclude them, and in tests to validate design. What worries me -- in this economic environment that we are in -- is that sizable essentials in the various technical areas I've mentioned -- very much need to be accomplished.

Unfortunately, full blown efforts in any one of the areas can absorb a lion's share of available funds. So we must find ways of integrating these efforts to kill more birds with one stone. Some Air Force studies of different approaches to reducing operating costs -- and integrating technical efforts -- will be covered in later presentations in this session. I hope they provoke a few thoughts and ideas.

This meeting is a good place to start because we have present both the people who understand the sophisticated technology and the people who can best bring about the needed changes. I would suggest that a session in all of your future meetings be set aside for interfacing with the other disciplines that are closely related to yours -- with the objective of identifying mutual interests and promoting helpful participation.

I want to reemphasize the importance of early, timely application of your shock and vibration considerations in the system development process. We see time and again that our operational problems could have been prevented by appropriate action in the early stages of development. The importance of this early action is true regardless of the type of procurement approach -- whether a carefully phased fullscale development program, ala the F-15, or a prototype program, ala the lightweight fighters.

There is a crucial early period in which commitments are made when it is imperative that the essentials of all the technologies, including shock and vibration must be considered. Decisions made at this time permanently impact the ease of difficulty of achieving acceptable standards of safety, reliability, and maintainability.

The Military Services and NASA have important roles in the future of our country. We both need dependable systems with superior performance to meet our responsibilities. We have made great progress in this regard, but we must make new systems even better. We need the help from the different viewpoints you represent in developing new techniques and knowledge for doing the job of qualification, reliability, maintainability, safety, and quality assurance even better.

And let us not become so enthralled with the beauty and elegance of the methodology that we forget that the primary purpose is developing systems with the needed performance, on time, and within costs.

I really have appreciated this opportunity to speak to Army, Navy, and NASA as well as Air Force specialists and their industry and University counterparts. All of us are facing the same problem. And the chances of finding some answers are much better with all of us working together.

If we can do anything to make your stay more pleasant and productive, please speak up. I hope you can take advantage of the opportunity to tour Wright-Patterson and visit the Air Force Museum Friday morning.

In closing, I must say I noted with some horror that this is the first national shock and vibration symposium in Dayton in almost twenty years. Please don't stay away that long again.

My pleasure to have been with you this morning . . .

INVITED PAPERS

STANDARDIZING THE DYNAMICS OF MAN*

DR. H.E. VON GIERKE
Director, Biodynamics and Bionics Division
Aerospace Medical Research Laboratory
Wright-Patterson Air Force Base, Ohio.

I was asked to apply the "human touch" to this Shock and Vibration Symposium and report on progress in analyzing and describing the mechanical response and performance effects resulting when the human operator is exposed to shock and vibration environments. Reflecting about this assignment, my thoughts went back 19 years to the last Shock and Vibration Symposium hosted here in Dayton at Wright-Patterson Air Force Base in 1955. At this symposium we had a special session on the biological-medical effects of shock and vibration, and I recalled the topics of the seven papers at this session[1]: Motion Sickness, Physical Response of the Human Body to Mechanical Vibrations, Damage to Animals due to Vibration, Vibration Tolerance Criteria, Hydrostatic Effects of Combined Tumbling and Deceleration, Downward Ejection at High Speeds and High Altitudes and finally, Acceleration Problems in Ejection Seat Design. All of these were excellent papers; many of them became classics in their field. And then I thought of the problems we are struggling with today, the problems which according to our present-day, highly overdeveloped research planning technology we hope to solve within the next three years. And to my surprise I had to confess: the problems today still sound very much the same! I was reminded of an old story: the graduate, who after years and years meets his old professor again and asks him after awhile: "Say, professor, are the questions in your examinations still so dreadfully difficult as they were in our times?" And the professor replies, "Sure, they are still so difficult. As a matter of fact, I still ask exactly the same questions." The old student is surprised and asks, "Why, if this is the case, are the questions by now, not known to all students and the answers are passed down from year to year?" "Well," answers the professor with a smile, "the questions are the same, but the correct answers change from year to year."

Although I was pretty sure that probably not too many of you were with us here 19 years ago and could argue with me how much the answers to the old questions have changed, these recollections forced me to take stock of the progress we made over these 19 years and to count our achievements. In the following I would like to share some of the results with you.

There is no question: the problems are still the same, some of them might even be more critical. Accident statistics and injury analysis give us today hard data, where 20 years ago we only knew that we had a problem. The analysis reveals to us if the man or the machine failed first, if the man failed due to physical injury or due to the inability to function normally and, most important, how these failures correlate with the environmental variables.

Human performance under vibration and buffeting is still an operational problem in low altitude, high speed flight, in helicopter and personnel carriers and in the surface effect ships on the drawing boards.[2] We are not only concerned how these environments effect mission capability and weapons delivery but also about their potential health effects due to repeated, chronic exposures. In a preliminary survey 87% of helicopter pilots reported back

*The research reported in this paper was sponsored by the Aerospace Medical Research Laboratory, Aerospace Medical Division, Air Force Systems Command, Wright-Patterson AFB, Ohio. This paper has been identified by Aerospace Medical Research Laboratory as AMRL-TR-74-129. Further reproduction is authorized to satisfy needs of the US Government.

symptoms after exposure to the helicopter environments for more than 300 flying hours. Spinal injury due to escape maneuvers was studied before the first ejection seat was ever tested more than 30 years ago. The problem is still with us today: The USAF has approximately 10 major injuries per year caused by ejection forces and the hospitalization and crew replacement costs alone are estimated to be between 10 and 50 million dollars per year. Windblast induced flail injuries to legs and arms still occur in roughly 7% of the ejectees (5 major injuries per year) and the probability of flail injuries increases with increasing airspeed (Fig. 1)[3][4]. All ejections initiated at airspeeds above 500 knots during the period January 1968 to December 1970 have resulted in fatality or major injury to the ejectee. Efforts to protect the most vulnerable part of man, his head, against blows and penetrating impact go back to pre-historic times: it is still difficult for us today to evaluate how much protection a helmet provides and how to quantitate the protection. In escape maneuvers helmet retention is a problem, again due to windblast forces (Fig. 2)[5][6]. The total injury rate to the head is twice as high, if the helmet is lost, a satisfying evidence that our efforts are useful. However, if we look at the distribution of head-neck injuries (Fig. 3), we find that with

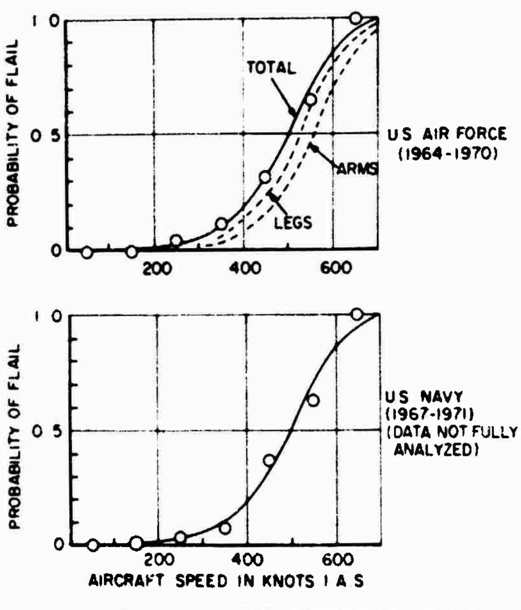


Fig. 1 - Cumulative flail injuries as a function of airspeed (USAF and US Navy) (From ref. (3)).

the retained helmet the probability of injury to the cervical area, the neck, increases by 50%. An injury mechanism might be at work through which the aerodynamic forces on the helmet increase the injury potential to the neck. These are just a few, almost arbitrary, military data points to emphasize that the old problems are still with us. In spite of all the solutions found they might even be more costly today in terms of human injuries and mission effectiveness due to the higher speeds, higher costs and larger number of our systems.

What progress has been made on the biomedical side to assist the solution of these problems? I will not talk about the technological/hardware solutions, but about our knowledge of man, his mechanical properties, his ability to withstand these environments and to function in them. Looking over the last 20 years I must say that progress has been very impressive. It was an exciting time during which a whole new discipline, biomechanics developed, an interdisciplinary specialty taught today at many universities. It is no longer the temporary meeting ground for physicians attracted by physical and engineering problems and the engineer or mathematician attracted by the possibility of biological spin-offs from his area of prime interest. It is a specialty in its own right supplying useful information to various medical areas, technologies and industries. The application of biomechanical knowledge to aerospace medicine and military technology is just one specific application of the broader body of knowledge. With respect to this specific application progress might be summarized in three statements:

1. For many practical situations of mechanical forces acting on man, we know how to describe man as a mechanical system. We know how the system reacts to various inputs, how its performance is effected by the mechanical responses and at what levels failure occurs.

2. Based on these data we can build mathematical models[7][8] which allow us the quantitative prediction of the body's response to force environments we never tested or experienced. We can give these models to the designers of hardware systems and specify human tolerance or performance limits in terms of model responses, in a language directly understandable and usable by the designer. These models of the human body can be combined with models of the hardware systems, e.g., the seat cushion, the seat, the restraint system, the tank, etc., and the overall man-machine response can be quantitatively analyzed.

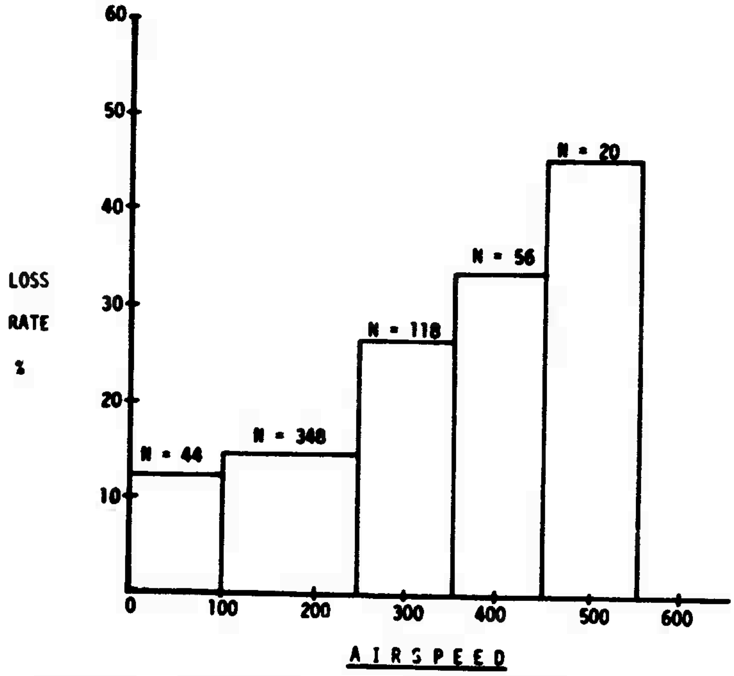


Fig. 2 - Helmet loss rate versus airspeed (from reference 5).

HEAD AND NECK INJURY LOCATION DISTRIBUTIONS

1 JAN 1968 - 31 DEC 1972

INJURY LOCATION	HELMET INTACT (NUMBER)	HELMET INTACT (PERCENT)	HELMET LOST/FAILED (NUMBER)	HELMET LOST/FAILED (PERCENT)
SKULL	12	23.1	11	26.8
BRAIN	2	3.8	3	7.3
FACE	22	42.3	17	41.5
EYES	2	3.8	2	4.9
EARS	3	5.8	2	4.9
CERVICAL AREA	11	21.2	6	14.6
	52		41	
	N = 48		N = 34	

NOTE: TWO INJURIES WITH HELMET STATUS UNKNOWN.

Fig. 3 - Distribution of head injuries after ejection; helmet retained versus helmet lost. (From ref. 5).

3. Our data base is large enough and the results reasonably consistent that we can make the step from individual observations to generalized responses; that we can standardize man's biomechanical responses for many applications in terms of military standards, national standards and international standards. The improved accuracy and cost saving from such standardization is obvious.

Let me illustrate these advances by a few selected examples, which might be of interest to you:

a. Whole Body Response Models.

To describe the kinematics of the human body under crash impact or during aircraft escape maneuvers, models of the type shown in Fig. 4 are being used.[9] They describe torso distortion and limb motion and impact of body parts with obstacles and canopy walls. In our program on the definition and reduction of windblast effects on the ejecting crewman, we are presently measuring in a wind tunnel the windblast forces on the various body segments of a live human subject.[14] Thereafter, the measured distribution of aerodynamic forces will be applied to this model to predict injury limits in response to these forces. In a kinematic model of the type in Fig. 4 the individual body segments are treated as rigid bodies. Therefore the model cannot describe the deformation of body parts or organs leading to physiological disturbances and injuries. For this, lumped parameter models of the whole body have proven very successful. An example of one is shown in Fig. 5[7]. This model, for which each individual parameter is quantitatively known, describes deformation of the spine, thorax, abdomen and neck under longitudinal impact loads and vibration and also explains the thorax-abdomen dynamics leading to injury from explosive blast. The same model is being used to elucidate the phenomena resulting from impact forces to the chest, as for example, when in an automobile head-on collision a driver's chest hits the steering wheel. The main responses of interest - the chest deflection leading to rib fracture and the internal thoracic pressure rise resulting in lung damage - are shown in Fig. 6[10]. The human body with the gross dynamic characteristics represented by these models presents a driving point impedance to the seat, when sitting, and to the floor, when standing, which is of interest for many design, loading and measurement problems.[11] Twenty years ago we started the first whole body impedance measurements in our laboratory; today we have enough data accumulated that we are close to an international agreement (ISO draft standard) on a standard impedance function to be used for example for the evaluation of seats and seat attenuation systems. The draft standard curve (together with the model configuration proposed to approximate the standard impedance) is shown in Fig. 7.[12]

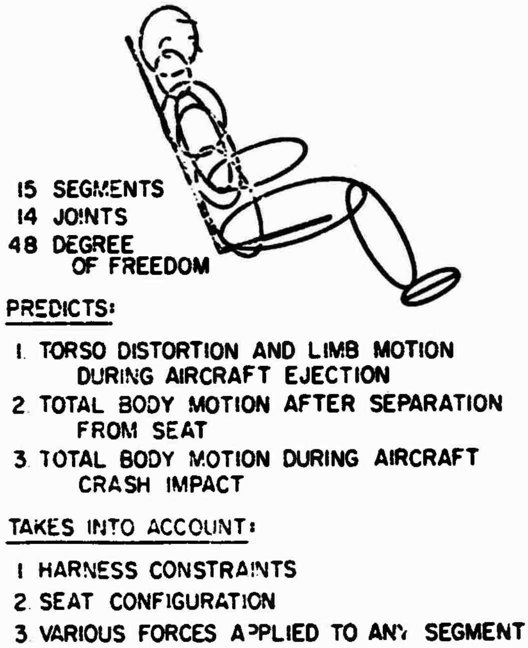


Fig. 4 - Kinematic model of the total body. (9)

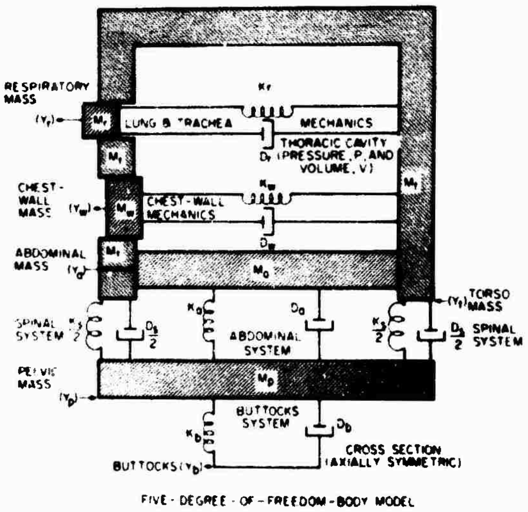


Fig. 5 - Lumped parameter model of the human body describing its response to longitudinal (G_z) impact and vibration, to infrasound and blast. (8)

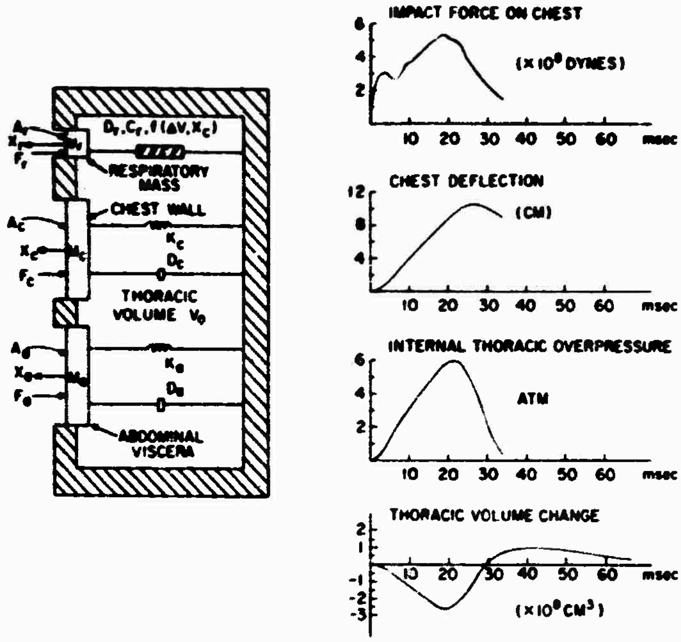


Fig. 6 - Thorax-abdomen model and its response to chest impact. (10)

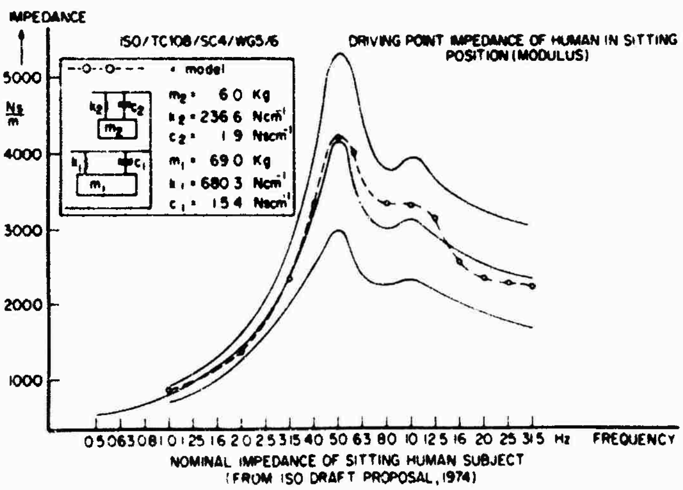


Fig. 7 - Proposed standard driving point impedance of human subject in sitting position. (12)

b. Impact Tolerance Models.

In the whole body response model of Fig. 8 the spine is represented by a simple, linear spring. To use this simple model of the spine to predict the probability of spinal injury under the short duration $+G_z$ accelerations produced by ejection catapults was perhaps the most important recent biomechanical contribution to solve AF and DOD problems.[13] As stated before spinal injury, i.e., compression fracture of the vertebral body segments is a frequent and extremely costly result of emergency escape. Although it was the USAF design goal to keep the probability of spinal injury to 5 per cent or less, analysis of the accident reports revealed that this limit was exceeded for several escape systems and that past military specifications limiting peak acceleration and rate of onset were not adequate and realistic. Using available data on the dynamics of the spine and breaking strength data obtained from tests of cadaver specimens a simple probability of injury model for spinal compression fractures was developed. The model considers only compression of the spinal spring loaded by a single equivalent body mass (Fig. 8).[14] The natural frequency of the system is for the average AF flying population (age 27.9 years) 8.42 Hz. The normalized maximum deflection of the spinal spring under the acceleration input, (normalized by the compression of the spring under the body's weight) is called the Dynamic Response Index (DRI) and correlated with the probability of spinal compression fracture. (The DRI is also equivalent to the peak force occurring normalized by the body weight.) The probability of injury estimated from laboratory cadaver data as a function of the DRI is presented in Fig. 9; the same graph shows the data points from operational experiences with various escape systems. The operational data points justified a shift of the curve to the right; i.e., an increase in breaking strength for a given DRI, a reasonable consequence of the strength and damping added to the isolated spinal column by the intervertebral disks, and by the ligaments and muscles of the live subject. The DRI vs probability of spinal injury relationship in Fig. 9 is now used in our military specifications as a design and evaluation tool for the development of ejection seats[15] and crew escape modules.[16] Its usefulness and advantages have been proven over the last 5 years. It has been adopted internationally as Air Standard by the Air Standardization Coordinating Committee (ASCC). One of the main advantages of this model methodology is that it allows the evaluation of complex acceleration inputs to which previous human exposure criteria were not applicable or incorrect. The model can be combined with the dynamic characteristics of seat cushions for a quantitative definition and prediction of their effectiveness with respect to spinal injury potential.

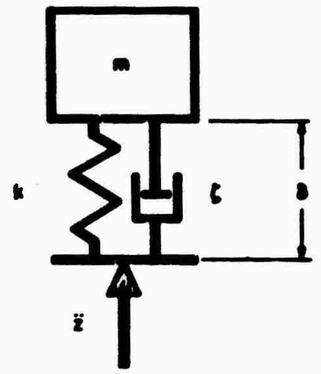


Fig. 8 - Spinal injury model. m is mass (lb-sec²/in.); δ is deflection (in.); ζ is damping ratio; k is stiffness (lb/in.); G_z is acceleration input (in./sec²); DRI = $\omega_n^2 \delta_{max}/g$, where DRI stands for dynamic response index; ω_n is natural frequency, $\omega_n = (k/m)^{1/2}$ (radians/sec); and g = 386 in./sec². (From references 14 and 15)

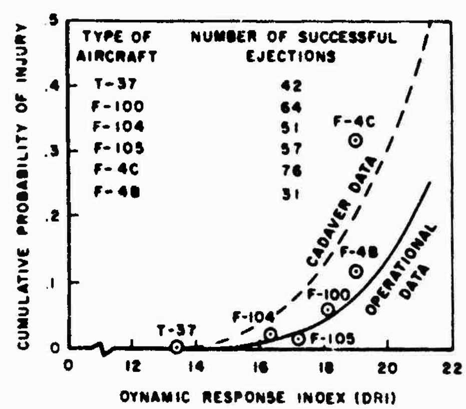


Fig. 9 - Probability of spinal injury predicted from cadaver data compared to operational experiences with various US Air Force ejection systems. (From reference 14)

This model for evaluating longitudinal spinal loads is only the beginning; much more refined models of the spine are being worked on to predict not only the occurrence of injury somewhere in the spine but to predict the exact location of the injury and its dependence on spinal curvature. For this purpose each vertebral element must be represented as a separate entity. Examples of such advanced spinal models are shown in Fig. 10.[8] For the evaluation of transverse acceleration loads as they occur in capsule landing and crash impact situations dynamic injury models for the other body axes are under development. We foresee an injury model which will finally allow us the prediction of probability of injury for all body axes and eventually for multiple repetitive impacts coming from various directions.

c. Evaluation of Human Vibration Exposure

Twenty (20) years ago some individual studies on what were considered excessive levels of human vibration exposure with respect to comfort and health were available. At the Shock and Vibration Symposium in 1955 the Chairman of this morning's session presented a study for Mr. Getline reporting on vibration levels and acceptability judgments in military aircraft.[17] The paper culminated in the proposal of a tolerance limit curve, the WADC vibration tolerance limit, which was used for many years in military specifications. At the same symposium a paper was presented on vibration exposures so violent as to result in pathological damage to the heart and viscera of animals. Little was known at that time how animal experiments can be interpreted and extrapolated with respect to their consequences for man. Here again the following years brought increased activity in this field: The models describing man's response to vibration, which were mentioned before, were supplemented by models describing the responses of various animal species and they all were related to each other by dimensional scaling laws.[8] On the basis of such scaling laws the quantitative interpretation of animal injury studies in terms of human sensitivity became possible. Parallel with these studies on injury mechanisms human subjective tolerance and performance capability when exposed to vibration environments for various time periods were studied. The combined body of information was extensive and convincing enough that this year, after 10 years of study and negotiations, agreement was reached by the member nations of the International Standards Organization (ISO) on an International Standard on the evaluation of human vibration exposure.[18] Basically, the same approach was already adopted years earlier as a US Military Specification.[19] The standard gives exposure limits for various exposure times which should not be exceeded without adequate justification or precautions since above these limits the the risk of health impairment is imminent. At acceleration magnitudes of half of the exposure limits "fatigue" is likely to occur and on many performance tasks required from operators in vibration environments "proficiency" starts to

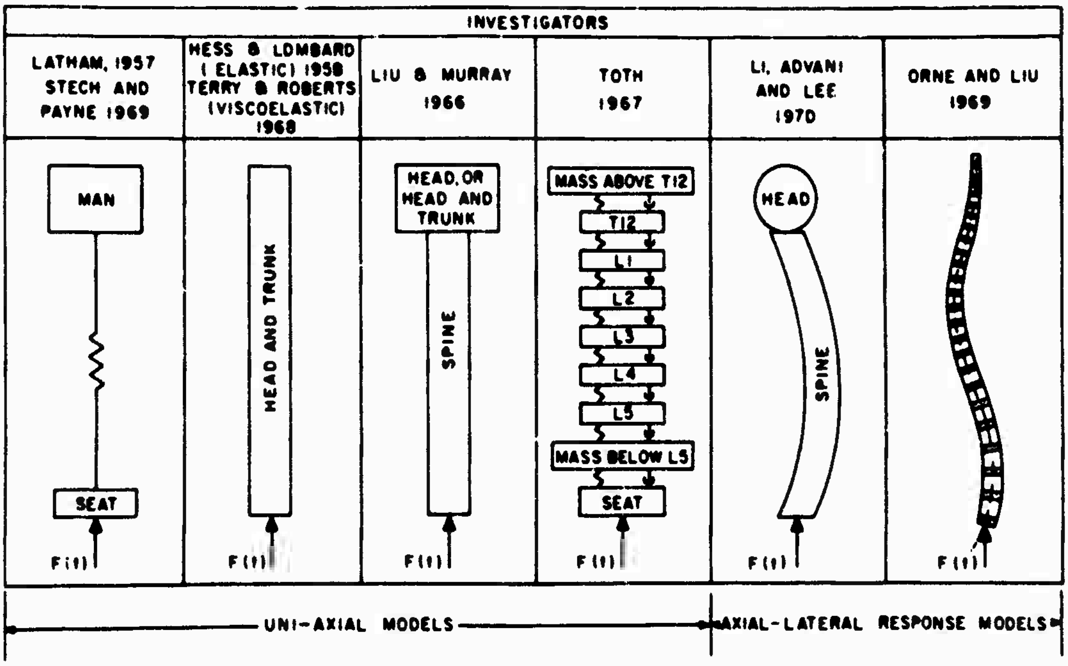
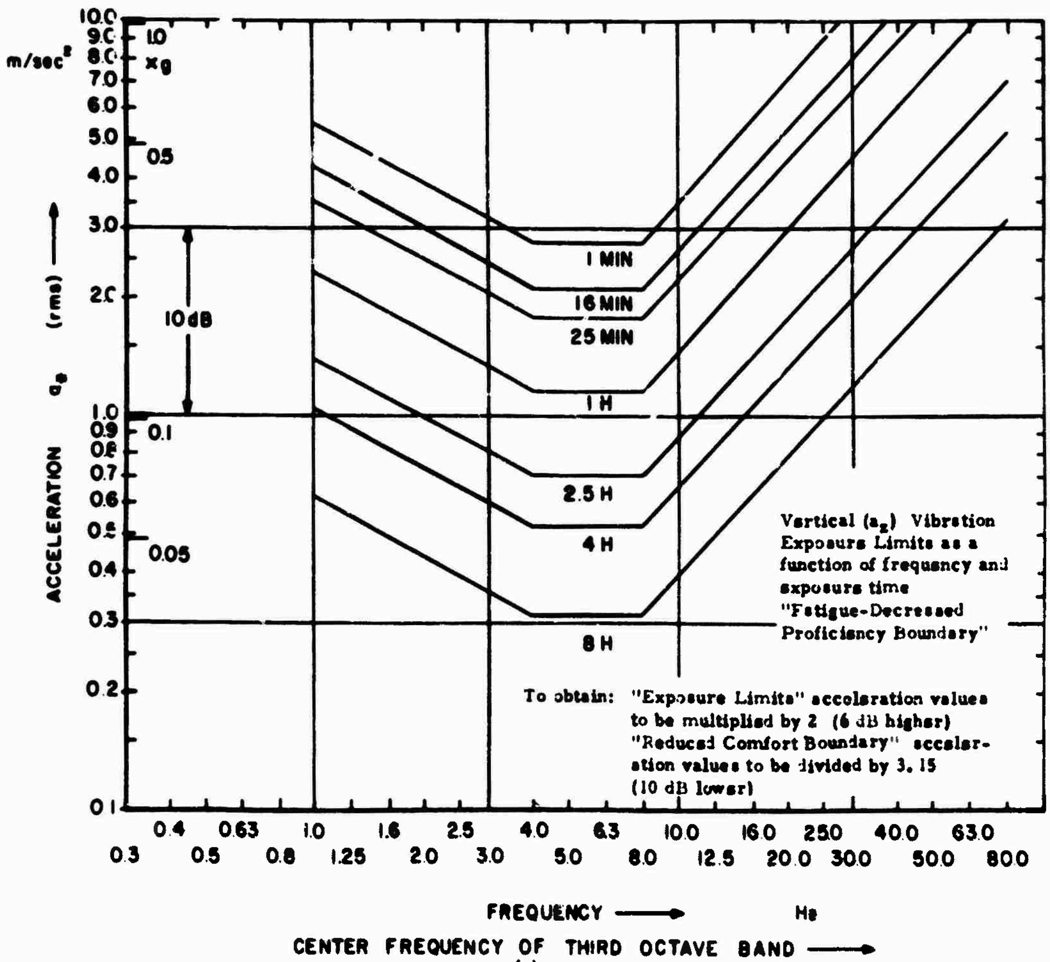


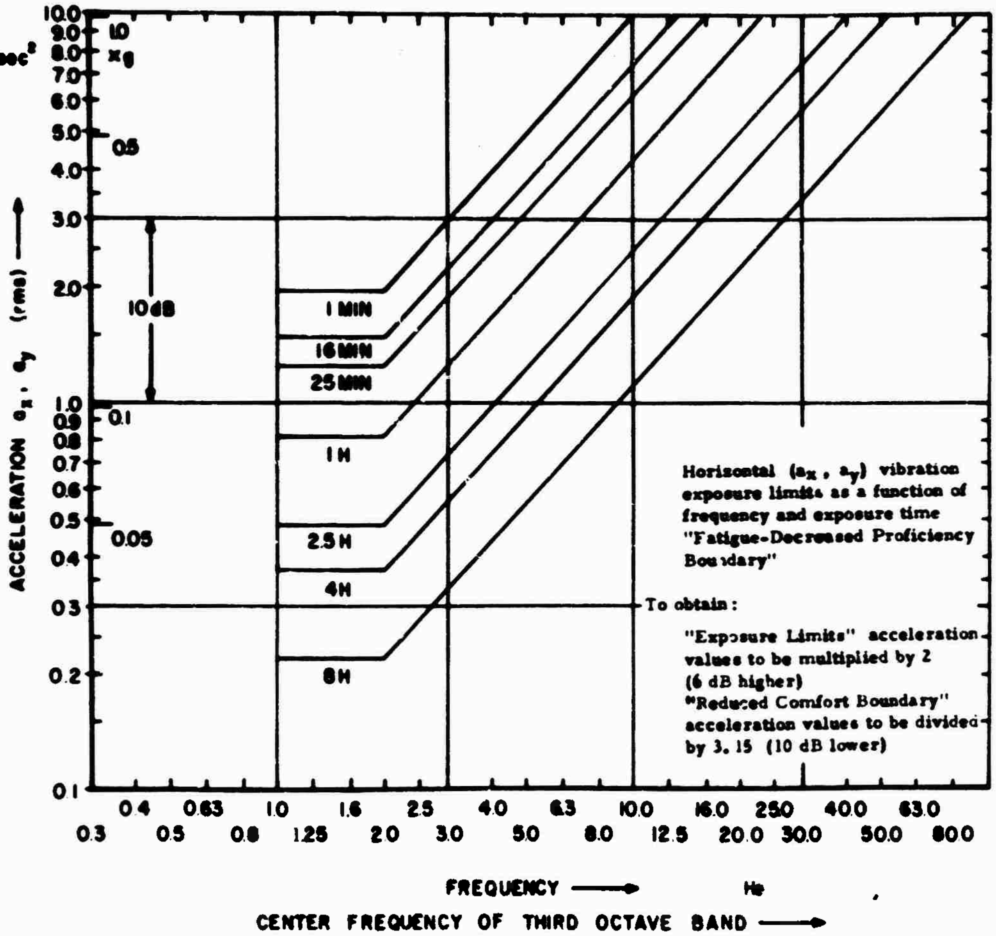
Fig. 10 - Representative examples of modeling approaches to the human spine. (From reference 8. See reference 8 for additional references).

decrease. These limits for operator proficiency were taken primarily from pilot ratings. Needless to say, that the design of the operators' tasks will have a marked influence on the vibration level at which proficiency is actually impaired. The frequency dependence of these limit curves depends primarily on the body's biomechanical response and is largely explained by the models of the human body: exposure limits are lowest in the frequency range in which body resonances amplify the vibration input. For vibration in direction of the longitudinal body axis the "fatigue/decreased proficience boundary" is shown in Fig. 11a. For the "exposure limits" these curves must be raised by a factor 2. To obtain the boundaries for "reduced comfort," which might be selected as the design limits for passenger compartments of transportation vehicles, the curves must be lowered by a factor 3. For the various octave bands the acceptable exposure level decreases with the exposure time as indicated in Fig. 12a. For transverse; i.e., chest-to-back or side-to-side vibration the main resonance frequency of the body is lower and the standard curves have the shape shown in Fig. 11b and 12b. Agreement on these vibration exposure standards was a very important step forward. Before the limits could be formulated, a uniform methodology for measuring and analyzing human vibration environments had to be developed. This step by itself was very important and worthwhile since it will result in



(a)

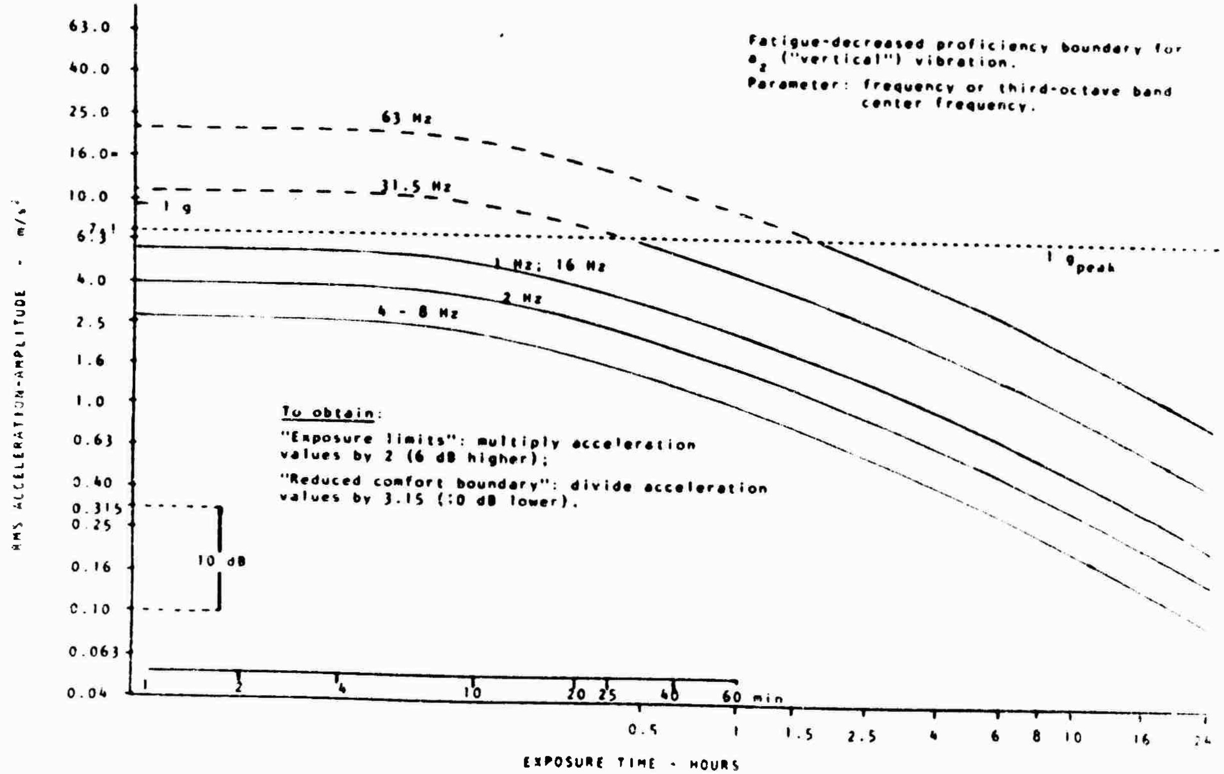
Fig. 11 - The standardized limits for human vibration exposure: "Fatigue/decreased proficiency boundary" as a function of frequency. a. for longitudinal (Z-axis) vibration, (From reference 18)



(b)
Fig. 11 (Cont'd) - The standardized limits for human vibration exposure: "Fatigue/decreased proficiency boundary" as a function of frequency. b. for transverse (X- or Y-axis) vibration. (From reference 18)

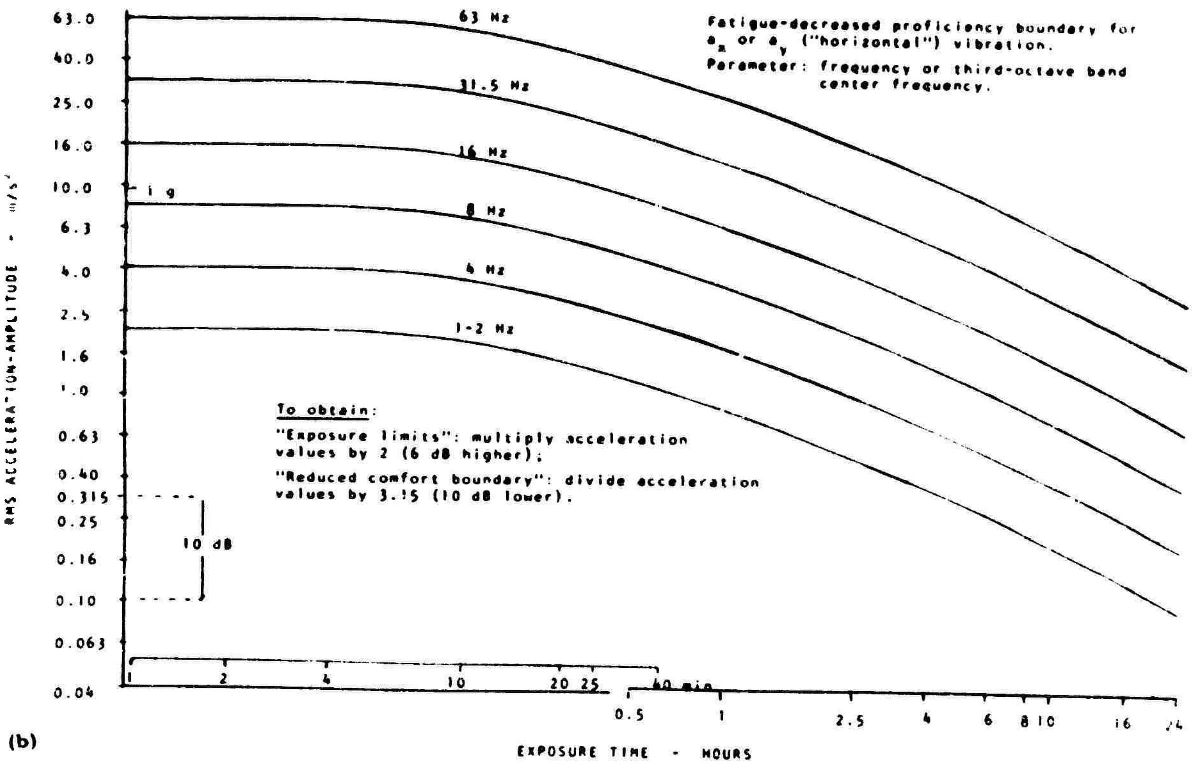
more reliable and uniform data collection in the future. The standard for the evaluation of human vibration exposure is basically applicable to all vibration exposure conditions and situations, to civilian as well as military situations, to aircraft, ground vehicles and ships as well as to buildings and factories. (Some adjustment of the curves up or down might be indicated since the military population might differ with respect to physical fitness from the general population, since tasks differ with respect to vibration interference sensitivity and comfort at home is evaluated with a different yardstick than comfort in a subway; but the basic framework applies to all situations and the curves provide positive guidance for all design requirements.) This standard has also been adopted as Air Standard by the ASCC after slight adaptation to the military aviation requirements. An application of the standard is illustrated in Fig. 13:[20] Here the vibration spectra during high-speed, low-level flight are compared for a B-52, B-58 and an F-4; only in the F-4 are the vibrations expected to have an effect on pilot fatigue and proficience after approximately 2 to 4 flying hours.

The vibration exposure standard discussed covers the frequency range 1 to 100 Hz. Below 1 Hz human response and exposure limits are governed by motion sickness, a phenomenon not only highly variable from subject-to-subject and from situation-to-situation, but also a phenomenon depending on other sensory inputs besides whole body motion. Visual inputs play a major role in its occurrence and adaptation is also known to be an important factor. Although motion sickness is no serious disease and results directly in no permanent effects, its debilitating



(a)

Fig. 12 - The standardized limits for human vibration exposure: "Fatigue/decreased proficiency boundary" as a function of exposure time. a. for longitudinal (Z-axis vibration, (From reference 18)



(b)

Fig. 12 (Cont'd) - The standardized limits for human vibration exposure: "Fatigue/decreased proficiency boundary" as a function of exposure time. b. for transverse (X-or Y-axis) vibration. (From reference 18)

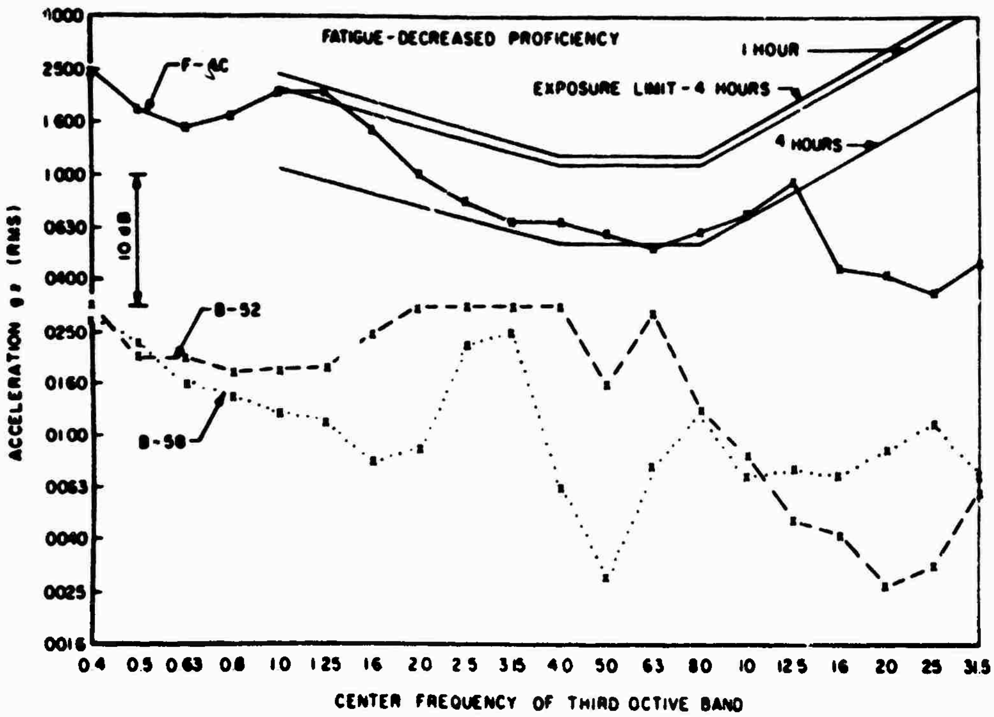


Fig. 13 - Maximum Z-axis third octave band acceleration levels measured at the pilot's seat in low-altitude, high-speed flight missions over mountainous terrain. The 1 and 4 hr "fatigue/decreased proficiency" boundary from Fig. 11a is also indicated. (From reference 20)

consequences and effects on any task performance are well known. Models have been advanced to account for the phenomena and their dependence upon the physical stimuli but their practical usefulness is limited due to the large variability of responses. In spite of these problems the requirement for some uniform guidance with respect to this frequency range was so strong[2] that the ISO working group is preparing an amendment to the vibration exposure guidelines which proposes "severe discomfort boundaries" for the 0.1 to 1 Hz range for various exposure times (Fig. 14). [21][12] Exposure up to these levels is expected to result in less than 10% motion sickness in the general population. Whenever for military use extensions of human transfer functions to this very low frequency range are needed the shape of these curves is being recommended as design guidance.

Another standard in preparation relates to human vibration exposure to hand-transmitted vibration, as they occur with most power hand tools (pneumatic and electric tools, chain saws, etc.). Habitual use of many of these tools has been found to be connected with various patterns of disease involving blood vessels, bones and joints of the exposed hand and arm. Tentative exposure limits are presently under consideration by the ISO working group dealing with this subject. These limits are shown in Fig. 15.[12]

In spite of the progress in the standardization of human responses to vibration, research on this subject is continuing. A new approach which might be of particular interest is the application of manual control theory to the evaluation of the operator performance on control tasks of defined complexity. This approach results in human operator models which allow us the calculation of an operator's tracking performance derived from the vibration interference with the display and control interfaces (Fig. 16).[22] By this methodology the operator's error can be analyzed in terms of a component correlating with the signal input and two components caused by the vibration effects: the vibration correlated feedthrough and the additional remnant, or noise, in the control output (Fig. 17). The application of this approach might best be illustrated by an example: Pilot tracking performance under vibration was investigated as a function of control stick location (side versus center) and stick

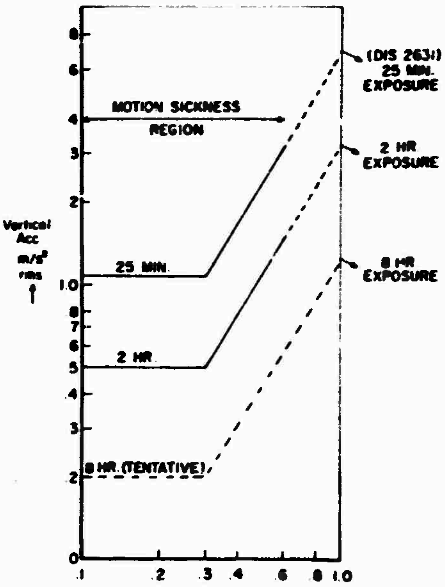


Fig. 14 - Proposed "severe discomfort boundaries" for various exposure times for the 0.1 to 1 Hz frequency range. (12)

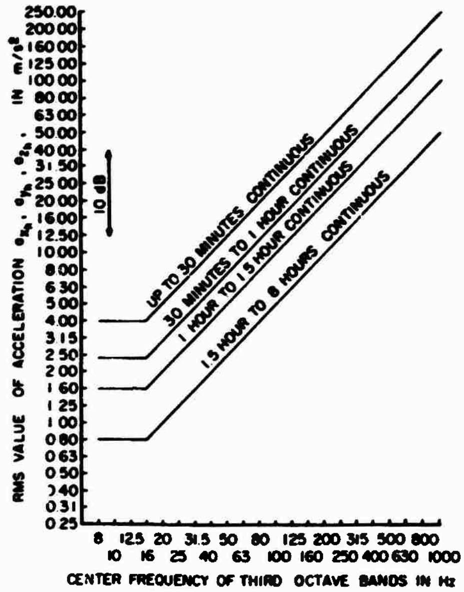


Fig. 15 - Proposed standard exposure limits for hand-transmitted vibration as transmitted by power hand tools. (12)

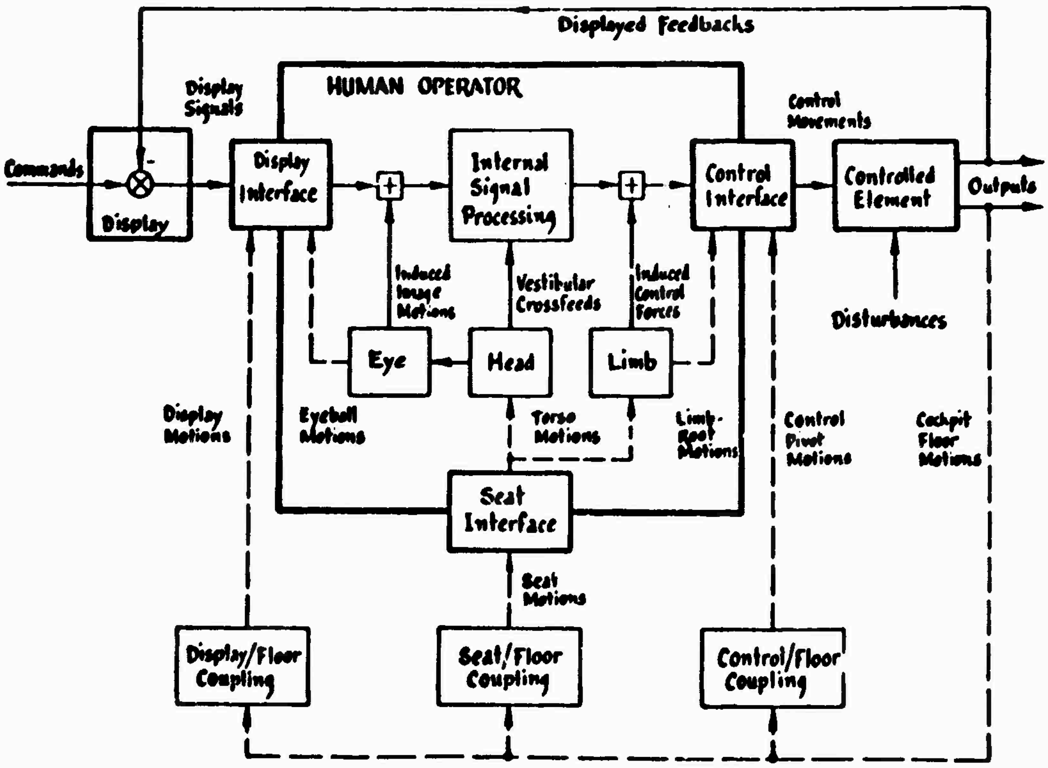


Fig. 16 - The human operator performing a manual task in a biodynamic environment. (From reference 22)

dynamics (stiff versus spring stick).[23] Separation of the errors into their various components exhibited the differences shown in Fig. 18. Theoretical prediction of the errors by use of the model results in good agreement with these data (Fig. 19). A practical application of these results and the model's predictive capability are shown in Fig. 20: In the vibration environment there is an optimum stick gain at which the output error is a minimum. The minimum is caused by the summation of the vibration feedthrough increasing with stick gain and the tracking related error (remnant) decreasing with increasing stick gain.

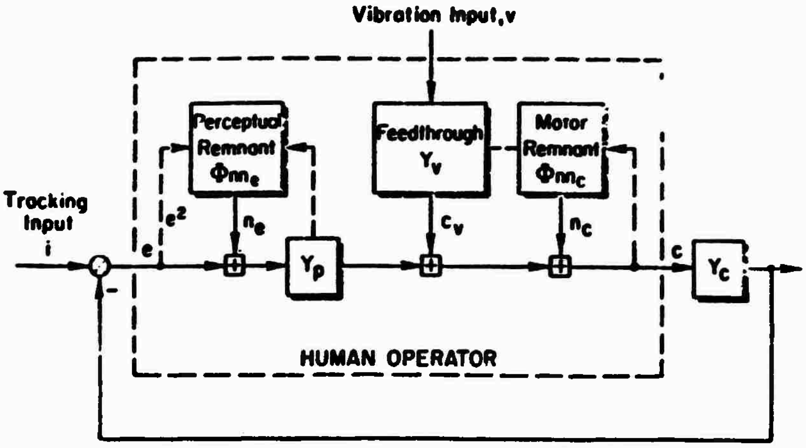


Fig. 17 - System model for vibration effects on manual control performance. (From reference 22)

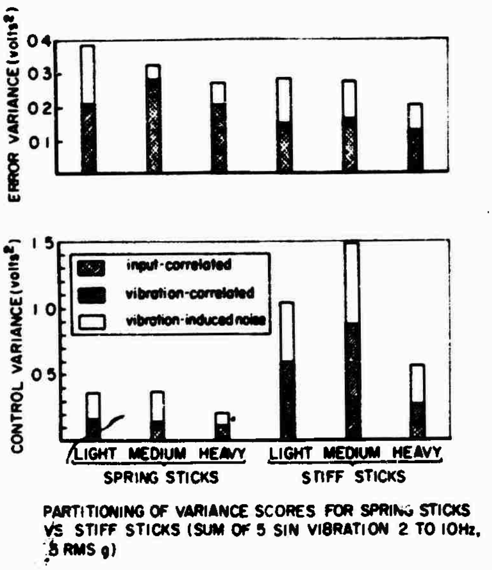


Fig. 18 - Pilot tracking performance under vibration (side stick versus center stick): components of error. (From reference 23)

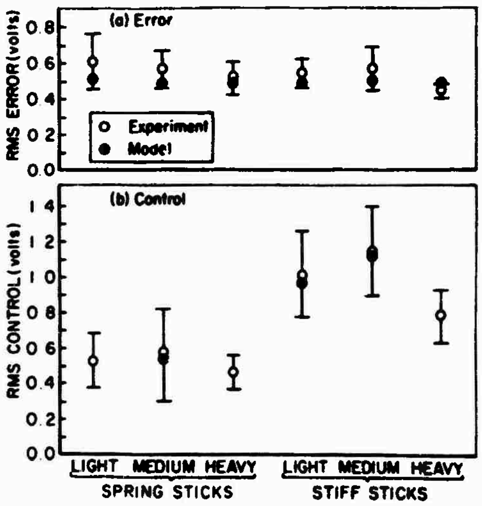


Fig. 19 - Theoretical prediction of errors by use of human operator model. (From reference 23)

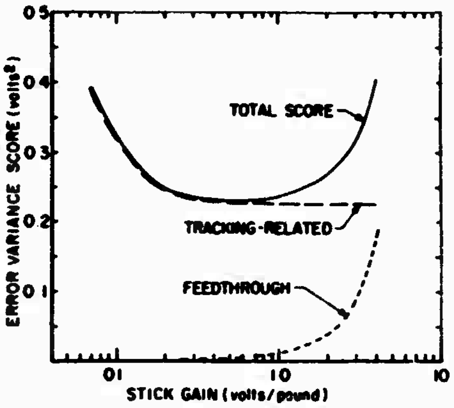


Fig. 20 - Output error versus stick gain for tracking performance under vibration (0.3 g_z rms). (From reference 23)

Summary and Conclusions

I hope this brief review of progress in biodynamics over the past two decades has illustrated the important advances made in defining and predicting man's response to shock and vibration environments. The emphasis of my remarks centered on the recent development of standards in this area since they indicate the practical application of the generally agreed upon results. These standards should be of interest to the designers of all transportation vehicles - water, land, air and space - and the designers of buildings as well as heavy machinery. My remarks did not do justice to the progress in basic biodynamics, the tremendous knowledge gained on the material properties of bone, tendons and all living tissue and the theoretical advances forming the foundations for the application of the modeling technologies I described. It is obvious, although it is unfortunately frequently not acknowledged these days, that the practical applications are only available and sound if they are based on firm and broad basic knowledge. The isolated pieces of knowledge of this technology base might have no direct practical application at all!

Most of the research results and standardization efforts I quoted originated from military requirements and R&D efforts. Many other programs profitted from these advances in biodynamics over the last two decades: The Manned Space Program of NASA; the Highway Safety Program of the Department of Transportation; the Occupational Safety Program of the National Institute of Occupational Safety and Health; programs of the Environmental Protection Agency; and, last but not least, biomedical engineering in general in support of clinical medicine.[24] There can be no doubt that even if old problems are still with us new problems stimulated and broadened the field. And the answers available today are far ahead of what we knew 20 years ago.

The reason for reviewing this information at this symposium is twofold: First, I think, the information and standards presented should be of interest to you and should be used by you, whenever man is exposed to shock and vibration environments. Second, it is important for you to realize that your technology, your measurement and testing techniques and your theories are being used in biodynamic research and that we are grateful for your contributions.

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THE RIVET GYRO STORY

by

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The objective of the Rivet Gyro Program is shown in Fig. 1. It is a combined AFSC/AFLC program to improve the operational reliability and lower the logistic support costs associated with Air Force systems, subsystems and equipment through maximum utilization of USAF organic resources. It is the last six words of that statement that make the Rivet Gyro Program different from other Air Force programs that have similar goals and objectives. Rivet Gyro means that when a job is to be done, it will be done by Air Force people using Air Force facilities and Air Force resources.

As shown in Fig. 2, the program started in May 1972 when General Ryan, Air Force Chief of Staff, indicated that he was dissatisfied with the low reliability and high logistic support costs associated with a certain aircraft's Inertial Navigation System. General Ryan's concern was reflected in a Program Management Directive to the Air Force Systems Command that not only said that the faults were to be found and fixed by Air Force people but a six-month, start to finish time constraint was imposed upon the program. What appeared in May of 1972 as a one-time challenge to our responsiveness and our in-house engineering capability has evolved into a program of major stature.

When a Rivet Gyro investigation is directed, 60 days are allowed to burrow into the problem and present a proposed plan of attack. From that point on, four months remain to complete the investigation and produce the final results.

As I mentioned earlier, Phase I of the program was an investigation of an Inertial Navigation System. It, like all subsequent phases, was completed on schedule and, as I will show you on a summary chart, it was demonstrated that the operational reliability of the system could be essentially doubled and that a logistic support cost in excess of 70 million dollars could be avoided.

Phase II, also directed by General Ryan, focused our attention on a Navigation Computer and a Scan Converter in one type aircraft and an Inertial Nagivation System in another type

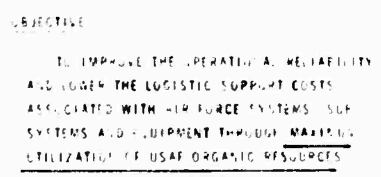

Fig. 1 - The Rivet Gyro Program - Objective

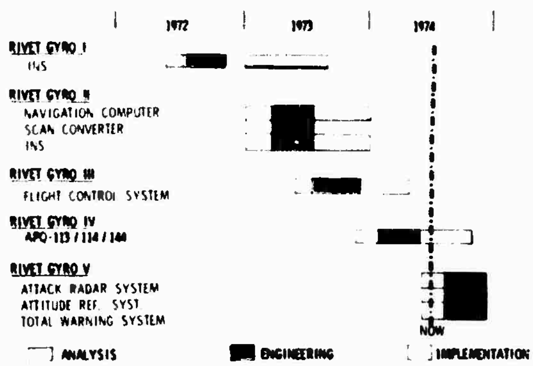

Fig. 2 - The Rivet Gyro Program Phases

aircraft. These investigations were conducted while simultaneously the Rivet Gyro Office was involved in directing the Air Force-wide implementation of the recommendations which had been accepted as a result of the Phase I investigation. The implementation phase of any Rivet Gyro investigation is the most important phase for it is obvious that it is not enough to know just what is wrong and what could be fixed. The payoff is when it does get fixed, and it takes just as much creativity, imagination and ingenuity to create a rapid upgrading of inventory capabilities as it does to find the faults and find the fixes.

Phase III came into being because the Commander, TAC, became concerned over what he defined as uncommanded inputs into an aircraft's Automatic Flight Control System that resulted in serious flight safety incidents and aircraft accidents.

Phase III was followed by Phase IV within which we undertook an independent look at the low reliability and high logistic support costs associated with an Attack Radar System.

Under Phase V, we are investigating a Terrain Following Radar, a two-gyro heading reference platform and an infrared tail warning system. We are on schedule and we will report the results of our investigation at the Air Staff on the first of December and from what I see at this point in time, the Rivet Gyro magic has worked again.

The key elements of any Rivet Gyro Program are diagramed in Fig. 3. They are to get reliability up and logistic support costs down. Our challenge is to do it on an exact fixed schedule that will not exceed six months and to do it with Air Force people and Air Force resources. It means that we must find the problem, find the fix and demonstrate the

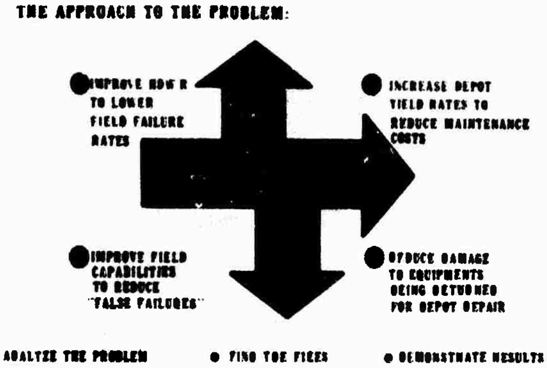

Fig. 3 - The Rivet Gyro Program - The Approach to the Problem

results which, if necessary, include the prototyping of hardware as well as ground and flight testing to validate our recommendations.

Our approach is to look at all possible factors that can contribute to low reliability and high logistic support costs. It means that we have to find out why the hardware fails and, with that information in hand, fix it so that the hardware being provided to the field units is as good as we realistically can make it. Quite consistently, we have found that the reliability of inventory equipment can be improved by upgrading the quality of the piece parts of which it is made. However, to get to that point, a lot of technical work goes on to isolate the principal piece-part offenders. Only through laboratory analysis of failed piece parts can we determine why the part failed, but with that information in hand, the fix may be very obvious.

We put technical teams into the field to try and understand the problems from the users point of view. One NCO talking to another NCO can produce valuable information on the problems we search.

Our field trips frequently disclose errors or deficiencies in maintenance technical data or the need to revise maintenance practices or procedures. When that is the case, we try to get the necessary revisions, in at least prototype configuration, into the field before our six-month clock runs out.

To make sure that nothing is overlooked, we even evaluate the adequacy of transportation packaging to make sure that the equipment is not damaged during transit. In five out of the six major investigations that we have conducted, AFLC packaging engineers have been able to improve upon the adequacy of existing packaging while at the same time lowering the cost of the packages.

We work very closely with the people in the Technological Repair Centers of AFLC. No matter what kind of business you are in, it sometimes helps to have an independent organization look over your shoulder. Sometimes amazing things happen. For example, AGMC was having trouble with air bubbles in repaired accelerometers. We said that we would take a look at their manufacturing procedures. We did and found nothing. However, the bubble problem has mysteriously gone away.

Not all problems are that easy to solve. For example, we became concerned about what appeared to be a high scrappage rate of very expensive Magnatrons used in one of our operational aircraft radar systems. Our initial thought was that perhaps, if the Magnatrons were bad, some part of them could be salvaged or the Magnatrons rebuilt cheaper than the price of new ones. It took over four months of very exacting work to uncover the fact that many Magnatrons were being scrapped, not because they were bad, but because of a very unique characteristic of the test equipment being used to check them at the depot level. It was erroneously indicating that they were bad. It took PhD talent borrowed from the Air Force Avionics Laboratory to really get to the bottom of the problem. In any event, our interest in looking at depot practices and procedures is to improve the effectiveness of their operations, thus hopefully reducing logistic supports costs.

If I may, I will use our investigation of the inertial navigation system, pictured in Fig. 4, as an example of the Rivet Gyro methodology. At the time that we initiated our investigation, there were over 2300 systems in the inventory with an average mean time between failure (MTBF) of 40 hours with an annual logistic support cost in excess of $24 million a year.

It is relatively easy to establish an MTBF model for that INS. Our model, shown in Fig. 5, indicated that the computer contributed the most to low system reliability while the depot repair of the inertial platform contributed the most to the annual logistic support costs.

It is relatively easy to derive from the Air Force Data System which subassembly within the LRU listed in Fig. 6, contributed most significantly to the unit's failure rate. Obviously, our attention focuses upon those subassemblies that were the greatest offenders.

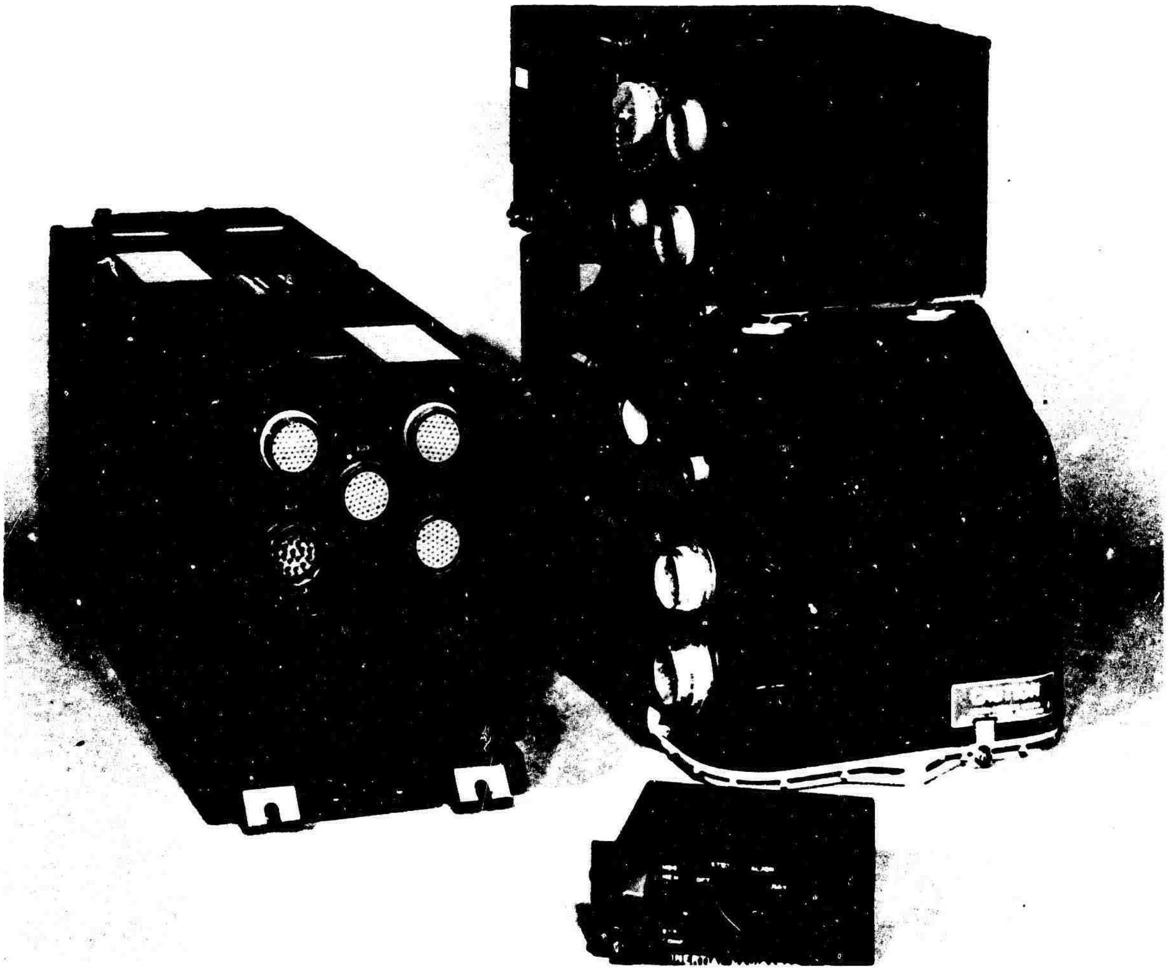

Fig. 4 - Inertial Navigation System

From the Air Force Data System it is also possible to find out down to the piece-part level within a subsystem which piece-parts fail most frequently. Such data are shown in Fig. 7.

It is at that point in the process that the real technical detective work starts.

In the case of the INS we found a significant number of piece-parts, such as cat-whisker diodes shown at the top of Fig. 8, which are technically obsolete by today's standards and can contribute to the intermittency of equipment under vibration. We also found many piece-parts that were being electrically overstressed because of high voltage transients present in the aircraft primary power system. As a matter of fact, we proved that by merely upgrading the quality of the piece-parts within the computer that it is possible to more than double its reliability. We also found that in terms of cost that it was cheaper to use currently available piece-parts of high quality than it was to continue to procure limited quantities of obsolete piece-parts.

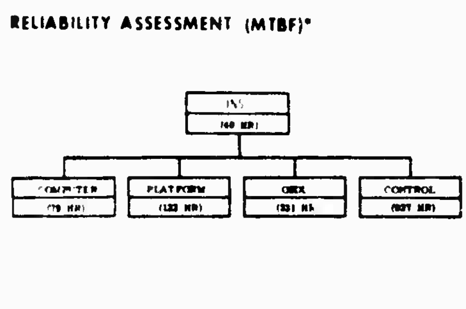

Fig. 5 - Rivet Gyro-Phase I - Overall Reliability Assessment (MTBF)

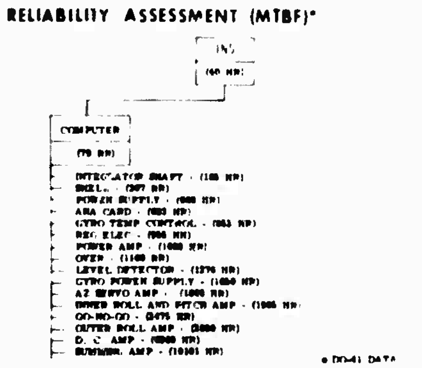

Fig. 6 - Rivet Gyro-Phase I - Computer Reliability Assessment (MTBF)

When we see variations in MTBF between Air Force Bases using identical equipment, we become very curious. These variations, illustrated by Fig. 9 , are classically driven by what we refer to as the people, paper and procedures problem. Unauthorized maintenance practices and unorthodox operational procedures can and do have a drastic impact on equipment reliability and logistic support costs. Our field investigations center on uncovering these deficiencies and correcting them. It is not infrequent to find that by merely improving operational crew debriefing procedures that a dramatic improvement can be made to field maintenance capabilities.

It is not unusual to find during our field visits that the field maintenance personnel have a need for test facilities that had not been provided to them to more effectively perform their field maintenance function. In the case of the INS several technical organizations had found the need for a system hot mockup and had designed and fabricated one of their own as shown in Fig. 10.

With this knowledge in hand and evidence of the value of a system hot mockup in the field, we designed and produced four well engineered prototype hot mockups as shown in Fig. 11, and provided them to technical organizations for a formal evaluation. The Rivet Gyro hot mockups have proven to be so effective in improving field maintenance capabilities and lowering logistic support costs that a total inventory quantity is being produced by Air Force personnel at Newark AFS.

As I said, in six out of seven of our major investigations, we have found that transportation packaging has been inadequate and costly. The inertial platform, as it was shipped from Newark AFS to the field, consisted of 13 discrete pieces (Fig. 12) which were frequently

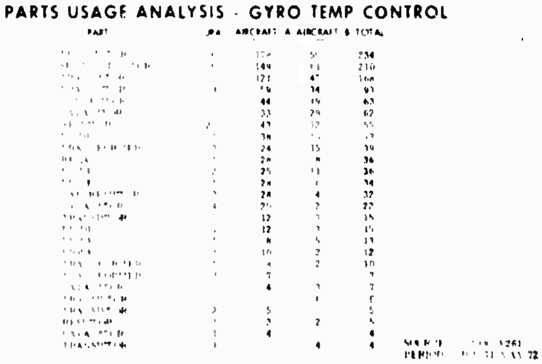

Fig. 7 - Rivet Gyro-Phase I - Parts Usage Analysis - Gyro Temp Control

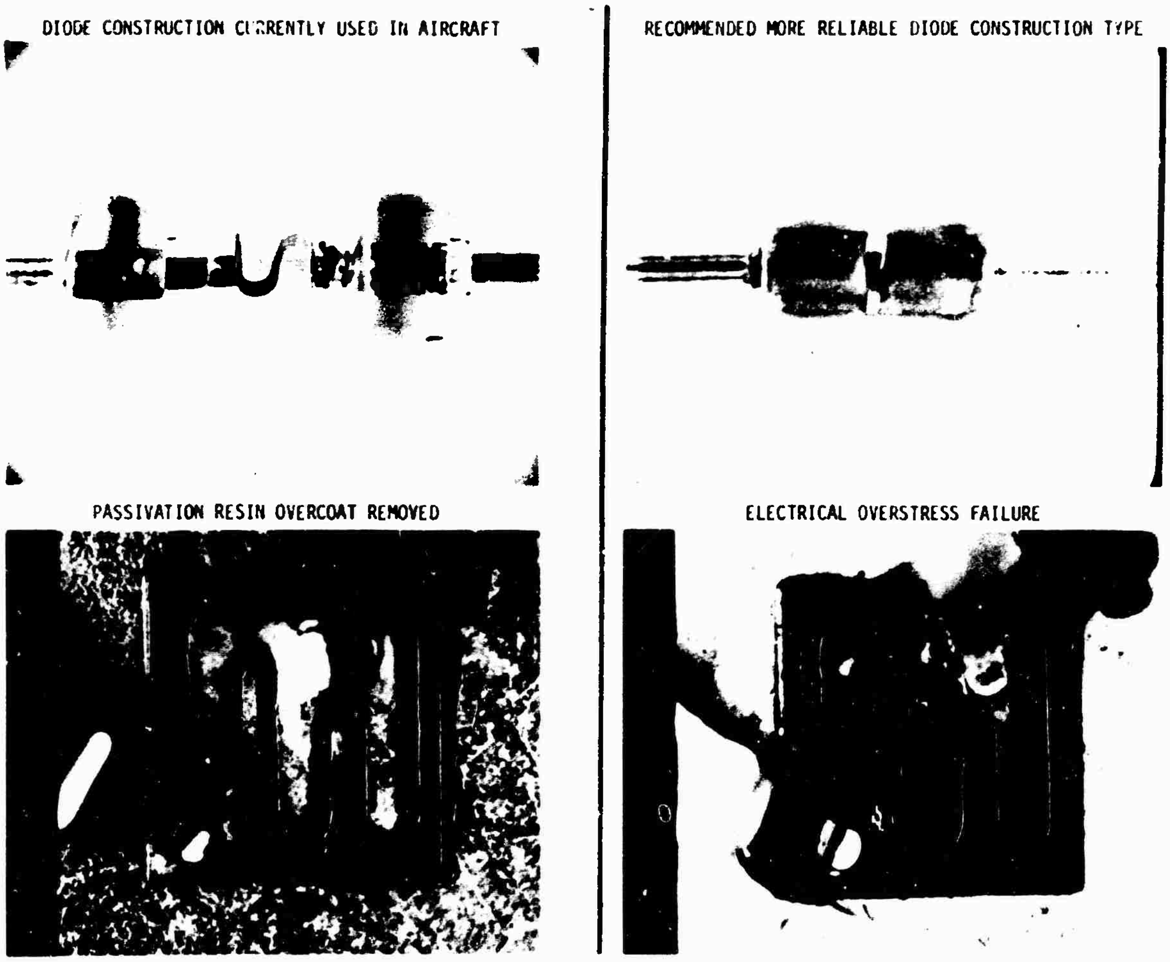

Fig. 8 - Piece Parts

lost, discarded or destroyed when the unit was received in the field. Therefore, the platforms, when they had to be returned to Newark for repair, were shipped in whatever container was available.

The Air Force Packaging Evaluation Agency of AFLC undertook the job of redesigning the packaging and as a result of their redesign effort, a unitized container shown being drop-tested in Fig. 13, is being used throughout the Air Force.

In addition to asking for data that through analysis takes us to the piece-part level of failure, we also asked that all failed piece-parts such as shown in Fig. 14, be forwarded to us for detailed failure mode investigation. During our investigations of one of the Inertial Navigation Systems, possession of failed piece-parts provided major clues as to the primary causes of system failure. For example, we found a significant number of

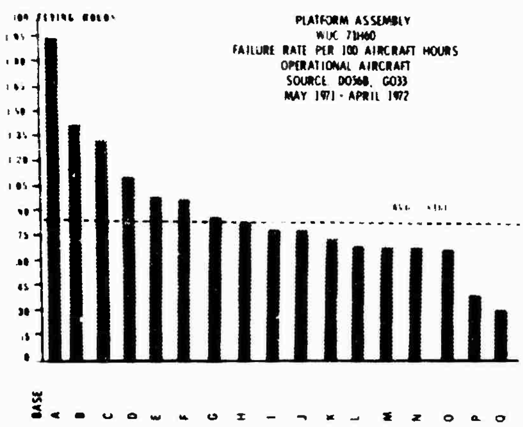

Fig. 9 - Failure Rates of Platform Assembly at Various AF Bases

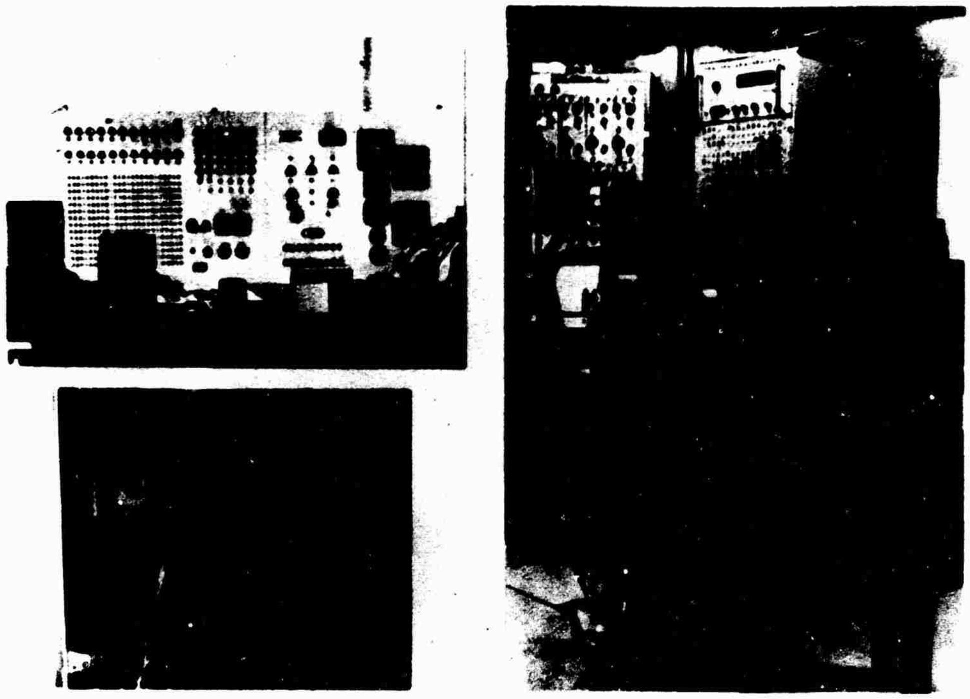

Fig. 10 - System Hot Mockup Developed in the Field

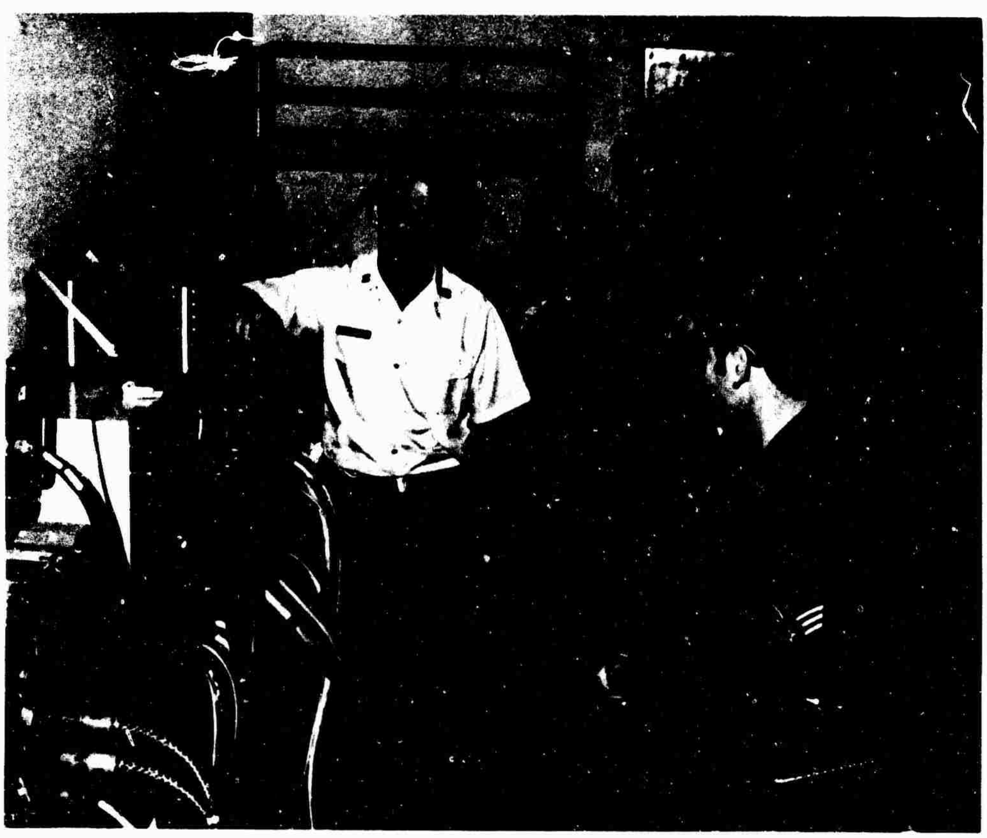

Fig. 11 - Rivet Gyro System Hot Mockup

Fig. 12 - Inertial Platform Packaging

Fig. 13 - Drop Test of Single Package for Inertial Platform

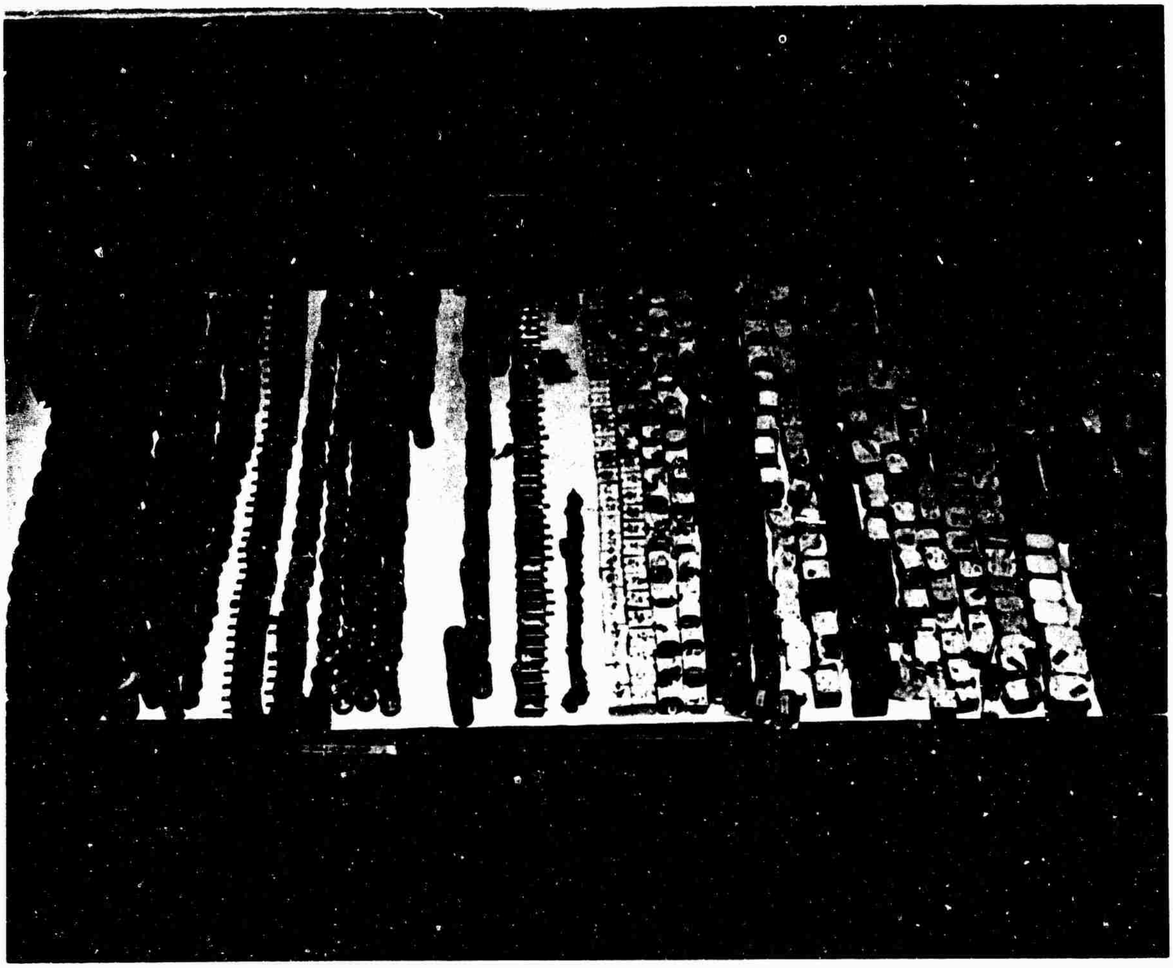

Fig. 14 - Failed Piece-Parts

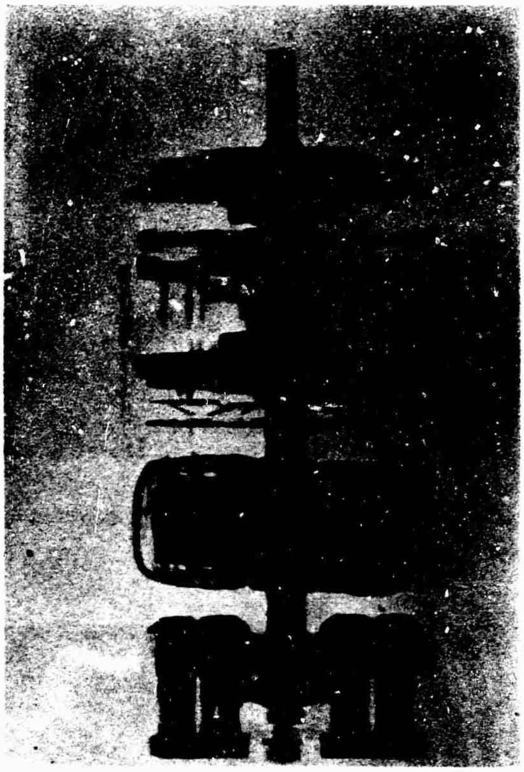

Fig. 15 - X-ray of A.C. Transformers

rotating AC transformers that had experienced premature failure in the field because of poor quality control at the vendor's plant, as shown in Figs. 15 and 16. To uncover this deficiency, we used the x-ray facilities of the Air Force Materials Laboratory. As a matter of fact, we used the x-ray technique very frequently so that the physical configuration of the article is preserved before we disassemble it for evaluation. We found a large quantity of cord wood electronic modules that had been removed and rejected on the depot repair line. In our attempt to determine which piece-part within the module caused the failure of the module, we uncovered the fact that better than 80% of the modules were totally functional. The cause of the problem lay in the practices being followed within the depot repair process. It was the practice of the repair activity to sequentially remove and replace suspect modules until the unit from which they were removed passed all performance tests. The removed modules were being discarded for the lack of a bench test capability to determine which of the modules that had been removed were in fact the offending item. The solution was very simple and highly cost effective. We designed a module tester and provided it to the repair activity for their use.

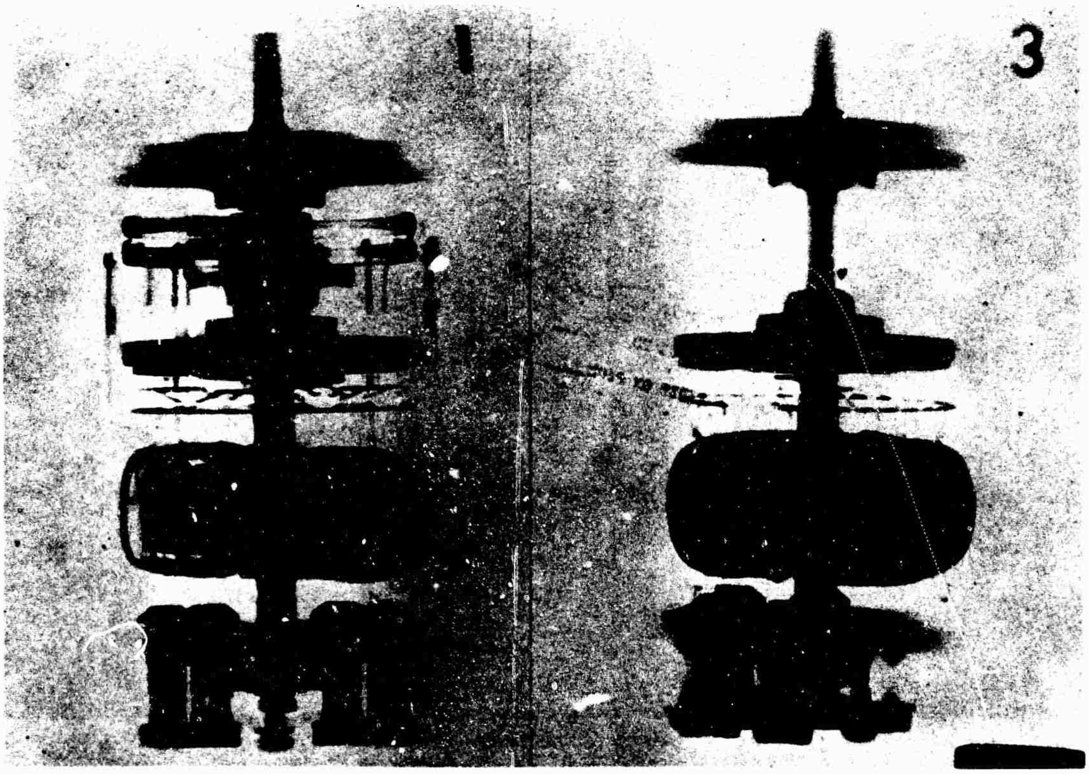

Fig. 16 - X-ray of A.C. Transformers

When we looked at several of the cord wood modules, we found that because of corrosion, the pin connectors no longer existed. A laboratory analysis indicated that the problem was caused by someone in the "total process" switching to acid core solder which, under high humidity conditions, produced hydrochloric acid. It was at this point in time that in a loud voice we said, "WE ARE GOING TO FIND OUT WHO IS USING ACID CORE SOLDER." To be honest, we never did find the culprit but our voice must have been heard because the problem has disappeared and the reliability of the unit in which the modules are installed has more than doubled.

We found great numbers of electromechanical devices, such as motors, synchros, and resolvers, as the only problem with the unit and the reason for its failure was the choice of the lubrication applied to the miniature bearings within the unit. We not only proved that the lubrication was wrong and that a better lubricant was available, but also that the failed units could be econonically reclaimed by merely replacing the failed bearings with bearings containing a proper lubricant. There is currently a new family of bearings lubricants that can extend the useful life of many of our small rotating devices by a factor of five to ten. It is now our task to try and convince the vendors who produce the articles that change to the lubricants is also cost effective for them. As a matter of fact, I was bluntly told by one miniature bearing manufacturer that he wasn't interested in improving the life expectancy of his article. He liked it the way it was. However, we did get his attention when we told him that we intended to share our information with his competitors.

When we conducted our investigation of the Navigation Computer, Fig. 17, as you would expect, we found a need to upgrade field maintenance and technical orders, to improve transportation packaging, and to eliminate low quality or obsolete piece-parts from the equipment itself. However, the major contributor to system failure was being caused by water that was entering the unit through the windows in the lighting plate on the face of the computer control box with the results shown in Fig. 18. The computer control box is located on the left-hand side of the rear cockpit. It is directly below a hole in the side rail which is part of the hatch latching mechanism. Whenever the canopy was open in the rear, the water ran through the hole and fell directly upon the face of the control unit. To my knowledge, there had been nine engineering attempts to design a method of keeping water from going through the hole. All were unsuccessful. The Rivet Gyro approach said let's assume that we can't keep the water from going through the hole so why not catch it on the underside. Our fix was a simple tin trough with a long plastic hose on the bottom.

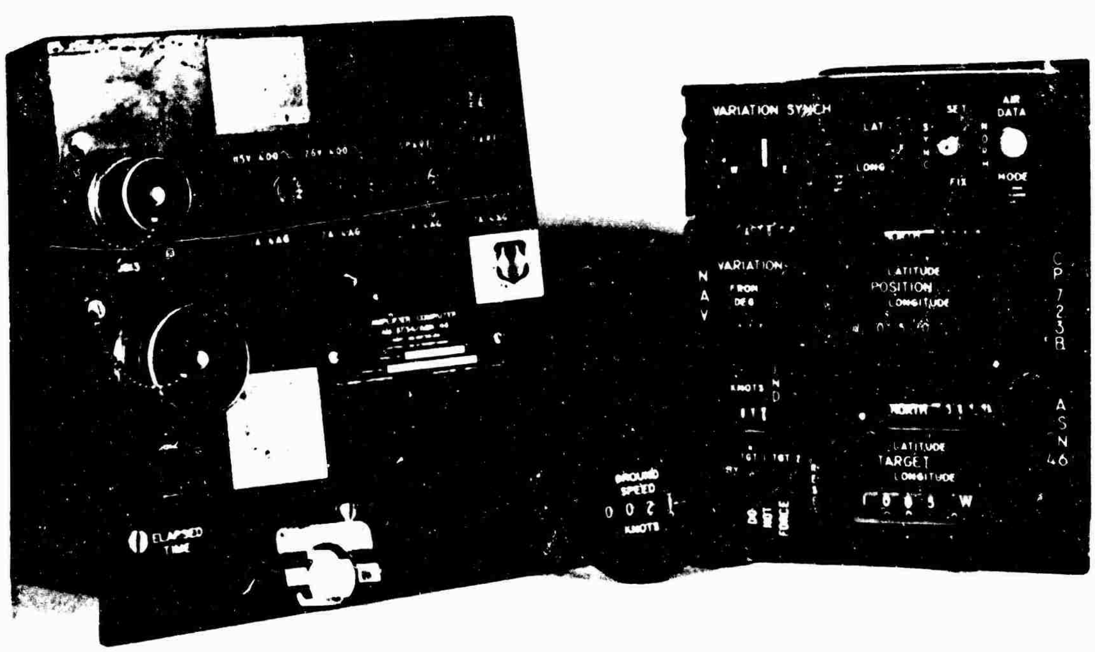

Fig. 17 - Navigation Computer

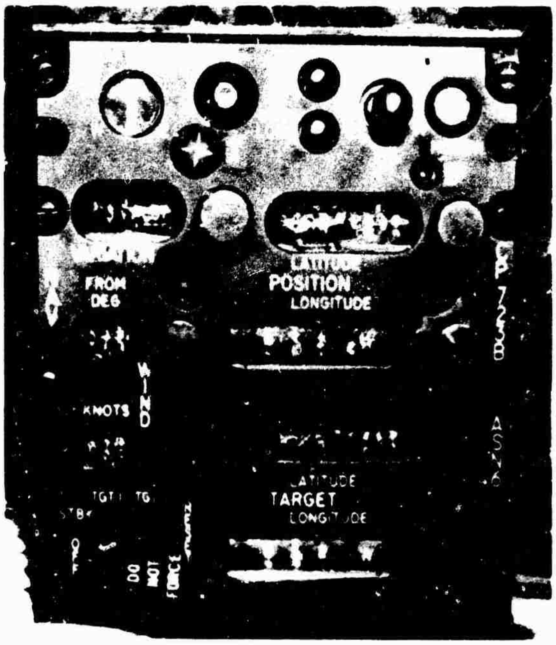

Fig. 18 - Effects of Water Penetration

It mounts directly beneath the hole on the side rail of the cockpit as shown in Fig. 19. When we showed our "fix" to TAC, they asked, with a smile on their faces, whether the tin trough was an example of ASD system engineering. Our answer was "Try it, you'll like it". TAC had only one other question and that was, "Where does the water go?". Our answer was, "Into the bilges of the aircraft where it has always gone. It will just no longer be filtered through the computer". TAC ran a six-month field evaluation of the ASD water diverter and, sure enough, it worked. At the present time, we are building a total inventory quantity within our local shops and the entire fleet will be modified in the next few months.

Our investigation of the Flight Control System was as difficult as any Rivet Gyro investigation that we have conducted. Technical order deficiencies and field maintenance errors were easy to uncover but they could not explain the cause for autopilot malfunctions. Through extensive laboratory analysis and evaluation, we found the three things shown in Fig. 20, each of which could

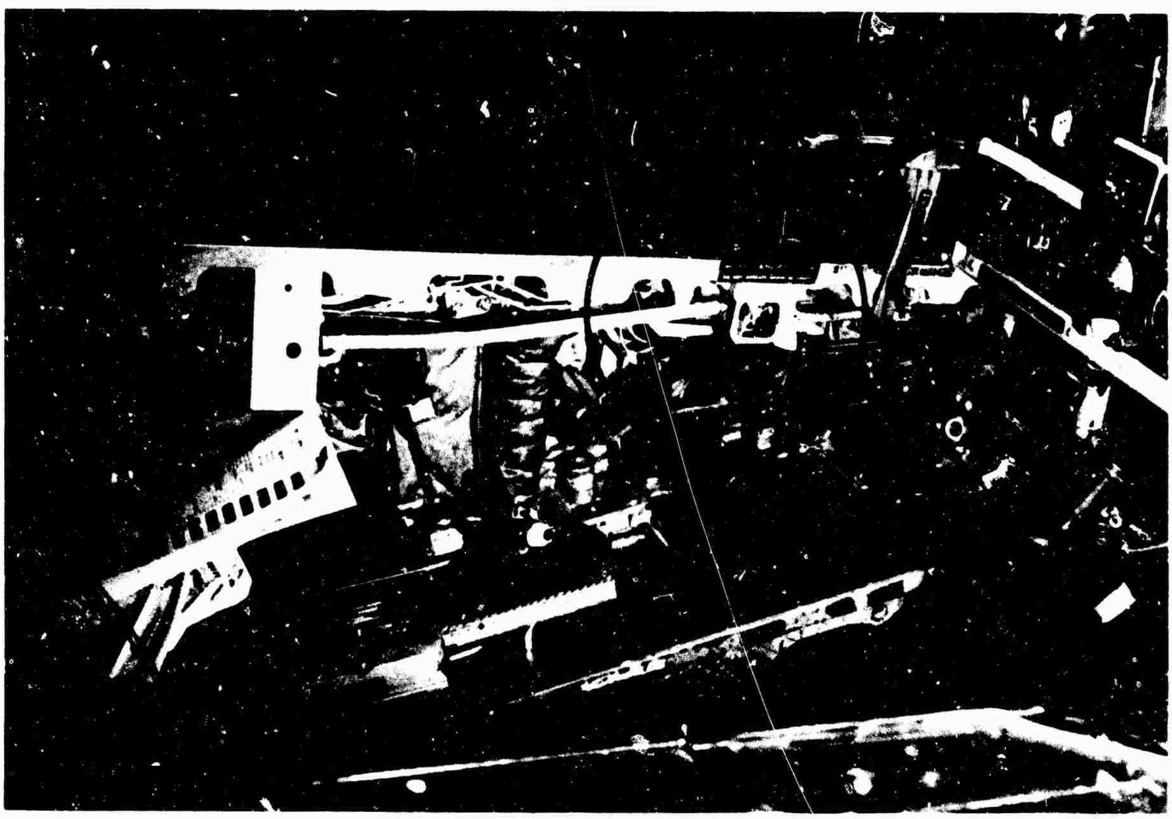

Fig. 19 - Rivet Gyro Water Diverter

explain one or more modes of system malfunction. We found a significant number of CL-65 wet-slug tantalum capacitors in the control amplifier which have the very nasty characteristic of periodically shorting internally and then curing themselves. A shorting capacitor at the right place in the amplifier can drive a flight control surface hard over. We found that the wrong lubricant was being used on the bearings in the spin axis of the rate gyro which was causing frequent spiking on the output of the rate gyro. This condition can also drive a control hard over. Our final finding was that there was inadequate particle filtration in the hydraulic system that fed the stabilator actuator. As a matter of fact, it was found that upon occasion that the filter that was in the system ahead of the stabilator actuator could go into a back-flush mode thereby dumping all of the entrapped contaminates into the system. The fix was obvious. It meant changing the currently installed filters with finer grain filters and eliminating the override bypass provisions of the old filter.

- "UNCOMMANDED INPUTS" CAN BE CAUSED BY -
    - TANTALUM CAPACITORS IN CONTROL AMPLIFIER
    - RATE GYRO SPIN AXIS BEARINGS
- "INADVERTENT AUTOPILOT ENGAGEMENTS" CAN BE CAUSED BY CONTAMINATION OF THE HYDRAULIC SYSTEM
- STABILATOR ACTUATORS HAVE AN UNEXPLAINABLY HIGH INFANT MORTALITY RATE

Fig. 20 - Flight Control System

We are currently working on an Infrared Tail Warning System. The system is located on the top of the aircraft's vertical stabilizer shown in a horizontal position on Fig. 21. One of our major investigations concerns itself

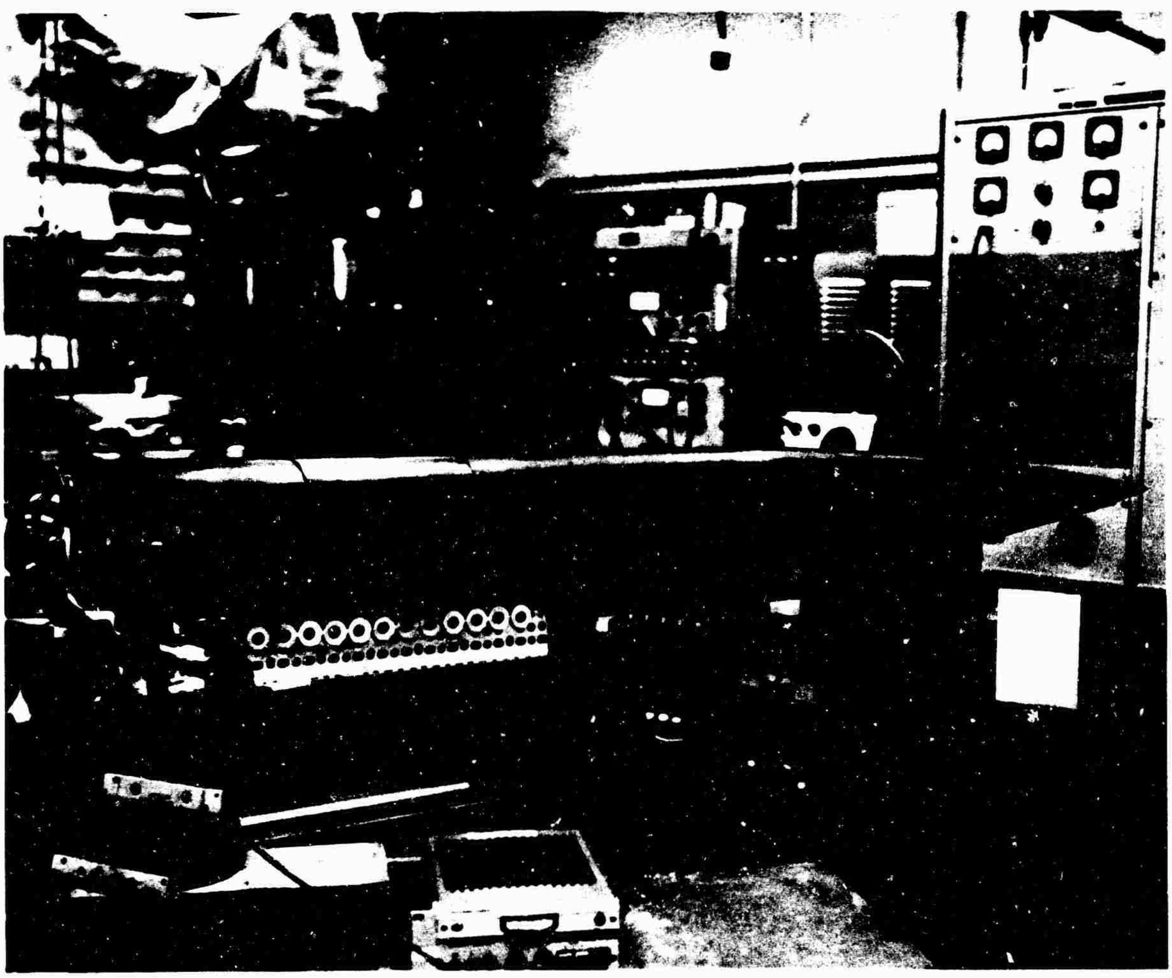

Fig. 21 - Infra Red Tail Warning System

	MTBF FROM	MTBF TO	10 YR LOG SUPPORT COST AVOIDANCE	DIRECT COST
PHASE I	40 HRS	70 HRS	70+ MIL (VAL.)	400 K
PHASE II				
	36 HRS	40 HRS	10 MIL (EST)	18 K
	26 HRS	40 HRS	26 MIL (EST)	30 K
SCAN CONV	INDETERMINATE			(95 K)
PHASE III				
FLT. CONT	(50% REDUCTION OF INCIDENT RATE)			(435 K)
PHASE IV				
	[illegible] HRS	52 HRS	10 MIL (EST)	200 K
		TOTAL	120 MIL	651 K

Fig. 22 - Rivet Gyro - Box Score

GOALS FOR FY 75

- CONTINUE TO INVESTIGATE HIGH BURNERS
- INITIATE NEW SYSTEM INVESTIGATIONS
- SPREAD THE RIVET GYRO CONCEPT THROUGHOUT THE PRODUCT DIVISIONS OF AFSC AND THE LOGISTIC CENTERS OF AFLC

Fig. 23 - Rivet Gyro - Goals for 1975-76

with really understanding the extremes of the shock and vibration environment that the equipment faces as it is flown on the aircraft. In our parts collection process, we have seen dozens of vibration isolators that have structurally failed. The Air Force Flight Dynamics Laboratory and Materials Laboratory are jointly conducting in-house investigation as to the cause of these failures. It appears at this point in time that the resonant frequencies of the scanner as mounted on the isolators in the fin cap match the frequencies of the bending vibration modes of the vertical stabilizer. Evidently, the isolators are not alleviating the vibration but making it worse. In fact, because there is no sway brace, the scanner has been impacting against the fin cap during flight. Furthermore, preliminary measurements obtained using digital model analysis techniques at the Air Force Materials Laboratory indicated that a proposed approach of stiffening the isolators does not appear to be feasible since the isolators would then become stiffer than the fin cap structure to which they are attached. The net result of all of this is that apparently since the isolators did not function very well at high frequencies, the vibration can best be reduced by hard mounting the scanner in the fin cap, thereby eliminating the failing mounts and improving the reliability of the system. Of course, we're continuing our laboratory tests out at Wright Field on this fix right now. It is one of the most dramatic, I think, examples of underestimating environment, misunderstanding it, not anticipating it, but having found that there was a deficiency, not moving out fast enough to correct it.

Why is Rivet Gyro important and why is it receiving attention, recognition and support at the highest levels within the Air Force? To me the answer is obvious. The "box score" for the first two and one-half years of Rivet Gyro operation, shown in Fig. 22, indicates that the reliability of systems can be improved, that logistic support costs can be reduced and that the Air Force has the organic capability to solve many of its problems in a minimum of time with a minimum of total investment cost.

A new Rivet Gyro Program Management Directive is being issued by Hq UASF which states the desire of the Air Staff: (1) to continue to address the logistic support "high burners" and develop a methodology than can be applied to our current development and acquisition programs, and (2) to expand the Rivet Gyro Program throughout the field elements of AFLC and the products divisions of the Systems Command.

AVIONICS RELIABILITY

LT COLONEL BEN H. SWETT
R&D Director, Analysis Division,
Deputy Chief of Staff for
Development Plans, Hq Air Force
Systems Command, Andrews Air
Force Base, Washington, D.C.

In July 1973, General Robert T. Marsh was DCS/Development Plans at Headquarters, Air Force Systems Command. He was given a briefing on a study of the A-7 aircraft. One of the findings was that black boxes didn't black box very well. At the end of that briefing, General Marsh asked the set of questions shown in Fig. 1, concerning not only the A-7, but the reliability of avionics equipment in general. From the last three of these questions, we inferred a major underlying question and assigned ourselves four study tasks.

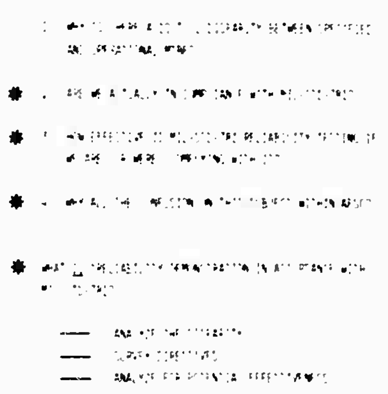

Fig. 1 - Avionics Reliability

We started out by taking a look at tne Military Specifications for various kinds of electronic equipment. These are given in Fig. 2. Each specification in the left-hand column of Fig. 2 contains a paragraph entitled "reliability", which doesn't really say anything — it merely refers to Mil Standard 454, Requirement 35, also entitled "reliability", which also doesn't really say anything. It refers in turn to Mil Standard 785. That document is largely organizational and procedural in nature. At the heart of it, it says, "For reliability predictions, see Mil Standard 756, and for reliability demonstrations, see Mil Standard 781."

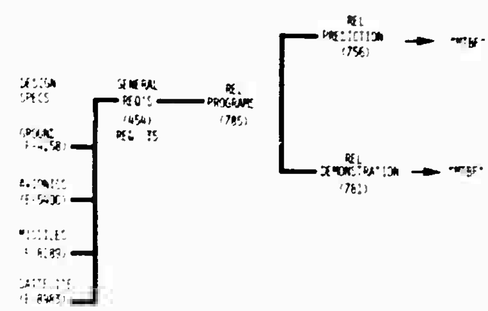

Fig. 2 - Electronics Reliability: Analysis of Directives

We took a look at those specifications. As indicated in Fig. 3. Specification 756 addresses the prediction of reliability, based on mathematical modeling. The predictions respond to the complexity of the equipment design, its expected usage, parts count and parts failure histories from such documents as Mil Handbook 217. We can largely ignore the last two bullets on this silde. They were part of the original study, but it turned out the correlation indicated here was a fluke. The reason we got a correlation was because somebody divided the predicted value by 10, as the entering argument for a reliability growth curve. The growth never happened, so the beginning of the growth curve correlated to the eventual field reliability, which was also one-tenth of the prediction.

- CALCULATIONS BASED ON:
  - COMPLEXITY OF THE EQUIPMENT
  - EXPECTED SYSTEM TYPE (AIRCRAFT, SPACECRAFT, GND-BASED)
  - PARTS FAILURE HISTORY (MIL-HDBK-217)
- PREDICTS: (A-7D AVIONICS)
  - OPERATIONAL MTBF X 2.04 (X .86 TO X 5.94)
  - TRUE MTBF X 1.05 (X .52 TO X 2.16)
- WHY ARE MIL-STD-756 PREDICTIONS MORE ACCURATE THAN MIL-STD-781 DEMONSTRATIONS?

Fig. 3 - Reliability Prediction (MIL-STD-756)

As shown in Fig. 4, Mil Handbook 217 contains parts failure rates, based on tests under thermal stress. It summarizes failure rates by part type, providing with each a set of environmental K factors designed to relate test results with field experience under various environments of use. These K factors multiply the basic failure rate — or derate the test MTBF, which means the same thing — "to account for failures caused by other than thermal stress." Mil Standard 756 summarizes the K factors by environment of usage. As you see, shipboard and fixed ground applications are approximately as failure-producing as the laboratory tests. Interestingly enough, so are satellites on orbit — not necessarily a benign environment, but a very predictable one. For manned aircraft applications, the K factor for all types of electronic parts averages 6.5 — which means the laboratory tests capture about 15 percent of the field failure rate for avionics parts. Missiles and satellites on launch and boost, of course, have a tremendous K factor, due primarily to the extreme shock and vibration regime during those phases of the mission.

- MIL-HDBK-217 CONTAINS
  - BASIC PARTS FAILURE RATES FROM LABORATORY TESTING UNDER THERMAL & ELECTRICAL STRESS
  - "K FACTORS" FROM FIELD FAILURE RATES USED TO MULTIPLY BASIC RATE "TO ACCOUNT FOR FAILURES CAUSED BY OTHER ENVIRONMENTAL STRESSES"
- MIL-STD-756 SUMMARIZES "K FACTORS"

SHIPBOARD, FIXED GROUND		X	1.0
MANNED AIRCRAFT		X	6.5
MISSILES		X	80.0
SATELLITE:	LAUNCH & BOOST	X	80.0
	ORBIT	X	1.0

- IMPLIES

  OPERATIONAL ENV (AVIONICS) = THERMAL & ELECT STRESS X 6.5

Fig. 4 - Reliability Predictions (2)

I would like to add one other comment to this list: the factor of equipment operating time. Shipboard and fixed ground equipment face their stress environment for a long time. So do satellites on orbit. But the tremendous shock and vibration that missiles face is primarily for 10 to 15 minutes. Aircraft, for which the K factor is not as large as missiles, face a very dirty, very complex, very failure-producing environment for a period of years. So it is my opinion that aircraft applications represent the worst case for electronics equipment, because they combine a tough equipment with long periods of operating time.

We'll take a quick look at Mil Standard 781. Figure 5 indicates that it is basically a statistical document. Now, there are numerous people both in and out of the reliability community who have assumed that reliability is a subset of mathematical statistics as a discipline. Therefore, to be a reliability engineer, one must first be a statistician. I'll have more to say about that in a minute, because it is part of the problem.

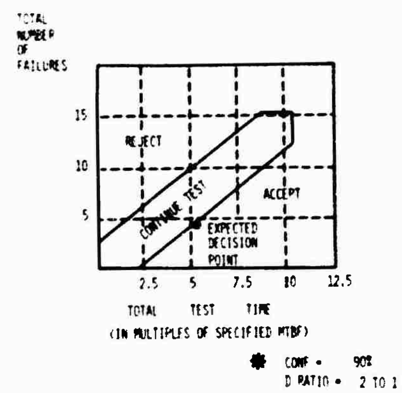

Fig. 5 - Mil-STD-781 "Test Plans" (1)

This is a test plan out of Mil Standard 781. It plots total number of failures on the ordinate, against total test time on the abscissa. As long as the demonstrated MTBF — that is, the ratio of total test time to total number of failures — remains between the lines, the test is continued. If and when it cuts the upper line, the equipment is rejected. If and when it cuts the lower line, the equipment is accepted as having passed the test. A very simple, straightforward sort of thing. I think even my copilot could have used it. However, when I encountered these test plans, they bothered me, because this is not a normal way to plot confidence interval calculations. So I replotted them several times, in several different ways.

Figure 6 is one of them. I plotted the ratio of demonstrated MTBF to specified MTBF on the ordinate, and total failures on the abscissa. Now, for those who are statistically inclined, there's about 15 minutes worth of text that goes with this slide. But for those not so inclined, the only point is over on the right side, where it indicates this test plan will accept any demonstrated MTBF greater than about two-thirds of the specified value. In other words, it will accept a third less than the specified MTBF.

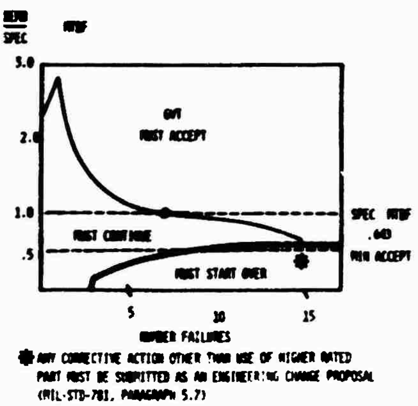

Fig. 6 - MIL-STD-781 "Test Plans" (2)

Figure 7 gives the test conditions called for by Mil Standard 781. Now, some of us isolated and naive staff officers at the Headquarters had assumed this Standard contained a menu of stresses which could be tailored for any given equipment. That is not exactly so. It calls for this same package of five stress types, and any tailoring is within the package. And anytime someone taiks about "781 stress levels," he's talking about thermal stress, in terms of temperature range.

Figure 8 is the 781 reliability test for avionics. It plots temperature range against chamber time. Down there, where it says, "-54°C," the equipment is turned on. It's heated with the chamber, hopefully at a rate of 5°C per minute, to the upper value called for by the contract. It's held there for two hours; the equipment is turned off, and the chamber is chilled back to the starting temperature. Vibration is applied for ten minutes per hour of equipment operating time. Input voltage is varied by tenths, in steps. And the cycle is repeated until the equipment is accepted or rejected by the statistical test plan. That is the 781 test.

- TEST CONDITIONS:
  - TEMPERATURE CYCLING
  - EQ ON-OFF CYCLING
  - INPUT VOLTAGE CYCLING
  - VIBRATION
  - EQ OPR TIME
- "STRESS LEVEL" = TEMP RANGE
  - "E" = -54 TO + 55c (-65 TO + 131F)
  - "F" = -54 TO + 71c (-65 TO + 160F)
- "TEST PLAN" = TEST TIME REQUIRED FOR STATISTICAL CONFIDENCE
- HOW REALISTIC ARE THESE TESTS?

Fig. 7 - MTBF Demonstration

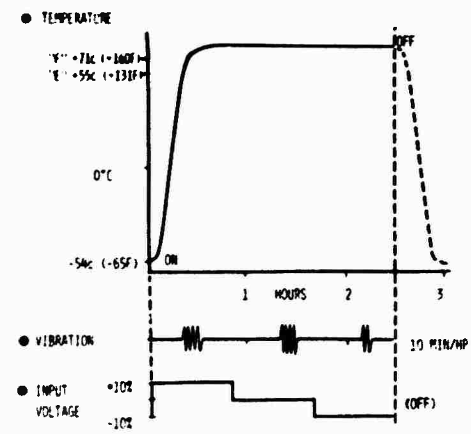

Fig. 8 - MIL-STD-781 Test Conditions/Cycle (1)

I wondered how realistic that thermal stress was, so I scratched around and found there were some people at the Air Force Flight Dynamics Laboratory who knew about such things.

I'll have to tell you a side story at this point. During one of the first times I gave this briefing, an individual in the audience stopped me at this point and said, "Colonel Swett, you're talking about reliability, aren't you?" I said, "Yes, I am." And he said, "Then what are you doing talking to the Flight Dynamics Laboratory?" I said, "Sir, I think your question is an indication of the problem I'm attempting to address."

On request, the FDL Combined Environments Group gave me some measured temperature data from the forward looking radar compartment of the A-7 aircraft, on desert, tropic and arctic mission profiles. These are shown in Fig. 9. They show that the 781 temperature range at least covered the extremes — and might in fact be an over-stress, since the majority of the time is spent in the waist of those measured temperature curves.

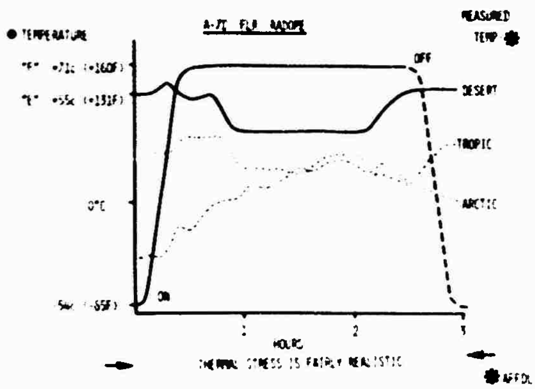

Fig. 9 - MIL-STD-781 Test Conditions/Cycle

Figure 10 shows the vibration stress called for by Mil Standard 781. It is the only one call for: 2.2 g_{rms}, single axis, sinusoidal vibration, at one non-resonant frequency between 20 and 60 Hz. And you'll recall from a previous slide that it is only applied for 10 minutes per hour of equipment operating time. The first time I briefed this at the Flight Dynamics Laboratory, five people in the audience suddenly burst into laughter at this point. Apparently, they knew what was coming next.

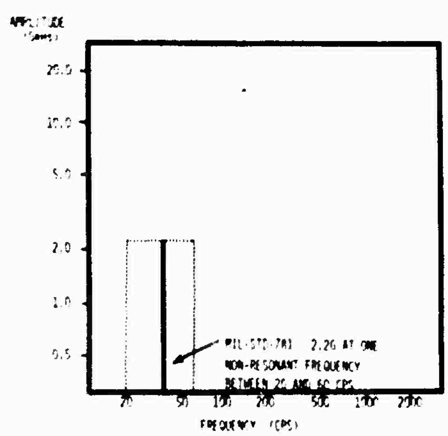

Fig. 10 - A-7D/E Vibration Levels: (1)

What happens when you overlay the measured vibration in that same compartment of the A-7 is shown in Fig. 11. Obviously, there are infinitely more frequencies and their harmonics operative in that compartment than the 781 test covers, any one or any combination of which could be producing resonant type failures.

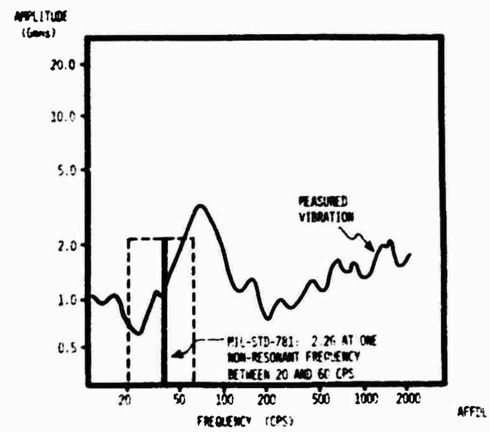

Fig. 11 - A-7D/E Vibration Levels: Forward Looking Radar

Figure 12 shows what happens when you fire the guns. You'll notice the neat multiples of 100 Hz produced by the M-61 Gatling gun in this aircraft. The conclusion of these slides is — and there is other experience data to back it up — that the Mil Standard 781 vibration is totally inadequate for avionics testing of any sort. And this leads us to an interesting observation.

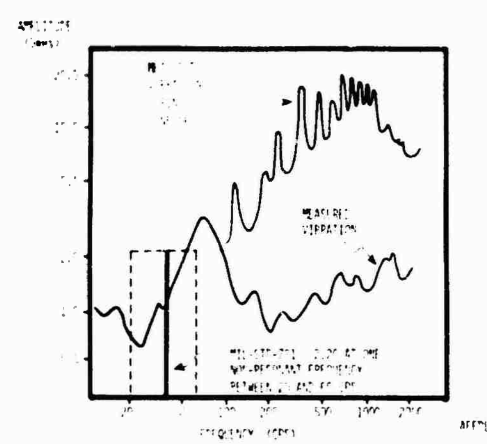

Fig. 12 - A-7D/E Vibration Levels: Forward Looking Radar

With the vibration test being found unrealistic, we see from Fig. 13 that 781 applies basically the same stresses as the laboratory tests used to produce failure rate data for Mil Handbook 217. On the right of the Figure, it indicates that both 217 and 756 failure rates must be multiplied by an average of 6.5 for avionics, to account for failures caused by stresses not applied during the test. But 781 does not derate its test results at all. Therefore, if the 6.5 holds, one could expect about 6.5 times worth of optimism in 781 test results. In other words, 781 is also capturing about 15 percent of the field failure rate for avionics.

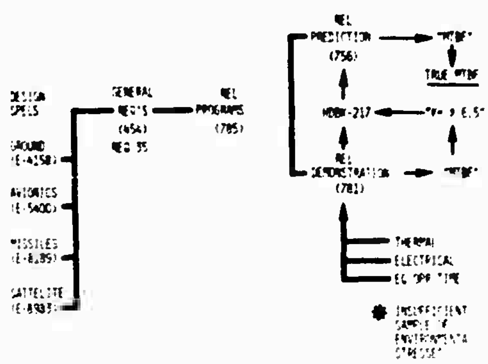

Fig. 13 - Electronics Reliability: Analysis of Directives

I have checked a number of other equipments, and I find a rather tight grouping, from about 5.9 to about 6.9, among correlations between actual failure rates during 781 tests and actual failure rates in the field. I need more correlations of this type, but that's what I have to date.

On Fig. 14, we turn from analyzing what 781 is, to an analysis of what it does. It says, "Mr. Program Manager, if you have equipment with any specified MTBF listed on the left of the slide, and a contract that calls for '781 Test Plan III,' the highest MTBF you can legally hold the contractor to demonstrate in the chamber is shown in the second column." That demonstration will be under conditions of thermal stress. Both Mil Handbook 217 and Mil Standard 756 say that demonstrated MTBF must be divided by about 6.5 for avionics applications. Therefore, the most you can legally expect to see in the field is as shown in the third column, labeled "true MTBF."

- WHAT DOES THIS ANALYSIS OF MIL-STD-781 "TEST PLANS" MEAN TO THE SPO?
- TEST PLAN III (A-7D AVIONICS):

SPEC MTBF CONTRACT	DEMO MTBF (.643 SPEC)	TRUE MTBF (DEMO / 6.5)	CHAMBER TIME (10.5 SPEC)
1500	964	148	93 MO (7 1/2 YRS)
1000	643	99	62 MO (5 YRS)
500	321	49	31 MO (2 1/2 YRS)
250	161	25	17 1/2 MO
125	80	19	9 MO

— BUILT IN —

- SPEC MTBF TRUE MTBF (FIELD) = 10.1 TO 1
- CHAMBER TIME = TRUE MTBF X 125 (MORE, IF REJECT AND RETEST)
- SPO'S HANDS TIED WHEN CONTRACT IS SIGNED.

Fig. 14 - MIL-STD-781 "Test Plans" (3)

If you will look at the point where specified MTBF is 1000 hours, and come across to an expectable true MTBF in the field of 99 hours, you can see where the typical 10-to-1 disparity between specified MTBF and field MTBF comes from. It is built into Mil Standard 781.

However, we use specified MTBF, via the test plans, as the primary basis for total test time. So, if we try to increase field MTBF by simply increasing specified MTBF, we wind up driving total test time as shown at the right of the slide.

Thirdly, and perhaps most importantly, is the predeterminism inherent in Mil Standard 781. These outcomes are locked in from the time a contract calling for 781 is signed. The SPO cannot control test results, because total test time and accepted MTBF are determined by the frequency of "relevant" failures during the test. So he cannot control program schedule, either, to the degree that it is dependent on 781 test completion.

You can recall from Figure 1 that General Marsh asked (question 2) whether we were complying with 781. Being an efficient, and therefore lazy staff type, I didn't have time to survey the Command, but our own supplement to the Air Force reliability regulation (AFR 80-5) was indicative. As shown in Fig. 15, it specifically requires compliance with 781 for production avionics for manned aircraft. It defines "munitions reliability" as "probability of success" rather than MTBF, thus neatly excusing the munitions people. Then it added the escape clauses listed below. "Exempts." Now, if you look at the first two of these, it says, "Items with low production quantity (how low not stated), or high MTBF (how high not stated), are excused.'' If you read between the lines, you can see the word SAMSO. This was one of the ways that SAMSO used to opt out of 781-type reliability testing about 8 or 9 years ago. And if you recall that missiles and satellites on launch have a K factor of 80, you can see why it is most appropriate for them to do so.

AFR 80-5, RELIABILITY PROGRAMS, AFSC SUP 1, AUG 1969

- REQUIRES COMPLIANCE FOR "COMPLEX ELECTRONIC EQUIPMENT FOR MANNED AIRCRAFT SYSTEMS"
- DEFINES "TRANSITIONS RELIABILITY" SO AS TO EXEMPT FROM 781
- EXEMPTS
  - -- LOW PRODUCTION QUANTITY
  - -- HIGH MTBF
  - -- EXPLORATORY DEVELOPMENT
  - -- ADVANCED DEVELOPMENT
  - -- SPC DEVELOPMENT
  - -- PROTOTYPES
  - -- RPV's

- WHERE IS THE RIGOROUS TESTING WE KNOW WE ARE DOING?

Fig. 15 - MIL-STD-781 Compliance

At this point in the study, we felt we had an answer for our underlying question, ''What is testing in accordance with Mil Standard 781?'' It seemed to boil down to something like this: ''Take production avionics equipment for manned aircraft, and cook it for a long time,'' which made us wonder where all the other testing was.

We went back to the design specifications and, of course, as you're all well aware, those three Mil Specs have a major paragraph entitled, ''Scope of Tests.'' It reads, ''Compliance with this specification requires environmental testing in accordance with MIL-STD-810, Environmental Test Methods.'' And, lo and behold, as shown in Fig. 16, there are all the other stresses. The Spec for ground electronics defines its own test conditions, but the way the other three are written makes it quite apparent these environmental tests predate the attachment of the reliability world, which is grafted on by the addition of the reliability paragraph. And there is something else here that I think is extremely significant. There is really no cross-referencing between these two families of Military Standards. There is one requirement in Mil Standard 454 that says, ''For fungus testing, see Mil Standard 810,'' but other than that, there is no cross-referencing. They are two absolutely independent sets of Military Standards.

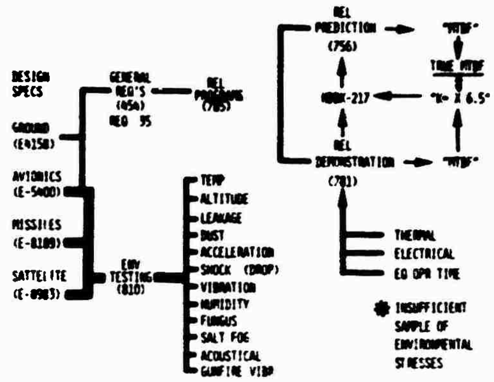

Fig. 16 - Electronics Reliability: Analysis of Directives

I understand there's going to be a hot and heavy panel session on 810 later in this symposium, but Fig. 17 will give you an idea of what I thought when I read it. I said, ''Yeah, those look like a fair sample of the stress types, but where is the equipment operating time?'' Apparently, equipment operating time was not conceived as a stress when 810 was written. Now, since you are in the vibration area, you know that failures caused by vibration are heavily dependent on exposure time. That goes for aircrews, too. A person doesn't go deaf the first time he rides in a B-47, but after a couple thousand hours, the effect takes place.

- CHAMBER TESTS BASED ON:
  - FAIR SAMPLE OF OPERATIONAL STRESSES
  - LITTLE EQ OPR TIME (MAX ≅ 24 HRS)
  - VISUAL INSPECTION + GO/NO GO CHECKS
  - SEQUENTIAL RATHER THAN COMBINED ENVIRONMENTS
- PREDICTS ? (NO "MTBF" OUTPUT)
  - "COVERS FACTORS THAT CAUSE 50 TO 40 % OF FIELD FAILURES" (FDL)
  - IMPLIES: $\frac{\text{810-TYPE TESTING}}{(2.5 \text{ TO } 3.3)}$ + TRUE MTBF

Fig. 17 - Environmental Testing (MIL-STD-810)

Just to digress a little, a couple of years ago, I went in for my annual physical, and there was an old medical technician running the audiometer check. He bet me a six-pack he could tell me which airplane I had spent most of my time in. Well, I couldn't pass that up. So he ran his audiometer check, studies the results, and said, ''That's easy. You're a B-47 type.'' I asked him how he knew, and he replied, ''You're stone deaf at 4000 Hz.'' That was the center frequency of the J-47 engine.

I found it amazing that, in almost all instances, the equipment is submitted to the stress while turned off, returned to ambient conditions, and then given a functional check or a visual inspection. This might be appropriate for stresses it will see in storage, transport, or ground handling, but not for the stresses it sees in its operating environment.

810 applies most stress separately, in sequence, rather than in combinations. This weakens the overall test by allowing some failure mechanisms to escape undetected. However, some of the more recent test methods, such as the temperature-altitude-humidity combination called for by Test Method 518, reflect greater awareness of this, and are designed to correct it.

How do 810 test results correlate to field reliability? There is no way to tell, because the equipment is not operated long enough to provide failure rate data. I've seen some studies that attempted to make such a correlation, but there isn't much basis for comparison. When this was first briefed, some people in the Headquarters were astonished, because they had thought all the shake, rattle and roll testing we do somehow had something to do with reliability.

Figure 18 shows the 810 test methods applicable to avionics, the chamber time required for each, and the equipment operating time, where "spot check" means 15 minutes or less. It occurred to us that this lack of sufficient operating time was probably the specific gap in then-current test procedures that 781 was designed to fill. Unfortunately, 781 was wired on the side, instead of being incorporated into the mainstream of the program. And then it drifted into being considered a subset of statistics, as a discipline.

TEST	TIME	
METHOD	CHAMBER	EQUIPMENT
ALTITUDE	3 HRS	SPOT CK
HI TEMP	38 HRS	SPOT CK
LO TEMP	AS SPECIFIED	SPOT CK
TEMP SHOCK	24 HRS	SPOT CK
TEMP-ALT	50 HRS	24 HRS
SUNSHINE	120 HRS	SPOT CK
RAIN	2 HRS	SPOT CK
HUMIDITY	792 HRS	6 HRS
FUNGUS	672 HRS	SPOT CK
SALT FOG	96 HRS	SPOT CK
DUST	28 HRS	12 HRS
EXPL ATMOS	N/A	N/A
LEAKAGE	2 HRS	VIS CK
ACCELERATION	5 MIN	3 MIN
VIBRATION	3 HRS	3 HRS
ACOUSTICAL NOISE	1/2 HR	SPOT CK
SHOCK (DROP)	N/A	VIS CK
SPACE SIMULATION	24 HRS	AS SPECIFIED
TEMP-HUMIDITY-ALT	95 HRS	SPOT CK
GUNFIRE VIBRATION	4 HRS	4 HRS

Fig. 18 - Environmental Test Methods (MIL-STD-810)

Now let me step out of the briefing for a second. In my opinion, reliability is not a subset of statistics. Reliability is equipment performance, over time or attempts to operate, under realistic operational stress. Perhaps I can say it to this audience without being stoned. It seems to be that statistics, like many other mathematical techniques, makes a very good servant, but a very poor master. But in the reliability world, statistical considerations have become the master.

Figure 19 is what our original staff study wound up saying to General Marsh, and then, at his request, to a lot of other people. Now I would like to address the third bullet under item one.

1. THE 10 TO 1 DISPARITY BETWEEN "SPECIFIED" AND "OPERATIONAL" MTBF.
   - IS NOT PRIMARILY DUE TO THE OPERATIONAL REPORTING (AFM 66-1)
   - IS BUILT INTO MIL-STD-781
     - 6.5 TO 1 FROM TEST CONDITIONS
     - ≈2 TO 1 FROM TEST PLANS
   - ANY DISPARITY LESS THAN 10 TO 1 MEANS THE CONTRACTOR DELIVERED HIGHER MTBF THAN REQUIRED.
2. ONLY AVIONICS ARE REQUIRED TO COMPLY WITH MIL-STD-781 OTHER ELECTRONICS PRODUCT LINES USE:
   - MIL-STD-756 PREDICTIONS, WITH FEEDBACK
   - MIL-STD-810 ENVIRONMENTAL TESTING EXPANDED SO AS TO PROVIDE RELIABILITY DATA OUTPUT

Fig. 19 - Findings (1)

We have found numerous cases where a contractor delivered more reliable equipment than he legally had to. After this briefing came out, one contractor representative came to my office quite disturbed, and said, "I hear that you're going around throwing rocks at the avionics contractors." I said, "Not at all. I'm throwing rocks at the Military Standards." He said, "Well, we built one of the pieces of gear in the A-7. We delivered more reliability than we were required to, and we knew at the time we were doing it, and we took a financial loss in the process."

He got his data together, and I got mine, and I checked it out. He was right. His company had delivered about 2.7 times the MTBF that would have passed all the tests. Why did they do it? Future sales potential, corporate image, that sort of thing. And it did cost the company a large number of bucks. I felt it shouldn't have.

Finding number three (Fig. 20) was not universally popular in the Command. But from the foregoing study, we couldn't really say anything else. Figure 21 expands on the last item of the preceeding Figure.

5. MIL-STD-781 TESTING WOULD NOT BE AN EFFECTIVE DEMONSTRATION OF AVIONICS RELIABILITY IF IT WERE FULLY COMPLIED WITH, BECAUSE

- BUILT IN DISPARITIES LIMIT TEST EFFECTIVENESS
- STATISTICAL DETERMINISM DRIVES TEST TIME AND IMPACT COST OF PROGRAM DELAY
- LIMITING TEST TIME DRIVES TEST RESULTS TOWARD MEANINGLESSNESS
- NO MANIPULATION OF "SPEC MTBF", "D RATIO", OR CONFIDENCE LEVEL WILL CORRECT THE 6.5 TO 1 DISPARITY DUE TO TEST CONDITIONS

6. THE CONFUSION ON THIS SUBJECT WITHIN AFSC

- IS NOT MERELY DUE TO SEMANTICS
- THE PROBLEM IS BASICALLY INSTITUTIONAL

Fig. 20 - Findings (2)

"RELIABILITY" (781) AND "ENVIRONMENTAL TESTING" (810) ARE TWO SEPARATE WORLDS WITHIN AFSC.

THEY ARE SEPARATED BY

MIL SPECS AND STANDARDS
AIR FORCE REGULATIONS
ORGANIZATIONAL STRUCTURE
PRODUCT TYPE
VIEWPOINTS, ATTITUDES & TERMINOLOGY

RELIABILITY (781) IS:	ENVIRONMENTAL TESTING (810) IS
WELL ORGANIZED	LEFT TO SPO'S AND AFCMD
SIDELINE TO EQUIP DEV	MAINLINE OF EQUIP DEV
STATISTICS	SPEC COMPLIANCE
UNREALISTIC TEST CONDITIONS	UNREALISTIC TEST PROCEDURES
POOR PREDICTOR OF TRUE MTBF	NO PREDICTOR OF TRUE MTBF
(OPTIMISTIC BY 10 X)	(NO RELIABILITY DATA OUTPUT)

Fig. 21 - Problem

I believe the fundamental problem is institutional, rather than technical. The reliability world, as symbolized by 781, and the environmental testing world, as symbolized by 810, have been institutionally isolated within the Command and, as it turns out, within the contractors own organization and elsewhere as well. They are separated by the Mil Specs and Standards, by our own regulations, and by our organizational structure. They are appropriately separated by product type – you have to take a different approach for different usage environments. But perhaps most significantly, they are separated by the viewpoints, attitudes and terminology of the people who inhabit these two worlds. As my immediate supervisor once mentioned, reliability people tend to be "ethnically statisticians."

The reliability world is basically well organized. We have reliability-maintainability focal point throughtout the Command. We do not have a similar structure for environmental testing.

Reliability is, like it or not, a sideline to the main thrust of equipment development, somewhat as the Chaplain is often a sideline to a staff meeting.

Reliability is based on statistics; environmental testing is based on spec compliance.

Reliability applies unrealistic test conditions; environmental testing applies unrealistic test procedures.

Poor correlation to field reliability, and no correlation.

We feel it is between these two stools that avionics reliability has presently fallen, and that this is a major reason why we of the development community provide the embarrassing opportunities which keep our friend Jack Short so dramatically employed.* Jack's got one of the best shows in town, as you've just seen and heard. But from the developers' point-of-view, his success is embarrassing, because he has solved a lot of problems we should have caught before he ever had a shot at them.

When we briefed this first phase of the study to General Marsh, he said, "Good. Now see what you can find out about putting these worlds together." So I tried. And that became the second phase.

The first thing I did was to say, "We've got to do something about those statistical test plans; they're driving the program managers crazy." What I did was to come up with a new kind of statistical test plan (Fig. 22) that cuts test time, and controls it, without sacrificing statistical confidence.

*Short, John E., "The Rivet Gyro Story," invited paper, 45th Shock and Vibration Bulletin, Vol. 1.

- REDEFINE "SPEC MTBF" • PRESENT "MIN ACCEPT MTBF"
  ELIMINATES "DISCRIMINATION RATIO" WHICH
  - IS DESIGNED TO LIMIT PRODUCER'S RISK
  - INCREASES TEST TIME AND COST
  - CAUSES SEMANTIC CONFUSION
- LIMIT MAXIMUM GVT RISK (TIME, COST, AND ACCEPTED MTBF)
  - ACCEPT/REJECT ON TIME TO N TH FAILURE (DEMO MTBF)
  - ALLOW CTR RISK (AND GVT RISK BELOW LIMIT) TO VARY WITH MTBF ACTUALLY DEMONSTRATED DURING TEST
  - STRUCTURE FINANCIAL INCENTIVES TO REWARD CTR FOR HIGHER DEMO MTBF, LOWER GVT RISK (% FEE, UNIT PRICE)
- ALLOW CTR TO SELECT HIS OWN DESIGN POINT (756 PREDICTION) ACCORDING TO THE REJECTION RISK HE IS WILLING TO ACCEPT
  - COMPETE DESIGNS THRU QUAL TEST
  - DEVELOP AVIONICS INDEPENDENT OF AIRCRAFT PROGRAMS
  - "REJECT" = GVT OPTION TO DECLINE PURCHASE
  - SAMPLING NO LESS STRINGENT THAN QUAL TEST (+/- DEMO MTBF = +/- % FEE, PROGRESS PAYMENTS)

Fig. 22 - Equipment Level DT&E Program (Proposed): Standard Test Plans

Figure 23 puts three things that are lying around together. We test avionics performance under room-ambient conditions. We test reliability under conditions that capture about 15 percent of the field failures. We apply a variety of environmental stresses, but without operating the equipment enough to test either performance or reliability.

- COMBINE PERFORMANCE, RELIABILITY, AND ENVIRONMENTAL QUALIFICATION TESTS INSOFAR AS PRACTICAL
  - SELECT STRESS TYPES AND LEVELS
  - DESIGN PROCEDURES TO MAXIMIZE DATA
- CANCEL TESTS SUBSUMED BY COMBINATION
- APPLY COMBINED-STRESS TESTING FOR
  - PARTS RATING
  - EVALUATION TEST
  - QUALIFICATION TEST } PROTOTYPE
  - SCREENING ("BURN-IN")
  - PRODUCTION SAMPLING } PRODUCTION

Fig. 23 - Equipment Level DT&E Program (Proposed): Test Conditions and Procedures

Then any of the present, separate tests we have combined should be cancelled to prevent duplication.

And the more realistic, combined performance/reliability/environmental test should become the backbone of equipment development and contract compliance, both by levels of assembly and by phases of the acquisition process.

In Fig. 24, the stress types listed at the upper left can generally be combined in the same chamber. If you start with the temperature/altitude/humidity chamber from Mil Standard 810, Test Method 518, you can add the random vibrator, and in some cases, the acoustic horn. If you start with the typical 781 chambers, you can add all these stress types except altitude — which has a tendency to implode the chamber and distress the neighborhood.

STRESS TYPE	STRESS LEVEL	ENV K FACTOR	
THERMAL CYCLE	-54°C TO +65°C (5°C PER MINUTE)	6.5	1.85*
RANDOM VIBR	.2G²/CPS, 20 TO 2000 CPS	2.5*	
INPUT VOLTAGE	DESIGN VALUE +/-10%, PLUS TRANSIENTS	?	
HUMIDITY	95% AT 0 ALTITUDE AND +15°C	?	# TEST
ACOUSTICAL	140 DBL, 2000 TO 20,000 CPS	?	
ALTITUDE CYCLE	0 TO 55,000 FT (2500 FT/MINUTE)	?	

PLANNING FACTORS	1.5 (1.2 TO 1.8)*
COST TO ADD RANDOM VIBRATOR	$ 90K TO $100K EACH*
CHAMBER OPERATING COST	$175 PER HR (INCLUDING OH)*
EQUIP TEST TIME	270 HRS PER MONTH*
CHAMBER/EQUIP TEST TIME	×1.85

* COMPILED FROM CTR EXPERIENCE

Fig. 24 - Equipment Level DT&E Program (Proposed): Test Conditions

The stress levels shown are arbitrary — just a grab for the middle of the distribution of known avionics stresses. General Phillips requested that, at this point, never to fail to make the following announcement: "Those who are not to be considered as the standard stress levels for avionics." He went on to say that what he wants to see is something like 110 or 120 percent of the measured level for each stress type, for that equipment, in that location, or the same percentage of a defendable stress prediction. He said, and I quote, "Let's do it right one time, shall we?" If you know General Phillips, you know that the more slowly he speaks and the more carefully he enunciates, the closer he is to killing somebody. So I didn't think he was kidding.

In the upper right corner is the 6.5 K factor for thermal stress. Below that is a 2.5 K factor for random vibration. I was able to find a contractor who had tested some late production samples of equipment already in the field, under that level of random vibration. He obtained 40 percent of his field failure rate, and he also got 80 percent of the workmanship defects in 10 minutes — you know, cold solder joints, loose screws backing out — that sort of thing.

In theory, if you were to apply thermal and random vibration at the same time, you should get about 55 percent of the field failure rate, which is a K factor of 1.82. In fact, I found another contractor who had tested some equipment with a known field failure rate under that combination of stresses, and he got 54 percent — the 1.85 factor shown. That's pretty close. However, those are the only documented correlations I could find for that combination of stress types. They're not enough to hang your hat on, but they do indicate what I'm trying to say.

Even though we've been testing for a long time, I couldn't find K factors for the rest of those stress types. But I did get a lot of feedback in the form of less than thoroughly documented information, which says the stress combination shown should produce something in the order of two-thirds of the field failure rate for avionics. The factors in brackets show the range of estimates I received.

The planning factors show it costs something to go to combined stress testing. However, most of the contractors I've talked to already have that sort of test facilities, only they have part of it in one building and part of it in another. So I don't think it's going to require us to build a completely new world of test chambers. We are going to have to put together some of the test facilities we already have. It is also going to cost more per hour to test under combined stress, but — from everything I can gather — not that much more.

Figure 25 shows a profile of one way those stresses might be assembled for an aircraft. You'll notice, on the left ordinate, that I plot temperature upside-down, with altitude on the right ordinate. Well, that's because I'm an airplane driver, and when you go up, it gets cold. Ever since Icarus, that is. When he went up, it got hot, and his wings melted off. But since then, whenever you go up, it gets cold.

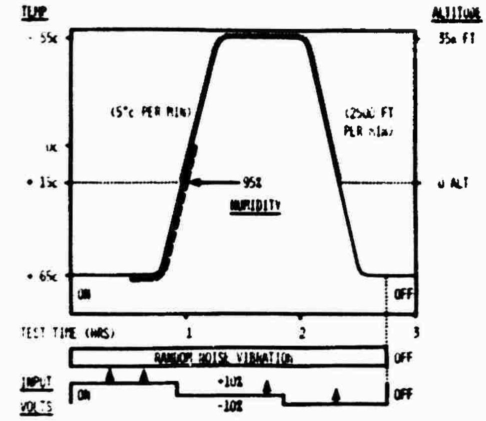

Fig. 25 - Equipment Level DT&E Program (Proposed): Test Conditions - Combined Stress Profile

The equipment is turned on when it is heat-soaked, to get the termal pulse. Humidity comes in while the chamber is hot, then the chamber is cooled and, as temperature crosses through the ambient range, altitude starts up. Shortly thereafter, the humidity can be expected to condense, frost or freeze, and sublimate. That produces a whole battery of failures you don't get any other way. It also produces an unbelievable family of malfunctions that don't duplicate on the ground. I spent a couple of years as an avionics maintenance supervisor in SAC, and that particular family of malfunctions drives a maintenance supervisor crazy.

Keep the equipment operating high and cold; keep it operating while the chamber returns to its starting conditions, and then cycle it off and on. This is one of a number of possible profiles.

However, a very important point: random vibration and acoustic noise — if you can get it— should be continuous while the equipment is operating. I don't know any airplanes that only vibrate for 10 minutes per hour.

Input voltage should include transient spikes. As Jack Short mentions, a large percentage of failures are caused by voltage spikes that pass circuit breakers, but blow transistors like popcorn. Let's put those in the test. Instead of testing for a theoretical, stable, beautiful power supply, let's test for the real world of dirty electrical power, as part of the reliability test.

In Fig. 26, the thermal stress is put back together to show that thermal realism is not lost by going to my proposal. As you'll notice, this profile looks more like a desert mission, while 781 looked more like half an Arctic mission. Both may be a temperature overstress, except for the conditions at turn-on. Maybe we should turn the equipment on heat-soaked one time and cold-soaked the next, with the majority of the test somewhere between. That would be tailoring the test to a measured stress profile.

In Fig. 27, measured vibration in the A-7 radome is overlaid with the random vibration that the contractor used when he got 40 percent of the field failure rate for a similar piece of avionics. General Phillips stopped me at this point. He said, "Colonel Swett, that vibration is not adequate for that piece of equipment". I said, "Yes sir. It is only shown as an example of frequency coverage". An engineer in the back of the audience said, "I would use a lower level of random vibration, to cover that exponential slope right above where it says 'MIL-STD-810', and I would add a sinosoidal to pick off the hump at about 60 Hz". He started to say something about the gunfire vibration, but General Phillips said, "Yes, I understand how to do it right. I'm just concerned that we are doing it so very wrong".

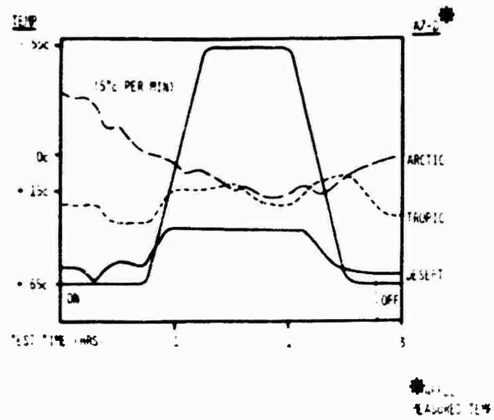

Fig. 26 - Equipment Level DT&E Program (Proposed): Test Conditions - Combined Stress Profile

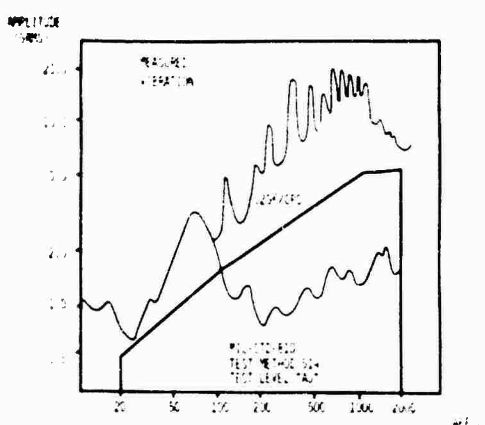

Fig. 27 - A-7/E Vibration Levels: Forward Looking Radar

I am somewhat embarrassed to show Fig. 28 because it sounds a great deal like motherhood. However, we don't do those things together, anywhere in our present test programs.

- OPERATE, EXERCISE, EQUIPMENT DURING APPLICATION OF COMBINED STRESS
- MEASURE, RECORD, ELECTRICAL AND MECHANICAL PERFORMANCE OUTPUT
- COMPARE TO PERFORMANCE LIMITS IN EQUIPMENT SPECIFICATION
- RECORD MALFUNCTIONS (PERFORMANCE OUT OF SPEC LIMITS)
  FAILURES (PERFORMANCE REMAINS OUT OF LIMITS)
  EQUIPMENT OPERATING TIME AND NUMBER OF DUTY CYCLES

→ PROVIDES
- OBJECTIVE MEASUREMENT
- PRE-ESTABLISHED SUCCESS/FAILURE CRITERIA
- MAXIMUM USEFULNESS OF DATA
  - FAILURE-FREE DUTY CYCLES/TOTAL CYCLES = MCSP
  - EQUIPMENT OPERATING TIME/MALFUNCTIONS = MTBM (MAINT DEMAND)
  - EQUIPMENT OPERATING TIME/FAILURES = MTBF (LOG DEMAND)

Fig. 28 - Equipment Level DT&E Program (Proposed): Test Procedures

Let me hit the second bullet from the top. It's about time we started going to automatic recording of equipment performance during the test, and get out of the subjective bit of "relevant failures", "non-relevant failures", "independent failures", "dependent failures", and all that. "Failure" should mean "failure to perform within stated limits".

I'd also like to expand on the last three points at the bottom of the slide. Failure-free cycles divided by total cycles approximates Mission Completion Success Probability — which is the operators' view of reliability. Operating time over malfunctions approximates Mean Time Between Maintenance — which is what drives the field maintenance people. And operating time over failures is demand on the supply system. It really should say "supply demand" instead of "logistics demand", which is a far broader term. Some AFLC people thumped me over the head for that. The point is, these are the operator's, the maintainer's, and the supporter's views of hardware reliability, and we — the developers — should be giving them the kind of data they need.

Figure 29 shows which Mil Standard 810 stresses could and could not be combined. The test methods for avionics are listed in the order called for, with the days required and the cost for each. The asterisks indicate those tests that could be subsumed by a combined stress test for avionics. They account for about two-thirds of the time and cost of the current 810 program.

By the way — and I have made this point a number of times —, some people have tried to save money by cancelling environmental tests.

	MIL-STD-810 TEST METHOD	DAYS REQ'D	$(x1000)
* 1.	502 LOW TEMP	8.5	$ 26.94
* 2.	501 HIGH TEMP	4.5	$ 18.68
* 3.	500 ALTITUDE	6.5	$ 19.50
* 4.	504 TEMP-ALT	9.0	$ 23.46
5.	505 TEMP SHOCK	7.0	$ 14.17
6.	512 LEAKAGE	1.0	$ 2.81
7.	510 SAND & DUST	4.25	$ 12.85
8.	513 ACCELERATION	4.0	$ 19.99
9.	511 EXPLOSIVE ATMOSPHERE	2.5	$ 7.52
10.	516 SHOCK (DROP)	5.5	$ 16.50
*11.	514 VIBRATION	10.25	$ 38.54
*12.	507 HUMIDITY	20.5	$ 19.05
13.	508 FUNGUS	14.0	$ 8.50
14.	509 SALT FOG	5.0	$ 5.91
*15.	515 ACOUSTICAL NOISE	5.0	$ 15.60
*16.	518 TEMP-ALT-HUMIDITY	12.7	$ 49.27
*17.	519 GUNFIRE VIBRATION	5.0	$ 21.00
	FINAL REPORT	11.25	$ 11.71
	TOTAL	139	$335
	* SUBSUMED BY COMBINED TEST	-82	-$232
	REMAINING ENVIRONMENTAL TESTS	57	$103

AFFDL-TR-71-32 (UPDATED TO FY 74 COSTS)

Fig. 29 - Equipment Level DT&E Program (Proposed): Environmental Tests

But I submit they didn't save a lot of money, because there's not a lot there to be saved. It's mainly, I think, that these tests get cancelled because they are viewed as a nuisance. There just isn't all that much money in the 810 tests, in any program that I looked at.

Now, Fig. 30 is the most important one of this study. It shows how — believe it or not — that shock and vibration engineers should be involved in writing reliability requirements. You may not have seen it this way, but I do. I'm recommending that we do three things for any given avionics development program. The first is to go to a statistical test plan. The second is to go to combined stress test conditions. And the third is to use the leverage provided by a lower K factor to restate required MTBF for the chamber test.

In this table, I started with required true MTBF in the field, and put in the environmental K factor to get required MTBF under test conditions. Test time and number of failures are the acceptance criteria. Total time divided by total failures experienced during the test is the demonstrated MTBF, under those test conditions. This must then be derated by the same environmental K factor to get expected true MTBF under field conditions. For the first time, we have both ends of the equation on the same ground.

- ACTIONS SIMULTANEOUS:
  - TEST PLAN "5TH FAILURE, 10% GVT RISK"
  - COMBINED-STRESS TEST CONDITIONS
  - REQ'D FIELD MTBF RESTATED (NOT REDUCED)

	REQ'D TRUE MTBF (FIELD)	ENV K	MIN ACCEPT MTBF (TEST)	TEST TIME (xMIN)	TEST TIME (HRS TOTAL)	N+1 FAIL	DEMO MTBF	ENV K	TRUE MTBF
PRESENT (TP III)	77 HRS	x6.5	500 HRS	x20.6	10,300	÷ 16	643	÷ 6.5	99
"5TH FAILURE"	77 HRS	x6.5	500 HRS	x 9.27	4,643	÷ 5	927	÷ 6.5	143
COMBINED STRESS	77 HRS	x1.5	115.5 HRS	x 9.27	1,070	÷ 5	214	÷ 1.5	143
	SAME		23%		10%		33%		144%

→ MIN ACCEPT MTBF 115.5 HRS
→ 90% CONFIDENCE ACCEPTED TRUE MTBF = REQ'D FIELD MTBF

Fig. 30 - Equipment Level DT&E Program (Proposed): Qualification Test (Example)

The top line of figures comes from the program I have used as an example. Minimum acceptable MTBF was 500 hours, but the requirement didn't say whether this was to be in the chamber or in the field. However, equipment of this same type now has about 35 hours MTBF in the field, and the operators would like 50. So where did the 500 come from? A "rule of thumb". The operators don't know that 6.5 environmental K factor is in there, and they don't know that 781 Test Plan III will accept two-thirds of the specified MTBF. They just know that the last time they say equipment with a 100 hour specified MTBF, they got about 10 hours in the field. And that is not an uncommon field MTBF for avionics. So now, when they want 50, they ask for 500. And the developing command planners add a "times two" discrimination ratio on top of that.

On the second line, we changed from 781 Test Plan II to the "5th Failure" plan I'm proposing as a replacement. This doesn't change anything in the equation until we get to the acceptance criteria. There, it reduces the test time multiplier from 20.6 to 9.27, and allows 5 failures instead of 16. This increases the lowest demonstrated MTBF the test will accept — and so expected MTBF in the field — by about 44 percent, and maintains the same amount of "government decision risk" as 781 Test Plan III.

On the last line, we are going to a combined stress package with an environmental K factor of 1.5 instead of the present 6.5, and if you'll recall, a large chunk of the difference in K factors was from adding the random vibration. This is where you can help in the requirements process: we need better K factors. This 1.5 factor is presently a "best guess".

With a soundly documented K factor for any given combination of stresses, one can go back to the operating command and say, "Hey buddy, I know why you asked for 500 hours minimum acceptable MTBF, and I know what you have in the field now. But there's a new ball game in town: I'm going to use more realistic test conditions, so to meet a 77 hour field requirement — which is more than twice what you have now — you can ask for 115.5 instead of 500. The number isn't important to you, as long as you get the reliability you need, but it makes a lot of difference to the program manager.

It will help him deliver your equipment on time". I've tried this idea on them for size, and they say, "Sure. Why not? But prove it to me first, before I lower my requirement". And that's why I need help in documenting the K factors.

When I take this restated requirement of 115.5 hours minimum acceptable MTBF in the test, and run it through the new test time multiplier, I've got a 90 percent reduction in qualification test time — for no loss of statistical confidence, and no less than a 44 percent increase in field MTBF.

Figure 31 compares test time, cost and expected field MTBF between the example program where everything was done strictly according to present Mil Standards, and that same program after incorporation of the three recommendations of Fig. 30. Both have a period for "test-and-correct" reliability growth, a qualification test before production, all-equipment burn-in and production sampling. All the numbers shown are specific to the program being analyzed — with a minimum acceptable MTBF in the test of 500 hours or its equivalent — but the percentages are not.

	TEST TIME (MONTHS)	TEST COST ($x1000)	ACCEPTED TRUE MTBF
● TOTAL:			
● PRESENT	36.8	$ 8,806	99
● PROPOSED	6.3	$ 1,655	142
	- 30.5	- $ 7,151	+ 43
	(- 83%)	(- 81%)	(+ 44%)
● PRODUCTION:			
● PRESENT	22.9	$ 5,516	114
● PROPOSED	6.9	$ 1,824	143
	- 16.0	- $ 3,692	+ 29
	(- 70%)	(- 67%)	(+ 25%)
TOTAL:	- 46.5	- $10,775	+ 29 TO +43
	(- 78%)	(- 75%)	(+ 44%)

→ DISPARITY BETWEEN SPECIFIED AND TRUE MTBF REDUCED FROM 10:1 TO 1:1.24

Fig. 31 - Cost-Benefit Comparison

By combining performance, environmental and reliability testing insofar as practical at the equipment level, we can — and there are several very conservative assumptions in this — expect to see something in the order of a 75 to 80 percent reduction in total test time and cost. And it is the test time, especially the time before the production decision, that is really hurting the program managers. The present 781 tests take so much time, and are so difficult to control, that program managers have to choose between delaying their programs or cancelling reliability demonstration requirements and producing unqualified equipment. And we see this in program after program.

The monetary saving from reduced test time more than pays for the additional cost of more realistic test facilities and the higher cost-per-hour of the new test, provided we use what we now have instead of starting over from scratch.

As shown at the bottom of Fig. 31, these three actions, taken together, reverse the present disparity between specified and field MTBF — from 10-to-1 to not worse than 1-to-1.24 — which was the problem that General Marsh asked us to investigate in the first place.

And there is one other effect of combined-stress testing, not mentioned in this briefing. It may well be the most important effect of all. Any contractor who knows his equipment must pass a qualification test under combined stress at realistic levels, and that all production units will face a burn-in under those same conditions, will have to design a different piece of equipment than he would to withstand a short exposure to 810 test conditions and a reliablity qualification under 781 conditions. This effect is real. Some of our programs are putting these concepts into their contracts now, and the result has been redesign of equipment or parts of equipment. It is this kind of attention to design that must take place — along with "test-and-correct" improvements — to improve the reliability and the ownership cost of avionics equipment in the field.

PANEL SESSIONS

MIL-STD-810C

A Panel Session

Moderator: Rudolph H. Volin, Shock and Vibration Information Center
Co-Moderator: Allan Piersol, Bolt Beranek and Newman Incorporated

Panelists: David Earls, Air Force Flight Dynamics Laboratory
Kenneth Herzing, Honeywell Incorporated
Joseph Gaudet, Sanders Associates
Al Tipton, Rockwell International
Peter Bouclin, Naval Weapons Center
Eugene Laboissonniere, U.S. Army Electronics Command
Robert Sevy, Air Force Flight Dynamics Laboratory
Dr. Alan Burkhard, Air Force Flight Dynamics Laboratory

Mr. Earls: Many of you have probably never seen MIL-STD-810C, so to bring you up to date we have decided to show you some of the new things that are in it that would be of interest to the Shock and Vibration Community. The new vibration tests are principally random but there is a new sinusoidal vibration test for helicopter equipment. We have new random vibration tests for equipment installed in jet aircraft, for equipment installed in external stores, and for assembled external stores on helicopters. MIL-STD-810C was in pretty bad shape as far as external stores were concerned and that was one of the areas that we in the Flight Dynamics Laboratory attemped to upgrade. There are also some acoustic noise tests for external stores, the assembled external store, and a special test for stores with cavities that are open to the airstream. There are some stores, leaflet dispensers and items of this type, which have cavities in them. When the bottom opens up the aerodynamic flow across those cavities causes some bad resonances. The vibration test for external stores for helicopters is sinusoidal, it covers the same frequency range of 5 to 500 Hz, but instead of dwelling at the resonances of the equipment the dwell is at the four rotor blade passage frequencies of some particular helicopter. The tests are geared for the number of missions and acceleration versus weight during dwell. They also vary according to the direction of vibration, longitudinal, lateral, or horizontal, so that you can tailor a test to the number of missions. One of the things that we are trying to do, and where we have been criticized so much in the past, is to tailor the test for the individual application. We have felt in the past that they were too rigid and you couldn't tailor them for an individual application, so we are trying to improve that. I think we had one curve for cargo vibration in the original MIL-STD-810 and then we had a group of them in MIL-STD-810B; now for general cargo logistical mode, rail, air, sea, and truck or semi-trailer, you can choose one level, 1 1/2 g from 5 to 200 Hz, and cycle sinusoidally at 84 minutes per axis. However there may be some changes before it is printed. Since random vibration for jet aircraft is one of major emphasis I will show you the curve that is in MIL-STD-810C and you will notice that it goes to 2000 Hz and it is a typical random vibration curve. It starts out at fifteen Hz, then it rises at 4 dB per octave, it is flat in the region of 300 to 1000 Hz, and it falls off to 2000 Hz at 6 dB. You will notice that the lower level, $.04g^2$/Hz, is usually generated from the runway roughness and low frequencies and that doesn't vary too much from airplane to airplane; but the higher level, at Wo, does change so you can't just go to MIL-STD-810C and say use this curve – you have to compute Wo before MIL-STD-810C can be used to specify a random vibration test for jet aircraft equipment. This is a list of definitions that you must have to apply the vibration. Vibration comes from two sources as far as the development of this part of the standard is concerned, aerodynamic flow and jet engine noise. For the aerodynamic flow portion you have to know whether the external flow is smooth or turbulent, you have to know the q of the airplane, which is the aerodynamic pressure at which the airplane is being flown; usually high speed low level flight is one of the maximum q conditions. You also have to know the number of missions for the equipment on the airplane and you can determine the test time for each axis. In order to predict the take off vibration that occurs because of the engine you must know the diameter of the engine, the velocity of the engine exhaust, and the location with respect to the engine including the distance and the angle with respect to the exhaust. One aspect that we have introduced into MIL-STD-810C is functional and endurance testing. The item of equipment shall function according to its detailed specifications as it should in the airplane at the functional level; the endurance level is for long term effects to see if the equipment would last through the life of the airplane. The Wo level is related to K q^2. To compute the Wo you must know the q of the airplane, either the maximum q or the q at high speed low level flight. For most of our fighter bombers q is about 1200 for high speed low level flight and that was the q that we used for establishing our fatigure test for the aerodynamic flow induced vibration. The same thing applies to the endurance level. In order to basically raise the level for the endurance test we multiply W_o by $(N/37T)^{1/4}$ and this raises the functional level. However for airplanes we would use a q of 1200 in doing that because that is where you accumulate the most time, we wouldn't use a q of 1800 to 2000. For the endurance level we would use a q of 1200 and then the number of missions (N) that the airplane is designed for divided by three times the test time (T), all to the 1/4 power, and the three means that one third of the flight time of the airplane will be spent at low levels and high speeds. We do the same thing at a functional level and at an endurance level for the jet engine noise induced vibration. The functional level is related to the diameter of the engine, the velocity of the engine exhaust, the noise level of the engine, and the location with respect to the engine; that is for things in the tail behind the engines principally. The endurance test level is similar to the aerodynamic test level only the factor is $(N/10T)^{1/4}$. This is a random vibration envelope for equipment installed in external stores. You will notice again we have the W_1 and W_2 levels and you will also notice that we have the F_1 and F_2 points, so that the break-off points where the curve is flat are adjustable. This is all done so that you can tailor it to your particular application because we don't want to overtest, or undertest. The W_1 level is .005 for the functional levels and then you would multiply it by $(N/3T)^{1/4}$ for the endurance level. You would just use the value of 1 in the specification for the functional level. The W_2 level is related to q divided by the weight density, w^2, of the store and this is 5×10^{-5} tq/w^2 $(N/3T)^{1/4}$, however you would use 1 in place of that value for the functional test. F_1 is 10^5 $(T/R)^2$, where T is the thickness of the skin and R is the radius and this is based on shell theory; it is for round or circular stores and that is where the resonance frequencies occur. F_2 is 1000 Hz higher than F_1. The foregoing is for equipment in the store. For the testing the whole store they use the same formulas as were shown previously to compute W_1 and W_2. The only difference is the points F_0 and F_1. F_1 is the same as before but F_0 only goes to 100 Hz, or up to a maximum of 500 Hz, since you are testing the entire store and it is hard to excite the upper frequencies. The acoustic spectrum for assembled external stores is based on the same philosophy, i.e., the functional test and the endurance test. The functional test is based on the q at which the store will be flown, the distance from the front of the store, and how blunt the store is, that is the angle beta: the blunter it is the worse the vibration condition will be near the front so that is related to the distance from the front of the store. The q at which store is being flown is not necessarily the q of the airplane; the airplane might fly at a q of 1800 or 2000 bare but it won't do it loaded with ECM pods or bombs. So it could very well be 1200 or less for carrying assembled stores. This is the same idea as we have in vibration tests for external stores. The store shall work satisfactorily at functional levels, and in the endurance test it has to work satisfactorily afterwards.

Mr. Herzing: I thought I might discuss some of our experiences that we have had with store programs when we utilized MIL-STD-810B criteria in an attempt to qualify not only the store as a system but the store cargo. Because many of the stores are multiple applications stores we have many opportunities to test the cargos by themselves. When we began in the store testing business I think it was general practice among the agencies that were requesting our services to specify that the stores would be tested as equipment installed in airplanes which is category b in MIL-STD-810B. In utilizing these criteria the first attempts were made to put the vibratory input in at what would be the store/aircraft interface, normally the top of the store where it attaches to the hooks to the bomb rack, and to control the input at that point in an attempt to force the system to respond to those types of inputs throughout the frequency regions. Our experience in doing that resulted in two types of failures, I guess the only two types that you could probably expect to happen. In one program we had a fully qualified store system that failed its first flight test. The store system had been fully qualified prior to being submitted to the customer for his initial flight testing, and during the flight test an electronic or an electromechanical device within the store system that controlled the rate at which the cargo came out jammed. Our second experience was with a store that could not possibly in any way ever pass the MIL-STD-810B sine sweep requirements without having catastrophic material failure, and yet the customer had on his own taken it upon himself to fly many of these stores, or prototypes of these stores, for many many missions without ever experiencing any type of failure. This pointed out to us and to our customers who were involved in our problem, that there were gross deficiencies in attempting to utilize the MIL-STD-810B equipment installed in airplanes as a qualification criteria for external stores. Because of the effort, primarily of these customers, we tried many devious schemes including utilization of measured data, the combining of arbitrary random test criteria from MIL-STD-810, but not necessarily part of that which is required by procedure I. We even, as you will recall, had a program where we attempted to qualify a store, and did in fact qualify a store, using a wind tunnel as a driving source; and last but not least we also arbitrarily combined the sinusoidal sweep requirements of MIL-STD-810B with some other arbitrary acoustic criteria. The whole point here is that we knew what was correct, but we didn't have any guidance from the military standards. Now we are looking at a new MIL-STD-810C, which while I am not here to sell it I am perfectly willing to say that it accommodates some of the obvious discrepancies that we recognized in using MIL-STD-810B; that is we finally have in MIL-STD-810C some guidance in the form of a separate category for equipment installed in stores, the assembled store vibration test, and the full scale acoustic test is an integral part of that assembled store vibration qualification test. It also provides us with some reference spectra which can be adjusted to accommodate variations in the maximum captive flight dynamic pressures, average store weight densities, skin thicknesses, and so forth. It also gives us a capability to do functional testing as well as the endurance testing; one thing that we probably overlooked in performing many of the early qualification tests to the MIL-STD-810B criteria was being clever enough to exercise the functional capabilities of the store and dispensing system during the vibration test program. Of course that is a difficult thing to do when you are doing a sinusoidal sweep because you always have to choose the appropriate frequency at which to exercise the function, and many of the functions, such as the cargo ejection, only happen once. Also I think a very important feature of the new criteria is that it establishes the necessity for performing a full scale acoustic test along with the vibration test and this is what we found in the experience that we had in the full scale wind tunnel test of the SUU-36 Dispenser system. We found that there were vibratory responses induced during that testing that couldn't be induced by any type of mechanical input; mechanical input would not get through the structure and into the cargo or to various parts of the structure that would be primarily affected. Also finally we have the capability of doing a separate test to simulate the captive flight and the free flight environments, and in many of the systems, that is an absolute necessity. During the type of test that we would perform on that kind of a system if the store is not a free flight store there is a good possibility that that test would be replaced by the cavity resonance test, since if the store is captive it most likely will end up with cavities once the cargo is ejected. This cavity resonance phenomena was in fact the mechanism that was responsible for the first store failure that we ever experienced, the first example that I related to you where we had a flight failure of a store that was completely qualified to the MIL-STD-810B curve C test requirements. In conclusion I would like to say that we are looking forward to the application of this standard and in fact we have already applied it to two stores systems during developmental testing. I can't say that the testing procedures have been completely debugged since I think there are some holes in the procedures, that we may want to discuss tonight, that will help to provide some guidelines so that a test that is performed at our facility, at a government facility, or maybe another contractor's facility will yield some sort of consistent results. I think during some of the discussions of the techniques we will probably run across those deficiencies.

Mr. Bouclin: I would like to address my remarks to vibration testing of external stores carried by jet aircraft. We at the Naval Weapons Center haven't had the opportunity yet to conduct the controlled vibration response tests as described in MIL-STD-810C. I would like to comment on the problems that we have had using the conventional single hardpoint vibration input control which is permitted by MIL-STD-810B. Some years ago we designed and manufactured a sophisticated, and I might add quite expensive, structure for three axis vibration of rockets, missiles, and free-fall weapons. This structure using hydrostatic bearings for cross axis restraint provided for attaching munitions to their respective launchers and bomb racks while positioned in normal flight attitudes. During the same time frame we also conducted captive flight vibration measurement programs on some missiles and free fall weapons systems; we were instrumenting principally in the guidance area but we also placed instruments throughout a number of these weapons systems. During these test programs we have always measured vibration at a hardpoint adjacent to the forward bomb lug or the launcher lug in the forward end of the missile, however input vibration is measured analyzed data and inputting this vibration to the store attachment points and attempting to control the response at this location, or any other locations along the weapons system, resulted both in an overtest as well as an undertest. These results suggest that the primary source of input vibration to the externally carried store due to jet aircraft carriage is not from the aircraft itself but rather due to the turbulent boundary layer or the turbulent flow around the store. For this reason I look forward to the MIL-STD-810C as a potential breakthrough in external store vibration testing, certainly controlled response testing requires far less in the way of capital expenditures for test equipment.

Mr. Gaudet: Well – It was only a few weeks ago that I saw MIL-STD-810C and of course the fact that we have been testing pods and the like for a number of years with relatively decent success turned me to say what are we doing now? We have another complicated specification that begins to look like a cookbook and we don't seem to get industry inputs into these specifications. Contrary to what Ken has mentioned, he found that they had problems in service on some of their pods, we probably have done something a little different; we did our regular qualification tests and on top of that we did one hundred percent "agree" testing on our pods and we haven't had any drastic failures in the field. So my interest is a little different, I am saying that we are happy with what we have right now and I want to know a little more about this MIL-STD-810C. Are we adding more burden to the contractor? I would certainly want to know more about how the MIL-STD-810C specification was arrived at, what criteria were put forth to come up with some of the limits and some of the requirements? I talked to Dave Earls a few minutes ago about a backup document because if any of you have ever had to go back to some of the specifications that have been put forth over the years, and try and find their basis it is impossible -- you just can't find it; with a standard this important it will be all encompassing and supposedly the answer to all our prayers. I would like to see the government come out with back-up data so that we who have to use the specifications at least know wat we are doing, we kind of feel left out. I also mentioned today about the industry input and I was told that there was a distribution of the MIL-STD-810C documents to the various military establishments for their further distribution to the users, but strangely enough there are a number of us on this panel who have never seen the document so it is a questionable thing. I don't disagree that the document is probably something that has been needed. It is an area that has always been one of question. When we talk about the method that they are discussing in here, supposedly it is going to save us a lot of force pounds in our shaker systems because we don't have to push these massive fixtures that we have all built up over the years; but I am one of these people who has to be shown a little bit before I spend my money and I need a little more information on the document.

Mr. Tipton: One of the primary responsibilities of a prime contractor is to determine the most realistic vibration levels and test criteria to be imposed on subcontractors, and do this for each specific weapon, missile, or aircraft system. For new aircraft systems this initially will probably rely on some prediction method that the contractor or the customer feels is most realistic, or perhaps a Military Standard such as MIL-STD-810B or MIL-STD-810C. If a current aircraft or missile is being modified or updated with new equipment, and if you have sufficient vibration test data available the task is much easier and realistic criteria can be specified fairly soon. If the initial criteria relies on a military specification or some prediction method it is probable that some conservatism might be employed. During a typical development phase the criteria should be refined within the economic constraints of your particular program and reflect changes in aircraft configurations and try to track changes in the structural design also if possible. If you have a large aircraft system with many many subsystems, such as the B-1, it might not be economically feasible to determine the vibration level for each specific piece of equipment and therefore some form of data enveloping, either on measured flight test data or predicted levels, would be still required. If you have a relatively small program with only a few equipment items you can probably economically determine the test environment for each specific piece of equipment. Especially these days I feel that vibration engineers must keep in mind that the test criteria where procured equipment is specified should be as realistic as possible – again within the frame-work of the financial constraints of your particular program. It is also realized that the subcontractors may feel that what the prime contractor thinks is realistic is somewhat less than realistic, however this effort should be made initially in a program to insure product reliability. Now all Military Standards have their good and bad points and MIL–STD–810 is no exception. I feel the primary deficiency of MIL–STD–810B was the lack of random vibration testing for aircraft equipment and for perhaps 20 years I think this specification has probably been outdated in that regard. There is no way that you can simulate in a sinusoidal vibration test the response of equipment to a random input. Also the test methods of MIL–STD–810C still rely on input vibration levels that are controlled at the shake table; possibly for larger equipment, some kind of a response control test might be more appropriate, or perhaps a vibroacoustic approach that some missile people employ. The random vibration test levels specified in MIL–STD–810C relative to our B-1 requirements are a little conservative. Next I would like to make some detailed comments on the random vibration test criteria for aircraft that appears in MIL–STD–810C. As you have seen on the slides there are two types of environments that generate the random vibration, the engine noise and the aerodynamic noise. In specifying the endurance test levels the mission life and the test time are factors. I feel that the curves might be somewhat conservative for most aircraft that I am familiar with; specification of endurance testing should be done by a more detailed mission analysis for each specific application because they are all different, however I realize that test specifications have to cover the world and some conservatism usually occurs when you try to specify a general requirement like this. The K factor listed for boundary layer vibration prediction is not too bad except that the K factor for external surfaces with discontinuities cavities, blade antennas speed brakes, etc. can vary widely, by at least an order of magnitude, from the particular value that is listed here. Also the minimum test levels specified here, $.04g^2/Hz$, seems a little high in my opinion. For after-hurning engines MIL–STD–810C recommends a vibration level four times higher than the non after-burning condition. It seems to me that this factor 4 probably could be replaced by more realistic parameter such as an exhaust velocity. Also the suggested spectrum shape in MIL–STD–810C for both boundary layer noise and engine noise is the same. I would expect considerable difference in the spectrum shape for boundary layer noise especially separated flow around external protuberances and some variation should be put in the standard to cover this particular situation; the general standard can't cover all of these details unfortunately. In closing I feel that most prime contractors would rather conduct the analysis required to specify realistic vibration levels and test methods for each particular project rather than to rely on existing or proposed general specifications.

Mr. Laboissonniere: I seem to be the only one on the panel representing the Army on ground equipment but I will give it a go anyway. As you well know MIL-STD-810C is presently in the stage of tri-service coordination. Just recently the Army completed its coordination for its various commands and submitted their recommended revisions. This evening I would like to discuss what effects the requirements in the shock and vibration area would have on the qualification of ground equipment for the Army as it is presently written. Perhaps the greatest challenge proposed by the new standard is the overtesting imposed by the 84 minutes of sinusoidal vibration testing in each plane on Army ground electronic equipment. Admittedly the old resonance search test with no resonances greater that twice the input was extremely controversial namely because different test engineers found different resonances, and different models of the same equipment exhibited different resonances. But this was not a test to accept or reject the equipment. The real test to accept or reject the equipment was the vehicle bounce test. Experience showed that if resonances in the range of 10 to 55 Hz could be reduced below twice the input on critical components you would have reasonable assurance of meeting procedure IX part 2, the vehicular bounce test. This test method was used primarily by the ECOM at Fort Monmouth. It was developed to simulate ground equipment destined to be either hard mounted or shock mounted in a military vehicle. The item under test mounted on a vehicular adapter plate, simulating a vehicular bed or mount, provided a structure to which repetitive shocks, controlled to provide a maximum 10 g level, were impacted on the test item. As you well know although it is difficult to define the ground environment, the vehicular ground environment is repetitive shock oriented rather than sinusoidal; however, because the repeatability of procedure IX from test facility to test facility was questionable the Army Commands at a special Army specification meeting chose to delete this procedure for new procurements and replace it with a TECOM proposed sinusoidal vibration test in spite of the fact that we know that the ground environment is shock oriented. We feel that the sinusoidal vibration test as proposed is more severe than the package test because it permits components at resonance to reach a greater amplitude, and this may cause many more failures because of the greater stress. The vehicular adapter plate test is essentially a random shock test which excites component resonant frequencies, but due to the nature of the test, does not allow the component resonant frequencies to reach their maximum displacement. It is probably a more realistic test for wheeled vehicles because of the random nature of the ground or road environment. Results over the years in the field environment has proven the acceptability of the package tester to test items destined for the field environment. In fact the 10 g maximum level was considered to be overly severe, and proposals had been made to reduce this level based on internal testing at ECOM. But let us get back to the new vibration tests and how they will affect the qualification of ground equipment for the Army. Really, they are not new vibration tests, but they are new to ECOM. Most electronic equipments installed in ground vehicles, equipment category f, are shock mounted. In accordance with test procedure VIII for equipment installed in ground vehicles, we will now have to vibrate our equipment for a minimum of 3 hours at a sweep time of either 15 minutes at 5 to 500 to 5 Hz per axis, or for 12 minutes from 5 to 200 to 5 Hz per axis depending on the vehicle in which the equipment is installed. The past philosophy was to eliminate all resonant frequencies on critical components below 55 Hz in the design of our equipment, in this manner the shock mounting system attenuated the inputs above 55 Hz. We still believe that we need not vibrate ground equipment above 200 Hz, and this applies to equipment installed in wheeled or tracked vehicles. Testing above 200 Hz is overtesting in the sense that the amplitudes are so small and don't represent damaging potentials, and the time and effort performing this type of test is very costly. With respect to the transportation of cargo equipment, category f, MIL-STD-810C specifies procedure X for equipment transported as secure cargo and procedure XI for equipment transported as loose cargo. It has been the experience at ECOM that the loose cargo test is more severe than the vehicular adapter test, also it has been our philosophy to test to the more severe environment when an item might be subjected to two separate similar environments; it is also our opinion that equipment transported as secure cargo is normally packaged for shipment by the packaging specifications. If the equipment is subjected to procedure X in this configuration you are again overtesting, since frequencies over 50 Hz are attenuated by the packaging material. If it is the intent to remove the equipment from its packaging and submit it to this test then we are wasting a lot of time, effort, and money, packaging material for shipment as secure cargo. If it is a requirement for equipment to be hard mounted in a wheeled or tracked vehicle, then procedure VIII should be applied. The Army has the added problem of man packed equipment. This equipment must be able to withstand vibrations normally induced during combat transportation and as loose cargo. These equipments are therefore subjected to procedure XI which is the loose cargo test. Now the problem becomes do we subject the same item to both the four hour plus sinusoidal vibration test and also the three hour loose cargo test? It is our opinion that only the loose cargo test should be run as it is more representative of the field environment. Another area where a MIL-STD-810C qualification test determines the acceptability of ground equipment is in the transient drop test. Admittedly the 26 drops from 48 inches were not a reasonable design requirement. The new recommended test for man packed and man portable equipments under 100 pounds, is to drop one test item on each face and corner for a total of 14 drops, and the second test item on each edge for a total of 12 drops. This is probably more realistic but in the R & D phase it means that we have to buy two separate equipments and this could be extremely costly. It appears that a test should be developed for the case where only one test item is available. I would be remiss if I didn't mention the problems imposed by MIL-STD-810C on equipment installed in helicopters, equipment category c. We do not agree with the vibration test for equipment mounted in helicopters as defined by Tables 514.2-IIIA, -IIIB, and -IIIC, and Figure 514, 2-3; this test calls for a 2 acceleration for equipment installed in locations other than equipment compartments, engine compartments, and tail rotor sections. We have vibration data indicating greater than 2 g vibration in the cockpit area of helicopters. In addition this vibration test calls for dwelling at the predominant helicopter blade passage frequencies rather than at the equipment's resonant frequencies. This type of vibration test is not suited for the Army's requirements since most of the Army's avionic type equipments are destined for use for all, or almost all, of the Army's helicopters and also several fixed wing aircraft. By vibrating at the predominant helicopter blade passage frequencies and not at its equipment resonances, we are in effect designing and testing the equipment for one particular helicopter. Table 514.2-IIIC lists a total of 12 different predominant frequencies for five Army helicopters. If a piece of avionics equipment is intended for use in each of these helicopters then it would have to be vibrated at 12 separate frequencies, and that is not the end of the problem created by this type of testing. Several standard army helicopters are not listed in the table and these include the CH-54, the OH-13, the OH-23, and the TH-55. In addition the table does not include the UTAS, the AAH, and the HLH, which are presently in a development stage, and also the advance scout helicopter, which is also presently in the planning stage. Since the predominant frequencies of these helicopters are in most cases not yet fully established it becomes obvious that this type of vibration testing is most certainly not suited to the Army's needs. The Army had recommended in its comments of MIL-STD-810C that the above paragraphs for equipment installed in helicopters be deleted. A new table has been recommended and submitted with Army comments. These recommended changes will be discussed at a future coordination meeting of MIL-STD-810C. I hope I haven't left the impression that I dislike MIL-STD-810C; it is a test document and it is a good one, its main problem is that it is greatly misunderstood by both government personnel and contractors. I believe that in the shock and vibration area for ground equipment we are making too drastic a change for procedures I, VIII, X, and XI and we have not spent enough time or effort to really investigate these tests which affect each other's equipment. Our goal should be to simplify and reduce the number of tests rather than introduce more tests. Continued coordination with the government and with industry is necessary to achieve this goal.

Mr. Piersol: I would like to start by noting that it is easy to criticize any specification and this one is no exception, but on balance I think. I would like to associate myself with the comments of Pete, Al, and Ken, that it is a vast improvement over MIL-STD-810B. With that said I would like to note a couple of things that have come to my attention. I myself have only recently seen this standard. I have been going through a few interesting exercises that I am sure other people will do. One of them deals with the comparison between aircraft levels and the stores levels and that seemed like an interesting thing to try. I assumed an F-4 carrying a Sparrow Missile, and a value of max q and then calculated the vibration test levels. Dave Earls made a comment earlier, which confused me a little bit, that you don't necessarily use max q – you use a common q, either way we get interesting results. I'm not sure of the cockpit levels in an F-4. I know something about the Sparrow Missile vibration levels. I am a little surprised that the aircraft cockpit is 8 times shakier than the Sparrow Missile. I appreciate the fact that the Sparrow Missile has a much higher weight density, but I also appreciate the fact that the cockpit is in an area generally where the boundary layer is a bit smoother, so perhaps somebody could comment on this. It first suggests that perhaps the aircraft levels are a bit conservative, or the stores levels are a bit unconservative, or maybe a little of both. I will note that I did check the stores levels against some measured data on the Sparrow Missile and I found that MIL-STD-810C in terms of the overall level would have predicted about 3.1 g's for the Sparrow Missile in a single mount position at a q of 1000 and actual measured levels at five critical component locations were 1.8, 1.8, 1.7, 4.7, and 3.2 g's, so the agreement is not too bad. I actually got overall g levels at two positions higher than the specification and remembering that the specification is smooth, that means that at some spectral location I exceeded the specifications by a substantial amount. But at least it seems unlikely that anyone will argue that the specification is too conservative. There are a couple of things that might be disturbing and I am surprised that they weren't mentioned. That is I am sure that one can contrive, and it very likely will happen in practice, that with all of these scaling rules someone will end up with a combination of weight density and q and so forth which will call for a vibration test level of 386 g^2/Hz or something like that. It would seem that in a specification of this type where that many scaling rules are involved it might have been wise to place specific upper and lower bounds on the levels saying that if your predicted levels exceed these bounds maybe you had better ignore the equation and just stick with the bounds. I note that in certain areas it did that and I think that might be worthwhile. No one discussed the connecting rod approach; I thought they would. That is given as an option and I wanted to note that there is some experience with that at Wright Field, and there is also some more experience at Point Mugu, both of which have generally been very successful. It saves a tremendous amount of time and money and when it works it seems to work amazingly well. There are cases however where it will not work, and we have run into cases of stores where it is not very effective, so it means that there will have to be some caution in the use of that approach. An obvious complaint that can arise in the overall store test is that two different contractors will be able to perform the tests and obtain different results since there is no specific guidance on where the control accelerometers are mounted for response control testing. That could certainly raise some objections, however the idea of going to response control tests, as I think everybody up here agrees, is certainly very wise as compared to the previous testing procedures.

DISCUSSION

Prior to the discussion Dr. Burkhard and Mr. Sevy joined the panel.

Mr. Hancock (LTV Aerospace): I understand that Method 519.2 is being revised again and is not as published in the MIL-STD-810C copy that is presently being circulated. Is that correct?

Mr. Sevy: No. Method 519 will follow on the termination of the completion of MIL-STD-810C as a coordinated document so we are holding off on it.

Mr. Hancock: Will we agree anymore on the second revision? The basic area of disagreement is that we believe that more discrete components are inherent in gunfire than the present method indicates.

Mr. Sevy: Yes. We are set right now at the fundamental, first, second, third, and fourth harmonic. Is that what you are talking about?

Mr. Hancock: Yes, and we would like to see about ten harmonics.

Mr. Sevy: You would like to see ten harmonics? That takes you up above 500 Hz and beyond. As you know we are substituting an acoustic test for the higher harmonics. That is the reason for the superposition of the four harmonics over the noise spectrum, and since the noise spectrum is continuous it will cover those ten harmonics.

Mr. Hancock: O.K. – It is a long argument. I guess I can refer back to our paper in the 40th Shock and Vibration Bulletin on gunfire simulation techniques as still being our preference. In regard to both vibration and acoustic testing of stores, we have looked at one store configuration on the A7A which was quite clean and the prediction methods currently existing yielded vibration levels approximately ten times those that were actually measured. It was a low density large store which was just the opposite of what Allan Piersol said a few minutes ago.

Mr. Burkhard: We found that the density of some stores becomes very low since they may have a lot of open area or light package material, such as in a dispenser where you have leaflets or something like that. The criteria are slightly conservative in that regard and we are currently looking at an in-house program to identify what we can do to the criteria to upgrade the area of low densities. The current prediction criteria are for fairly uniform stores and I think densities in the range of 40 to 150 pounds per cubic foot. When you are above or below that range the criteria fall off.

Mr. Hancock: It seemed to us that maybe the K factors might be changed slightly. It was as though the present method always assumes a separation on the aft end of the store and doesn't give proper leeway for the design of a store to avoid separation. Now I realize that in some flight profiles a slight separation will occur. Is that true? Did you assume separation predominantly?

Mr. Burkhard: In the aft end of the store you have separated flow or base pressure fluctuations, and these are reflected in the criteria by shifting the factor A which predicts the roll off rate at the high end; it will shift downwards and give you more low frequency requirements on the environment than would normally be there if you didn't have separated flow.

Mr. Hancock: I guess that the level that will be predicted with the present method would be more appropriate to a separated level than to a clean attached boundary layer level.

Mr. Burkhard: That is probably true because most stores in the aft end are reentrant in nature – you have the start of separated flow or they are truncated like the end of a missile.

Mr. Hancock: In the event that we can show that no separation should occur do we have leeway to reduce those levels?

Mr. Burkhard: It would certainly be reduced if you didn't have separated flow because of the reduced amount of excitation or turbulence induced into the boundary layer, which is a different situation than was used to set up the criteria. The criteria were based upon conventional types of aerodynamic shapes and structures.

Mr. Hancock: There is a paragraph at the front of MIL-STD-810 which allows submission of alternate test methods if data can be used to substantiate it, is it still there? One other comment on the aeroacoustic store testing; I think it would make sense to test a store in a progressive wave tube rather than in a reverberant chamber. I can't quite read that permission into the present MIL-STD-810C.

Mr. Burkhard: What condition you would want to do that for?

Mr. Hancock: Aeroacoustic loading or jet noise.

Mr. Burkhard: No. It is currently set up for a reverberant chamber. There would have to be some adjustment in the levels themselves to account for the difference in environment that you would use to simulate the flight environment, and the current criteria are reverberant criteria. There would have to be some adjustment and I wouldn't know what that would be.

Mr. Hancock: When we submit a test plan for a store would you consider a progressive wave test?

Mr. Burkhard: I would refer that to the acoustics experts in terms of what the required deviation would be or the variation from the current reverberant criteria. I don't know whether your submission would be up or down in terms of response.

Mr. Senn (Test and Evaluation Command): Mr. Laboissonniere mentioned that he had some reservations on procedure VIII. As I remember the original proposal was proposed by ECOM, it was massaged TECOM, and it went back; after going over it we agreed on it and the proposal that is going in now was resolved supposedly at the Army meeting in July. Do you repudiate that at this time?

Mr. Laboissonniere: I am not saying that we are going to drop this and we are going to change. I am going to have to live with this requirement of the 200 Hz. We are putting it in our specifications right now, I feel that we are going to have problems in meeting some of these things, and that is why I brought it up. I didn't say that we were taking it out. We have not had enough time to subject some of our ECOM equipment to this test; Mr. Biamonte had never done it to my knowledge. I started to subject one piece of man packed equipment that is vehicular mounted with shock mounts. I was able to get through one 3 hour cycle and I found out that my shock mounts failed and my equipment failed. I didn't have another piece of equipment that I could use to run some more tests so that sort of stopped me right there. But we are going to have to live with this and we will probably have to do more design work; we will probably have to have heavier equipment because we will have to stiffen components which we never did before, and I have a feeling that it will cost us more money. But it is in MIL-STD-810 and we are not recommending that it be taken out.

Mr. Senn: 300 Hz is the upper frequency for vibration tests of equipment mounted in tracked vehicles; in wheeled vehicles it is 200 Hz. You mentioned that you considered 500 Hz an overtest because when it gets up to 500 Hz it doesn't see anything since it is all damped out anyway.

Mr. Laboissonniere: This is because we feel that if it is on shock mounts they will attenuate inputs at that frequency.

Mr. Senn: Well then, you say that it really doesn't do anything when you get above 200 Hz, therefore it is a useless test and it is a waste of time.

Mr. Laboissonniere: Yes.

Mr. Senn: It is not an overtest then?

Mr. Laboissonniere: To the point that it will cost more time and effort to run the test it is an overtest.

Mr. Senn: An overtest is where you test something to a more severe condition. You also mentioned that you consider the bounce test to be the most suitable test for electronic equipment and that you have arrived at this conclusion after a number of in-house tests and different data. Can you provide us with the reports backing up that statement?

Mr. Laboissonniere: Yes.

Mr. Senn: On the helicopter test that is in the version of MIL-STD-810C that was distributed for comments, we withdrew that, and at the meeting in July an alternate proposal was made which I thought was rejected but ended up in the essential Army comments. Now do you agree with that?

Mr. Laboissonniere: Yes, we agree with the new test. I have seen the Army's comment, I have been over it with the other people, and they have agreed to it.

Mr. Senn: Yes, that is the one we proposed.

Mr. Laboissonniere: The only reason I brought out the other is we have so much testing that we have to do for all of these resonance frequencies and for all of these helicopters.

Mr. Senn: That is true and this is one of the reasons that we made our counter proposal, because we did not include all of the helicopters. For example, the UH-1 helicopter's rotor blade passage frequencies are 11, 22, 33, and 44 Hz. When you take into consideration the variations that you get from pilots, from equipment, and between helicopters, you will run from a few percent above to 10 percent below; then the blade passage frequencies of another helicopter are a multiple of 12 Hz, and still another one has blade passage frequencies in 32 Hz multiples. If you put all of these together and try to hit each one you would end up virtually covering the entire frequency range so you might as well sweep and be done with it. You agree with procedure X and procedure XI, the loose cargo test, because it is very much like procedure IX?

Mr. Laboissonniere: Yes. Procedure IX, that was the vehicular bounce test that was taken out. We are still sticking with the loose cargo test. Our problem is are we going to run both the sinusoidal vibration and the loose cargo tests? I believe you have a choice; we would run the loose cargo test.

Mr. Senn: The object of a test of equipment for the Army is to test it as close as possible to the manner in which it is used in the field. If a piece of equipment is soldier carried and is thrown into the back end of a truck or into a trailer then it should be tested in that way, and that is a loose cargo test. If it is something that is not carried in that way but is mounted as secured cargo then it should be tested that way. The determination as to how it is tested depends on how it is used.

Mr. Bowser (Aeronautical Systems Division): Recently the flight control community was given some vibration level requirements for a flight control computer in one of our projects, the Light Weight Fighter. The proposed spectral density levels were stated at one point and the community got rather upset and put up quite a fight to lower the requirements and they won; they were using the MIL-STD-810C requirement 514.2-IIA. Has there been any thought about such a sharp decrease in the vibration levels that were actually encountered? Have you considered the depth of the skin or the equipment?

Mr. Earls: I am not too familiar with that. Could you tell me where it was located?

Mr. Bowser: This was up in the forward area of the airplane. I guess that is zone one.

Mr. Earls: How many measurements did you have?

Mr. Bowser: I can't give you the exact number of measurements but there was a pretty sharp decrease.

Mr. Earls: I will give you one explanation that I think may be true without knowing the details of the problem. When we developed the MIL-STD-810C criteria we examined many pickups at many locations in many different airplanes; and so we took third octaves and averages of perhaps 18 of them, and we got the standard deviation and twice the standard deviation, and by the time you go through this you will fill in all of the gaps in the spectrum. One will peak at one frequency, one will peak at another frequency, and that is how you end up with this curve we have here, because by using many data points you will have many frequencies at different places. So you end up with a curve which would be applicable and you could use this equipment and cover 95% of the data within any of these types of airplanes. When you look at one or two individual pickups, or one location or two or three locations, within a very limited amount of data, it is very easy to be quite a bit different from the way the criteria developed. We can say from our criteria that the vibration will not be exceeded in perhaps 95% of the cases in many different airplanes; you look at one pickup or one location and you can look at the overall levels and say that they are way down. It is only peaking at this particular place while your spectrum has it all the way across. Those are the reasons it happens.

Mr. Wilkus (Aeronautical Systems Division): If I might add to Dave's comments; this is the Flight Control Computer of the F-16 that you are referring to, is that correct?

Mr. Bowser: Yes.

Mr. Wilkus: The specification as it is now does not account for a number of important variables which should be accounted for in the future, and which we are looking into now. One of them, for example, is that part of the flight control computer is actually secondary structure. One goes through the skin, through the bulkhead, and up to a secondary truss or beam for one end of the box. The other box takes off with some brackets from the bulkhead; so one goes from the skin through a bulkhead, through a bracket, and then into the box at the other end. It has been my observation that there are substantial losses in transit and situations like that, and this specification does not allow for that type of structural detail. We had hoped, in new specifications, to try to accomplish some of this largely through some distance parameter from the skin and that it would help to reduce those levels somewhat. I recall that that particular vibration level ran at fairly high Mach numbers and there is not a correction in the amplitude levels to account for fairly high Mach numbers on the order of 1.6 to 1.8; I think that correction would also help to bring it closer into agreement with the actual measured data. But we don't have a provision for it now and we will have to wait for it in the future modification.

Mr. Kana (Southwest Research Institute): First, let me say that I was one of the individuals here that did not see MIL–STD–810C before tonight so I particularly appreciated your preview of some of the changes in the standard. I would like to talk for a moment about the basic philosophy behind this standard as well as the one that was in MIL–STD–810B and in the others; a moment ago someone here alluded to the more or less "crutch clause" that appeared in the first page of the document, and of course it reminded us if for some circumstances it appeared that the standard is too severe or even under severe, then we are more or less obligated to look at field data and arrive at a specification that we feel is more appropriate. Personally, from my own experience that is certainly a reasonable approach and that is the approach that ought to be followed; however, quite often in the application of this standard that is not the approach that is followed. In particular there will be people at various levels in an organization that are not really familiar with the details of the standard and they feel that in not being familiar, the only safe road to follow is to stick strictly to the standard. Would it be appropriate to have a little bit more elaboration on this paragraph in MIL–STD–810C so that we could follow what I have called a little bit more reasonable approach?

Mr. Earls: There are many variations in using the standard, such as when prime contractors bid on a new weapons system they can usually have the prerogative during bidding of predicting the environment. I think the hang up is where is this rule to be applied? Who can do this? Is it the subcontractor, the prime contractor, or the Air Force? As far as the Air Force is concerned I look at this document as a place to go to get your test requirements when you first start writing your specifications; when you have better information about particular locations, great, you use it. You should do this because we can't, in MIL–STD–810C, forecast all of the data for everything you are going to build for the Air Force. I think it is applied in the Air Force according to major weapons sytems and it is probably a little more difficult when you get into the GFE areas, and I think that is mainly becuase of the lack of environmental engineering personnel who are able to make the judgments. We become involved where there are wrong judgments made because in applying it across the board we hope it is more realistic now; we are working toward that goal so that you won't make as bad mistakes as you have before because we are trying to tailor the vibration tests to the individual application just as much as we can. But there is still that perogative, it just depends at what level it is applied, and maybe you are at a level where it is pretty difficult to apply. It is hard to talk to a program manager when he has something in a contract if he doesn't have anybody to go to who knows the vibration business; there are people at the Wright-Patterson Air Force Base he can call on, and in general they are finding out who they are, but there are probably other procurements where they don't know who to call. We are in the process of building up that expertise in those types of people who can be called on to help you tailor your test, and especially if the specification doesn't cover your application.

Mr. Tipton: As I have said before prime contractors usually generate there own test criteria, and test levels. Normal procedures are to get together with the Air Force counterparts and mutually agree on good levels and criteria to be used. As with MIL–STD–810B, and still with MIL–STD–810C, it specifically states that a curve shall be selected from the tables and figures or by performing a detailed analysis of the expected vibration environment within a particular vehicle; it has been my experience that most do that at the prime contractor level.

Mr. Kana: Let me make one more comment on that and then let me pose a hypothetical situation, and actually it is not so hypothetical, because I have been through it and I would tend to guess that many people in the audience, and perhaps some of you on the panel, have had the same experience. In many cases when we receive a piece of electronic hardware for a vibration test the people at our organization bring it to us when they are well behind schedule to start with and in addition they don't know very much about vibration. Very little consideration has been given to the vibration up to that point; maybe when the specification has been typed for their review a mis-type has been made in the specification and someone doesn't even realize that until it is brought down to us. So we look at this and we begin to realize that there will be an overtest or an undertest, we point this out to them and they begin to feel very uneasy because they have this security blanket that they are falling under and you have pulled it out from under them. This is the situation that I am alluding to and it has happened any number of times in the past, and I feel that down at this level is where the real intent of the specification has broken down; therefore if some elaboration again in pointing out the philosophy of the specification, or if some clarification could be made of this, it would make the whole process much easier and in many cases it would save considerable amounts of money.

Mr. Piersol: Are you talking as a prime contractor? Isn't the prime contractor the one to go to with a problem of this type?

Mr. Kana: You are probably right in that but I am saying that in our case we are doing the test for another part of our organization which might be in the electronics group. Now the electronics group is behing schedule, they don't want to get involved with problems of this type, and therefore all they want to do is run the test and get it over with. Even if you have to give them the parts back in a bushel basket then everybody is unhappy.

Mr. Volin: This is not an uncommon case and it happens frequently because of the poor planning in the program.

Mr. Cline (Aberdeen Proving Ground): Carrying on with what Dr. Kana has said there are people who are concerned with perhaps a helicopter specification that was proposed. They are concerned because the Huey has four blade passage frequencies of 11, 22, 33, and 44 Hz, other helicopters may have some other number, and they look at all of the frequencies that they have to dwell at. I sat here a couple of hours this afternoon in a session that says do we test or do we analyze? There is nothing in this world that says you can't determine the dynamic behavior of your specimen and see how it will apply to these various helicopters. I think that what we are trying to do is make MIL-STD-810C too rigid, not to allow any flexibility in applying the test schedule. It has to be done. Who does it? Well the one that is responsible. There has to be somebody in the military, or somebody has to make these decisions. We have to bite the bullet and do it.

Mr. Curtis (Hughes Aircraft Company): To follow on Dr. Kana's comment about the "escape clause", which I suspect is the one paragraph of the standard that is never fulfilled. I think there is another situation where one should be able to take advantage of that paragraph and yet if you do you are a dirty guy. If one is answering an RFP that calls out requirements in accordance with MIL-STD-810 then that should include that paragraph; so if you can arrive at alternate requirements this is looked upon as being unresponsive, and so you suggest this to the proposal manager and he says I am not going to be unresponsive. So the end response is that you don't have the opportunity to take advantage of that clause.

Mr. Herzing: I agree with what you say and I think that this is a very real situation, we tried to circumvent it but we weren't successful. We attempted to do something that the government has done with air and water pollution, that is to propose an environmental definition study as part of the contract and to establish applicable environmental criteria as one of the first phases along with some of the parallel design efforts, and that wasn't only for the dynamic area. I had the same experience that you had in attempting to do that but I wanted to throw it out because it is kind of analagous to the environmental impact statement requirements for power plants and the like.

Mr. Piersol. I think that it should be noted however that apparently it can be done. If I am not mistaken on the B-1 all the requirements were pretty well developed outside formal specifications so it does occur.

Mr. Rothaug (Dayton T. Brown Inc.): Let me bring up a little different aspect of this from the test lab point of view. I understand that the procedures for externally suspended stores are loosely written on purpose so that they can be interpreted in different ways. For example the store can be suspended in various ways and vibration can be applied at any location that is suitable for your purpose, either in the center, in one end, or somewhere in between. You are looking for resonant responses and when you find resonances you have to match their response to the curve that is specified and calculated. In between these resonant points you can put in zero; there is no specified input in between these resonant conditions so that you can take advantage of that situation and first put your input into the place that will do you the least harm, and second put in nothing else at any other frequency. This certainly has disadvantages from our point of view, for one if I do a test, or if someone else does a test, there could be two completely different sets of results; second most of the time I work with subcontractors rather than some government agency, and I could get considerable pressure, I am not saying that I would bend to this pressure, but I could get considerable pressure to perform this test in a way that would be most advantageous to this subcontractor. And there are other problems also such as DCAS would like a written procedure so they know exactly what is going on where it is loosely stated; this could be a problem in delaying the test program until everyone is in agreement.

Mr. Bouclin: I have to agree with what you are saying. I think you are exactly right. As I pointed out earlier we have not yet attempted to do this point control, or controlled response vibration testing. When we do it, we will be doing it in somewhat of an experimental fashion. We will have to learn about this ourselves and I think then we would be able to have some comments that probably would be helpful; but at the moment I can do nothing but agree with you.

Mr. Herzing: I have seen a number of requests to do testing in similar manner as called for in MIL-STD-810C and I have never considered it experimental. When I have to respond to a quotation and tell them how much it will cost I have to define my methods pretty closely and it is a problem to us to have a specification like that that is not fully defined.

Mr. Burkhard: One of the intents in that standard is to try to use as much of the information that is available from structural response tests of the store, or extensive measurements taken in field usage on other stores, and try to obtain that same type of behavior in the test laboratory. The shaker store test as it is proposed is purely a low frequency test procedure. It is meant to be used in conjunction with a higher frequency vibroacoustic or acoustic test procedure to cover the complete frequency spectrum. Therefore the shaker test is set up to work out or to excite the low frequency beam type modes that occur in any type of a store which predominantly occur during takeoff, taxi, rolling, or passage through turbulent air during the flight profile. So you try to identify these types of frequencies in the initial search for resonant peaks, realizing under the assumption that the most significant damage that occurs in this frequency regime occurs in those bands around those peak response frequencies that represent beam or large deflection type behavior of the pod.

Mr. Rothaug: Dr. Burkhard, I agree with your philosophy but I still would like to see something written to further define the methods. I have seen a lot of flight data that shows frequencies up to and beyond 5K Hz, this test only goes, even with the acoustic test, to 2K Hz. Is there any reason why it is cut off at that point?

Mr. Burkhard: The specification does say that when you normally run a facility of this sort there are other higher harmonics, up to 5K, 10K and above, in the chamber, but the requirement is only in the same regime over which the equipment is previously required to operate, which is up to 2000 Hz. I think the Navy has had some experience where some equipment is sensitive to higher frequencies than 2000 Hz, and by using the acoustic test you can excite those higher frequencies in your store because the acoustic environment is there as opposed to using just a mechanical shaker test to excite those frequencies.

Mr. Curtis: We have had a lot of experience running response control tests and these have been done not only on external stores, but there are good technical reasons for doing them on any piece of equipment which is of sufficient mass and bulk and perhaps with well separated attachment points, that you should try to take account of the loading of the supporting structure; this was the original reason that we proposed this type of test. I would like to strongly suggest along the lines of Mr. Rothaug that a minimum input spectrum rather than a very loosely defined, if at all defined, requirement outside of a resonant peak should be included in this method so that you would know how to run the test. I also know as a contractor that the test will be applied to me, it will be applied as an equipment specification, and that I will be required to show system performance under those conditions regardless of what MIL-STD-810C says; so you have to look at it as being used in that way and you may not be around to tell somebody that it is really a structural test and therefore I shouldn't care whether the equipment performs or not. I would like to follow that up by asking whether you have given any consideration to permitting the use of a response control test for a piece of internal equipment, which might be a whole radar subsystem that weighs on the order of 600 or 700 pounds, that is all being tested together as a rack? There is exactly the same technical justification for response control as you have on the external store.

Mr. Beck (Boeing Aerospace): I would like to comment on arriving at your own test levels. We have done that at Boeing and when you win a contract sometimes you have to turn in a report to the vibration and acoustics lab showing how you arrived at those levels and this is fine. The problem that we seem to run into is that anytime somebody invokes a standard such as MIL-STD-810, and this gets into the project, it becomes sacred. I can understand that a vendor or whoever runs a test really has to adhere to that specification and there seems to be no way of deviating from it. Many times when we talk to the people that are responsible for avionics they don't seem to know that there is a dynamics laboratory at Wright-Patterson Air Force Base. There must be some way in this procedure that you could handle these special cases, such as a heavy items of equipment. We did this on the Saturn program with the Marshall Space Flight Center; we worked directly with the dynamics people and we wrote specifications for individual components, if necessary, getting data from a static test firing vehicle which is handy. I think we need that kind of a procedure that we could work between the Air Force, the prime contractor, and the subcontractors, on down so that you could work some of these problems in detail. These things tend to be arbitrary or very binding when they get into a specification. I appreciate what you are trying to do and I appreciate the random test being put in, but we need some way to handle the special cases, because when you have data that is scattered over a factor of 100 it is difficult to tell someone, well, that is the level. And that is what will happen because that is what you have to tell him, since if you don't have an airplane to fly there is not much justification unless you get with the people who designed that specification and talk it over among those that are knowledgeable; then maybe you can come up with something that is better for all parties involved.

Mr. Kana: This is of course part of the discussion that I brought up earlier and I agree that it is all part of this problem. I think that if or when an item passes a standard test in MIL-STD-810B or C of course everybody is happy; but when one fails that is where the problems start and we all recognize this. It seems to me there ought to be some kind of a mandatory clause which states that if an item fails that specification then you must look more closely at the environment, and in effect prove that this specification or some other one is more effective or more appropriate.

Mr. Senn: I don't think that we can expect to write a Military Standard that will correct all of the ills of government contracting. The MIL-STD-810 is a document that is used for testing those items. Now I can't speak for the Air Force because I don't deal in their kind of equipment; but the equipment that ECOM is testing has a multiplicity of purposes; they don't know in what helicopter it will be used, so they have to have a general purpose specification to which they can test it, and that is the reason they use MIL-STD-810. Now if they knew what vehicle it would be used on they could design their test around that vehicle and we will test it to that vehicle. You have paragraph 1.2 in the front of the MIL-STD-810C that says if you know a better way to do it, do it that way; you can't very well open the book any more than that, if someone doesn't want to take it he will not take it regardless of what you write in it.

Mr. Beck: There is one request I hope you put in and that is how to handle vibration isolators. With the 5 Hz minimum test limit you can cram all of your frequencies below 5 Hz and you isolate equipment in the B-1 or B-52 and it will be damaged. There are many people who misuse isolators. Do you have a minimum frequency requirement in MIL-STD-810C?

Mr. Earls: I think it is the 20 to 30 Hz range. I can't quote MIL-STD-810C exactly, but for aircraft applications we try to keep them in that range. I can't quote you the paragraph or number but if it is not there it will be. If equipment is to be tested off the isolators, where you don't have the isolators or the rack, you mount it on isolators with resonances between 20 and 30 Hz and a Q of 4 to 5 and test it as being mounted on isolators; but that should be for aircraft applications because that case you have has to be covered so that you don't put the isolator where it is going to tear everything up.

Mr. Hermes (Aeronautical Systems Division): I believe the Air Force has the same problems that the contractors have in terms of an equipment procurement cycle where the primary engineering control at procurement is the electronics engineer, and normally the environmental engineer or the vibration engineer are left out in the cold. I think hopefully the Air Force will come up with a solution to the problem that everyone has touched upon; that is MIL–STD–810C cannot be used totally without deviations, without thought, without analysis, and that there needs to be some negotiation between the contractor and the Air Force on a technical plan. As I see the solution under the present circumstances, if a contractor has problems specifically within with the ASD procurement program he should ask the ASD engineering office, or ask the procurement office, for technical specialists either in vibration or any other environment. I think the contractor has that right and I think if the equipment procurement engineer doesn't know who that person is he should be forced to go find him. I think if we had that, or at least if we went in that direction, and I am sure ASD is going in that direction, we could resolve all of the minute perturbations to MIL–STD–810C on a technical level between two engineers who know what they are talking about; and hopefully if we reach that stage, I think we can resolve a large majority of our present problems.

Mr. Pusey: It seems to me that maybe we are missing some people in the audience that we ought to have. Perhaps General Stewart ought to be here and perhaps the commanders of the Naval Sea Systems Command, the Naval Electronics Systems Command, and the various Army commodity commands; because one of the most frequent complaints that reflects on this problem that you have raised is that you can't use the escape clause because you can't get to the right people even if you have the proof. Many elements of the Defense Department; the equivalent to the Air Force SPO's or the Type Desks in the Navy, don't have the right personnel and they get a document such as MIL–STD–810C or MIL–S–901C. A contractor can come to this project office and say here is proof that we don't need to cost the government all this money, the levels should be lowered or what not; and this person says I am not going to take that upon myself and he doesn't really know where to go to talk to somebody to give him good advice, and even if he did he wouldn't take the chance. So I really think the problem is a little bigger for deviations from specifications and I don't know how to solve it, but maybe we are getting a little improvement in those areas. I hope so.

Mr. Root (National Waterlift): I think Mr. Pusey's comments brings us back to this second support document. I know as a second tier subcontractor we could use it at times to know the limitations of these formulae, how to apply them, and what are some of the guidelines; maybe we need a second document to go along with MIL–STD–810C and maybe this would answer some of these other questions.

Mr. Volin: If I am not mistaken originally the plan was to have two documents, a methods document and a levels document but I think that this was something that fell by the wayside. Mr. Root's point is very well taken.

Mr. Root: I don't think so much that, I think we need a philosophy document to go along with it. Something that I can take to the project people and say here is what the Air Force and the Army and the Navy intended when they wrote this document. Here are the limitations, here are the escape clauses and so forth.

Mr. Volin: I think the intent is quite clear and that is to get the most reliable equipment for the least possible cost. The problem is very often that in trying to interpret the specification something gets lost along the wayside and people say what does this mean, what is behind it. I think this is what you have to try to convince your project officer of.

Mr. Moskal (Rockwell International): I'd like to get a clarification between a specification and a standard. Now MIL–STD–810 is referred as a standard and all I have heard tonight was specifications. It has always been my impression that a standard was a guideline.

Mr. Earls: I couldn't agree more. You write your specification from the standard once you know what equipment you will buy and you have some idea where it will be used; you use the standard to find what test procedures are applicable to your item in its location and from that point on I don't think you really have to refer to MIL–STD–810 except for the mechanics of the test. I think we get into a very vicious circle in the detailed specifications when we say test according to MIL–STD–810. I haven't brought this out yet; maybe this would be a good time to do so but failure criteria are a real nebulous area. How do you know when you have a failure and what do you do about it? Very often the detailed equipment specification will say test according to MIL–STD–810 and then if there is a failure nothing is said what they do about it.

Mr. Moskal: Well perhaps we should consider changing MIL–STD–810 to MIL–S–810.

Mr. Earls: No, you can't do that because you write detailed specifications for a particular piece of equipment or for a particular airplane, and you use what is applicable. MIL–STD–810 can't be applicable to everything you just take that part for your application and write your specification; the specification is contractually binding.

Mr. Moskal: In other words we are talking about MIL–STD–810 as a guideline?

Mr. Earls: Somebody has to take that and write a specification.

Mr. Feroli (Aberdeen Proving Ground): One more word about this "escape clause". I remember that we were working on this MIL-STD-810C during a tri-service meeting and somebody suggested that we eliminate this "escape clause"; well with a firm unanimity that we have never seen before the three services were completely opposed to eliminating that "escape clause" and a question came up as to how should we write it in; the answer was let's put it in capitals. So I think that for the benefit of the contractors it is important to realize how significant the services felt that this "escape clause" was; but I want to point out one more thing. We feel that in deviating from MIL-STD-810C the burden of proof lies on the person who wants to deviate, it isn't the other way around. He has to have pretty good proof that the environment that his item will experience will not be that as explained in MIL-STD-810C.

Mr. Wilkus: In connection with some of these comments that are made, this is a problem of "don't confuse me with the facts my mind is made up." You people think that you have run into this. I think that it is incumbent on the technical people in relations with management to become acquainted with us the technical experts. I am disappointed that people think that a specification or a standard is being imposed on them, and incidentially I think the standard only signifies that it has been coordinated among the services. We don't start with MIL-STD-810 and try to get somebody to prove that they can deviate from it in ASD. In all of our major contracts we start out in the very beginning, before the proposals and the requests for proposals come in, and require that the people make realistic estimates; we don't start with MIL-STD-810, it is only a guide. We want the prime contractors to make a reasonable estimate of these environments and derive the test requirements. We want to participate in that and agree with it. We change these if reasons for that arises so I think that there seems to be some wrong impressions here. For example we discuss the specification requirements, we try to arrive at a reasonable answer, and based on technical considerations revise it if it is needed; we spend a lot of time, we find out what is happening and as another gentleman pointed out, you get the equipment too late and he is trying to show that it will pass a test and it won't even begin to pass one. We don't intend that the qualification test be used to find out if equipment will pass the test. We hope that it only validates that the development and the testing and the evolvement of a design have been done. It shows that the work we presume that you have done, has been done. Frequently as it has been pointed out, nothing, no test, nothing of any kind, has been done until way down stream.

Mr. Hancock: When will the myriad of specifications, such as MIL-T-5422 be revised to reflect the standards of MIL-STD-810?

Mr. Earls: MIL-T-5422 is not presently recognized by the Air Force. It was deleted by the Air Force and you use MIL-STD-810 instead of it so we are interested in revising MIL-T-5422 in the Air Force

Mr. Hancock: What about MIL-E-5400?

Mr. Earls: We have convinced some avionics people that using MIL-E-5400 is the wrong approach and that random vibration is the right approach. I think it will be easier to convince them once we get it into MIL-STD-810C. General Philips, the Commander of AFSC is an old SAMSO man who developed random vibration tests for the Atlas missiles; he is sold on it, we are all sold on it as a technical community, and it looks as if it won't be any major problem. I haven't approached any other people than the avionics people; we have run random vibration tests for them, it has really opened their eyes and they have been for it. I haven't tried to talk to those who administer the specifications.

Mr. Hancock: What is your prognosis for Naval Air Systems Command acceptance and wide useage of MIL-STD-810?

Mr. Bouclin: I am afraid I can't answer that question directly. Test requirements as they may be defined in MIL-STD-810, probably do not come to the attention of the program managers back at the Naval Air Systems Command. Program offices at places such as the Naval Weapons Center arbitrarily make a decision as to the test levels that will go into the specifications that are already written; those of us at the working level may never even see them until once they are out on the street and have been signed off by the Naval Air Systems Command.

Mr. Hancock: I would like to comment on procedure 515, Acoustics. This gets to one of the key issues and that is the amount of tutorial material that is contained in the standard. I believe that in one of my written comments to Dave Earls I suggested that we do something about educating test engineers and specification writers with regard to reverberant test chamber sizes, specimen sizes, and lower limiting frequencies such as some of those such as ANSI 1.21 has contained recently. Is it possible in this C revision to address that sort of thing?

Mr. Earls: It is possible if you have a write up that fits in there. I think there could be more guidance as to test chamber size and low frequency capability of the chamber. It is not too late for it as long as it's straight-forward.

Mr. Hancock: The problem is quite frequently the lower frequency for acoustic tests of cigarette size packages which are installed aboard a missile in a one cubic foot compartment is down to 8.3 Hz or so; and theoretically they should be tested in an outdoor size chamber. We normally use a 400 or a 5500 cubic foot chamber which is still a little ridiculous because there is no way that you can run that low frequency in that small chamber. Of course it is always accepted since there is no point in having the 8.3 Hz test spec on it to begin with. That is an example. I don't know how you would put it into th standard.

Mr. Tustin (Tustin Institute of Technology): It seems to me that the test engineers who run tests need a great deal of guidance that isn't presently found in the standards that perhaps should be in the standards or in some closely referenced supporting document. Such things as an acceptable fixture, acceptable instrumentation, and pick up locations, should be defined better than they are. Questions concerning tracking filters are often not answered by the standards; also what is an acceptable sine wave, or how much distortion might be permitted? I would like to go along with Mr. Rothaug's previous comments that the test labs need considerably more in the way of procedures and guidance.

Mr. Volin: To answer one of your comments I noticed in the current proposed version of MIL-STD-810C that there is a minor discussion on the use of tracking filters but I would certainly agree with you that it could be amplified as to their use and limitations.

Mr. Rothaug: Perhaps one answer to this would be to write into the MIL-STD-810C the requirement for detailed specifications, and even beyond that, detailed test procedures that would have to go back for approval before tests are run.

Mr. Tipton: It is probably impossible in a general standard to get anything detailed however I think the "escape clause" is something that people look at and I don't see any reason why it couldn't be expanded to include more realistic test procedures.

Mr. Rothaug: I didn't really mean as a general specification, but for a particular type of equipment, or even major equipment, such as detailed aircraft specifications written around MIL-STD-810C which would be further defined. The standards would exist on its own as a standard for general use and then specifications would be written around the particular type of equipment that the services are procuring.

Mr. Tipton: Well, if MIL-STD-810 is imposed and it doesn't give you any "escape clause" to generate the applicable test methods I don't know what to do. The way I interpret MIL-STD-810, it gives you an "escape clause" on the test curve to be used but not on the particular test method to be employed.

Mr. Rothaug: My previous point was that the test methods were not defined, and that the specification would further define those test methods for each particular application. This is to be assured that the Air Force or whoever purchases the equipment will eventually get material that has been properly tested.

Mr. Tipton: There is one approach to that and it is not done on all contracts but it is generally done on major aircraft contracts where test procedures are either sent from the customer to the equipment contractor, or to the airframe contractor, and that is MIL-STD-810 can only be used as a guideline on each particular test for each particular equipment. I believe whoever does the testing has to be imaginative in generating his own particular unique test requirements for a particular piece of equipment; and when those requirements aren't addressed in MIL-STD-810C, and actually most of the detailed test requirements that may be unique are not addressed in MIL-STD-810C, so someone has to use imagination in arriving at the best way to perform a test. Sometimes there are requirements for test plans which go through an approval cycle, and other times they are not, but I think the only way to handle these unique cases is to write them up on a separate basis.

Mr. Curtis: I would be against trying to put too many procedural type requirements into MIL-STD-810C. It seems to me that a military standard is a requirements document with enough "how to" kind of information in there so you generally know what you are supposed to do; but that has to be followed up by a detailed test procedure prepared by the people who will do the test. That has to include a lot of information that you never could put into a general military standard because not only do you have to include in that procedure how you are going to create, control, and measure the environment itself, but most of the tests require that you have to perform the functional testing on the equipment. Those requirements are unique to whatever you are testing and you have to have that one consolidated document that tells both sides of a test team how they are supposed to work together; you just couldn't possibly generalize that kind of information. I don't have the numbers with me but the spectral density levels for internal avionics that are called out in MIL-STD-810C appear to be quite high compared to the measured data on avionics equipment in a number of aircraft that I have in hand. I recognize that it is an envelope of the data, but even as an envelope, it seems quite high. Was there a conscious increase of that level to provide a known margin in establishing those levels?

Mr. Dreher (Aeronautical Systems Division): There are no factors of safety or tolerance margins in it, we only tried to envelope the data. We found the mean value of the data from each airplane, we found the standard deviation, and then we added 1.6 standard deviations to the mean value which brought us to about the 90 or 95% coverage point, so we covered 95% of the data. The 5% of the data that was above it generally was in the order of 3 dB above this 95% coverage level. Perhaps we can look at it in a little different vein; instead of taking the 95% point, suppose we took the mean value of all of the data and said let us consider the mean value as a reasonable average since we have a fair data spread, and let us use that as criteria for the level that we feel is going to be there. Then I would ask myself if that is the level that is really going to be there and if I want to add a factor of safety to it, then what factor of safety do I want to add? There are a number used in our industry. I know the structures people use a factor of safety of 1.5 or 3 dB, in design. I like a factor of safety of 6 dB because it gives me a little comfortable feeling; if I consider adding 6 dB to the mean value then I would find that I would be close to the 95% data coverage point. That is, it is about a 6 or 7 dB spread in the data from the 4 airplanes that we looked at between a mean and a 95% coverage point. Now you may prefer to test to the mean and not use a factor of safety and say let us take our chances out in the airplane, let it fly around – we may or may not get failures; but my own position and design experience is to have some factor of safety in the system and that is how we arrived at those levels. I want the confidence that the actual levels are going to be less than those that I know my piece of equipment can take.

Mr. Forkois (NRL): I haven't seen this last version of MIL-STD-810C but I wonder if anyone has considered that the levels of vibration should be associated with the weights of the equipment and to what parts of the structure they are attached? It seems to me that the energy is distributed in a different way according to these factors.

Mr. Piersol: There is a mass attenuation curve applicable to aircraft equipment and the levels for external stores are based on a mass density.

Mr. Forkois: I am not familiar with specification in as much detail as you are, but it just seems to me that maybe if you have something that weighs 2000 pounds and it is attached to the main hull girder of the aircraft, or the fuselage, this would indicate that you would have a different vibration environment than if you put it on the tip of the wing. Perhaps you should have some sort of a scale which is directed toward this concept.

Mr. Earls: It is in there and it is the same idea that was in MIL-STD-810B. When equipment weighs above 80 pounds the mass attenuation factor can be applied when you calculate your test curve. It goes down 6 dB for 160 pounds, so when it is 6 dB down we get one fourth of the level.

CO-MODERATOR'S SUMMARY

First our introductory comments, an excellent presentation was made by David Earls who described the main differences between MIL-STD-810B and C and then in a series of comments from the stores application, he described deficiencies as he saw them in MIL-STD-810B and he pointed out where he considers the improvements to be in MIL-STD-810C. Peter Bouclin made comments for stores, making a special point of the hard point mount called for on MIL-STD-810B and its elimination as a desirable feature. Joseph Gaudet pointed out that he has had no particular problems with MIL-STD-810B and that he would like to see more information on MIL-STD-810C since it is a new standard and he hasn't had difficulty with the past one. I think this does note that we are all going to have to have a period of introduction to develop some confidence in this standard. Al Tipton, for the case of aircraft, noted deficiencies in MIL-STD-810B particularly the lack of a random vibration requirement and he noted some of the deficiencies that he sees in MIL-STD-810C. Finally Gene Laboissonniere discussed the problems that are associated with the ground equipment and pointed out a number of deficiencies in the application of MIL-STD-810C to ground equipment. We had a lot of good discussion, and it seems that perhaps three points were predominant. Considerable discussion ensued on the problems of deviating from MIL-STD-810C, that is whether or not the "escape clause" can be executed and we have had several views on that; in summary it appears that if sufficient justification is available deviations can be obtained, this is a standard not a specification. Probably the easiest time to do this would be in the generation of the specification. There was some discussion about including backup philosophy. I think there is general agreement that that should not be in the standard, the standard is long enough now; but there has been some discussion that maybe it would be worthwhile as a support document for the standard. For the case of external stores tests Allen Curtis pointed out something to me that I didn't know; apparently there is no specified level between the peaks in the lower frequency range for the store vibration tests and that certainly is an important point that is worthy of consideration. We also had a comment noting that when MIL-STD-810C was applied to a very low mass density store that reasonably high levels resulted; this supports one of my earlier comments that there probably should be some sort of bounds since it is very easy to run into situations that are outside the range of the MIL-STD-810C data, and generate unreasonable levels.

TEST OR ANALYZE

A Panel Session

Moderator: George Amir

Panelists: Harold M. Forkois, Naval Research Laboratory
I.B. Irving, Johns Hopkins University Applied Physics Laboratory
Grant C. Schoonmaker, Bell Telephone Laboratories
Ronald L. Eshleman, IIT Research Institute
Robert M. Mains, Washington University
Clifford S. O'Hearne, Martin Marietta Corporation

Mr. Schoonmaker: Actually there really isn't that much division between the two different design techniques of analysis and test as the separation of the tables might indicate. I really didn't anticipate being first and even at that I think in order to maintain my position as a test supporter I am not going to give a completely unbiased presentation. We know that there are a number of factors that enter into the choice of a design technique whether it be testing, analysis, or the combination of the two; in fact the title of the session is incorrect it should say and/or. There are factors such as severity of the environment, complexity of the structures, criticality of the particular design mission of the equipment in the environment, and there are economic considerations as well. We at the Bell Telephone Laboratories perform many analyses and you will have an opportunity to hear a paper tomorrow on an analytical study of a community dial office and its resistance to various seismic events; but before I go any further I would like to take a look at one equipment cabinet. I heard someone in the audience say how would you like to analyze that and that is my point. My first slide shows a rather complex structure it is rather large and some of you may be familiar with it. It has a rather complicated electromechanical function. These are traveling wave tubes that are mounted in the cabinet in a rather unusual way, there are 40 thousand volts wandering around in that cabinet, and there is a rather complex structure underneath it, including tube sockets and the like. If I ask you to simply design this so it won't fly around the room that is a very simple matter; but if I ask you to design it so that it will survive an atomic blast and work during that event and afterwards, then I think we have a slightly different situation. You will admit that this is complex. My second slide shows a diagram of the inner components of the traveling wave tube; can you imagine how many high priced analysts, how many days, hours, or month of analysis would be required to properly model this? And having done that, input the necessary environment at some interface which you might be able to choose and have any degree of assurance that it would survive the environment? I think you would agree that it is not possible. Just in case you think that this is not a completely atypical situation I have a third slide that shows another large cabinet that is filled with electronics that more or less control the design of the interior of the cabinet. My last slide shows another rather complex piece of electromechanical gear, all of these were required to fulfill a mission to survive an atomic blast and operate during and after. To summarize my opening remarks there are a lot of things that you can do with testing that you can't do with analysis.

Mr. Forkois: Engineers, whether of the design variety or stress analysis-specialist variety, are confronted with a vast spectrum of unknowns. These can be placed in two categories, known or anticipated unknowns, and unknown or unanticipated unknowns. The former unknowns are generally amenable to the level of knowledge of established procedures or methods of technology and experience of seasoned engineers. The latter unknowns are the troublemakers and in many cases turn into rutheless killers. Examples of the latter are the inflight explosions of several Comet Jet Aircraft, crashes of several of the Electras, the F-111 crashes, the collapse of the Tacoma-Narrows Bridge in the state of Washington in the thirties, and more recently the collapse of the Point Pleasant-Gallipolis Bridge across the Ohio River. The causes of the disasters suffered by the Navy through the loss of the Thresher

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and Scorpoin submarines remain baffling mysteries. In certain instances although the technology may exist in the literature its application to a current design is overlooked or disregarded by the pressures of the schedule makers. The latter scenario may convince juries of criminal negligence in addition to granting civil penalites. Failure of the Comet was attributed to a fracture origin at the welded reinforcement for the window openings combined with a brittle aluminum alloy and the cold temperatures of high altitudes. A complete laboratory test of alternating ground-air-ground pressure cycling, combined with simultaneous high-altitude cold-temperature to ground-temperature cycling, in a suitable facility capable of handling the entire fuselage would have revealed this design defect. In the case of the military F-111 crashes, imperfections in a wing structure forging were a source of fracture origin. Extensive non-destructive testing of these critical parts involving the use of more exotic metals would have shown these defects. The commercial Electra aircraft engine pods were isolated on very soft low-frequency vibration isolation mounts which responded violently to turbulent flight conditions. This violent response caused a wing collapse with total disaster. This design error could have been uncovered by a proper wind tunnel test simulating non-steady air-flow. The failure of the Tacoma-Narrows bridge was also caused by turbulent wind conditions exciting a very low frequency structure into destructive instabilities.

I think materials properties are a weak point in many analyses. More specifically analysts use material property values based on slow strain or stress rates applied in so called static testing machines. Under actual shock conditions it has been estimated that stress rate values in simple structures are as much as 30,000 times greater than in the static tensile testing machine. The effect of stress or strain rate is not normally considered in engineering analytic investigations with any significant degree of confidence. The assessment is further complicated if temperature effects are a design parameter.

Electronic equipment is usually housed in cabinets made of aluminum, (for non-magnetic requirements), dimensions parallel the configuration of the human anatomy, that is 72 inches high, about 24 inches X 24 inches on the base, and about 26 inches deep, and they weight on the order of 1000 to 1500 lb. These structures have accessibility and maintainability requirements which detract significantly from the amount of continuous material which can be included in the calculation for moment of inertia in determining deflections in bending. In one case an analyst's calculation indicated a loaded fundamental natural frequency of 55Hz. My testing experience indicated that 20Hz was optimistic. The actual value under test was 17Hz. Thus the effective moment of inertia, as determined by the natural frequency of the actual testing, indicated a value considerably less the calculated one. By employing the design technique which involved the use of a greater number of tight fasteners the sheer effectiveness of the front and rear panels was substantially increased. This increased the structural homogenity of the cabinet and its natural frequency was correspondingly increased to 30Hz, suitably in excess of the maximum upper forcing test frequency of 25Hz and a corresponding Transmissibility Ratio of less than 3 to 1 was attained. So these are the two major items in which I think the analysts are weak. Even though they do good work there are serious unknowns.

In many cases the building of a model and conducting tests is less expensive than a computer analysis especially for smaller items. The experienced designer, cognizant of the lack of data on the dynamic properties of materials, will use a "factor of safety" in his strength estimates and rely on testing as much as possible to cope with these unknowns. In addition the "unknown" effects of defective material with regard to fracture initiation and the "unknown" contribution of fatigue to structural weakness should reinforce our conviction that testing is necessary and should be employed to the greatest extent possible to cope with these "unknowns" involved in the design process.

Mr. Irving: I pondered the question of to test or analyze and it appears to me that I have a choice to test or to analyze. After pondering the question I have come up with a simple and quite obvious solution, you test everything that is small and you analyze everything that is big. This would leave the burden of the decision to less technical and more administrative group personnel. This I fear is too simple a solution and would meet with great opposition, primarily because it deprives us of the responsibilities for which we have all been trained. In my simplified opinion vibration testing is the art of discovering and measuring resonances. Vibration analysis is the art of determining when and where a resonance will occur and the effect of the resonances on an object. Good mechanical design is the art of eliminating a resonance or putting it in a place where it will least affect the object. In my opinion testing is a nuts and bolts thing, it is very real, it is like the value of pi, it is a real number, and it can be carried out to some required accuracy. Analysis to me is paper, lots of paper, it is imaginary like the square root of minus one, but without it of course we cannot return mathematically from infinity. Both of these things I believe are intuitive to the good design engineer. From my experience I have found some design engineers who really dread analysis but nearly all the design engineers I know dread testing, we as human beings do not like to be found wrong. I think I would be remiss in my duties as a test engineer if I did not bring up the old example of something that has been nicely designed, it has been analyzed, it has been built, it has been tested and it works very well. Build another just like it and it should work just as well; but suppose some person doesn't tighten the nuts and bolts as well as the first person did. Suppose he runs out of nuts and bolts and the fabrication engineer decides he should use rivets instead. Suppose he runs out of a certain item and changes manufacturers of a subsystem, component, or an element; will the unit work? That is the first question, maybe. Will it work as well? You really can't find out by sheer analysis you have to test and actually I feel that both should be used to assure quality. And if I may steal a statement from Ralph Nadar, both must be used if someone's life and limb depend upon its satisfactory operation. I like to think of testing as a protection. Testing is costly, and because of its expense it may well be viewed by many of your administrative personnel as the biggest protection racket that has been thought of since the Mafia. Testing is a protection for the design engineer, for the analyst, and the fabrication engineer; it is a protection against mad scientists, inventors, suppliers of inferior products, and sloppy workmanship. Recently I inherited a motor scooter; a moter scooter is a motor, a handlebar, a tractor seat, mounted on a set of casters, and with that statement I have analyzed it. Fortunately the state in which I live will not allow me to take that on the road until both it and I have been tested; testing is a protection even a protection against yourself.

Mr. Eshleman: The relationship between analysis and test with respect to problems has been debated long and many times before this. Whether the engineer decides to test or analyze or do a combination of both depends upon his background and his facilities. Despite that our testing friends seem to indicate that testing is cheap and analysis is expensive, I assure you that this is not always true; there is certainly a balance somewhere between them. I like to look at the design program from an overall point of view where you use some of both and you end up with engineering. I like to think of analysis as the type of thing that you do to get into the "ball park" with the design. If you are designing something with which you have had a great deal of previous experience the designer has the art already and he can put it on the board with a minimum of analysis; but if it is something that is pressing the state of the art or is unusual, I think that some sort of engineering analysis is certainly justified to get into the "ball park". Thus you do not have to test ten or fifteen items before you get one that works; that certainly has to be a balance in cost that you must consider. I like to think of two different kinds of testing; one is a basic testing for properties and materials which Harold Forkois mentioned and he is certainly right that much of the strain rate effects data that we use in analysis comes from static tests when we are trying to analyze dynamic phenomena. This is what I call characterization tests and it is something that we haven't done enough of; and we could do a better job in our analyses of new structures and machines if we had some of this basic material characterization or component characterization at high strain rates. Admittedly we must get that from testing. The second kind of testing is performance testing within the environment and this is after the product has been built and you want to see if it will work. This we might call environmental testing. There is one area where I think I have used analysis profitably, and this is in a case where we are studying the safety of some sort of a vehicle or machine that in the process of its function may become unstable or it may destroy itself. I don't think it is a good idea to test those sort of things. One example that I am thinking of is vehicle handling where you want to know the safety of the vehicle near its stability limit. Of course there is really a high risk in testing there and when you have a high risk in testing it costs more money. This is a place where analysis is really effective when you test a mechanism or a vehicle up to a certain level, maybe at three quarters or at half level, to verify your model and do the rest of the testing so to speak on the model; I think models are very effective in this way. In conclusion some of the criticism of analysts comes from what I call the number crunchers that model problems with an infinite or a finite large number of degrees of freedom when they really don't need it, and they crunch a lot of numbers out and don't even use them. Now there are testers that do this also. They put a lot transducers on a model and get a lot of numbers that maybe they won't even use. But these are both extremes of what I call bad engineering practice.

Mr. Mains: I don't know whether I like being classified as an analyst. I like to think of myself as an engineer who does analysis sometimes, tests sometimes, and mixtures most of the time. Analysis can extend from a few numbers on the back of an envelope, to careful numbers done by hand, to moderate computer operations, to immoderate computer operations. By immoderate computer operations I mean that kind of calculation which has been mechanized to the point where the man is out of it as much as possible, and has lost all contact with the physical reality of what he is supposed to be analyzing. I don't like to see this done because I don't like to have it happen to me. I like to keep close physical contact with what is going on so that I can apply some judgement to it. Testing ranges from something as simple as cutting out a cardboard model on your desk and pulling it around pushing on it to see how things deform so that you can decide what kind of analysis to do. The simpliest kind of model would be that. I keep a spline in my file cabinet and I frequently get it out and twist it and wave it around and look at and see what the deformed structure looks like so I can see what to analyze in the first place. A more complicated model would be one that would be built to scale and perhaps of a different material in order to get such things as flexibility numbers, or stress numbers that you could not reasonably get by analysis; and then there is always a full scale prototype test that is done to either to verify an analysis or to develop data for the next design and the next prototype, and home in on a final design. I guess my point of view might be summed up in this way. Analysis or test? Each problem is different. Each one needs to be sized up on its own merits and as much analysis is done as is needed, and as much testing is done as is needed, to insure that the product performs its intended function efficiently and at a minimum cost. You cannot hope to do more than this.

Mr. O'Hearne: I think that we have an awfully difficult topic today because you have to put so many qualifications and consider so many contingencies to discuss it intelligently. I think the reason that I accepted this position was that I was sitting in a lecture room shortly before I was called to sit on this panel and one of our product course analysts was giving a lecture on design to cost. One of his slides in his talk showed that one of the principles of design to cost is more test and less analysis. Well naturally I objected to that because I though that everyone knew that in general tests cost 100 times more than analysis for the equivalent information; in fact I know of some informal studies that have been made that have shown that. So I questioned him and he wasn't quite prepared to answer me, it was a small part of his lecture and I believe it was something he had copied. There is a sort of friendly rivalry between test and analysis people similar to that between battleship sailors and tin can sailors in a fleet smoker. After I made my point our structures and environmental test lab manager across the room said I don't agree with you Mr. O'Hearne and everyone had a little laugh and that was the end of it. Of course I don't have the luxury of following that sort of thing up, whether more test and less analysis means what I think it means in my product course analyst's lexicon, but I think it is something like a recent remark by the President of the Ford Moter Co; he said what the American public wants is economy and they will pay anything to get it. I think the dichotomy between test and analysis is not quite appropriate in terms of people, at least in the types of organizations that I have worked in. That is without all of the qualifications, you can think of a test as just another analysis in which you have used a very special purpose analog computer erected in the structures lab. Really if there is a rivalry it is between the people who do the test; the type of instrumentation and power supplies, the method of analyzing the data in the laboratory, and the type of computer to be used. My circumstances are that I have a decision to make on which computer to use, and it is usually my own decision.

DISCUSSION

Mr. Reed, (Naval Surface Weapons Center): I think it is sort of questionable that anybody should argue about whether to test or to analyze. I think the whole problem is a matter of working together. I think a good example would be to select a wife based primarily on the results of a NASTRAN model, it would be foolish not to do a little testing; but on the other hand it would be foolish to do it based purely on testing. I think the separation that you gentleman had at the beginning of the session was somewhat symbolic. Based on my limited experience, a designer only comes to an environmental test facility when he wants to deliver his hardware or to pick up his hardware, and an environmental test engineer only talks to a designer when he is going to call him some names because he brought this piece of hardware that obviously couldn't pass the environmental tests. I think that the whole problem is the fact that designers and test engineers really don't work together enough. I have been in somewhat of a unique situation in that I work in an environmental test laboratory and by some means that I don't question, I just accept it, I have been able to so some modelling. I have had the capability of running a NASTRAN model, or some other model, and then putting it on a shaker, a shock machine, a temperature chamber, or some other test equipment. It reinforces your confidence in a test and it reinforces your confidence in the models. To argue these two positions is sort of representative of the whole problem. As in the case of the like the examples that were shown on the slides, obviously once those things are built the easiest thing to do is test them. But they are obviously not going to survive any kind of nuclear shock so why didn't you find that out by some kind of analysis before hand. I think that you should be working together.

Mr. Forkois: To specifically answer your problem, in which do you have more confidence? In my experience they seem to think that the analyses are perfect, and there is no error because there is a high order of mathematics in there. When something fails in a test they always pin point the test and say that it is wrong. They have a ten cent item to fix but they are reluctant to do this; they spend more time on an analysis of what is wrong with the test. If they would just spend ten minutes time to correct the failure in the equipment they wouldn't have any problem whatsoever. This is the point; it is easy to find an error in a test and everybody who has eyes can see it. But it is very difficult to interpret, analyze, and find errors in someone's analysis.

Mr. Eshleman: Mr. Forkois says analysts believe that their numbers are exactly right. I have found many test engineers who also believe that their numbers are exactly right, that there is no error in the instrumentation or where you put the transducer; but I think anyone who says that is not an engineer. I think there are extremes on either side.

Mr. Maines: We had an old saw around the Applied Physics Laboratory which used to go like this: Everybody believes the test except the guy who made it, and nobody believes an analysis except the guy who made it, or you can turn that around if you like. The point is that unless an individual has been intimately concerned with the process he doesn't have much faith in it. We used to require that the designers come down to the environmental test lab and take part in the testing while it was going on so that they could see what the equipment was doing and therefore be able to design better the next time. This is a constructive way to go, it helps if you can also get the environmental test people up into the design area once in a while. It can be done.

Mr. Schoonmaker: The obvious intention of putting those diagrams up there was to excite someone to the point where they would say well obviously you can't just test that unit there must have been some design done and obviously there was and there was quite a bit of analysis done prior to the testing that took place. One possibility exists for better cooperation between the test personnel and the analyst. I have found in many instances the test engineer is not incorporated into the program at the onset and in a number of instances he finds himself on the tail-end of the program. Perhaps the other test people could corroborate this, or maybe it is a unique problem, but I have experienced this. There could be a greater understanding if this cooperation took place at the beginning of a project.

Mr. Paladino, (Naval Sea Systems Command): The panelists have all presented good points. Analyses can get you in trouble when you try a new procedure that you haven't really tried out on a design; by this I mean in the early days we in the Navy tried to predict frequencies for our propulsion systems and it was catastrophic. But as we went along and we got experimental data, underway data, we were able to put the empirical values to the analytical program that gave us credence for making these predictions. Today the Navy must use analysis for the propulsion systems in any new ship. If we built before we did the analysis we would have multimillion dollars of shafting, and turbines, and reduction gears that wouldn't be worth very much. Analysis does another thing, if we find we have critical frequencies that we can't tolerate at sea we can make an adjustment to location of thrust bearings and/or steady bearings. There is a place for the analyst and there is a place for the experimenter.

Mr. Forkois: A lot of these things are subjective and the cracker barrel philosopher always found out that the intelligent people always tended to agree with him, so I think we have this problem. However when people do analyses we have people who are involved in concepts and they symbolize these concepts in equations and these are general equations. You can't get numbers from them because they don't have constants, they don't have the emperical data, and they don't have the constitutative equation in which to put the constants. The engineer must arrive at numbers and he must arrive at numbers which are in the ball park. I do analysis myself even though I am in the test area, but I find the analysis people over do it many times; I think Dr. Mains will agree with me that a lot of the analyses could be shortened and simplified a good deal and more reliance placed on a good test.

Mr. Cole, (Naval Surface Weapons Center): I guess we are all saying what is obvious. It seems to me the point of the discussion is I agree with each speaker who speaks but disagree at the extremes. Shouldn't the point of the discussion be when is the best time to test and when is the best time to analyze rather than try to arbritrarily set up barriers? Obviously we all know that we have to both test and analyze.

Mr. Mains: That is a very good point. I think that what ever dichotomy exists between analysis and test results from the test being looked upon as the last step in the operation to be held off as long as possible in the hope it might go away. What really ought to happen is that the designer and the test engineer should be working hand in hand every step of the way, and there are many simplified tests that can be made during the course of the design which will anticipate future difficulties and allow you to correct them. I for one very much favor concurrent development testing with analysis.

Mr. Forkois: Very well put Dr. Mains and I agree with you one hundred percent. I think what we need here is a definition perhaps of the different kinds of tests. There are all kinds of tests and I ennumerated some of them but did not present them in my opening statement. We have developmental or experimental of tests, these are XN1 tests in which we test a new product to determine its feasibility. And of course the Navy has many of these tests; we make a limited number of samples and then they are put aboard the ship. The Navy has groups which then test these items aboard ship; the fact that you have passed certain tests does not mean that it will automatically be placed aboard a fighting ship. It means that there are compatabilities and interferences that have to be analyzed and it has to be integrated with the general military concept of waging a war. Later on we come along and everybody says we want five thousand of these things right away, so we issue a contract and we have a preproduction test and its function is to test out all of the changes that might have been made from the original because I am sure that none of us are really perfect to begin with. Then we go into the production phase and the value engineers, who are looking for a penny here and a penny there, often change a design and do not tell the designer of the change. I recall a simple test of push button made by one of the manufacturers, who had been making these for years; all of a sudden the push button was making contact under shock conditions when it wasn't supposed too. They immediately though that there was something wrong with the machine. Inevitably it is never the product it is always the machine. They brought it to my lab I tested it and I said that something was seriously wrong, perhaps they changed the material. They went back and they found out that the copper strip wasn't hardened, somebody had changed the process so that the spring was actually deforming plastically or it didn't have the right constant. These are things which can cause trouble; a simple push button is no great shakes for any of us here but it is very important for the fleet. Then we have tests which I call quality control tests and they are screening tests. These tests don't simulate anything in particular. We give them a 75 or 100g halfsine pulse shock test; this is a good screening test and these products, when used in an integrated system, will probably function very well because they have been screened and many of the troublesome things have been worked out. It does not mean necessarily that it would work when it is interfaced with the larger system, but at least you know you have something that is fairly good. Then we have the reliability testing which Colonel Swett mentioned yesterday and I was surprised to learn that the Air Force doesn't operate their equipment during reliability testing, I am sure the Navy does. So I think we have to keep in mind the fact that testing is actually part of the design process. I was trained as a designer and I use the test machines as analogs. They are my computers and I have faith in them.

Mr. Root, (National Waterlift Co.): You have mentioned the development type tests and I can think of many development tests that have been poorly run because the engineer didn't do any analysis ahead of time. He didn't know where he wanted his accelerometers or his strain gages and yet I seem to think that you have said that you can test without analysis.

Mr. Forkois: No sir! That is not what I said at all. I was trained as an engineer and I am familiar with differential equations and computers, but the problem is one of making sure you have the right people. In many cases in the electronics industry the electronic engineer has cognizance of the equipment and when they get into the mechanical engineer's field many times we have a disaster and a catostrophe. I think industry has realized this and in recent years it has given the mechanical engineer a little more prominence than just the electron chaser.

Mr. O'Hearne: I would like to respond to that especially since Mr. Forkois has used the word bad analysis a few times. I would like to have the privilege of using the phrase bad test and I agree with Mr. Root that it happens quite frequently in development. This is particularly true on the substructural level in the laboratory, where boundary conditions have nothing to do with reality, inputs have nothing to do with reality, and there is also a tendency to compromize when this is realized. Let us shake it and maybe we will get something out of it, which I think is ridiculous but I have seen it. Perhaps the way this should be put together is that all test and analyses need more criticism. My personal feeling is that there is a tendency not to be critical in the test and analysis area the way we are in the design area. The designs are frequently reviewed, the designers have to show their trade-offs, and tell their reasons; but when it comes to discussing the tests and analysis all they want to know is the results, not how it was done, why it was done, and they take those numbers. I think one way to resolve question of bad test and analysis is for organizations to be more critical and have more people look at what is being done; probably the best way to do it is to have the analysts or the test engineers themselves ask for the review the way it is usually done with the design.

Mr. Slupek, (Ingalls Shipbuilding): In the area of shock and vibration we frequently have the problem of whether to analyze or test when it comes to qualification. Now I think the real question that should be addressed is if you were to buy an item what would you be willing to accept in the area of either test or analysis in order to realize a certain degree of confidence, knowing that you are the one that will have to pay the bill? I think there are factors that affect our decisions, for instance reliability requirements. A vendor will supply items or components for designs in which there are no reliability requirements, no mission essential characteristics, or the items may be hazardous to personnel. In other areas we have to consider the ability to correct deficiencies under actual usage. For instance in the shipbuilding industry, or in the operation of a ship, you can correct certain conditions during trials, whereas in the missile field you just have a single chance, and therefore you will have to perform a great deal more testing to make sure that you gain this particular confidence. What classes of equipment could be considered as being the best candidates to exempt from testing?

Mr. Mains: There is only one catagory of equipment to be exempted from testing and that is the equipment that is so big that there is no way to test it. Everything else should be tested.

Mr. Caruso, (Westinghouse Corp.): I think the decision whether to test or analyze is already made for you. For instance the gentleman from the Navy mentioned that many times testing is just too prohibitive at a certain stage of the game; also cost may prevent you from testing so you might initially decide that it would be best to analyze. Schedules will often decide whether you have time to build the test fixture needed or assemble the tools needed to test, in which case or an analysis would be the way to go.

Mr. Reed: I think again this is a negative comment but the problem is that neither test nor analysis is very definite. The vibration test is probably a good example. If you give me a piece of hardware and you tell me you want it to pass I can put the control accelerometer somewhere to make it pass. If you give me a set of natural frequencies and you give me the drawings for something I can probably make the natural frequencies come out there, but the analysis is just not that finite a thing, nothing is. I don't think that you should miss any opportunity to assure yourself that something is going to work or function as it should, therefore I think that the two, test and analysis, go together very well.

Mr. Forkois: I think perhaps the discussion is evolving into something different. Perhaps the dichotomy between testing and analysis is merely a symptom of something a little more grevious, and perhaps it has to do with our industrial organization and the values we put on what we are doing. The Navy had a Systems

Performance Effectiveness Committee in which this was extensively analyzed and there will be an article, which Dr. Eshleman is preparing, that will appear later in the Shock and Vibration Digest in which we think there is lack in the administrative function diagram in the way things are done. We should get the environments into the concepts and into the thinking of people from the beginning, from the upper to the lower levels and the things other considerations that General Stewart mentioned yesterday. There are other considerations involved besides shock and vibration. We have safety, we have cost, we cannot afford to be so wasteful, and we cannot afford to be cavalier in these things. Remember if we ever get into a war your son may be aboard one of these ships, and you want him to have the best. I think perhaps we need a review of the whole situation from top to bottom. I think that this discussion is merely a symptom of something that is a little more grevious and General Stewart very wisely mentioned it in his opening remarks which were well taken.

Mr. Volin, (SVIC): The cost and the availability of the hardware was one of the prime considerations that has not been mentioned in the question of test or analysis. There are many programs now in our conditions of austerity where we do not have the spares, the developmental test models, to test. As a result we have to test where we can and we often have to qualify by analysis where testing cannot be done; this is either because of the lack of hardware, or maybe in some cases, as Doctor Mains and others have mentioned, the item is just too large to run any kind of a test on it whatsoever.

Mr. Schell, (SVIC): I think I can build a shaker to shake this whole Convention Center, if I were given enough money, and believe it or not some people have tried to do things like this. I recall a few years ago that some people were building a shock machine to put in a very short drop off time sawtooth shock test to a 1500 pound item. This becomes very difficult to do because you can't get short drop off times on large heavey items without a lot of effort. So I think that you can test anything that you want to but you have to make some decision as to how much money you have to spend.

Mr. Schoonmaker: Just a comment on Mr. Volin's statements. It is possible, even in the face of current monetary or fiscal problems to utilize testing eventhough we have very large structures where the cost would normally be prohibitive; this would give a very good example of the compatability of the two design techniques, and analysis and testing. It is quite possible to perform an adequate and not terribly costly analysis up to a certain point in a structure. Then as the complexity of the structure becomes much greater at the subsystem level the analysis up to this point could then be used to as an input to a subsystem. The testing cost would be relatively low and the analysis cost would be relatively low as well. The combination of the two might yield something very worthwhile. In addition to what Mr. Schell said about shaking very large buildings or pieces of equipment, there have been and I am sure you are all aware that many very large buildings have been tested. Obviously these are not destructive type tests. They are impedance type tests where the structure is characterized. It is possible to shake five story buildings, or buildings with a reasonable number of stories, characterize their responses.

Voice, (Airforce Flight Dynamics Laboratory): A preliminary evaluation and a review of past tests should be necessary in performing research and development type of tests because these tests have often been done previously and this can save the money of testing. Sometimes when you are testing at an environment at either extreme, so that if a given stress is required to fatigue a certain object after a certain amount of time, if your predicted stress is far less than perhaps a tenth of that amount I don't think it would be necessary to test. If it should be extremely high, because the environment would be much more severe, testing would not be necessary, I think we can just say it will fail. After we decide that there have not been any repetitions or extremes we should test and we should evaluate the data the next day after the testing rather than waiting several months. I have seen this happen many times, sometimes years after the test was completed. This will help us to decide the direction of the test, and to compare our data against our preliminary prediction; then we can decide whether something is wrong with the test or if something is wrong with the analysis. A final analysis and test report should be made as soon after the test as possible.

Mr. Slupek: It has been my experience that both management and the customer prefer testing so we tend to test to the maximum extent possible; however the rule that we generally follow is that it must be cost effective in all cases, not only in terms of money but in terms of people themselves and the mission.

Mr. Fortenberry, (Jet Propulsion Laboratory): I wanted to share a little 20/20 hind wight with the panel. The Langley Research Center, Martin Marietta Corp., and JPL had finished an all up test on the Viking spacecraft and that test required extensive pretest analysis just to determine if we could perform the tests, much less perform it without damaging the spacecraft; looking back I think it probably would have been possible to accomplish that test without the extensive analysis but I am also equally sure that without the analysis it couldn't have been done within the cost and schedule that we were provided.

Mr. Forkois: A previous speaker made a good point and I think that this is where the Shock and Vibration Information Center could help. When you have a contract to do a test for the Navy you are not doing it the first time, it has been done before. One of the big difficulties is the lack of accessibility to data on previous items which have qualified, and many times I think the Navy has qualified equipment on the basis of similarilty or previous tests. But in these cases you always get into the hassle of whether the unit is truly representative of the previous test. No matter what you do, according to Murply's law, you will have trouble there is no simple way of doing this. I think that the military can indicate through their computers when they have a successful test and the type of equipment that was tested; if we have this information many of you can query a computer data bank and it might give you something that may be similar to the problem that you have in hand. Another problem in design and analysis is the lack of continiuty in personnel; about every five years, as many of us here will testify, we go through this business of what is a g or whether you should use shock mounts. The Navy has the problem of educating many of these people and many of these companies who do not have the technical knowhow or competance, and then the Navy has to perform an educational function through its laboratories so that we have some continuniuty.

Mr. Chen, (Jet Propulsion Laboratory): For the past few years we have been doing structural dynamics in the Viking Orbiter project and we as engineers perform both the analysis and the test. We specify the test, and the instrumentation locations, and, our experience indicates that this is quite satisfactory. As an aside the Viking spacecraft was designed according to the analytical loads analysis. Maybe this is just one data point as to how test and analysis should be balanced.

Mr. Reed: Sitting here and listening to all these comments I think the problem is if this were a boiler makers convention there would be no problem. The technology exists to predict the stresses in a pressure vessel or a boiler; the analyst would say we can tell you what the stresses will be and the test engineer would test it at 150% of the design level or whatever after the boiler was designed. The problem is the lack of technology or the lack of confidence in the technology. I don't think any analyst will sit down and come up with his mode shapes, natural frequencies, or his transient responses and have a high level of confidence in his answers; there are many questionable areas. I think the total problem is that at the present time the state of the art is not sufficiently tested to get the confidence of the designer or anybody else and maybe that is the point. If you worked to make this a black and white issue and eliminate all analysis it would be a little hard to imagine that our technology will get us to the point where we can do an accurate analysis if the analysis capability is completely denied.

Mr. Mains: You should have been at the Pressure Vessel and Piping Conference at Miami in June where I presented a paper on the plastic analysis of heat exchanger expansion joints in front of a bunch of boiler makers. The state of the art isn't all that good; if you look at closures on pressure vessels, expansion joints, or inlets, that is nozzles, this is far from a closed issue, we need to learn a lot more about it.

Mr. Forkois: I think the learning process includes accidents and disasters as much as we don't like to think about them. I think they are real life tests and we are shocked by the results of these real life tests and we try to learn from them. They are in fact providing empirical data for both analysts and testing advocates.

Mr. O'Hearne: I believe there is a great deal of confidence in certain types of analysis, for example the normal modes of primary load carrying structure of the conventional type. We have been doing a good job of predicting them for years, and the final test usually shows that they are accurate, at least in the first few modes, to a few percent. I have read recently that there is some thought being given to eliminating the ground resonance survey on the Space Shuttle because of cost. I don't know whether that is going to be done or not but it is a surprise to me that it would be done; it certainly shows that someone must have a great deal of confidence in analysis.

Mr. Amir: I have a question for the panel. If we talk about the real design, the designer is faced with the decision to size his structure, and the question is how does he do it? Well there are many methods, one of them is to perform some calculations. The question is can we use today's sophisticated methods to support the designer especially where cost is very critical, and the schedules and the requirements are tight? There is a need to use sophisticated methods, can we use them comfortably and support the designer to produce his design?

Dr. Eshleman: There is no doubt that we can support the designer with analysis in most situations. I think most engineering analysts will have some confidence if its a reworked job. I think he has some information with which to start and it is similar to an iteration process. I would hope that particularly if some tests have been run that the results would feed back and the designer could use this information and shorten the iteration process over building prototypes and testing them.

Mr. Mains: I think the problem does not arise very often if the item to be designed is primarily structural. The difficulties usually arise when it is either an electromechanical or an electrochemical problem or some combination where the structure is simply the supporting shell and the engineering attention on the function of the electronic gear or the electrochemical gear is overemphasized. As a case in point I recently spent a good bit of time in hearings over an electrochemical item which was being manufactured by a supplier and sold to the Navy; would you believe that they sent the preproduction prototype in without ever once having performed a vibration or a shock test? They had performed electrochemical tests and they went into the preproduction test with no previous environmental test experience and were quite amazed to find that it didn't pass. The item failed to pass the preproduction tests many times, finally the contract was terminated and the manufacturer claimed that the problem was that the elements were overtested. If he went into the preproduction submittal without any previous shock and vibration testing in the development process he had a hole in his head. But you get into difficulty in these combined media or situations, where the structure is the supporting shell and something else is the primary function such as electronic gear, batteries, or search lights, because the primary effort is not on how to make the thing hold together.

Mr. Senn, (U.S. Army Test & Evalustion Command): Your comment about overtesting is familiar I have heard it before. If we have any choice on how to test an item we will put it in the real environment and run it, drive it, fly it, or whatever you have to do with it to get enough statistical data to know whether or not the item will survive in the real field. When we don't do that it is because we don't have enough time or money to do it. Then we go to the substitute which is a laboratory test and therein lies our problem, because you not only have to rely on the analysis to develop the test, and there is a certain lack of confidence in that analysis as well as the test; that is why we prefer the real environment. I don't see how you can have a laboratory test without analysis or analysis without a laboratory test and know what you are doing.

Mr. Mains: I agree with you. I get students from an aircraft company, some of whom work in the test laboratories and some of whom work in the design area. They tell me that it is not all uncommon to change the dimensions, the scantlings, on a part of the aircraft by as much of a factor two on the test floor after all of the analyses. When they get it down on the test floor they frequently find that it needs to be changed by a factor of two so don't have much faith in analysis or too much faith in test, question everything.

Mr. Dillon (Jet Propulsion Laboratory): I sympathize with one of the gentlemen who mentioned that the electromechanical parts were usually under the cognizance of the electronic engineer, that the mechanical engineer was divorced from the picture, and that most of the design proof was done by testing. I am in the electronic packaging area of JPL and over my life time I have noticed a trend away from the detailed testing and more into analysis to both save money and get a level of confidence that the equipment will pass these tests. I think that this trend will continue in the future and there will be less detailed testing as people learn to have more confidence in the techniques of analysis.

Mr. Mains: Those fellows in the aircraft company have a lot of confidence in their analysis and it is the filtering out of these people into other industries that is beginning to make analysis more popular there. I know this too intimately; I shudder, I tell my classes that I pull out my little five inch slide rule and say if you can't solve it with this you probably don't understand the problem. That is not entirely true but it makes a good point, especially after they have turned in designs with eight significant figures because that is how many they get from their little pocket digital calculators now. I wonder where our next generation of responsible design engineers will come from if they are allowed to go through life thinking if there are eight digits it must be right. The computer output from the analyses of the supports for electronic gear may be printed out to eight digits too but that doesn't mean it is right either. "Garbage in garbage out" you know.

Mr. Dillon: That is true and I won't argue that fact all I have said is that over the past fifteen or twenty years analysis techniques have improved and with the aid of the computer and the analysis techniques that we now have I think that the trend toward detailed testing will drop off. Testing is required but I think it is more in line with establishing that the materials are capable of sustaining the design loads and verifying their quality.

Mr. Mains: I think most of this is a result of the proliferation of finite element analyses to such an extent that where we used to do things on a judgement basis and simplify them down to the point where we could do some analysis, now instead they divide it up into an infinite number of finite elements and let the computer grind away without really examining the meaning of it all. I have a collegue who wants to do a stress analysis of rail cross sections with finite elements when this was all done years ago, in the thirties to be exact, with the infinite finite element, that is the zero gauge length strain gage photo-elasticity. He apparently didn't know that this had been done but he still wants to do it with finite elements; something is out of joint here, something is wrong. We need to ask ourselves now what am I really trying to do and how much do I need to do to get results; not Ok I have a program so we will shovel some numbers into it and see what comes out.

Mr. Forkois: If what the gentleman said is true that detailed testing is going to decrease and that you will perform cosmic analysis which will ignore all these things, then all I can say is that in about ten years from now you will have another session just like this wondering what is wrong. It scares me to think that what you predict will ever happen, and I just hope that it will not happen.

Mr. Fortenberry: I don't want to frighten you further but it has been my privilege to do a little interfacing with the Space Shuttle Program and we are looking at the possibility of deploying certain experiments from a Space Shuttle. We have met with some of the astronauts, we met with quite a few of the cost experts on the program, and the trend, at least as far as the Space Shuttle Program is concerned, is to fly the cheapest thing you can and if it doesn't work up there bring it back. This trend toward less detailed testing seems very real where that program is concerned.

Mr. Root: I would like to return to Dr. Mains' comment on this practice of carrying so many significant places. I think our analysts are at fault, I have worked with both designers and analysts and we have many analysts that carry things out to eight and ten places and quote it to the designer as if this is the gospel; I think the designers are just picking this up from the analytical people.

Mr. Mains: That could be. I recall a few years back the company that was designing and producing the control rod mechanisms for one of the reactors told us that we would have to change a material from 75 PH to 150 PH because we calculated the stress in this control drive mechanism under shock and it was 50,023 rounds per square inch and that the material was only good to 50,000 psi. I asked him what happened to the .38 after the decimal point and they didn't even know what I was talking about. These were both designers and analysts.

Mr. Eshleman: I think some of the discussion of the trend toward more analysis somewhat contradicts what I heard a couple of weeks ago at the ASME Design Technology Conference in New York; here we seem to be talking about the trend toward more analysis and less testing. There was perhaps a different group of people at that meeting but the concern was that the students that came out of college adapted immediately to the ways of industry in which the people in industry claim largely has very little or no use for analysis. I spoke to someone from United Aircraft in Canada about this trend toward analysis and he claimed that the new engineers would depend heavily on analysis on the first problem, the first design. These are very complicated rotor bearing systems. When the new engineer went to the lab and took some measurements on the system and nothing would approach his predictions he became completely disenchanted. Then he claimed that they would drift back and when they found out what it was all about and that you couldn't depend on one or the other things became better. So I think there are some different views on the problem.

Mr. Schell: One of the factors that is entering into a possible deemphasis of testing is its high cost. Rather then reduce testing we should examine some of the highly sophisticated techniques that are used in testing and in data analysis as well as the amount of laboratory equipment and man hours that are being spent in testing, and look for some simpler means of testing to demonstrate the equality of the piece of equipment. The Navy has used MIL S 901 Shock Machines for many years and are very simple. When the equipment is subjected to one of these tests the Navy has a strong feeling that will survive a similar type of near miss explosion in an ocean. It is a simple test,very inexpensive to run, and yet it gives good results; I think that maybe we are overly sophisticated in our testing techniques and that is where our money is going.

Mr. Beaulieu (Picatinny Arsenal): The Army has seen of the problems of the analysts and test engineers with the SAM-D Missile and they have put it under the CS square program which is a cost system so they could keep track of the costs of testing and analysis. This program has brought the test engineer into the program along with the designer and the analyst immediately. The end result has been that the environments have been monitored and changed, and the test programs altered quickly to respond to new design changes before it even got into the test lab. It looks as if it is a complicated system and it is a very expensive million dollar costing and controlling system but maybe some good will come out of it to monitor the cost in testing. The other factor in the high cost of testing is how do you write MIL-STD 810, put in temperature chambers, and put in particular requirements on a test and expect to do it cheaply? I just can't put the two together. Maybe we could discuss narrow band temperature requirements, low gradients, exact controls, multiple point controls.

Mr. O'Hearne: The SAM-D Missile has just completed a ten flight control vehicle flight test program and I was recently watching some movies in which they showed the firings of the missile at the White Sands Missile Range. A "Calcomp" plot of particular motion variables, such as attitude or lateral acceleration, would be on the screen and then as the missile flew you would see the actual in the test. The actual lay on the predicted everytime, and naturally the control test vehicle flight test program was not just for that purpose but I couldn't help think, if that product cost analysts were sitting in that room what he might have thought about the difference in cost between that Calcomp plot and that flight test.

Mr. Burns (Consultant): As far as the cost of test is concerned and whether or not we are too sophisticated you can't discuss that question until you discuss the purpose of testing. For example Dr. Mains mentioned that his students from the test labs sometimes had to double scantlings by a factor of two, but I wonder if those same students ever considered cutting the scantglings in half? If your only question in a test is whether the equipment is sufficient to purpose it is a relatively simple test. If the question is whether this is the most efficient that is a completely different question, and how much you spend on the test depends on how important the answer is.

Mr. Mains: Well they throughly strain gaged one of these aircraft structures and they frequently reduced the section but they also frequently increased the section; and when they increased or decreased the section it may easily have been by a factor of two.

Mr. Burns: With respect to elegeant efforts as MIL-S-901C efforts the question is are we simply after insurance or are we after design improvement?

Mr. Mains: Even after the changing of sections on the test floor in the aircraft business, this still doesn't tell you whether it will fly stably, whether it will land safely, or whether it might flutter.

Mr. Burns: Of course the ultimate test is when it is in service.

Mr. Mains: Yes. The reason for the sometimes apparently sharp definitions of requirements in testing are just like the case I mentioned a moment before where a manufacturer claimed that he was wronged by having his contract terminated because really what was wrong was that things were being overtested. In this particular case the shock machine was set up for a half sine pulse at perhaps 60 g and 10 miliseconds with a dead mass load; when the manufacturer's gear was put on the shock machine it necessarily had some elastic feedback into the input and so the pulse was no longer a nice clean sinusoid, it had some second harmonic on it and it read 70 g instead of 60 g so he claimed that it was an overtest. How do you write a specification that will educate the manufacturer to understand that he is out of order in making that kind of a claim when it is just done exactly as it was supposed to do?

Mr. Rich, (Consultant to ONR): Over the past twenty years the Navy has been running many full scale ship tests in which, instead of simulating the shock conditions on a machine, the ship itself has been subjected to what you could call simulated battle conditions. A couple of problems have come up, some of which have been alluded to here, but one of them I think is very important. We speak of overtesting but a large number of failures that we have had on these shipboard tests were things that had never been tested and probably were never analyzed. Dr. Mains mentioned the fact that frequently equipment is designed for function and then very little is done before it is put on shipboard. I have the feeling at times that something was done, the equipment was designed in somebody's laboratory for a function, it might have been electronic or electromechanical, and then in preparation for the shipboard environment it was painted grey and put on shipboard, and that is about the extent of the environmental preparation, so we have had a large number of failures of equipment that was never designed for this type of environment. The second type of failure that occurred has not been alluded to at all and I think it is important to bring it up. In many cases the equipment on board the ship was tested and it passed some sort of an acceptance test but there were failures: we found occasionally that the failure was in an offspring of the equipment that was originally tested. Some piece of equipment, perhaps switch gear, was tested and over a period of ten or fifteen years and all of the production was considered as shock proof: however over the years a number of changes were made, while they may have been small, their cumulative effect was to get an offspring that didn't look at all like its parent and it was still considered as being acceptable by the Navy. This is another area which became fairly prominent in these tests. Is anything being done about this at the present time?

Mr. Forkois: I really don't know if there is an answer to your question. The Navy has a qualified parts list and I know my own testing work has decreased in amount. I have a feeling perhaps many tests that are performed are not valid at other places. This is not meant to denigrate whatever they are doing, but it is a fact that sometimes we are coming in with tests which are way off base. You brought up some valid points and one was this idea of qualifying a part because it was tested ten years ago and then saying that's fine we don't have to test that we will save $500. And of course they put it into the assembly and it doesn't work and they have a failure. They can't have the $500 to test it but they will spend $500,000 trying to find out what is wrong with it. This is one of the things that just continues, at least in my experience, and it is just the tail chasing the dog continuously.

Mr. Volin: Those of you who were present at our opening session yesterday morning may have heard Mr. Short discuss the Rivet Gyro Program. In that discussion he mentioned some cases where testing and perhaps even analysis that should have been done was overlooked. As a result of these oversights they often had to go down to perhaps the piece part or the element level in order to find the culprit and find a fix. If testing and analysis are done carefully at the beginning, we can obtain reliable equipment and we don't have to go back and spend perhaps ten or more times than the cost of the original test or analysis trying to find and correct failures.

Mr. Hanks, (NASA Langley Research Ctr.): Just for the record I would like to point out what NASA in doing with regard to the Space Shuttle Program to cut the cost of testing, and in so doing we are leaning toward analysis. We are developing analysis and a 1/4 scale model of the Space Shuttle at the same time. We will use the analysis to guide the testing of the 1/4 scale model and then use the test results to improve the analysis, and finally to test components of the shuttle. We will also use analysis to put the test results of these components together to produce the whole vehicle, to eliminate testing on the entire vehicle.

Mr. Ibrahim (NASA Langley Research Ctr.): There are two dangerous problems facing our modern technology: the first is over analysis and the second is overtesting. I wonder about an analyst who runs a program that occupies the whole storage of a computer and takes twenty hours to solve a six or seven thousand degree equation together. He gets some results out of it and says that is my analysis. At the same time I wonder about the test engineer who uses a fifteen ton shaker, or perhaps twenty ton shaker, and who very soon will need an atomic bomb to shake some structures. I hope that the analysts and the test engineers will spend more time on simplifying their techniques and then the problem may be solved. Simple analysis and simple testing is the solution for the problem but people are now developing computers and equipment for vibration testing, and the techniques that they are using were developed by Kennely and Pancu in 1947. I don't say that one technique is bad or that another technique might be good but we have to improve existing techniques and develop new techniques for testing.

The analyst has to work to produce simpler testing techniques and this I guess will help to solve the problem.

Mr. O'Hearne: I might make a brief remark at this point about the test people who did not depend on their analysts by perhaps mentioning one concrete example that I can think of. There was an aircraft in which the flight test department got the notion that they were carried away with the weight reduction program. They took a little balance weight out of the rudder; and the result of that was an heroic pilot brought airplane back with two thirds of the tail missing. For some reason they had not consulted engineering.

PANELISTS CONCLUDING REMARKS

Mr. Irving: After listening to all of the discussion I have come to the conclusion that the test engineer is about the best friend and analyst has. I think that the time has come now where we have to go a little bit beyond just friendship and I say this because there are companies who have made a marriage between the test engineer and the analyst. We have analysis equipment driving shakers and we have shaker equipment doing analysis. I think that now it is time for us as individulas to get together and make this same marriage.

Mr. Schoonmaker: I certainly agree with those remarks and I would just like to say that this reiterates some of the considerations that I feel are should be taken into account. In the marriage of analysis and testing and they are equipment complexity, the operational criticality of the equipment under question, the severity of the environment, and last economic considerations. Certainly analysis may be adequate for some very non critical function and very low severity environment, but for very complex electro-mechanical gear I would feel that there is still a place for testing.

Mr. Forkois: I of course agree with two previous summaries. However, I did try to emphasize the fact that there are unknowns that we do not know about and even if we knew about them we don't know how to handle them, we don't know how to put them in our equations, and we may not even know how to put them into our tests. So I think I was trying to emphasize a little philosophical thought that maybe we should have a little more humility and indicate that when we don't know we just don't know.

Mr. Eshleman: Test or Analysis? I think today we have come to the conclusion that we need a little bit of both. I think that one of the problems is that when we work on cost problems that there has to be a trade off on who does what. I think the point that Mr. Ibrahim made a little earlier about a simpler test and analyses is good. And it would be worthwhile to spend more time looking for those things. It gives us a side effect to where we can actually have a better feel for the hardware that we are testing when we use simpler techniques.

Mr. Mains: I would like to urge each of you if you are in a managerial spot to do the most that you can to get your people to the point where they are not just test engineers, or just analysts, or just designers, but whatever their primary function is that they are able to carry over into the other parts of the operation so that they have a better understanding of what they are doing and why. You won't find many people who are all three but it would be nice if we were all somewhat of all three; and those of you who are not in managerial positions I would urge you to try and find ways to get up out of your tunnel and see how the rest of the operation goes and take part in it as much as you can so that you will understand the interface between your work and theirs as well as possible.

Mr. O'Hearne: Yes and we shouldn't overlook the need for the deep specialist in every organization. We need both breadth and depth. I would like to reiterate my opening remarks about the central position of the analyst. He is the person who uses the test laboratory and the computer laboratory and I think he has the central responsibility because he is the man responsible for the theory of what is being done with respect to the design. In addition to that I think that though we have bad analyses the solution to those circumstances is not to test in the place of analysis, or to have a test engineer control the test except for his instruments and his on-line analysis equipment. Another point that I made during the course of the afternoon is there is far more need for a critical review of what we do because we do many things badly, both test and analysis.

SEISMIC

SEISMIC SIMULATOR FOR SILO CONSTRAINED MISSILE GUIDANCE PLATFORM

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A test facility for subjecting a precision inertial guidance platform to simulated seismic disturbances is described. The facility controls, simultaneously, four independent motions applied to the inertial guidance platform. These are three rotational and one of three possible linear motion with accuracies of one arc second rotational and .001 inch linear. Details, unique features, and technical problems that were encountered are presented.

INTRODUCTION

A test facility for simulating silo constrained missile response to ground motion was designed and developed by the Environmental Test Laboratory at the Autonetics Division of Rockwell International. The ground motion that is simulated is a computer solved equation representing possible field service conditions. The nature of this motion, caused by random inputs (phase and amplitude) is complex sinusoidal and results in a different program or scenario for each set of constants and variables in the computer solution to the equation. The requirements for the range and accuracy of each discrete part of the facility were beyond the capabilities of existing equipment. This condition seemed to create an insurmountable task to assemble not yet developed parts into a functioning facility.

The test item was to be an inertial guidance platform with roughly a 22 inch diameter, spherically shaped case with equatorial mounting pads. The platform weighs approximately 90 pounds. A standard value of 5 lb-in-sec^2 for the moment of inertia was satisfactory for facility design purposes. The requirements were: rotational accuracy of 1 arc second, 80 dB dynamic range, linear accuracy of .001 inch, 70 dB dynamic range, DC to 5 Hz frequency response, simultaneous multi-axis motion, and a motion derived from a complex waveform equation. After screening existing equipment specifications and reviewing the requirements, it was apparent that a digital approach with precision equipment was necessary. A three degree of freedom rotational system would be necessary. However, a three degree of freedom linear system was determined to be beyond the scope of this program. A four degree of freedom test system (3 rotational, 1 linear) with flexibility of linear direction was conceived as being possible to develop in the time frame and cost allowed and to satisfy the objective of the test program.

The seismic simulator that was built can be viewed as a 40 inch high, 3 axes gimbaled assembly, itself subjected to linear motion by being attached to a precision, 38 inch by 52 inch, linear slip table. (See Figure 1) The slip table base is capable of being positioned to provide linear motion in the N-S or E-W or NW-SE direction. The rotatable base is attached to a free standing reinforced concrete seismic mass. The complete test facility consists of four major components plus a group of ancillary items which primarily involve detection and monitoring devices. These major components are 1) seismic reaction mass, 2) a three axis gimbal system for 3 degrees of rotation motion, and 4) an Integrated Control Console designed to provide for command and control and a monitor of all the various elements of the seismic simulator.

SEISMIC REACTION MASS

The reaction mass is essentially a solid concrete block 14 ft. x 14 ft. x 8 ft. deep, steel reinforced, of 2500 psi mix design. A 6 ft. 6 in. square by 3 ft. thick section of the top southeast corner of the block was omitted during the monolithic pour of the block so as to provide a recessed "mounting base" for the translational motion slip table. The "mounting base" is of unique design and

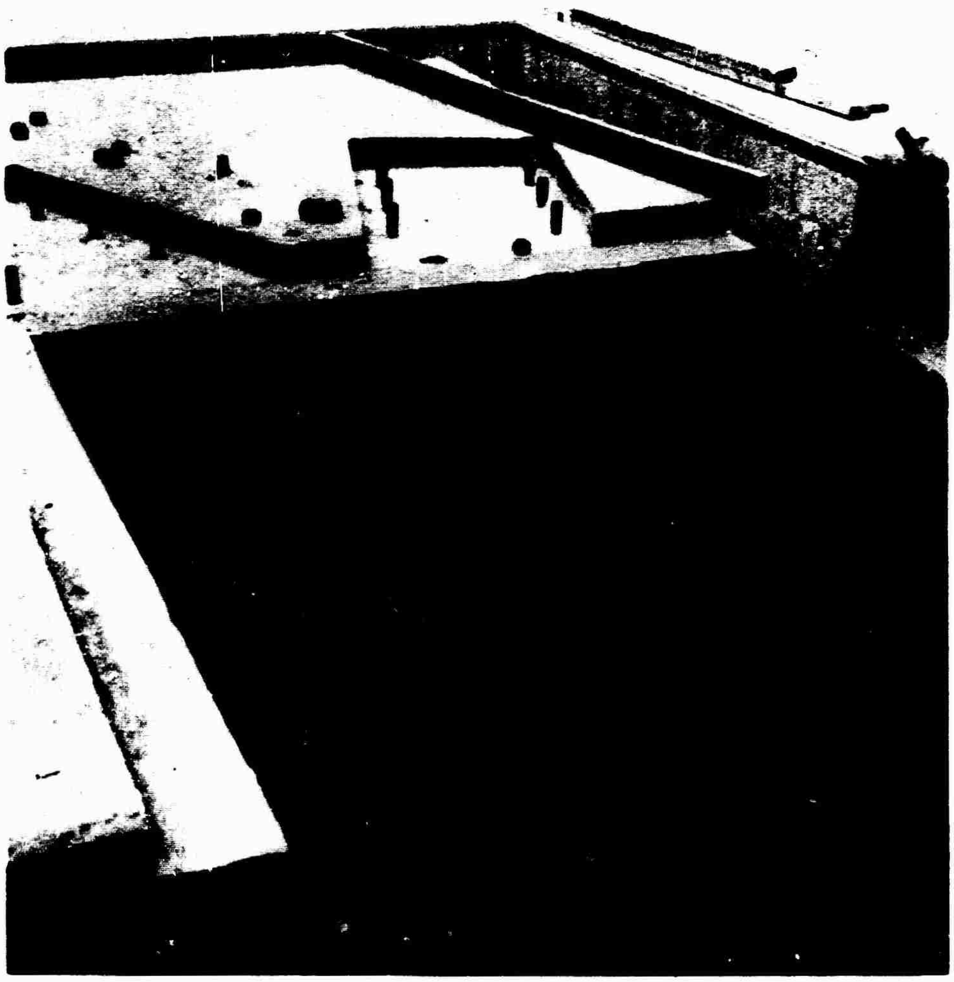

Fig. 1 - View of seismic simulator with test item mounted

required a high degree of precision not normally encountered in this type of construction. The base itself consists of two surface plate precision ground granite blocks, one on top of the other. The bottom granite block is anchored to the concrete mass by 16 one inch diameter steel bolts which extend entirely through to the bottom of the concrete mass. (See Figure 2) Design requirements dictated that positioning the bottom granite block level was critical since the reference level of the entire motion simulator was based upon it. To accomplish this, three hydraulic jacks were imbedded in the concrete mass beneath the granite block in a three-point suspension layout. A special casting resin, possessing a very low shrinkage coefficient was employed as a grout between the concrete mass and the granite block. Precise leveling of the granite block was accomplished during the set-up period of the casting resin (approximately 2 hours) by adjusting the hydraulic jacks. At the end of a 24-hour resin cure period, the second granite block was set on top of the bottom granite block. A swivel pin recessed in the geometrical center of the bottom granite block mates to a corresponding bearing in the center of top granite block, thus providing for rotation of the top granite block with respect to the bottom block. The precision ground mating surfaces of the two granite blocks incorporate a flotation air bearing to support the load of the motion simulation equipment mounted on top of the upper granite block; the purpose of this bearing is to accommodate rotation of the system for selecting the direction of the translational input. Both granite blocks were fabricated by Mojave

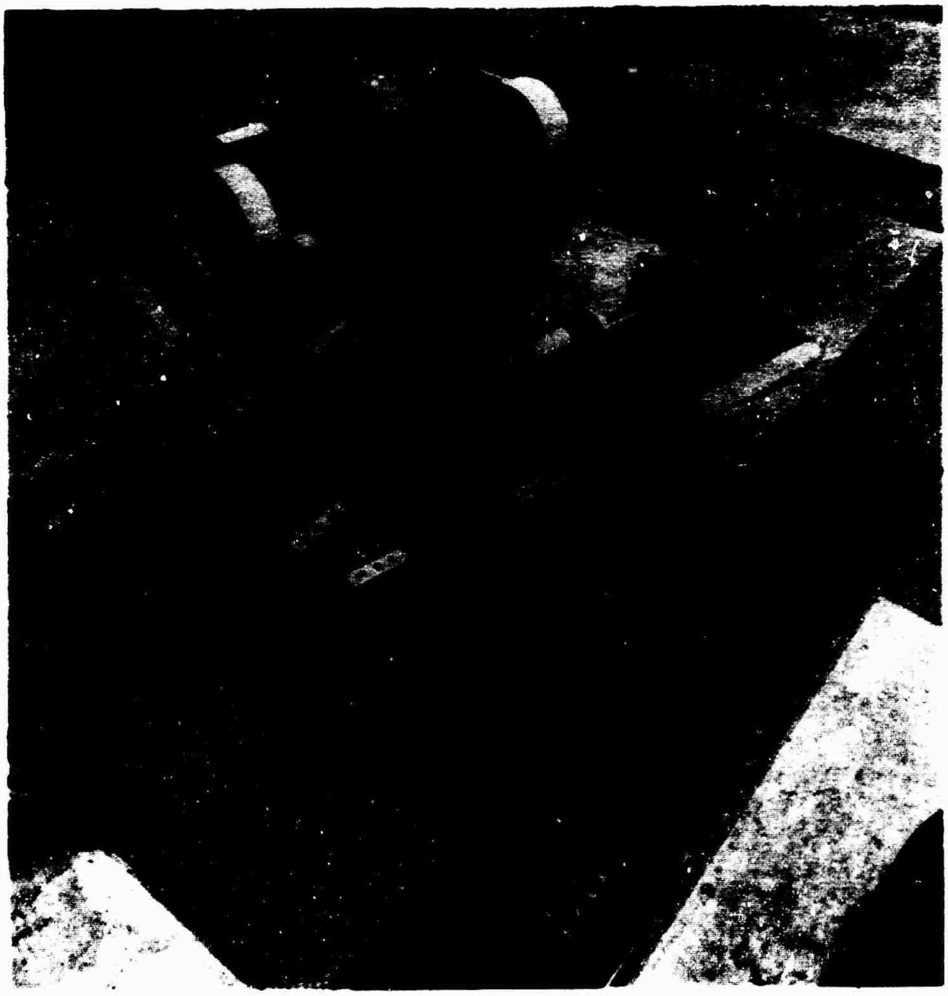

Fig. 2 - View of bottom granite base and positions for actuator base mounting

Granite. Physically, these blocks are 1) bottom: 66 inches x 66 inches x 16 inches thick, and 2) top: in the shape of a octagon, 60 inches side to side and 9.5 inches thick. The final error from level achieved as measured on the top granite block was 2 sec in the north-south axis and 20 sec in the east-west axis.

The upper granite block is the base for the hydrostatic slip table. By rotating the upper granite block on the air bearing, the slip table may be positioned to any one of three azimuth positions: i.e., north (0 degrees), northwest (315 degrees), or west (270 degrees). These positions are indexed very precisely by a tapered index pin so as to ensure alignment with the translational actuator stroke path. Kimbal Industries designed or specified much of the mass, granite base, and slip table interface elements.

Mounting pads are provided on the mass which attach the translational actuator to the concrete seismic mass. These are steel plates approximately 2 ft. x 3 ft. x 3 in. thick. These plates were also precision leveled and grouted into the prescribed fixed positions. Further, to provide maximum stability each pad was anchored to the concrete mass by six 1-inch diameter steel bolts which extend entirely through the eight foot thickness of the concrete mass itself in the same manner as the granite block. (See Fig. 2)

One of the features of this portion of the facility is that the seismic mass supporting the motion simulator itself is free-standing. There is no mechanical contact with the laboratory building other than the soil upon which it sits. There is a two foot space between the seismic mass and the retaining walls of the pit in which it is situated. This provides for isolation in that disturbances from outside sources affecting the simulator during extremely low level motion are reduced. This isolation also reduces the effect on the sensitive test site equipment during large seismic motions. Measurements during no programed input and at various frequencies and amplitudes in the range of the seismic programs, indicated at least 4:1 isolation between the seismic mass and the building floor. That is, the quiescent noise on the building floor was greater than 4 times the noise on the mass. During simulated program motions in the linear N-S and E-W directions, the mass moved at least 5 times the detected motion on the building floor. The motions measured covered 0.1 $\widehat{\text{Sec}}$ to 2.0 $\widehat{\text{Sec}}$. Cable trays were installed in this isolation space to provide a convenient and unobtrusive means of running utility lines to and from the motion simulator. The seismic mass, including the granite blocks, weighs approximately 108 tons.

TRANSLATIONAL MOTION SIMULATOR

This system is comprised of two main components: 1) the slip table, and 2) the actuator. Item 1) is a set of four special Team Corporation long-stroke hydrostatic bearings upon which is assembled a Kimbal Industries mounting fixture that also serves as a structure uniting the four bearings into one integrated moving member. Item 2) is an MTS Systems Corporation electro-hydraulic actuator which provides the driving force to the fixture bearings. The system incorporates three unique adaptations developed to help achieve the desired high degree of accuracy from very small displacements to the very large.

First, each bearing is mounted on a unique Team Corp. adjustable block. The adjustment range is in micro inches. These blocks were developed in order to make the table linear stroke nonrotational for the full 15 inch programed stroke. Each end of each adjustable block contains a chamber with a grease fitting access. A common auto grease gun is used to pressurize the chamber. Each chamber requires a different pressure up to approximately 3000 psi to level the block, thus obtaining the desired nonrotational motion. (See Figure 3).

Second, a special linear transducer of the Linear Variable Displacement Transformer (LVDT) type is used as the position feedback source for closed loop control of the actuator. This transducer is in reality two LVDT's mounted on the same shaft. One has a full range of 1 inch for small displacements while the other has a range of 20 inches. Normally, the closed loop control transducer is a part of the actuator itself, however, for this application the LVDT is mounted on the slip table as a means of improving accuracy.

Third, the coupling between the slip table and the actuator consists of a high frequency isolating elastomer. The purpose of this elastomer is to reduce or eliminate pressure pulsations, which are generated by the actuator hydraulic pump, from reaching the test item or being observed as acceleration levels in any part of the moving fixture. The actuator hydraulic pump configuration and motor speed were selected to have pump pulsations at 140 Hz, outside of any calculated gimbal resonance. During the initial design phase of the program, it was learned that Dr. D. M. Onysko of the Forrest Product Lab in Ottowa, Canada, had achieved some success with elastomers for the same purpose on a human factors motion simulator. The supplier of the Canadian actuator and our actuator, MTS Systems Corporation, provided four sets of elastomers to our facility. It was necessary to conduct experiments under seismic-simulator loading conditions to determine which elastomer under given conditions would provide the best isolation. The only problem encountered was insufficient precompression. During maximum acceleration, decoupling occurred leading to oscillation. These experiments were accomplished during the initial checkout phase of the facility. Since these isolation pads are inside the actuator servo loop, it became necessary to find the best compromise between the true transmission of applied motion and optimum of damping for higher frequencies. Also, a larger than normal "line tamer" accumulator was designed into the hydraulic system to initially reduce the hydraulic pulsations at the actuator input.

Two hydraulic power supplies are used for the translational motion. One is for the linear slip bearings and the other is the power source for the linear actuator that produces the linear motion. Both the power supplies are 2500 psi capability and are located outside of the test site room to minimize noise.

ROTATIONAL MOTION SIMULATOR

This equipment is a 3-axis rotational motion simulator that utilizes the translational motion fixture as its base. The 3-axis gimbaled system is a modified Carco Electronics Model S-460 structure. This system is a DC torquer drive motor configuration with tachometer servo stabilization. The gimbals are, for the most part, solid cast magnesium with two end supports on the two inner gimbals and a single end support on the outer gimbal. The gimbaled structure

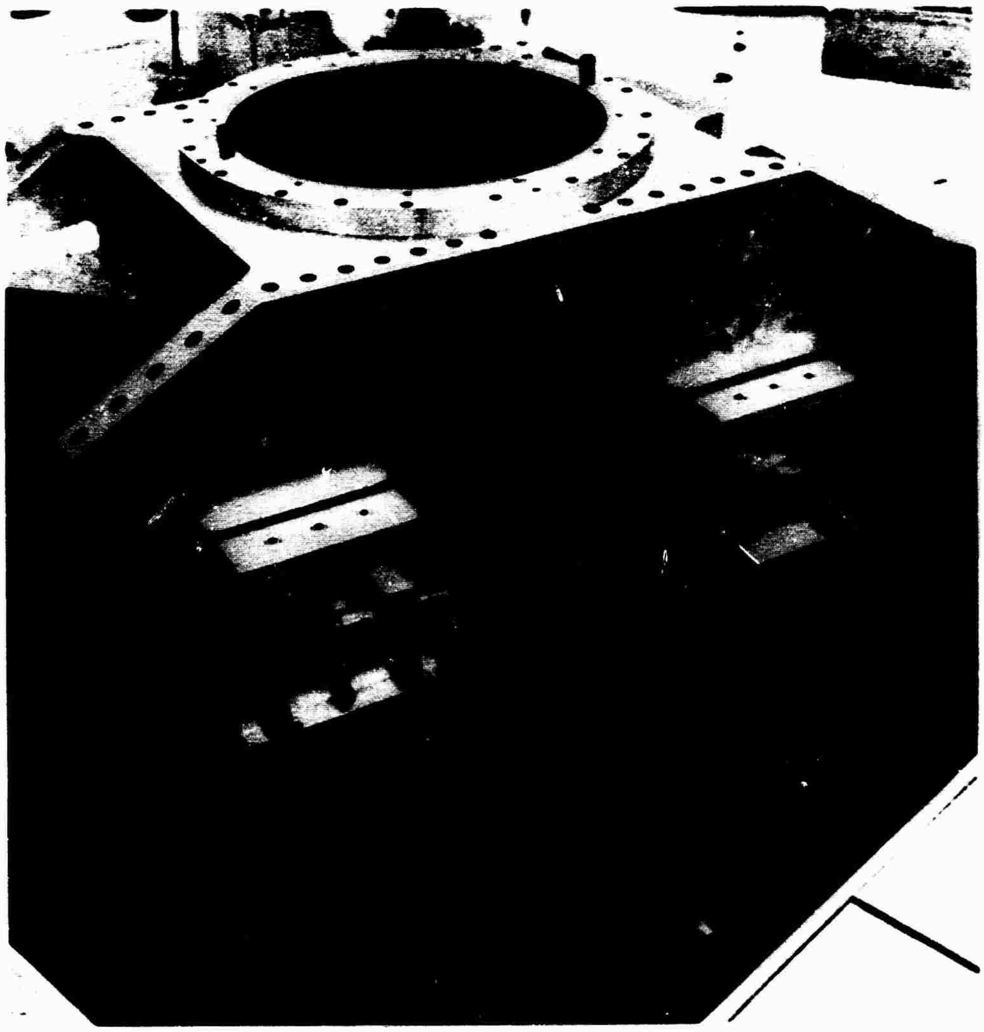

Fig. 3 - View of top granite octagon mounting base and slip table

would essentially fit in a 4' cube. The inner gimbal will accept a 24" diamter spherically shaped test load. (See Fig. 1)

Because of the unusual requirement for such a large, motion producing piece of equipment to sustain side forces, (itself being subjected to motion), the single end support of the outer gimbal was of special design. Two bearings, stiffened plates, and precision dimensions for base interface were required.

Because the test item was to be monitored extensively, special gimbal frames were built with 3 optical access holes. These access hols were in line with and 5" above the gimbal coordinates. The drive motor and inductosyn stators located in the housings were machined to allow maximum optical access without degrading electrical or mechanical performance.

Each axis has a 720 pole inductosyn position transducer which is connected with a digital system for .0001° resolution. There are removable stops at ±5° in all axes and reduced-power switches at ±3 1/2° on all axes. Programable motion must be within these limits unless the limit switches and stops are removed. (Then motion up to approximately ±60° can be obtained on two axes and ±100° on the other axis, limited by cable routing.)

Removable locking pins on each axis maintain near zero position for power-off conditions, repeatable to within approximately ± .0030° which far exceeds design requirements, but after extensive use, a desire exists for a less deviate pinning position.

INTEGRATED CONTROL CONSOLE

The control console consists of a 3 bay Autonetics console attached to a 2 bay Carco Electronics console. The Carco console contains the electronics exclusively for the 3 gimbals, such as motor drive power amplifiers, resolver and inductosyn circuits, digital read-in and read-out chassis, power supplies, etc. The Autonetics portion of the 5 bay console contains all data processing circuits. The digital signal processing circuits contain de-multiplexing, input arithmetic, output arithmetic and multiplexing functions. The arithmetic circuits convert ± binary coded decimal signals to circular coordinate values of binary coded decimal signals for the Carco input information and reverses the process for the output information. This console also contains such devices as the MTS hydraulic linear control electronics, 8 channel strip recorder and seismic tiltmeter electronics.

The number of possible displacements for motion is 5 axes (3 rotational and 2 linear) multiplexed in a time sequence as follows: Roll, Yaw, Pitch, Linear North-South and Linear East-West. Linear NW-SE motion must be selected from and programmed into one of the 2 available multiplexed linear time slots.

NOTE: Only one linear motion at any time is used.

The dynamic range of position information required for the test was 80 dB. Due to the typical analog recorder range of up to 40 dB, analog recorders using a conventional method were not considered. However, analog recorders were available for this program, and their limitations and use with digital information were evaluated and found to be suitable. A digital signal of up to 20 binary coded decimal bits was required to define any position and was the method selected. The 20 bits were split into 2 groups of 10 bits and labeled coarse and fine information and required 10 tape recorder channels for the 10 bits. Thus a total of 10 segments at 0.5 milliseconds between them resulted in each motion of the seismic simulator being updated every 5 milliseconds or 200 times per second. Three more channels were required, one for clock, reset, and sign (polarity from 0). Thus 13 channels of the 14 channel wideband recorder were dedicated to supplying position information. The 14th channel had special pulses and IRIG format time code "B" multiplexed together. The 14th channel information signals were kept separate and distributed separately from the 13 channels of position information.

The Integrated Control Console input circuits for each recorder channel consist of one stage of amplification and filtering, adjustable one shot delay, and data latches and clock synchronization. After the 5 axes of motion information is demultiplexed at the latches, plus and minus BCD information is available. The linear actuator electronics accepts this format of information and North-South or East-West is thus selected. However, the Carco electronics are wired for rotational values from 0° to 359.9999°. It is necessary to convert ±BCD values to circular values. The arithmetic circuit accomplishes this function. When the linear input is required to be E-W, the roll angle is set to 90°. (Also a 45° input can be selected for use of N-S or E-W information or other special programming.) The roll position information is converted to 90° ± values by selecting the desired angle with front panel control of the arithmetic circuits.

For the recording of digital information (of the programed motion obtained), the above process of arithmetic functions and the multiplexing of the information back into the original format is repeated in a reverse sequence.

The Integrated Control Console accepts digital signals from either of two sources during actual operation. The main signal source is pre-recorded BCD formated data, processed by magnetic tape. An alternate input is from the Xerox 9300 computer in real time control. (This computer is also the source for the magnetic tape program.) The magnetic tape is normally processed for input to the control console at the data control center within the Test Building, about 300 feet from the test location.

Local control can be by digital or analog or manual or any combination of digital or analog signals.

PERIPHERAL EQUIPMENT

Other equipment used at the test site consists of a linear and angular accelerometers and their associated electronics, a linear displacement monitor and remote readout, an electro-optical tracker (Physitech), biaxial tiltmeter, and communication equipment.

The Endevco Corp. linear accelerometer monitors motion from ±1g to approximately ±50 micro g's limited by physical and electrical noise. Two Systron Donner angular accelerometers monitor motion from ±1 radian/sec^2 to approximately ±1.0 milliradian/sec^2, again limited by physical and electrical noise. The roll axis accelerometer is ±0.5 radian/sec^2 maximum. The linear displacement monitor has .0005" resolution and .001" accuracy. The optical tracker is used for monitoring motion, with limits and accuracy varied depending on the lens used. It is also used for closing a servo loop on the roll gimbal during a change in position of the linear drive assembly. The gimbals (hence test item) remain at the same compass heading while the linear drive axis is oriented to a new compass heading. The biaxial tiltmeter is a Rockwell International commercial version of the Minuteman tiltmeter. It is used to monitor the seismic reaction mass for seismic disturbances during a test and detects motion to one percent of an arc second (1/3 arc second is the typical observed value due to motion programing during a test).

TECHNICAL PROBLEMS

Technical problems, encountered during the build-up phase of operations, were serious due to the extreme time schedule required and consisted of the following major areas of endeavor: (a) seismic mass, (b) biaxial tiltmeter, (c) tape recorder alignment, (d) electrical noise, (e) linearity of Carco servo loop, and (f) Carco/A.A. Gage fine-coarse switch circuits. A brief explanation of these problems is listed below:

(a) The seismic mass preparation problems started with the initial hole-in-the-ground. Between awaiting decisions on new federal regulations on safety and the completing of the walls of the hole in the ground, the earth dried out and some of the dirt sides of the pit collapsed. The contract had to be renegotiated, and the new contract called for enlarging the evacuation area nearly 4 times the original floor area to prevent cave-ins. This increased costs and, more importantly, delayed the scheduled completion time.

(b) After the initial installation of the biaxial tiltmeter in the middle of the seismic mass, a slow change in the tilt of the mass was indicated. A factory check of the tiltmeter electronics revealed satisfactory operation. The question of seismic mass stability versus tiltmeter stability was answered by extensive checking which showed an anomaly in the tiltmeter sensor which was repaired.

(c) The tape recorder alignment problems became evident, only after trial runs were performed. Deviations in head alignment are allowed by IRIG standards as ±.001". Extreme tolerances were discovered between the recording recorder and the playback recorder. Variable tape friction, skew, speed deviation, and tape temperature were problems to be reckoned with. Also, the odd channel-even channel dimension tolerances were beyond tolerance of the ICC circuitry. Individual channel delay circuits (one shot monostable multivibrators) were installed and the attendant rewiring of the computer-wired chassis was necessary.

(d) Electrical Noise - In the console, excessive susceptibility to electronic noise necessitated redesign of input circuits and digital clocking circuits. These problems became evident after test runs were initiated. After redesign and due to remaining infrequent noise "spikes" fed into the drive circuits, the source of the spikes was difficult to isolate. While investigating recorded tapes, the Xerox computer was discovered to be a contributor to some of the spikes. The problem was solved by Xerox. Environmental chambers outside the test site room on the same power lines were the main remaining contributor to the occasional "spike" felt by the system.

Electrical noise from laboratory equipment occasionally caused false digital bits and occasionally caused impulses in the analog signals. Considerable time was spent in diagnosing the source of and the input path of noise. Power line filters were installed. In addition, due to observing the noise and its results, an analog filter and a safety slow-shutdown circuit was designed and installed before the linear circuitry for the possible occasion of a tape recorder malfunction or other catastrophic failure. Further work of isolation and noise suppression was centered in the A. A. Gage digital circuits of the Carco 3 gimbal system.

(e) During the initial calibration and acceptance tests on the Carco unit, it was discerned that the linearity-frequency response of the system was not acceptable. The system response was acceptable in the area of 0.1 to 1 Hz, but a 5 Hz the gain was not within specification. Carco, after extensive effort, improved the servo loop response and the system was again recalibrated. The response of the system then met the specification criteria.

(f) The Carco Electronics digital circuitry procured by Carco Electronics from A.A. Gage exhibited a subtle problem during the checkout phase of operations. With a digital input signal approximately 1° in amplitude or greater and about 1 Hz, a switchover from fine to coarse error signal in the A. A. Gage sub-chassis can occur due to phase lag of the servo loop. The switchover is normal, but a transitory change in acceleration was occurring. This was due to a drift in the coarse error voltage and not matching the fine error voltage in a random manner.

Extensive investigation with experimental circuit changes were evaluated during the checkout phase. A series of discussions with Carco followed which resulted in a completely redesigned circuit, recommended by Autonetics. This circuit was installed, evaluated, and determined to be jerk-free and stable.

The results of all measurements indicated a completely acceptable performance of the entire seismic test facility. Rotational values from ± 1/3 arc second to almost ±3° and frequencies to approximately 5 Hz were programed in the various scenarios. Linear values from ±.001 to almost ±7" and to approximately 5 Hz were included in the programs.

TEST PROGRAM

The test program consisted of 17 scenarios or seismic simulation test profiles with different parameters, labeled A-1 through A-12 and B-1 through B-5. Each scenario was determined to be a distinct set of parameters by Ballistics Engineering.

Prior to the application of each test scenario to the test item, a complete recorder test run was performed (without drive power applied to supply motion). The primary purpose was to adjust the digital timing circuits for optimum time delays and compensate for variations in each tape scenario from a previous scenario and to verify the data on the tape. Due to the long time period required to obtain the complete set of 17 scenarios, different recorders, different computer to analog wiring dressing, and different equipment were utilized during the fabrication period. These variations influenced the recorded digital data bits timing in relation to the clock pulses and to each other, from tape to tape.

The scenario tape was played on a Sangamo 4700 recorder in the Data Center. The output was patch-cabled to the test area signal junction boxes' master patch at the Data Center master patch panel. The signals were received at the test site signal junction boxes and processed through the buffer amplifiers to establish ground isolation between the test site and the recorder location.

TEST MONITORING

The seismic motion was monitored for displacement and acceleration. The 3 axis Carco Electronics system utilizes digital position information for servo control. This digital information was available for monitoring and was used for all tests. Carco also has analog position information available, which was also used for all tests. The MTS linear system has an analog to digital and a digital to analog converter incorporated in the signal output and input lines. The digital position information from all four motions was multiplexed in the Control Console and the resultant output signals connected to the test site signal connection box. These signals were then connected via the master patch panel in the Data Center to a recorder for permanent record of the test. In addition, analog signals were monitored via the strip chart recorder. Position command and test fixture acceleration signals were the main signals monitored during a test. The seismic tiltmeter and position output signals were occasionally monitored. (See Fig. 4)

Also, an analog magnetic tape recorder located at the test site was connected to the 4 axis analog position information and three Davidson optical auto-collimator output signals. Time code formats IRIG "B" and "C" were used to correlate all recorders.

CONCLUSION

This unique equipment has been in service for over one year and has been a successful adjunct to inertial platform improvements. Because of the built-in flexibility, numerous special tests beyond the original concept have also been performed. Seismic environments of 1/40th scale were performed. This 1/40th scale was performed in digital mode for precise reference and was programed analog by a simple signal attenuation network attached to a D/A converter. Various combinations of analog, digital, linear, and rotational deviations from normal testing have ensued. Other special tests such as post launch and large assembly simulation have been performed. In addition, other engineering groups are preparing their test programs to include a proposal to utilize this facility.

SAMPLE OF MOTION

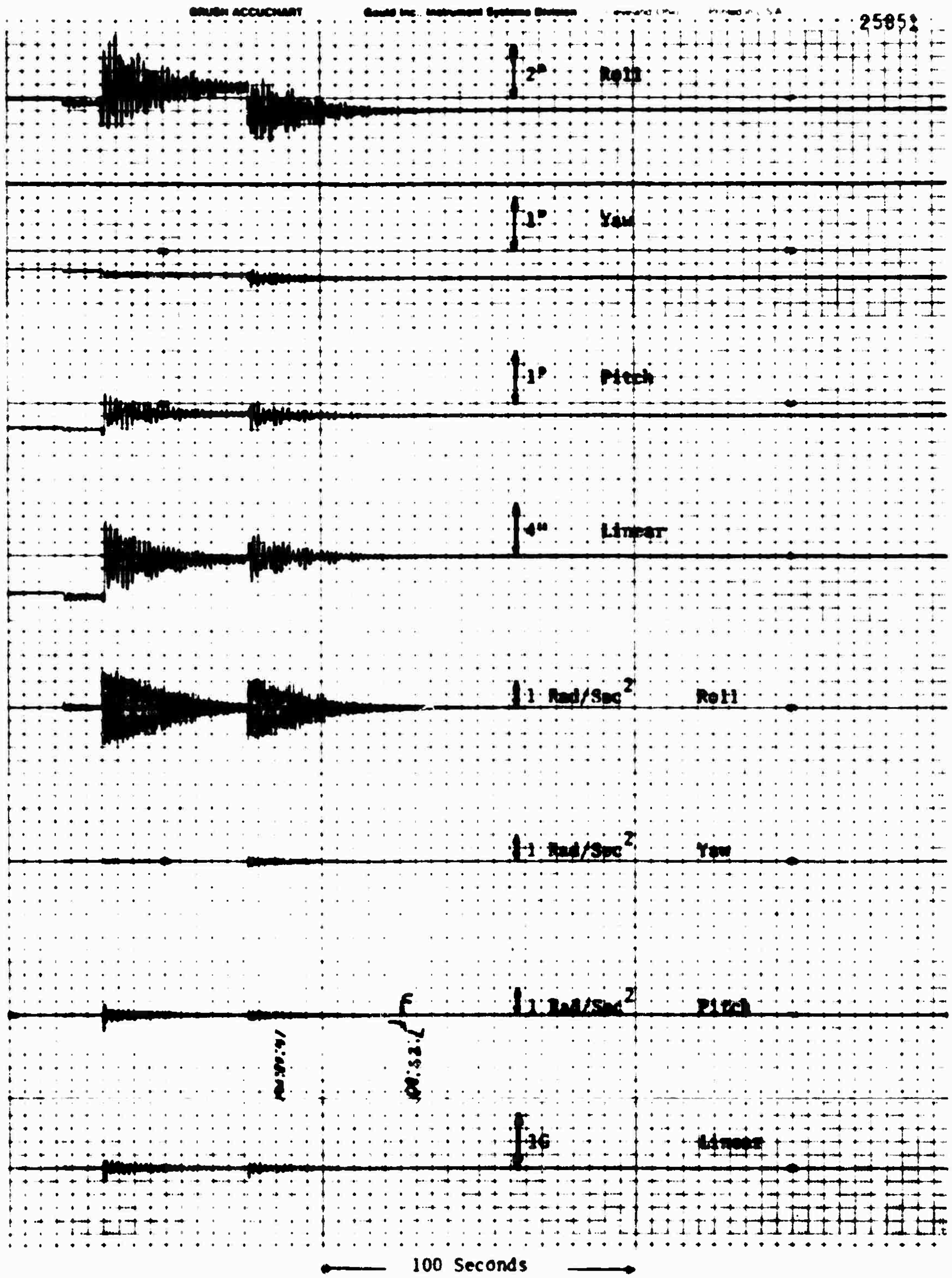

Fig. 4 - Portion of displacement and acceleration chart from sample test

EARTHQUAKE RESPONSE OF COMMUNICATIONS EQUIPMENT IN SMALL BUILDINGS

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Whippany, New Jersey

The earthquake response of multiple rows of equipment frameworks braced to the walls of small single story buildings is examined. A general procedure, based on finite element analyses of actual equipment and building configurations is developed for determining earthquake bracing loads. Dynamic equipment-building interaction is included and is shown to have a significant effect on the support system loadings. The results of the analysis are used to determine bracing loads for a wide range of office configurations.

INTRODUCTION

Earthquake protection of telephone buildings and equipment has always been a design consideration in areas of high seismic activity. In the past, design practices were provided by considering existing earthquake historical data to assess seismic risk, and employing available analytical tools to design buildings and equipment. However, these techniques were inadequate in many cases for analyzing complex structures. Great advances have been made during the last decade in seismic risk analysis and structural dynamics. Computer codes have been developed for analyzing the dynamic response of complex building and equipment configurations to earthquake excitations.

The earthquake response of multiple rows of tall slender equipment frameworks in small single-story Community Dial Offices (CDO) is studied in this paper. The buildings housing this equipment are typically constructed of either concrete block or wood framing. They are usually located in suburban areas where large and fast population growth has necessitated additional telephone facilities.

Typical Equipment-Bracing Configuration

Figure 1 is a schematic representation of a typical equipment-support structure. Equipment frame lineups are tied together at the 9-foot level by cross-aisle cable racks and steel angle braces (indicated as lineup braces). On the Common Distribution Frame (CDF) side, the frames are attached to the building wall by horizontally oriented steel angle braces. Equipment frames opposite the CDF are in many cases large distances from the outside wall and, therefore, not attached directly to it.

Failure Modes

The most probable earthquake-induced faillure modes of the CDO bracing system shown in Figure 1 are due to: (1) buckling of the wall braces under excessive load, or (2) tension failure of the connection between the wall brace and the building wall.

A series of tests were conducted to determine the loads required to cause the failures indicated previously. The wall brace connection to the building proved to be the weakest link in the support structure. As a result of this the primary objective of this study is to determine the forces in the wall braces.

This discussion pertains to the weak-stiffness direction as depicted in Figure 1. The configuration in the long direction is such that the stiffness of the equipment frames bolted together and to the floor is more than adequate for earthquake loadings.

ANALYTICAL PROCEDURE

The determination of loadings in the equipment bracing system and the resulting possibility of failure of this configuration are determined by the following general procedures:

1. The actual equipment-bracing-building structure is a continuous three-dimensional system, which can be modeled with beams and trusses into an equivalent two-dimensional mathematical representation, so that available dynamic computer programs can be employed.
2. The frequencies and mode shapes of the structure's equivalent mathematical

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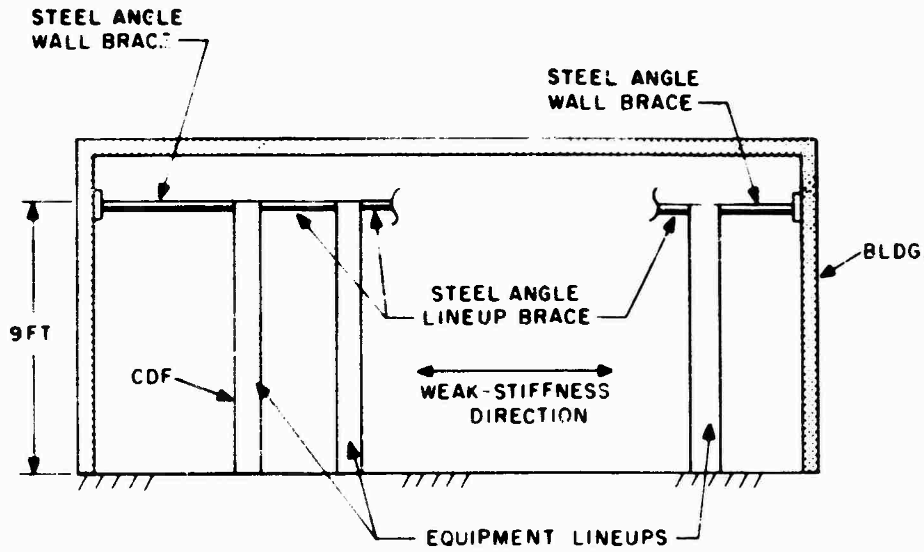


Fig. 1 - Typical CDO Support System

model are determined by employing a finite element program called SAP [1,2].*

3. The dynamic response of each node point in the structure is determined for the appropriate earthquake forcing function.

4. The peak loadings in the bracing system are determined.

These procedures are performed for a number of different office configurations.

Assumptions and Features Employed in Analysis

The entire analysis is linear from the frequency and mode shape determination to the calculation of peak loadings in the bracing system. This assumption is justified because of the small displacements, and hence elastic responses, that result from the dynamic loadings.

Coupled equipment and building responses are included. In most other seismic studies equipment and building responses are assumed to be uncoupled because the building mass is so much larger than the equipment mass. However, as shown later in this study the equipment mass is comparable to the building mass, and the coupling effect is significant.

To reduce computer time usage, only the weak direction horizontal motions are included. This is justified by the physical configuration of the system and some initial calculations, which indicate a negligible vertical response contribution to the loadings in the bracing system.

All connections to the ground are either pinned or clamped. The implication of this assumption is discussed in a later section. In addition, all members are considered as either beams or trusses.

Analysis of a Typical Office

To illustrate the analysis methods mentioned above, consider the floor and cabling plans of a model office, as shown in Figure 2. The main aisle running from left to right in the floor plan is used as a natural break in the physical layout. The analysis considers only the equipment shown below this aisle.

The "weak direction" of motion is from right to left in Figure 2. It is assumed that the lineups move in this direction uniformly, i.e., there is no twisting in any of the lineups. This should be a conservative assumption since it makes the entire mass of each lineup move in the same direction at the same time, thus increasing the maximum forces in the bracing system. This assumption makes it possible to model the entire three-dimensional physical system as a two-dimensional structural model, as shown in Figure 3.

Each equipment lineup is represented as a a vertical beam with geometric and physical properties determined by considering the whole

*Numbers in brackets designate references.

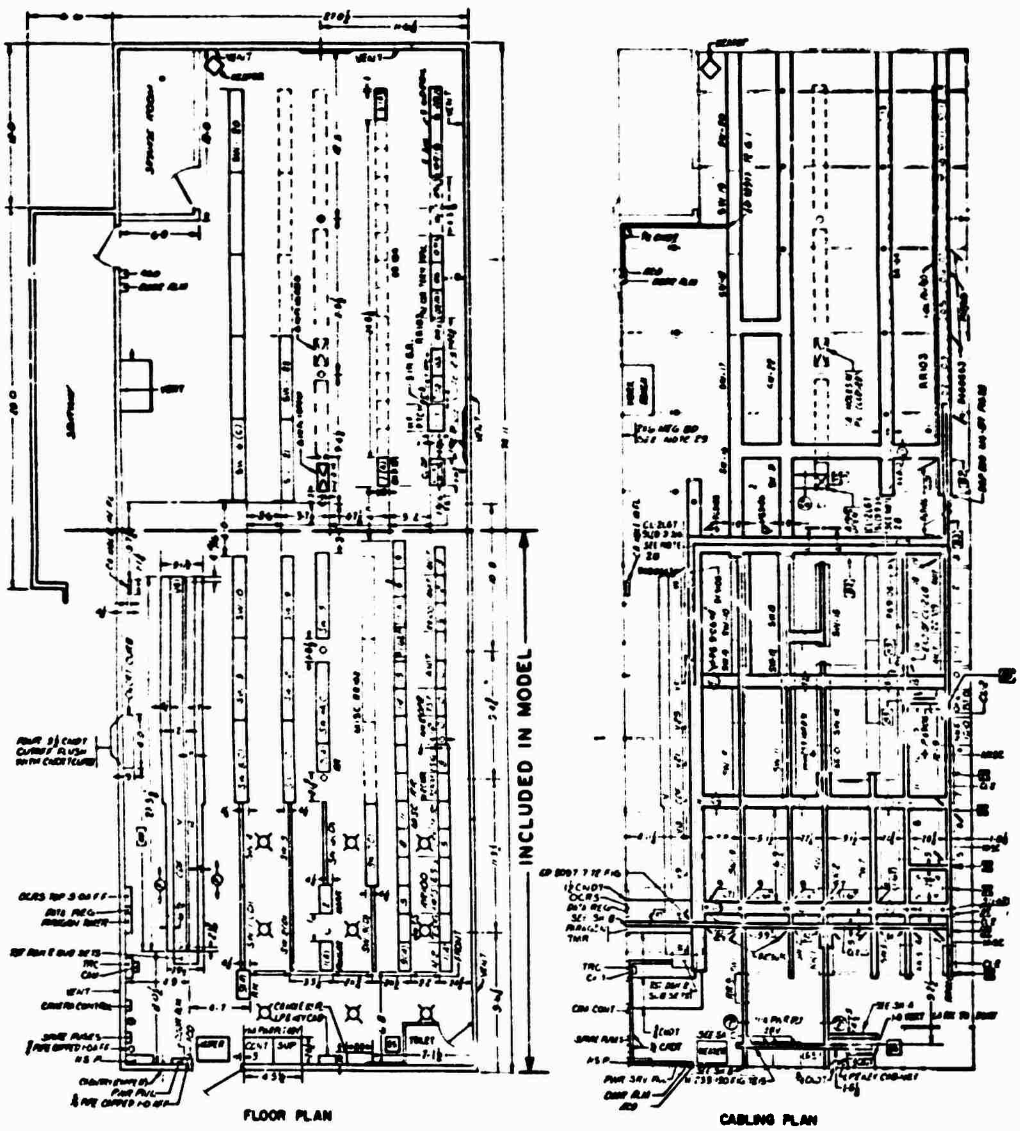


Fig. 2 - Floor and Cabling Plan of Typical Community Dial Office

lineup. Wall and lineup braces are modeled as trusses. Cables running in the direction of the lineups are assumed to be concentrated loads located at the top of their respective lineup.

The walls and roof of the building are modeled as beams, one vertical beam for each wall and a horizontal beam for the roof, using properties from the actual CDO building. The effect of the end wall mass is taken into account by doubling the weight of the roof section associated with the lineup model. The effect of the end wall stiffness is considered by adding two "phantom" trusses. These trusses are used to "tune" the lowest mode of the building to a specific natural frequency. This was achieved by uncoupling the equipment from the building and adjusting the area of the "phantom" trusses until the desired building frequency was obtained. The resulting building model was then attached to the equipment model, as shown in Figure 3.

In this particular example the lineups are represented as beams with "built-in" connections to the ground. In addition the building is

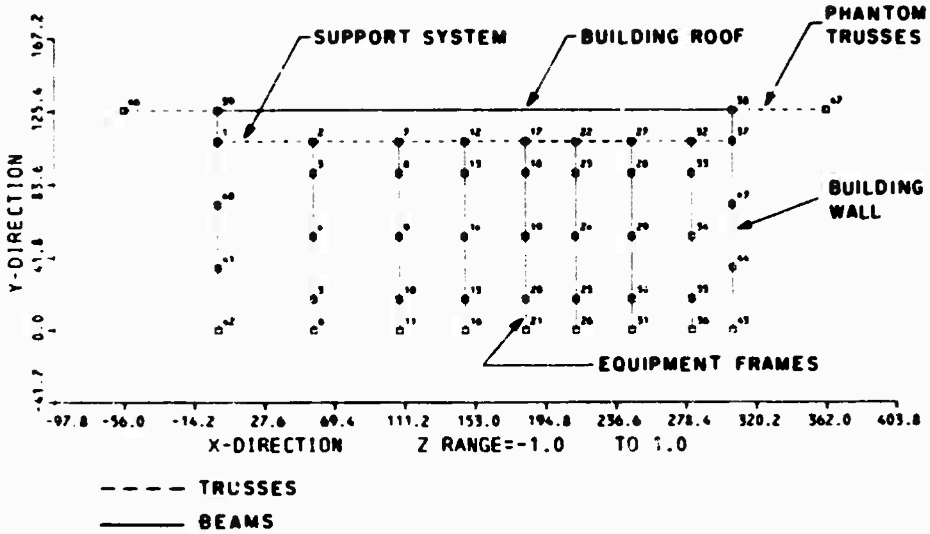


Fig. 3 - Structural Model of Typical Office

assumed to be flexible with a natural frequency of 10 Hz. This latter assumption is based upon the construction of typical one-story Bell System CDOs and documented [3] information pertaining to building frequencies. Other variations were made during the course of the analysis and are discussed in Results of Computer Analysis.

The computer program SAP was used to determine the natural frequencies and mode shapes of the model. Plots of the first four modes are shown in Figures 4a through 4d. Mode shape one, Figure 4a at 2.61 Hz, corresponds primarily to the movement of the CDF. This is expected because the weight of this lineup is much larger than any other lineup. The second mode shape, Figure 4b at 8.64 Hz, is a complex mode that includes coupled building and equipment motion. Thus, the 10-Hz building uncoupled frequency is actually reduced to 8.54 Hz when the effect of the equipment is included. The third and fourth modes, Figures 4c and 4d at 10.60 Hz and 10.90 Hz, represent equipment modes. Mode shapes similar to these occurred in all other CDO models, which included a CDF and a 10-Hz building.

For the dynamic analysis a modal damping value of 2 percent of critical was used for all frequencies. This assumption is reasonable for the equipment modes; however, building damping should be higher (5 to 10 percent of critical). Since the second mode is a complex building and equipment mode, using 2 percent damping results in an added degree of conservatism. The forcing function used is the acceleration-time history of the El Centro North-South 1940 earthquake, which has a peak acceleration of .315 g's.

Absolute accelerations and relative displacements of each mode in the model are determined. In addition, the maximum relative displacement of each node, the maximum relative displacement between any two nodes, and forces in the bracing system are computed.

Table 1 shows the forces in the lineup and wall bracing for this particular example. Notice that the wall brace loads are much higher than the lineup brace loadings. This is an expected result since there are inherently more lineup braces, and also, the wall braces support more equipment weight than the lineup braces.

TABLE 1

Wall Brace and Lineup Brace Loads for Typical Office Due to El Centro Earthquake

Nodes	Force Per Brace (lb)	
1-2	670	Wall Brace
2-7	227	Lineup Braces
7-12	142	
12-17	119	
17-22	132	
17-27	201	
22-27	289	
27-32	318	
32-37	857	Wall Brace

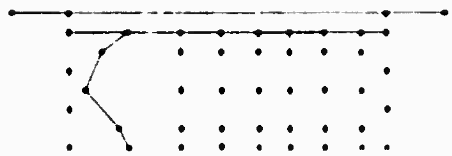


FIGURE 4a
MODE SHAPE NO. 1 FREQUENCY 3.610 Hz

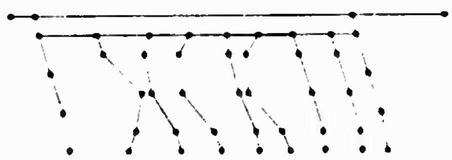


FIGURE 4b
MODE SHAPE NO. 2 FREQUENCY 8.640 Hz

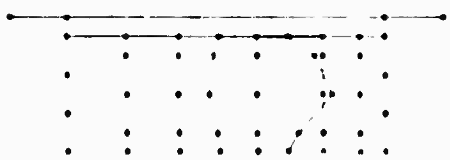


FIGURE 4c
MODE SHAPE NO. 3 FREQUENCY 10.600 Hz

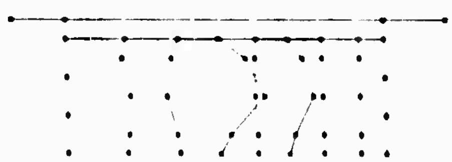


FIGURE 4d
MODE SHAPE NO. 4 FREQUENCY 10.900 Hz

Fig. 4 - First Four Mode Shapes of Typical Office

RESULTS OF COMPUTER ANALYSIS

In this section the analytical procedures are applied to a series of CDO models. The results of these computations are presented and synthesized into general results applicable to any CDO.

Model Configuration

Five CDO models, of various sizes, were analyzed by the methods previously outlined. The largest office was separated into three sections determined by the natural breaks in the equipment configuration. Each of these equipment sections was analyzed separately, making a total of seven different office configurations that were investigated. These seven offices were analyzed with various combinations of equipment connection to the ground, and building flexibility, resulting in a total of 14 cases.

Forcing Function

The North-South component of the May 18, 1940, El Centro earthquake is used as the basic design environment in this study. This is generally accepted as a "severe" earthquake environment. Figure 5 shows the time history and acceleration response spectra of El Centro.

Computer Results

The results for all of the computer runs (due to El Centro) are tabulated in Table 2.

TABLE 2

Tabulated Results of Computer Analysis Due to El Centro

Model*	Equivalent Length (ft)	Number of Braces		Total Wall Load (lb)		Force Per Brace (lb)	
		CDF Side	OPP Side	CDF Side	OPP Side	CDF Side	OPP Side
1-P-R	44	1	3	430	1,790	430	600
2-P-R	79	2	3	2,160	3,890	1,080	1,297
3-P-R	141	2	5	2,360	5,970	1,180	1,194
4-P-R	268	10	7	7,802	5,101	780	730
4-P-D	268	10	7	13,000	11,100	1,300	1,587
4-F-D	268	10	7	6,700	6,000	670	857
5-P-R	299	14	7	11,670	5,568	834	795
5-P-D	299	14	7	15,560	10,378	1,111	1,482
5-F-D	299	14	7	10,386	6,825	741	975
6-P-R	170	10	0	10,508	0	1,051	0
6-P-D	170	10	0	15,250	0	1,525	0
6-F-D	170	10	0	9,650	0	965	0
7-P-D	72	3	0	6,800	0	2,267	0
7-F-D	72	3	0	5,040	0	1,684	0

*Model Notation

- Office Number

  Seven offices notated 1 through 7

- Type of Beam Connection

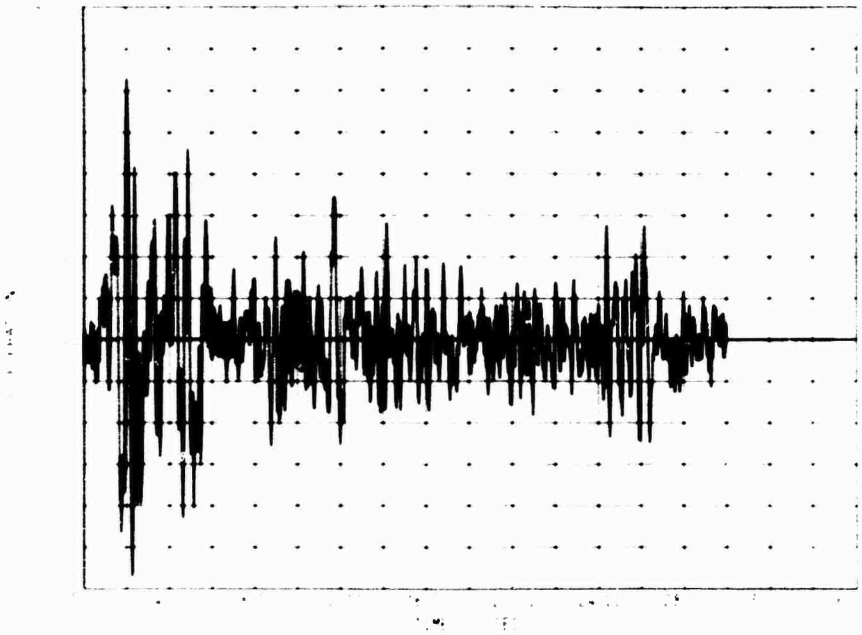
  Pinned (Simply-Supported) P
  Fixed (Built-in-End) F

- Building Flexibility

  Rigid R
  10-Hz Natural Frequency D

- Typical Usage

  Office 2 with built-in beams and 10-Hz building 2-F-D



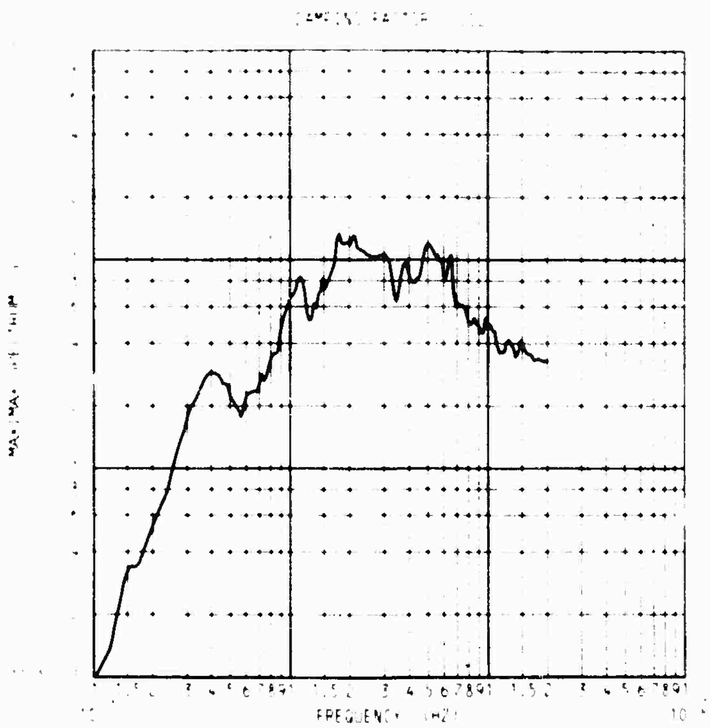


Fig. 5 - El Centro Earthquake and Acceleration Spectrum

Equivalent length is defined as the length of the CDF times four, plus the length of the remaining lineups in the office. It provides a number approximately proportional to the total weight of the equipment in the office, and can be used as an "office size" parameter.

Figure 6 is a plot of Total Wall Load versus Equivalent Length of Lineup for three types of models: (1) pinned base equipment and a 10-Hz building, (2) fixed base equipment and a 10-Hz building, and (3) pinned base equipment and a rigid building. The total wall load indicated is the sum of the CDF side and OPP (opposite CDF) side wall loads from Table 2.

It is obvious from this plot that there is an almost linear relationship between wall load and equivalent length for each of the three model configurations. This was expected since, as indicated previously, the mode shapes and frequencies of each office model are approximately the same. Thus, the major difference in the response for each model type is

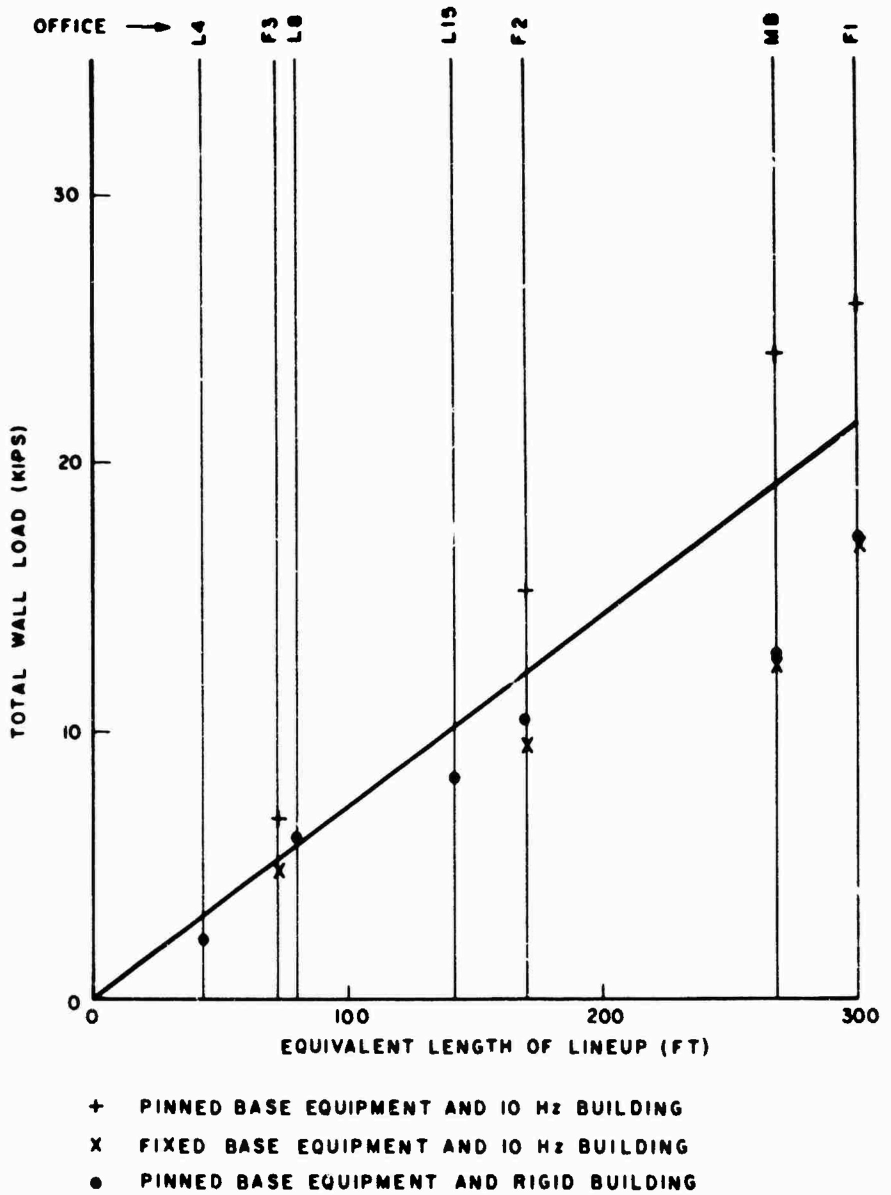


Fig. 6 - Total Wall Load vs. Equivalent Length

primarily a linear function of the total weight of the equipment.

Figure 6 also shows the large differences in response due to changing the building characteristics and changing the type of beam connection between the lineups and the ground. In both cases, the responses are almost double. Considering this data, two assumptions are made. First, since the 10-Hz building is a more realistic representation of the CDO buildings and has such a significant effect, only the results from the 10-Hz building models are considered. Second, since the connection of the equipment to the ground is neither pinned nor fixed, but somewhere in between, it is assumed that the true response is approximately halfway between the two different beam-end condition responses. Therefore, the solid line on Figure 6 represents the total wall load as a function of equivalent length (office size) based on these assumptions. This linear function can be used in conjunction with bracing failure levels to determine bracing requirements for any size office.

SUMMARY

A study of the earthquake survivability of wall-braced equipment in small buildings has been completed. Tests show that the most vulnerable part of the equipment-support structure is the connection of the equipment to the building walls.

Expected earthquake induced loads in the equipment-wall connection were determined by:

1. Developing a coupled building-equipment mathematical model representing the actual facility.
2. Determining frequencies and mode shapes of the model employing finite element computer codes.
3. Determining the dynamic response of the model due to the expected earthquake excitation.
4. Determining peak loadings in the equipment support-structure.

The unique part of the analysis was including dynamic equipment-building interaction. In most other seismic studies equipment and building responses are assumed uncoupled. However, for the specific facilities studied, where the equipment weight is comparable to the building weight, this coupling effect is shown to be significant.

These procedures were applied to a variety of building-equipment configurations. The results indicate that there is a linear relationship between total equipment weight (office size) and total peak support-structure loads. This linear relationship can be used to develop general equipment bracing requirements.

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1. Wilson, E. L., "SAP — A General Structural Analysis Program," Report No. UCSESM 70-20, University of California, September 1970.
2. Bathe, K., "Solution Methods for Large Generalized Eigenvalue Problems in Structural Engineering," Report No. UCSESM 71-20, University of California, November 1971.
3. Anderson, et al., "Lateral Forces of Earthquake and Wind," ASCE Transactions, 1952, pp 717-780.

DISCUSSION

Mr. O'Hearne: (Martin-Marietta) Was your forcing function the longitudinal motion of the floor?

Mr. DeCapua: Yes, horizontal motions.

Mr. O'Hearne: You didn't indicate how you solved the equations.

Mr. DeCapua: We performed an exact solution of the transient equations of motion in the modal analysis where all modes are included.

Mr. O'Hearne: Did you use a digital computer?

Mr. De Capua: Yes.

Mr. O'Hearne: That is a large number of cyclic forcing functions. Did you have a problem with numerical stability?

Mr. DeCapua: No we didn't have any trouble with the stability or with computer time in this problem. It is because we considered low frequencies so that the computation interval was not that small and the total computation time was not that large.

Mr. O'Hearne: I couldn't tell what boundary condition you used from the diagram of your mode shapes. Were the frames all fixed at the base to an unmoving floor?

Mr. DeCapua: The frames were, in some cases pinned and some cases fixed, we did both. The building was fixed to the ground and the floor motion was the lateral earthquake motion.

Mr. O'Hearne: That is what I thought and I would suggest that a better boundary condition would be to do the eigen analysis with the floor free to move longitudinally, in that way you don't have to mass couple the rigid body and you have a diagonal set of equations motions in the generalized coordinates. I think your frequency spectrum for this kind of solution would be better for those boundary conditions.

Mr. DeCapua: The boundary conditions thet I choee ere typicel of the boundary conditions in this type of problem, the inputs were es precise end es eccurate es we could make them so I don't feel thet we could improve the enalysis.

Mr. O'Heerne: I'm not telking ebout your ectual physicel boundary conditions, but the generelized coordinates in which you solve the equations. If you use the boundary conditions which I just indicated you would heve no mass coupling with the rigid body mode. You would have e different spectrum which might be more suitable for your solution. There wes e Bell Labs peper thet wes presented et this meeting lest yeer in which e set of modes, similer to yours, were used for the same type of circumstences.

Mr. DeCepua: It wes quite e bit different. Our input motions ere ectually the ground motions we ere not modeling the soil et ell. In that peper there wes e soil model end so you did get the rigid body building motion in the soil. That perticuler peper wes quite different from this one.

Mr. Butzel: (Boeing Compeny) Whet would heve happened if you hed eccounted for the soil motion? How would that heve influenced these maximum brecing loeds?

Mr. DeCepua: It is not thet we didn't eccount for it. It's thet the environment itself, the El-Centro Eerthquake environment, is the soil motion. There ere other models which ectuelly generete the soil motion in the enalysis. But we essumed thet the free field motions thet we heve ere the actual ground motions, end these ere the motions et the bese of the building. The El-Centro Eerthquake wes e meesured ground motion. So we ere not modeling any soil structure interection, beceuse this is the motion of the soil in our perticuler configuretion.

Mr. Butzel: Isn't it conceiveble that the size of the building that you enalyzed could change the epparent soil motion?

Mr. DeCepua: It is possible, but I think thet the size of this building wes small so that the soil motion eround it probably would not change significently. But it is quite different for lerger buildings.

Mr. Butzel: You mentioned thet these types of buildings are often of wood, brick, or concrete block construction; how would the type of building construction influence the size of the bracing loads that you cen tolerete? A concrete block building typicelly fells apert more easily then freme buildings under earthquake excitation.

Mr. DeCepua: The buildings themselves are also designed for eerthquake excitations. We arrived et loedings thet were exerted by the equipment on the building well end they were such thet both the freme end the concrete buildings could withstend them; the week link in the system wes the support structure of the equipment to the building.

Mr. Butzel: Mr. Schell suggested thet your results could conceivebly depend quite criticelly on the seme mass density of equipment. The fect thet you heve similerity in mode shepe behevior, what heppens if the mass loeding chenges so thet the modal ectivity is different? Would that chenge these results significently?

Mr. DeCepua: Yes it will. But we heve exemined the whole spectrum of community diel offices. Although the size of the office is different, they ell heve similer cherecteristics since they heve certein types of fremes end types of equipment; the line ups are such thet, in the entire spectrum of equipment thet we exemined ell hed similer dynemic cherecteristics. So we feel that ell of the offices ere within this aree. If they ere different then the results of course don't epply, and this depends on the differences in their dynemic cherecteristics.

Mr. Butzel: Do you heve eny wey of determining eheed of time when you ere within sefe limits to use this type of curve? When will you lose thet similerity?

Mr. DeCepue: We heve epproximately 100 of these offices in Celifornie, we heve exemined the equipment configurations, end we have convinced ourselves thet ell of them setisfy the same dynemic cherecteristics es this model.

Mr. Stehle: (General Electric Co.) You have analyzed a particular earthquake, the time history of the El-Centro Earthquake. How do you take into account the fact that it is not the most severe earthquake that you might consider?

Mr. DeCapua: Of course it is not the most severe earthquake it had a Richter magnitude of roughly 7.1. It is possible that California could experience an earthquake of Richter magnitude of 8 but we have also been doing seismic studies simultaneously to these structural studies to determine what is a typical earthquake with various return periods. For example, if we consider that our equipment has a fifty year life we would want to determine what is a typical earthquake that we could expect in California with a fifty year return period.

El-Centro fits these characteristics, a fifty year return period for your buildings, more precisely than a Richter magnitude 8 earthquake. So we feel it is a good environment for our facilities but I want to emphasize that it isn't the worst case. It is possible that California could experience a more severe earthquake but that probability is small.

SEISMIC ANALYSIS OF MOTORS FOR NUCLEAR POWER PLANTS

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and

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Qualification analysis of Seismic Category I rotating electrical equipment using the Response Spectrum plus Modal Analysis technique is described. Equations for a lumped-mass single degree of freedom system are written in terms of influence coefficients and solved using matrix iteration. Spectral accelerations are then used to obtain equivalent static forces for stress and deflection analyses.

Before atomic power became a practical large-scale energy source, consideration of seismic design was generally restricted to buildings and other tall flexible structures. With the recent construction and licensing of nuclear power plants, the potential hazard of an uncontrollable release of excess radioactivity as the result of an earthquake is of great concern. It is of the utmost importance that those systems and components in the plant which are vital to safe operation or shutdown (Seismic Category I) maintain both structural and functional integrity during and after the worst possible seismic disturbance. Included in this category are alternating-current induction motors used to drive pumps as well as fans for the cooling and recirculation of containment air.

As a part of the Safety Analysis Report required in the licensing procedure, documentation must be provided to show that all safety related equipment meets the seismic requirements of that particular plant. A properly designed test of the equipment under simulated earthquake conditions can provide accurate and reliable results, although special facilities are generally needed and the testing is rarely inexpensive. It is possible, however, to qualify apparatus for seismic duty through suitable analytical techniques, several of which will be discussed here.

The first step in any analysis is to develop a mathematical model for the system to be studied. Extreme care and good engineering judgement are necessary in establishing this model since the accuracy of the results depend strongly on how well the model represents the actual physical system. Development of some of the necessary parameters for analysis may require some supplemental testing.

An alternating-current induction motor consists of a wound stator which is pressed into a frame barrel, two end brackets, and a shaft/rotor core assembly supported by two bearings (Figure 1). Torque production is the result of the interaction of the magnetic fields of the stator and rotor. The end brackets are of cast iron and are well ribbed both internally and externally. The frame is also of cast iron. The assembly consisting of the frame, stator and end brackets is much stiffer and has a much larger mass than that of the shaft/rotor core assembly. It can be assumed then that the non-rigid system of interest for analysis is restricted to the motor shaft and the masses which it supports. The problem is, therefore, reduced to that of a beam simply supported by two bearings.

This type of construction is typical of motors through 400 horsepower up to NEMA (National Electrical Manufacturers Association) frame 449T. Maximum motor weight is approximately 2000 lbs. which is small compared to the building in which it would be located. Consequently, the motor can be modeled independently from the supporting structure. For cases where

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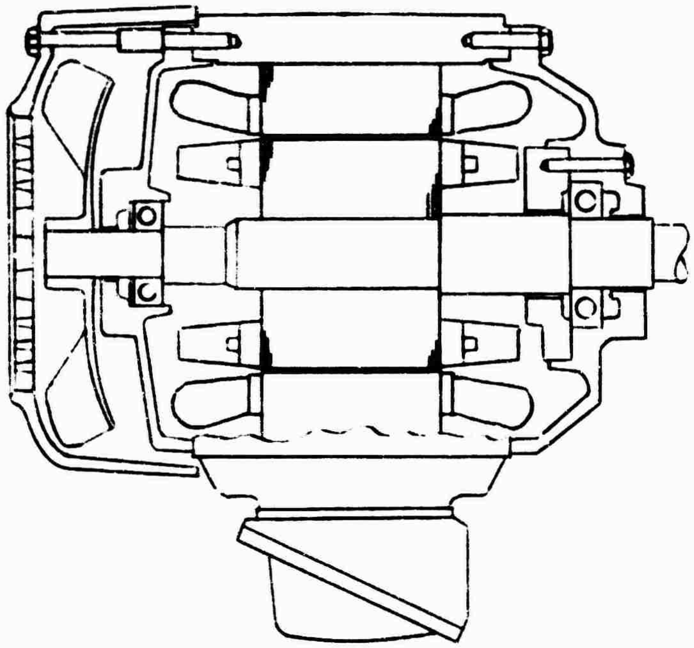

Fig. 1 - Typical construction features totally anclosed a.c. induction motor.

the mass of the equipment is no. ralativaly small or whera flexible aquipment supports ara usad, independent modaling is not justifiable and the antira systam must be considered in a single mathamatical modal.

A common application of an induction motor as Saismic Category 1 alactrical aquipment is in tha Reactor Containment Fan Coolar (RCFC) System. The function of this system is to provide cooling and air recirculation within tha raactor primary containment. The motor generally has a two spead winding. During normal operation, the motor performs at the higher horsapower and speed. In the event of a nuclear accident, a.g., a pipe ruptura, tha motor is switchad to low spead and horsepower operation. During accidant conditions, tha motor must function to circulate densa air containing high prassura, high tamperatura steam containing a decontaminant chemical spray intended to reduce the radiation laval of the containment environment. In line with tha conservative design principlas employed in nuclaar power plant dasign, it is postulated that a seismic avant occurs during just such an accidant.

Figura 2 shows a typical RCFC shaft/rotor cora assambly. A two-stage fan is illustrated, with identical impellers mounted naar the end of each shaft extansion. Betwean tha two bearing supports is the rotor core assembly. This system can be modalad as a lumped-mass system

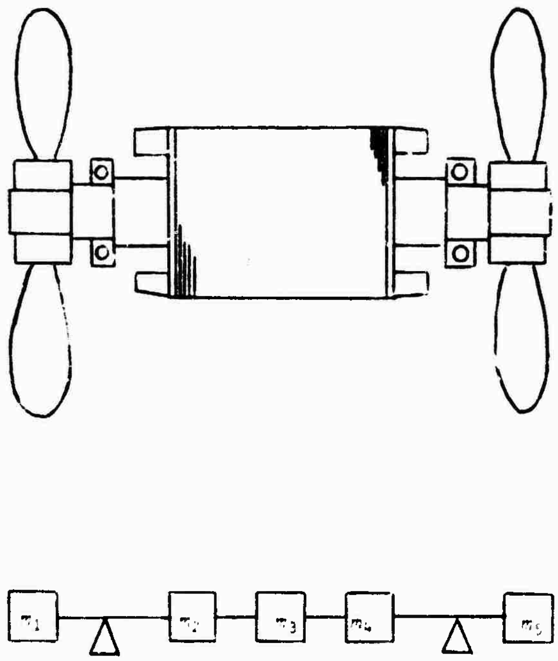

Fig. 2 - Shaft and rotor assembly with two shaft mounted impellers.

with the masses connected by massless elastic members through the interaction of the shaft and bearings. In selecting the number and location of mass points necessary to describe the system, the criterion to be used is that the shaft distortion be accurately represented by the displacements obtained. As a guide, mass points should be assigned to any component of mass having a deflection differing significantly from an adjacent mass.

For example, for the assembly shown, the mass of each impeller can be considered as acting over a fairly short length of shaft and each mass can be treated as a single mass acting at its center of gravity. On the other hand, the rotor core may be fifteen to twenty inches long. As a result, three or four distinct mass points would be necessary, each representing a section of the core and each located at that section's center of gravity.

Various industry standards and guides have evolved which deal with suggested methods of analysis and testing of electrical equipment. The most significant such document yet produced is probably IEEE No. 344 (Institute of Electrical and Electronics Engineers), Guide for Seismic Qualification of Class 1 Electrical Equipment for Nuclear Power Generating Stations. It is the intent of this guide to provide direction for establishing procedures which will yield data verifying that Class IE equipment meets performance requirements during and following a Safe Shutdown Earthquake (SSE). The SSE is that earthquake producing the maximum vibratory gound motion that the nuclear power plant is designed to withstand without functional impairment of those features necessary to shut down the reactor, maintain the station in a safe condition, and prevent undue risk to the health and safety of the public.

IEEE No. 344 permits the use of any justifiable method of analysis although two particular approaches are stressed - Static Analysis and Dynamic Analysis using the Response Spectrum Modal Analysis technique. Use of the Static Analysis is limited to rigid equipment which can be shown to respond as a single degree of freedom system. Seismic forces are obtained by multiplying the maximum expected seismic acceleration by the mass of the equipment, assumed to be concentrated at its center of gravity. In general, however, the equipment will not be rigid and a dynamic analysis will be required.

Several methods for dynamic analysis are available to the designer. Once the equations of motion for the system are written, integration can be performed. This can be accomplished either by use of an analog computer or by numerical integration. The difficulty encountered in using this method lies in defining an appropriate driving function - a time history record to be used as input. Other possible techniques include Fourier Analyses and the Power Spectral Density Analysis. Neither are commonly employed. The method suggested here and in IEEE No. 344 is the Dynamic Analysis via the Response Spectrum Modal Analysis. Since response spectra are more readily available from consulting engineers and contractors than are any other form of input parameters, use of the Response Spectrum Analysis is a more realistic and practical approach to the problem. The Modal Analysis gives as its results the natural frequencies, mode shapes and participation factors. The Response Spectrum is then used to provide the acceleration response of each mass point in each mode. Use of the Modal Analysis enables the responses in each normal mode to be evaluated separately, then superimposed to provide the total response. Each normal mode may be treated as an independent one degree of freedom system.

Consider the system shown in Figure 2. Assume that motion will occur only in the plane of the paper and that the system is vibrating in a normal mode. Vertical displacements are to be considered positive when upward from the static equilibrium position. For each mode point, the equation of motion can be written in terms of influence coefficients as

$$-x_i = \sum_{j=1}^{5} \delta_{ij} m_j \ddot{x}_j$$

Assuming harmonic motion and replacing $\ddot{x}_j$ by $-\omega^2 x_j$, the system of equations can be rearranged in matrix form as

$$[X] = \omega^2 \; [\delta] \; [M] \; [X]$$

Evaluation of the 5 x 5 influence coefficient matrix, $[\delta]$, is necessary before proceeding further. By definition, the influence coefficient, δ_{ij}, is the deflection at the coordinate i due to a unit force applied at the coordinate j.

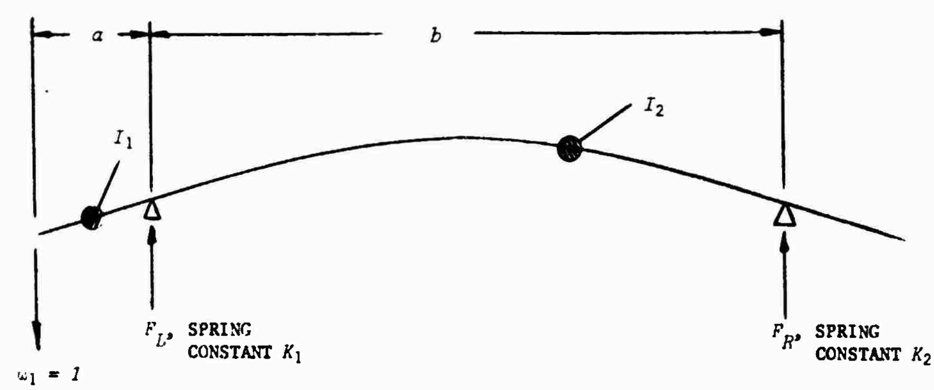

Fig. 3 - Evaluation of influence coefficient δ_{11}

By Maxwell's reciprocal principle, $\delta_{ij}=\delta_{ji}$. As a result, only 15 of the 25 influence coefficients will have to be computed.

The deflection at node 1 due to a unit force applied at that same node, δ_{11}, is the result of the angularity at the left bearing support, bearing flexibility and the cantilever beam deflection outboard of the bearing (Figure 3).

$$\delta_{11} = \Delta_1 + \Delta_2 + \Delta_3 + \Delta_4$$

where Δ_1 = deflection due to angularity at left bearing.
Δ_2 = deflection at node 1 due to left bearing deflection.
Δ_3 = deflection at node 1 due to right bearing deflection.
Δ_4 = cantilever beam deflection.

$$\Delta_1 = a\theta = -\frac{a^2 b}{3EI_2}$$

$$\Delta_2 = -\frac{F_L}{K_1}\left(\frac{a+b}{b}\right)$$

$$\Delta_3 = -\frac{F_R}{K_2}\left(\frac{a}{b}\right)$$

$$\Delta_4 = -\frac{a^3}{3EI_1}$$

The other influence coefficients are determined in a similar fashion. Once this has been completed, all of the parameters necessary to solve the matrix equation are available. The process of matrix iteration is suggested here as the method of solution.

The process is begun by estimating the configuration of the first mode and substituting the assumed values of displacements into the matrix equation then determining if the equality holds. In general it will not and the results of the first iteration become the starting point for the second iteration. Iteration continues until the assumed x_j are approximately equal to the calculated x_j. The result will be the fundamental frequency, ω_1, and the mode shape as defined by the values of x_j. For the second and higher modes and natural frequencies, the orthogonality principle is used to obtain a new matrix equation that is free from any lower modes. The iteration procedure is used again to solve the new system of equstions.

As each mode shape is determined, the modal participation factor, Γ_n, must be evaluated.

$$\Gamma_n = \frac{\sum_{i=1}^{5} m_i x_i}{\sum_{i=1}^{5} m_i x_i^2}$$

The modal frequencies now provide the input to the Response Spectrum Analysis. A typical response spectrum is shown in Figure 4. A response spectrum is a plot of the peak responses of a large number of single degree of freedom systems of different natural frequencies, at a damping value expressed as a percent of critical damping, to a specific input transient, in this case, a given earthquake motion.

Typical ground accelerations for even a relatively severe earthquake are generally rather small, a fraction of gravity. The magnitude of the acceleration at some location several floors up in the containment structure, however, can be much larger depending on the flexibility and vibration characteristics of the structure. Since the rotating equipment has been modeled independently from the supporting structure, an appropriate floor response spectrum must be provided for every level at which the apparatus will see service.

Each of two major horizontal directions plus the vertical direction should be considered separately but combined simultaneously. Unless the directional orientation of the unit is known, the analysis should be done so as to provide the most conservative results. For preliminary analyses where all three of these response spectra may not be available, and only a horizontal floor response spectrum is provided, the second horizontal direction is assumed equivalent to the first and the vertical response is assumed to be two-thirds of the horizontal response. For final qualification work, however, no assumptions should ever be made and all of the seismic criteria will always be available.

Proceeding on a modal basis, the spectral acceleration, S_{an}, can be read from the floor response spectrum as a function of natural frequency and percent of critical damping. The equivalent static seismic force at each mass point for each mode is then

$$F_{in} = m_i x_{in} \Gamma_n S_{an}$$

Using these equivalent forces, seismic stresses and deflections can be calculated by static stress analysis methods. Modal forces and deflections for each node are first combined algebraically to obtain maximum effects for each principle direction, then these three seismic stress and displacement values are combined with the normal operation loads by the root of the sum of the squares method.

The final step in the analysis is the comparison of the combined stresses and deflections with the allowable limits. This includes strength and alignment considerations as well as mechanical interference effects. Limits are established to insure that the motor will remain functional during the worst anticipated earthquake for the plant site. Some permanent deformation may even be acceptable as long as the motor maintains operability and structural integrity. Redesign or modification are, of course, necessary should any of the loads or deflections be excessive.

The procedure described above is not restricted to rotating machinery but can be applied to any equipment that can be modeled as a lumped-mass multi-degree-of-freedom system with mass free interconnections.

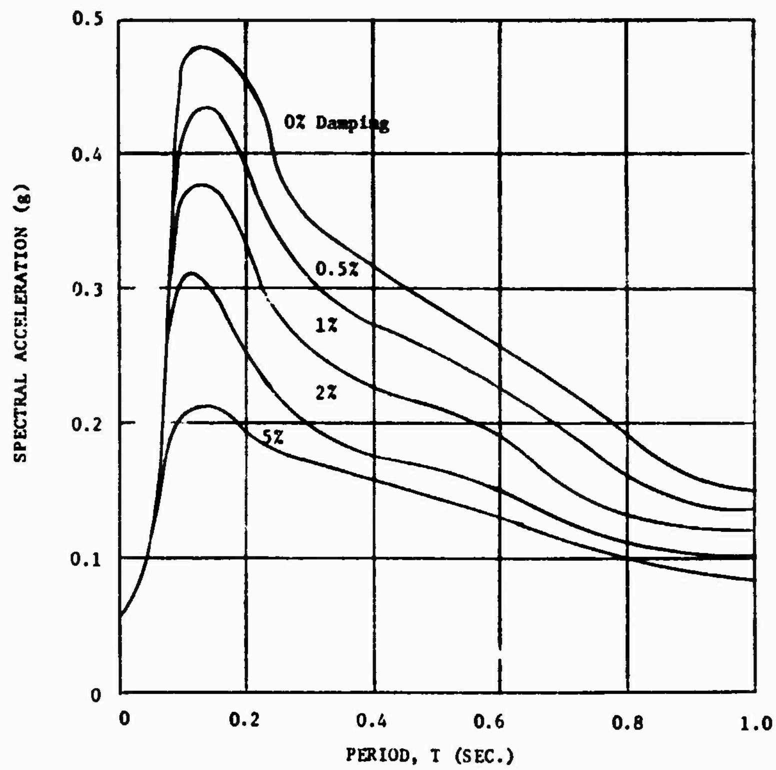

Fig. 4 - Typical earthquake response spectrum.

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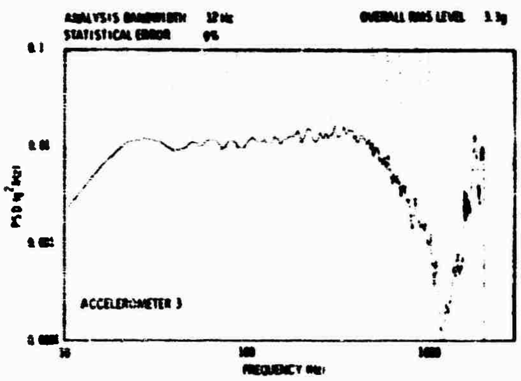


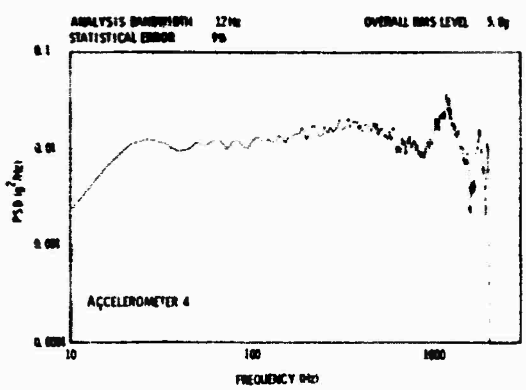


Fig. 3 - (cont.)

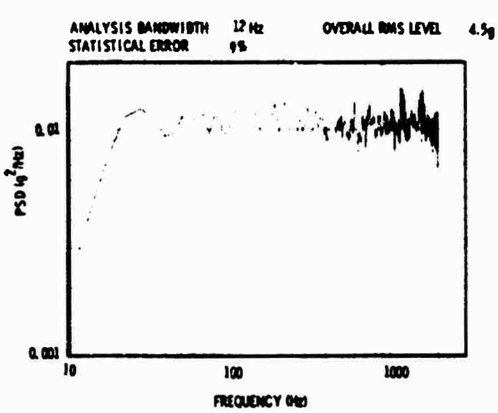


Fig. 4 - Average PSD for input signals

RANDOM-ON-RANDOM

Random-on-Random is a term used to describe a vibration testing environment that provides for superimposing several swept narrow-band random signals on a background stationary random spectrum. This technique provides a more realistic test for simulating environments where a significant amount of energy is concentrated in a few narrow bands but where the center frequency of each band is somewhat uncertain.

The implementation of this technique involves a change in the test setup program to input the band parameters and a change in the control program to dynamically change the reference spectrum during the actual test run. Up to five bands are allowed; each band is described by the following five parameters.

1. Start frequency - defines the start frequency for the band.

2. End frequency - defines the end frequency for the band.

3. Narrow random bandwidth - the program calculates from this parameter the number of frequency lines to raise for the narrow random bandwidth. This bandwidth is then swept from the start frequency to the end frequency.

4. Number of times to sweep band - this parameter defines how many times the band is swept during the test. The program calculates from this parameter and the test duration the number of control loops before stepping one line. The program will not allow parameters that require a faster sweep than one line per control loop. For reasons discussed in the next section, if the sweep is faster than one line per 15 control loops, the program writes a warning message onto the CRT screen to alert the operator that the narrow-band random spectrum will be swept too fast for good control.

5. G**2/Hz level - this is the g^2/Hz level of the narrow-band random signal.

The program allows for the bands to overlap. Note that the sweep for each band is linear with respect to time.

The control program initiates the reference spectrum with the narrow-band random superimposed at the start frequency; once the full test level is reached, the sweep is started.

approximately equal to the sixth root of the ratio of the return and reference spectrum amplitudes. The inverse Fourier transform of the modified drive spectrum is computed, and the resulting time array is used to generate the drive signal for the next control loop (i.e., for the next N frames). Each frame of the next control loop is further randomized by the array rotation, sine windowing, and overlapping operations. This ensures a drive signal that will be stationary and will not have discontinuities at the frame boundaries.

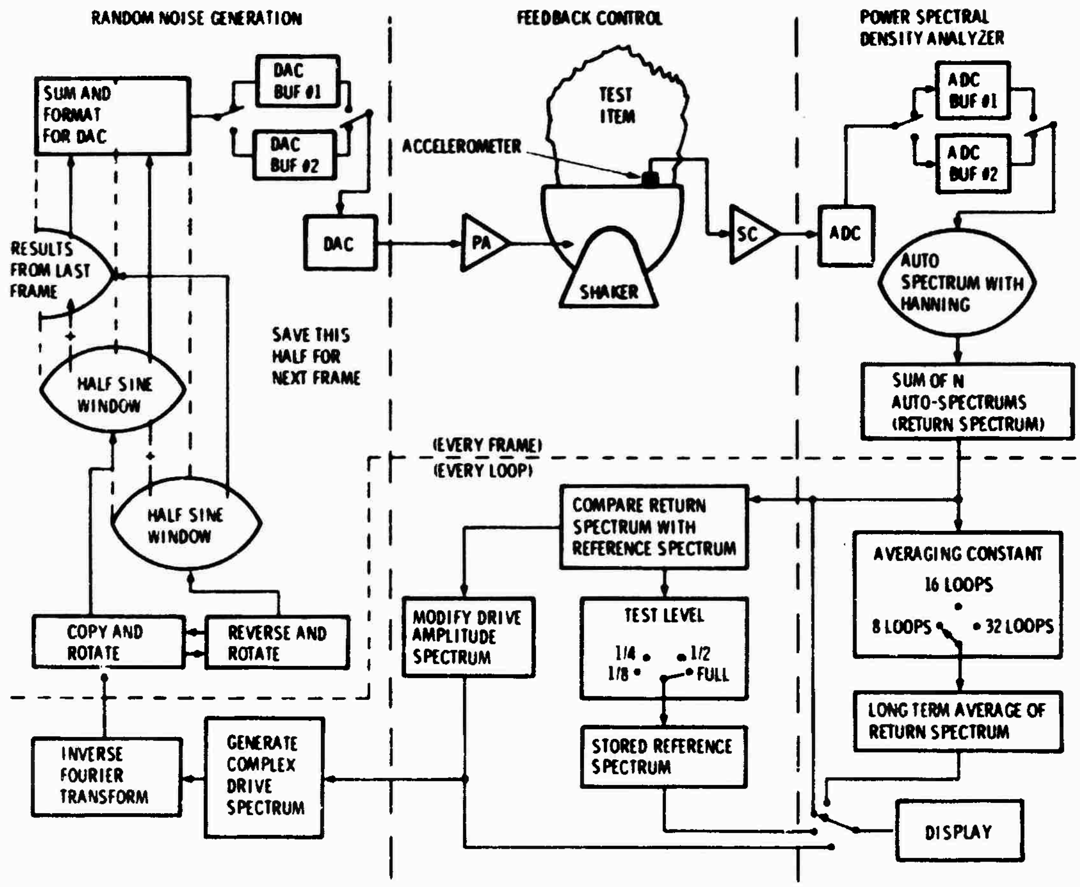


Fig. 1 - Functional block diagram for a digitally controlled random vibration system

AVERAGING

At high frequencies fixture resonances cause the acceleration to become nonuniform over the fixtures. Therefore, averaging the power spectral density (PSD) of several input channels can provide a better input definition on large test items than a single control input channel. Input averaging is implemented by sampling each of the input channels for one or more frames (ADC buffers) during the control loop as shown in Fig. 2. The PDS estimates for each input are then summed to provide an estimate of the average PSD. The average PSD estimate is compared with the stored reference spectrum and the drive signal is modified in the same manner as for a single input test. All input accelerometer signals must be normalized for the same sensitivity, i.e., g/volt, and each input has equal weight in the average.

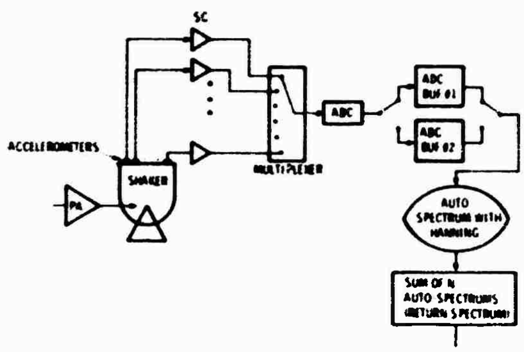


Fig. 2 - Modification of control algorithm for input averaging

The setup program asks the operator to input the number of control accelerometer signals to be averaged for the test. Up to six inputs may be specified. The program then calculates six choices for control response time; the operator selects the appropriate time based on his test requirements. (A short test will require a fast control response time.) The selected control response time determines the number of frames per loop for use in the control algorithm. The choices for control response time are calculated in such a manner that the number of frames per loop is a multiple of the number of inputs to average. This ensures that each input will have equal weight in the average. The control algorithm requires that at least four frames per loop be used. Table 1 shows the relationship of these two parameters.

TABLE 1

Correlation of Number of Inputs to Average and Number of Frames per Loop

No. of Accel.	No. of Frames per Loop 1	2	3	4	5	6	Choices
2	4	8	12	16	20	24	
3	6	9	12	15	18	21	
4	4	8	12	16	20	24	
5	5	10	15	20	25	30	
6	6	12	18	24	30	36	

For input averaging the control program initiates the analog-to-digital converter system (ADC) for the multiple sample mode. This mode causes the ADC to sample the first channel for one frame (buffer), the second channel for the next frame, etc., recycling after the last channel. The control algorithm modification, shown in Fig. 2, affects only the PSD analyzer section. The multiplexer was part of the existing ADC system. No hardware changes and only minor software changes were required to implement this option.

Input averaging was checked by running a test on a slip table. Four accelerometers were mounted at equal intervals down the center of the table. Figure 3 shows the PSD for each input; Figure 4 shows a plot of the calculated average of the four return spectrums. As can be seen from Figs 3 and 4 the average spectrum was close to the required spectrum 0.01 g^2/Hz); whereas, the individual accelerations indicated large variations in level.

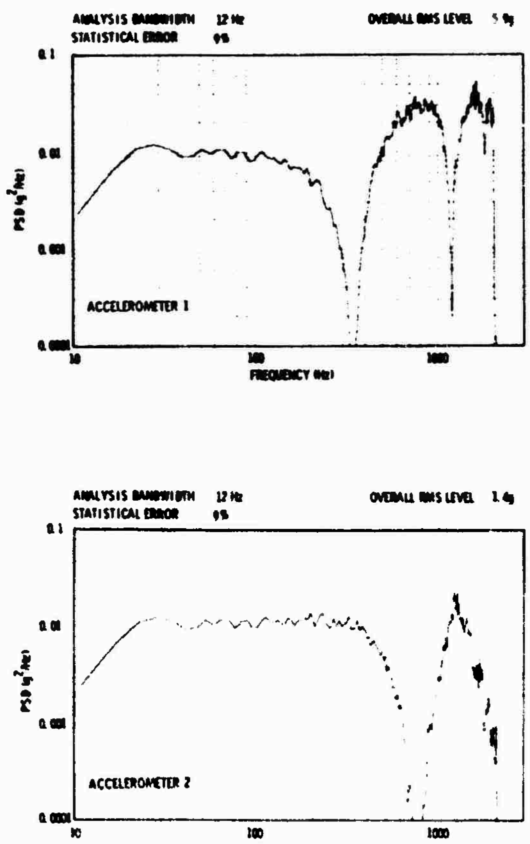
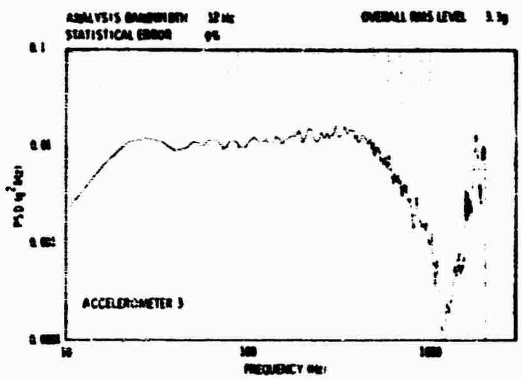


Fig. 3 - Power spectral density for return signals



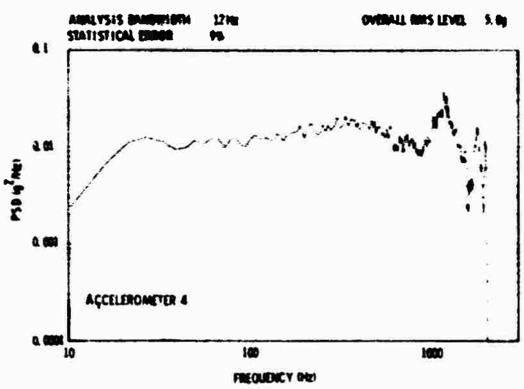


Fig. 3 - (cont.)

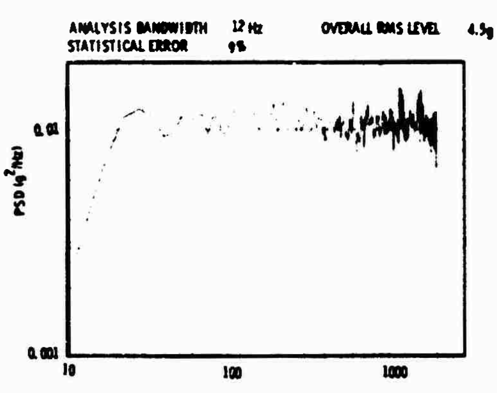


Fig. 4 - Average PSD for input signals

RANDOM-ON-RANDOM

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The implementation of this technique involves a change in the test setup program to input the band parameters and a change in the control program to dynamically change the reference spectrum during the actual test run. Up to five bands are allowed; each band is described by the following five parameters.

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5. G**2/Hz level - this is the g^2/Hz level of the narrow-band random signal.

The program allows for the bands to overlap. Note that the sweep for each band is linear with respect to time.

The control program initiates the reference spectrum with the narrow-band random superimposed at the start frequency; once the full test level is reached, the sweep is started.

The narrow-band random is swept by subtracting and adding the dashed vectors every N control loops as indicated in Fig. 5. Note that the end lines on the narrow-band random spectrum are raised to 1/2 level.

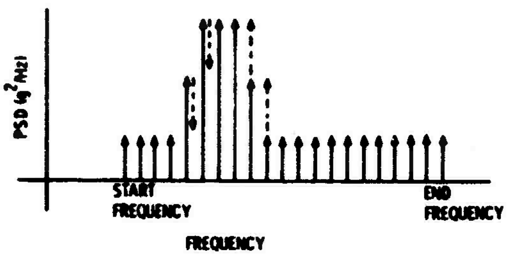

Fig. 5 - Narrow-band random on broad-band random background

The control algorithm updates the drive spectrum every control loop. The correction to the drive spectrum is approximately equal to the sixth root of the ratio of the reference spectrum amplitude to the return spectrum amplitude; therefore, the number of control loops needed to correct the level to within 10 percent of the desired level can be estimated. Figure 6 is a plot of the amount of change needed versus the number of control loops required to make the change. If the narrow-band random spectrum level is significantly higher (greater than 4 times) than the background random spectrum level, the two end lines on both sides of the narrow-band random spectrum must be changed by at least 2:1. From Fig. 6, this will require at least 13 control loops to correct the level to within 10 percent of the desired level. For good control fifteen control loops were selected as the minimum number required before stepping (sweeping) to the next frequency line. This will ensure that the system will have time to equalize the level of each new frequency line.

The effect of sweep rate and bandwidth of the swept narrow-band random spectrum is analogous to the response of a low-pass filter to a rectangular pulse input. If the pulse is too narrow, the output will approximate a half-sine pulse at a reduced amplitude. The output signal can only rise and fall so fast; thus, for short-duration pulse inputs, the output peak amplitude is proportional to the pulse duration. The narrow-band random bandwidth is analogeous to pulse duration; the sweep rate, to cutoff frequency. If the bandwidth is too narrow or the sweep rate too fast, the return spectrum will be rounded and reduced in amplitude.

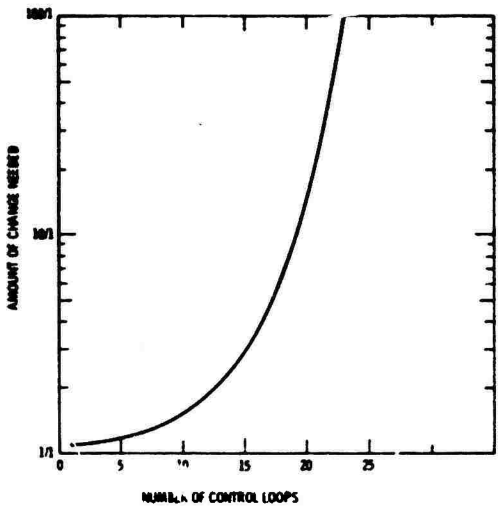

Fig. 6 - Amount of change needed vs number of control loops required to correct the level to within 10 percent of the desired level

In summary, the major change required to implement this technique was in the software required to specify the test. The only change required in the control loop was the code required to dynamically change the reference spectrum. No hardware changes were required.

To illustrate the "Random-on-Random" capability, a test was run with two narrow-band signals superimposed on a flat random background. The background random was specified at 0.01 g^2/Hz over the frequency range 4 to 2048 Hz. The reference spectrum for Band 1 (see Table II) was modified every four control loops, and the narrow-band random was four lines wide. Therefore, 16 control loops were required for the band to sweep by a frequency line.

TABLE II

Band Parameters

Parameter	Band 1	Band 2
Start Frequency (Hz)	10	200
Stop Frequency (Hz)	200	400
Bandwidth (Hz)	16	52
Sweep	1	1
Level (g^2/Hz)	0.1	0.1

Test Duration: 250 seconds

From the preceding discussion, the narrow-band random should exhibit rounded shoulders with a peak near the specified level.

The reference spectrum for Band 2 was also modified every four control loops. However, the band was 13 lines wide, requiring 52 loops for the band to sweep past a frequency line. The band should exhibit a flat top at the specified level with square shoulders.

Figure 7 is a plot of the return spectrum at 125 seconds after the test was started. As predicted, Band 1 is rounded with a level near 0.1 g^2/Hz; Band 2 has a flat top, also near 0.1 g^2/Hz.

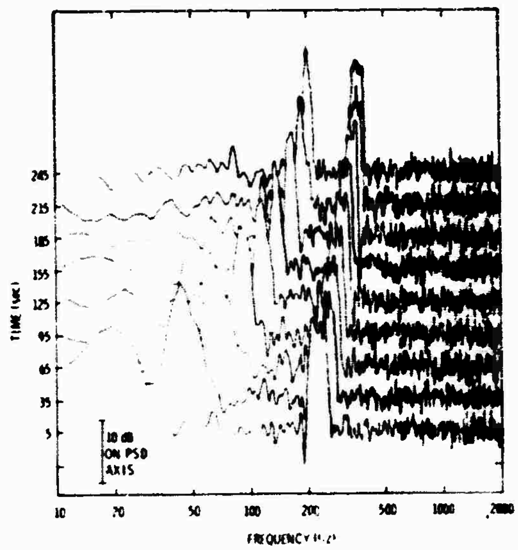

Fig. 8 - Random-on-Random test results

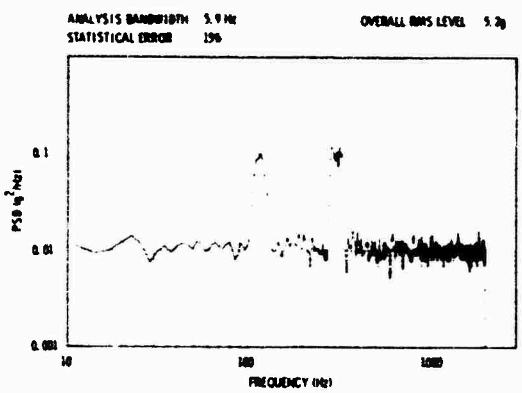

Fig. 7 - Return spectrum analyzed at 125 seconds after the test was started

Figure 8, a three-dimensional plot of the return spectrum, was constructed by analyzing the return spectrum at equal time intervals (30 seconds) and overlaying the plots. Linear time is measured along the vertical axis, log amplitude is measured along the vertical axis, and log frequency is measured along the horizontal axis. The curve at 125 seconds is shown as Fig. 7. Band 1 maintained its rounded shape at the correct level during the sweep. The apparent narrowing of the band is due to the log-frequency presentation of the data. Band 2 also maintained a relatively square shape during the sweep.

RANDOM LIMITING

In vibration testing, it is generally assumed that when the correct motion is reproduced at the control point of a test item, the motion at other points on the test item will also be correct. This, of course, assumes that the test item is driven at the correct point (or points), the proper boundary conditions are maintained, and the control spectrum accurately represents the field environment. In many cases, these assumptions are not even approximately met in the test laboratory when compared with the field use of the test item. For these cases, limiting is a useful concept to prevent overtesting. The limit concept assumes that field experience indicates that the response of various points on the structure will not exceed certain values; therefore the response during a test should also not exceed these values. Test techniques[3,4] that rest on the limit concept have also been suggested.

The obvious technique for limitation, using a digital control technique, is as follows: Measure the PSD of each of the limit channels concurrently with the PSD for the control channel. If the PSD for any limit channel exceeds its respective limit spectrum, reduce the drive signal at the appropriate frequencies. Such a system would be real time and should produce good results. The disadvantages of this method include the following: The number of limit channels which can be successfully handled is small. The exact number will depend upon the hardware available for a particular system. As the number of limit channels is increased, the control loop time must increase as significant additional time is required to process the limit data.

A second approach to random limiting has been suggested and implemented at Sandia Laboratories, Albuquerque. If the system is linear and the input Gaussian and stationary, the response at any point can be determined from the input, provided that the transfer function between the two points is known from the familiar equation

$$Y(f) = |H(f)|^2 \, X(f) \qquad (1)$$

where

$Y(f)$ is the response power spectrum,

$X(f)$ is the input spectrum, and

$H(f)$ is the transer function between X and Y.

The basic approach is to measure and store the transfer functions for each of the limit points. These can be measured during a short random test, a low-level random test, or a transient test. A transient test is currently used at Sandia. The response spectrum is then estimated from Eq. (1). If the estimated response spectrum exceeds the respective limit spectrum, the reference spectrum for the control point is reduced an appropriate amount. The method is outlined in block form in Fig. 9. The random control program is then run in the usual manner by using the modified control point reference spectrum $X_i(f)$.

The advantages of this implementation are as follows: The method is easy to implement. Any number of limit channels can be handled with no increase in complexity or in the control loop time. The principal disadvantage is that the limit control assumes a linear system and is open loop. That is, if an error is made, it cannot be automatically corrected during the test run. This disadvantage is partially offset by noting that the precision required for limiting a spectrum is often not so critical as for the primary control channel.

To implement the above approach, a method was provided for measuring the required transfer functions and to modify the reference spectrum. No hardware changes or changes in the basic control loop were required.

Data from a large number of tests are required to determine whether the above implementation is sufficiently accurate to satisfy most users or whether the more sophisticated method outlined first will be required.

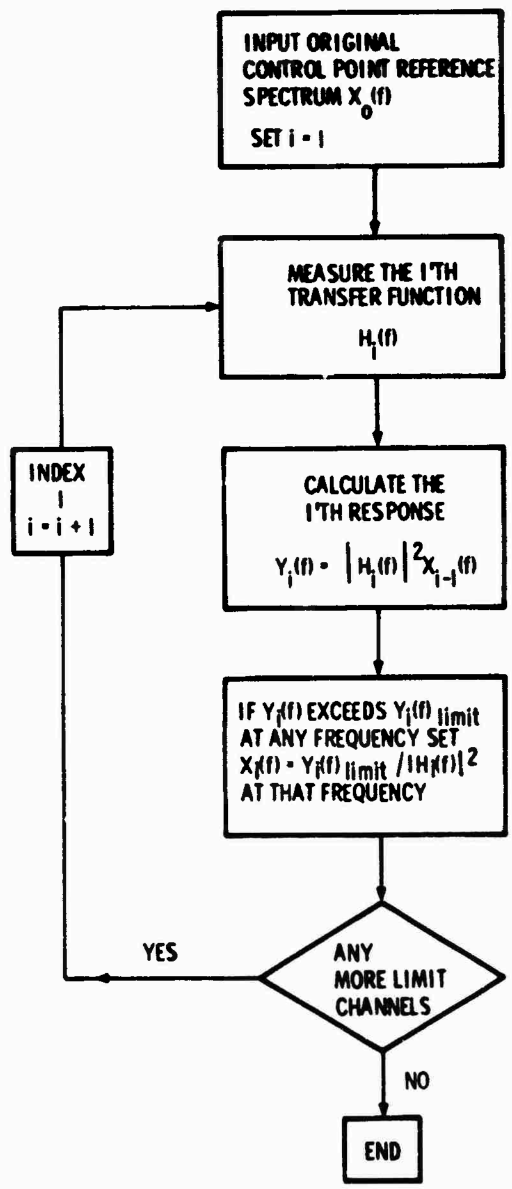

Fig. 9 - Block diagram of limit algorithm

An example to show the type of results expected is included. The same test setup previously used to check input averaging was used. In this case, Input 1 was defined as the control point. The control spectrum was set at 0.01 g^2/Hz from 20 to 2000 Hz. Input 4 was selected as the limit channel. Figure 10 shows the transfer function between Input 4 and Input 1. If limiting were not used Eq. (1) would indicate an expected response of 25 g^2/Hz ($50^2 \cdot 0.01$) at 350 Hz and 1 g^2/Hz ($10^2 \cdot 0.01$) at 1200 Hz. The limit spectrum was set at 0.02 g^2/Hz. Figures 11 and 12 show the results of this test. As can be seen from Fig. 11, the control spectrum was sharply reduced at the first two axial resonances of the slip table (approximately 350 and 1200 Hz) to limit the response at the end of the table. The limit was quite good (see Fig. 12). The third axial resonance, which is evident from the data (approximately 1800 Hz), did not require limiting.

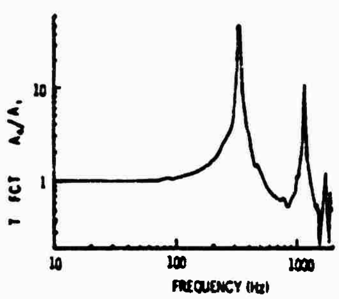

Fig. 10 - Transfer function between Input 1 and Input 4

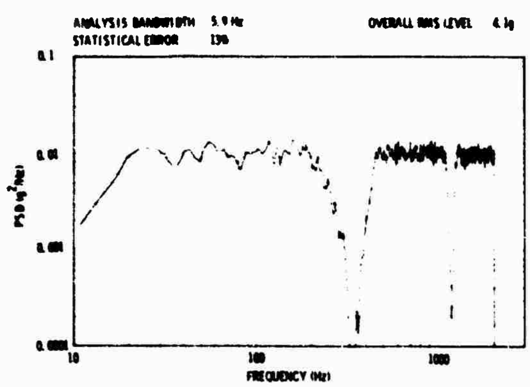

Fig. 11 - Response limiting--control spectrum

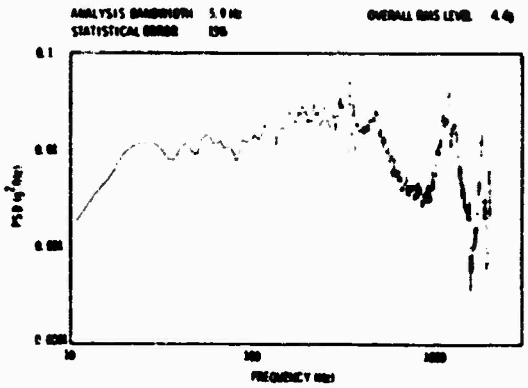

Fig. 12 - Response limiting--limit channel spectrum

CONCLUSION

The basic control algorithm for digitally controlled random vibration tests can be expanded to include more complex test requirements than the single control channel test: This paper has described three control extensions that have been implemented and tested at Sandia Laboratories, Albuquerque, New Mexico. These control extensions have shown the flexibility of digital control to adapt to new testing requirements without hardware reconfigurations.

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DISCUSSION

Voice: What sweep rate did you use on the particular example of the narrow band random on random that you showed?

Mr. Smallwood: In this particular case the test duration was 10 minutes and we swept over the band once in that period.

Mr. Curtis: (Hughes Aircraft Co.) In doing response control tests analog style, and using the transfer function in the same manner as you indicated, we found it necessary to use an iterative scheme to measure the transfer function at increasing levels so that we can take care of the nonlinearities due to changes in damping as the level increases. Have you had a similar experience?

Mr. Smallwood: Yes. I will admit that that is sometimes a problem with this particular method of limiting. To overcome this problem we try to use a short burst of noise which is near the desired input level in order to measure the transfer function.

Mr. Curtis: It seems to be, and it has been our experience, that the short burst that you try the first time should be a few db down because if notches are called for, and you are at essentially full level during that first burst, you may have a considerable amount of overtest during the get ready time. This may be enough to have a failure when you really shouldn't have one.

Mr. Smallwood: This is also true; however we have been using transients for this burst which is typically only 100 milliseconds long. I will admit that we generally run a low level test first to make our initial estimate of the transfer function. If that works we are in good shape if it doesn't we have to iterate. That is correct the iteration is done in an essentially manual fashion.

VIBRATION-INDUCED DOPPLER EFFECTS ON AN AIRBORNE SHF COMMUNICATION SYSTEM

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The vibration-induced Doppler effects on an airborne super-high-frequency (SHF) communication system were investigated by a flight test of the system mounted on a transport aircraft and communicating with a synchronous satellite. The Doppler effects due to the aircraft rigid-body motion are sensed by the inertial navigation system (INS) and removed from the signal by a computer. The remaining Doppler effects from antenna/INS relative vibration thus limit system performance. This relative vibration was measured and analyzed in terms of the parameters of displacement, velocity, acceleration, and jerk (rate of change of acceleration). A frequency analysis identified significant resonances of the antenna. A time analysis obtained the number and duration of the times the communication system would be inoperative during flight due to excessive vibration. The investigation revealed the potential severity of vibration Doppler effects at SHF frequencies, near 8GHz. These frequency-proportional effects will be greater for systems using higher frequencies, and such systems must be designed with vibration - induced Doppler effects in mind.

INTRODUCTION

The Air Force Flight Dynamics Laboratory has recently been investigating a new vibration phenomenon in airborne communication systems — the Doppler effects on the signal caused by the vibration of the airborne antenna. These effects are proportional to the signal frequency and are becoming more important as higher frequency bands are used, as in communication with airborne command posts(1).

To develop secure, reliable communication for the Advanced Airborne Command Post, the Air Force Avionics Laboratory has been investigating the use of super-high-frequency (SHF) communication terminals to communicate between aircraft and synchronous satellites(2). At these frequencies, near eight gigahertz (GHz), Doppler effects from the aircraft motion may shift the signal frequency by several kilohertz. This signal must be accurately followed in frequency and phase by the receiver. To acquire and track the signal, the aircraft rigid-body motions are measured by the inertial navigation system (INS). The resulting Doppler shift is determined and the receiver tuning is corrected by a computer. The remaining Doppler effects due to antenna vibration with respect to the INS are thus limiting factors on system performance. Degradation of signal or loss of tracking can result from excessive Doppler effects due to vibration.

The vibration-induced Doppler effects were expected to be particularly severe for this program because of the new modulator/demodulator (modem) to be used. The modem is the key component of the communication system. The proposed modem was phase-shift keyed and designed with tight tracking loops for enhanced performance. It was originally designed for a ground base in which the vibration environment could be ignored. For the airborne application, however, the design features which improved its performance made it far more sensitive to aircraft vibration Doppler effects.

To measure these vibration-induced Doppler effects, the Flight Dynamics Laboratory performed a flight test program on a testbed transport aircraft equipped with an SHF terminal communicating with a synchronous satellite. This test was part of a program to study several aspects of the airborne SHF communication system. The other tests are being reported separately, and some have been published(3).

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THE COMMUNICATION TERMINAL

The airborne communication terminal, shown in Figure 1, is a cargo aircraft with a twenty-foot long fiberglass radome housing the SHF antenna. Inside the radome is a 33-inch diameter parabolic antenna driven in elevation and azimuth by servomotors to point toward the satellite.

The components of the communication terminal are shown schematically in Figure 2. The signal received from the satellite was routed through the antenna pedestal to a parametric amplifier and thence to the receiver/transmitter. The transmitted signal went through a power amplifier to the antenna. A computer was used to command the antenna to point in the proper direction. The computer commands were based on the satellite location and on the aircraft position derived from the inertial navigation system (INS).

In addition to controlling the pointing direction of the antenna, the computer calculated the Doppler frequency shift in the received signal due to the aircraft velocity and commanded the modem to track the signal at the proper frequency. The modem used in these flight tests was based on an existing design which was not highly affected by Doppler shifts.

To adequately define the Doppler effects on the new modem, four parameters of the antenna vibration were needed. The displacement causes a phase shift in the signal; the velocity causes a frequency shift; the acceleration determines the rate of change of the Doppler shift; and the jerk (rate of change of acceleration) determines the abruptness of the Doppler shift rate. Each of these parameters affects a phase-lock tracking loop of the modem; the full four-parameter vibration environment was therefore needed to define all of the feedback loop values.

The Doppler frequency shift for a radio signal is approximately 1 Hz per MHz per aircraft Mach number in the direction of the satellite. For example, an 8 GHz signal being transmitted from the satellite to an airplane flying at Mach 1 directly toward the satellite (with the satellite just above the horizon) would exhibit a Doppler frequency shift of about 8000 Hz. During these tests, the satellite altitude above the horizon and the aircraft velocity produced a maximum Doppler shift of about 5000 Hz.

While the aircraft velocity produces a Doppler frequency shift, the aircraft acceleration causes a rate of change of this Doppler shift. An aircraft in a 60° bank (2g turn) will show a $\sqrt{3}g$ acceleration in the horizontal plane,

Figure 1. Airborne Communication Terminal.

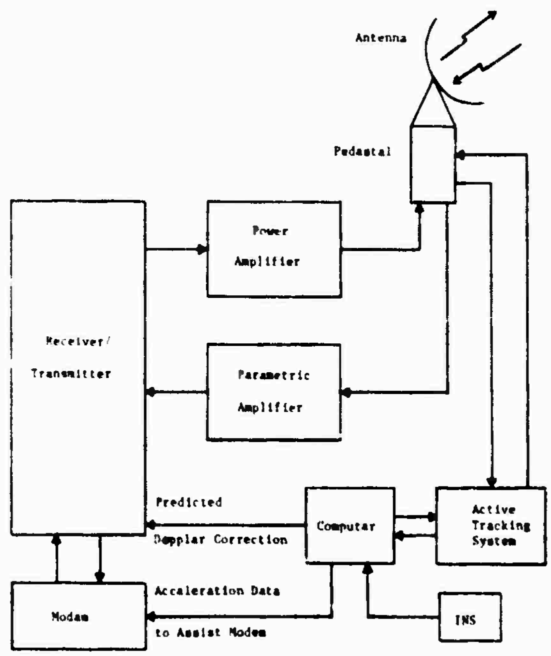

Figure 2. Components of Airborne Communication Terminal.

and if the satellite appears just above the horizon, this acceleration will produce a Doppler shift rate of about 450 Hz/s. The new modem had been modified to track under a maximum Doppler shift rate of 500 Hz/s, and this steep bank is thus a stringent operating condition. Any vibratory acceleration of the antenna would add to this Doppler shift rate and could cause it to exceed the modem operating range.

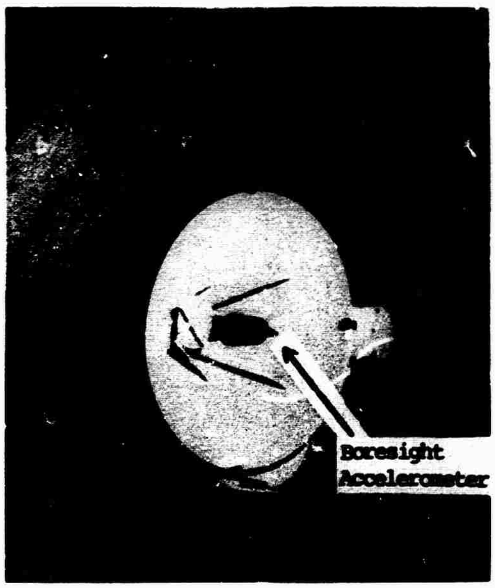

Figure 3. SHF Antenna Instrumented With Accelerometers.

Similarly, the new modem has maximum allowable values for the parameters of displacement, velocity, and jerk of the antenna. The risk of loss of signal tracking by the modem depended on the durations of continuous excessive Doppler effects, called exceedances. If the signal were lost, the chance of recovering it was also dependent on these durations.

FLIGHT TEST PROGRAM

To measure the effects of vibration on the SHF satellite communication, a flight test program was conducted. Figure 3 is a photograph of the antenna in the radome with the flight test accelerometers attached. Four crystal accelerometers were attached to the antenna edge to derive the angular acceleration in elevation and in azimuth. An accelerometer was positioned near the axis of the antenna to measure the antenna "boresight" acceleration. This is the acceleration in the direction of the satellite. Only acceleration in this direction causes Doppler effects on the signals. Figure 4 shows the INS instrumented with accelerometers to measure in the vertical, lateral, and longitudinal directions with respect to the aircraft. These measurements were used in combination with the antenna boresight acceleration to derive the uncorrected Doppler acceleration of the antenna.

Figure 4. INS and Orthogonal Accelerometers.

The antenna pedestal was instrumented with five accelerometers -- three orthogonal accelerometers to measure vertical, lateral, and longitudinal accelerations, and two other accelerometers to measure angular accelerations in pitch and yaw. The other terminal components -- the parametric amplifier, the power amplifier and modem, and the cooling pump -- were also instrumented with orthogonally mounted accelerometers. These sensors were used to measure the environmental vibrations of all the components of the communication terminal; these measurements have been reported separately in a limited-distribution report(4). The results are typical of previous measurements of cargo aircraft vibration environments(5,6). The communication system was also monitored to determine the signal/noise ratio, Doppler shift, and other parameters, in order to compare them to the vibration measurements.

The accelerometer signals were amplified and recorded by the compact instrument package shown in Figure 5. The package included automatic gain-changing amplifiers and an FM tape recorder. This instrument package weighed 115 pounds and was mounted on a table attached to the cargo deck just aft of the side cargo door.

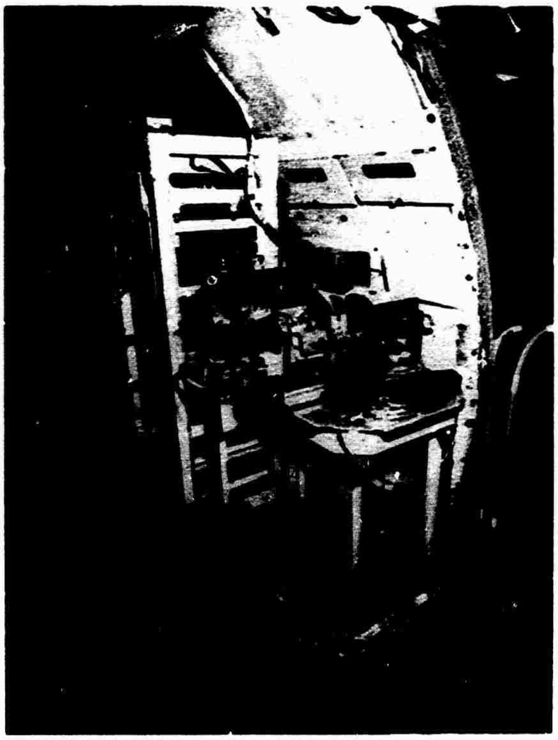

Figure 5. Data Recording Package Mounted in Aircraft.

In order to insure reliable communication, the terminal was designed to operate from before takeoff until after landing and during all phases of flight. This required consideration of the operating conditions taxi, takeoff, climb, cruise, 2g turn (60° bank), penetration approach (rapid descent with gear down and speed brakes extended), descent, and landing. When possible, the antenna was pointed at the satellite and the signal strength was measured. Otherwise, the antenna tracked a computer-generated signal and was pointed forward, laterally, or vertically. These flights were made from Wright-Patterson Air Force Base near Dayton, Ohio. In order to measure the antenna responses at high satellite elevations, one flight was performed under the satellite, which was above the equator over the Pacific Ocean.

DATA ANALYSIS AND RESULTS

Spectral analysis of the Doppler vibration test data was performed by the scheme shown in Figure 6. The analog signals representing the difference between the INS and antenna boresight accelerations were low-pass filtered and digitized to obtain data for the calculation of narrow-band acceleration spectra. These spectra were obtained from a digital computer and were plotted as rms accelerations in meters/second^2 versus frequency in 1.22 Hz bandwidths; they were then compared with the maximum allowable criteria under which the modem could operate. These spectra provided a quick-look at the overall problem to show possible adverse resonances.

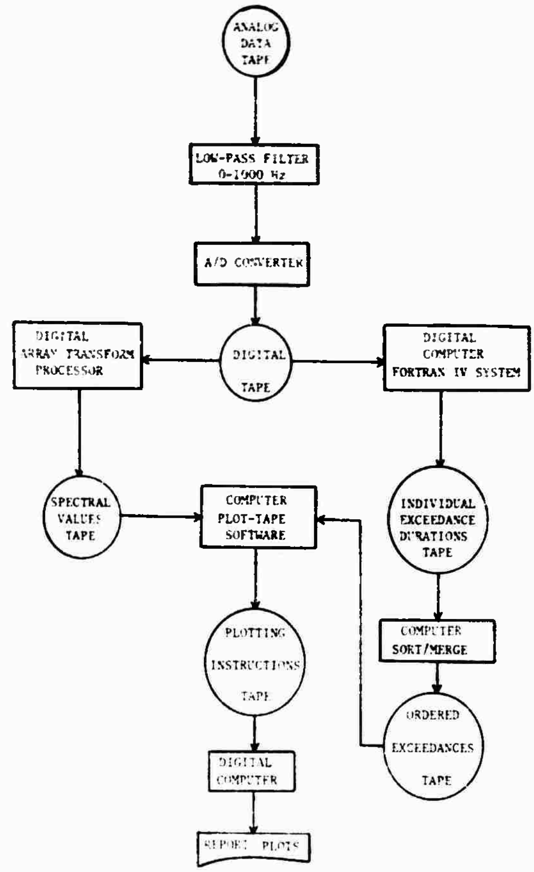

Figure 6. Flow Chart for Data Analysis.

Figure 7 shows the acceleration of the antenna in the boresight direction minus the INS lateral acceleration during cruise at 4000 feet altitude, with the antenna pointing laterally with respect to the aircraft. Superimposed on the acceleration spectra are four modem operational criteria for displacement, velocity, acceleration, and jerk. These criteria were provided by the modem contractor. The jerk, velocity, and displacement criteria were converted to equivalent acceleration by frequency transformations of $1/\omega$, ω, and ω^2 respectively. The figure shows that the displacement and jerk criteria determine the operational envelope. Vibrations at frequencies below about 3.5 Hz are limited by the

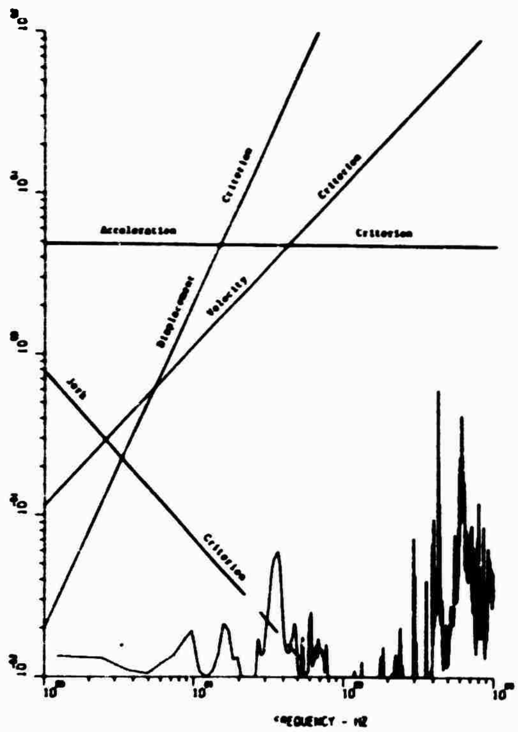

Figure 7. Antenna Boresight Minus INS Lateral Acceleration During Cruise (Antenna Pointing Laterally).

displacement criterion; conversely, vibrations at higher frequencies are limited by the jerk criterion. The jerk criterion was exceeded because of a resonance near 35 Hz, and the situation became progressively worse for higher frequencies. Fortunately, it was known from laboratory vibration tests of the antenna that the resonances at 35 Hz and higher are not rigid-body motions of the entire antenna, but are flexible-body modes. This means that there was no overall Doppler shift; instead, there were many small and opposing shifts. The result was a degradation of signal strength at the received frequency, but this was small compared to the rigid-body effects. For this reason, the jerk criterion is shown dashed at the higher frequencies.

Figure 8 shows the antenna boresight acceleration during a 2g turn. Due to the continuously changing attitude of the antenna during this flight condition, the INS acceleration was not subtracted from the antenna boresight acceleration. The overall response in this flight condition is similar to that during cruise, but the levels are higher for frequencies below 10 Hz.

Figure 9 shows the antenna boresight acceleration minus the INS lateral acceleration during rapid descent with gear down and speed brakes extended, with the antenna pointing laterally. Here the responses were much higher than those during cruise or the 2g turn, with the lowest resonances at 7.5 and 15 Hz showing up more strongly. These resonances caused the jerk criterion to be exceeded significantly. This was the most severe condition measured during the tests. Taxi, climb, and normal descent did not produce significant antenna vibratory responses, and the landing responses were of short duration. Displacement and jerk were again the limiting parameters at lower and higher frequencies respectively.

In order to quantify the severity of these responses, a time-history analysis of the digital data was performed, as shown on the right side of Figure 6. New computer programs were developed to determine the statistics of the numbers and durations of times during which each criterion would be exceeded during a particular flight condition.

Some typical results of this analysis for the acceleration criteria are shown in Figure 10 for conditions of cruise at 4000 feet altitude with the antenna pointing laterally. The acceleration used was the difference between the antenna and INS. The exceedances are shown in terms of the maximum length of time the

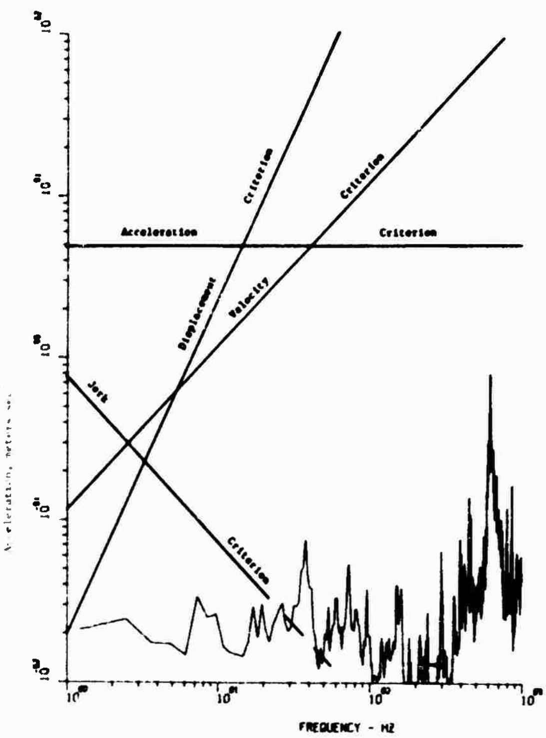

Figure 8. Antenna Boresight Acceleration During 2g Turn.

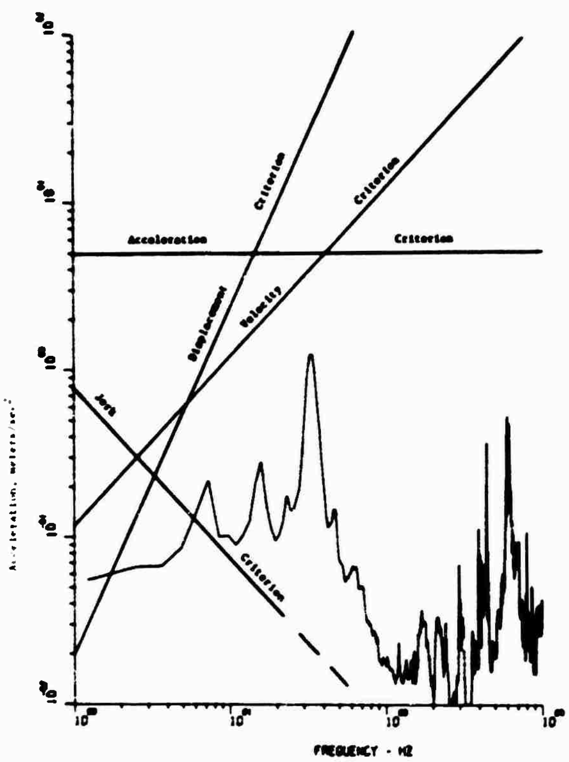

Figure 9. Antenna Boresight Minus INS Lateral Acceleration During Rapid Descent (Antenna Pointing Laterally).

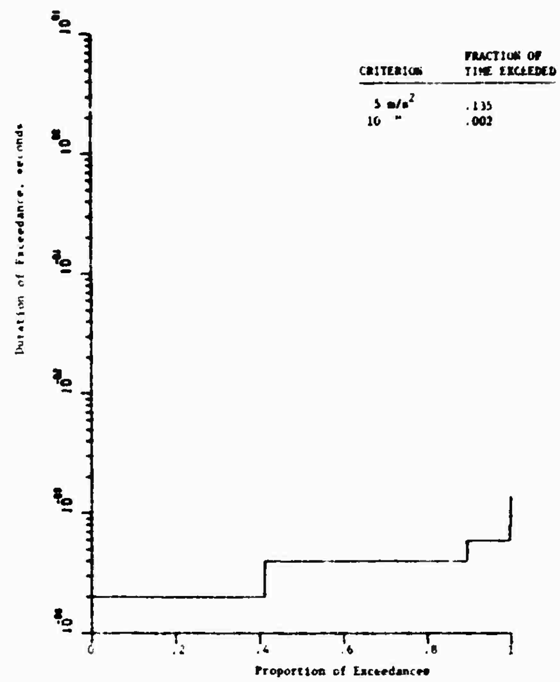

Figure 10. Exceedances of 5 m/s^2 Acceleration Criteria During Cruise (Antenna Pointing Laterally).

criterion was exceeded versus the proportion of total exceedances which were less than this value. The maximum time the 5 m/s^2 criterion was exceeded during cruise at 4000 feet altitude was 0.0014 second, and half the exceedances were less than 0.0004 second. The table shows the fraction of the total flight time during which the various criteria were exceeded. The 5 m/s^2 criterion was exceeded 13.5% of the time in this flight condition. Figure 11 shows the exceedances of the antenna acceleration during a 2g turn, with the antenna tracking the satellite. The maximum exceedance was 0.006 second, and the 5 m/s^2 criterion was exceeded 24.3% of the time. Figure 12 shows the corresponding exceedances of the antenna minus INS acceleration for the condition of rapid descent with gear down and speed brakes extended, with the antenna pointing laterally. The maximum exceedance was much greater for this flight condition, about 0.01 second. The 5 m/s^2 criterion was exceeded 35% of the time, indicating a serious degradation of communication. A complete analysis of all flight conditions is contained in a limited-distribution report(7).

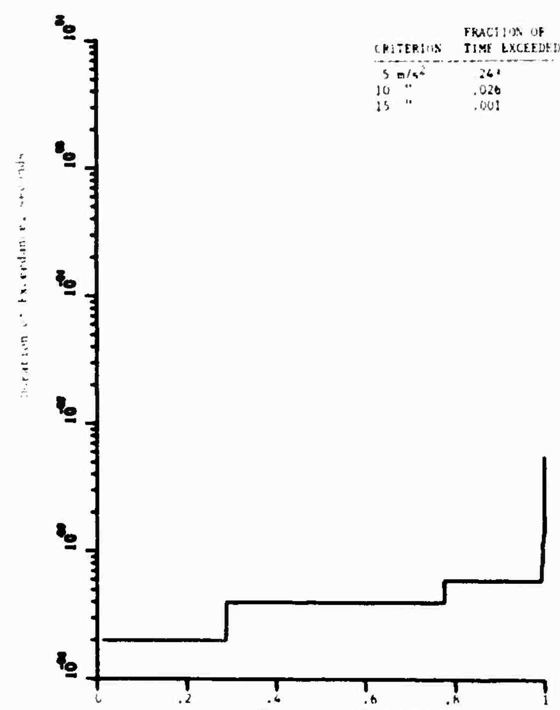

Figure 11. Exceedances of 5 m/s^2 Acceleration Criteria During 2g Turn.

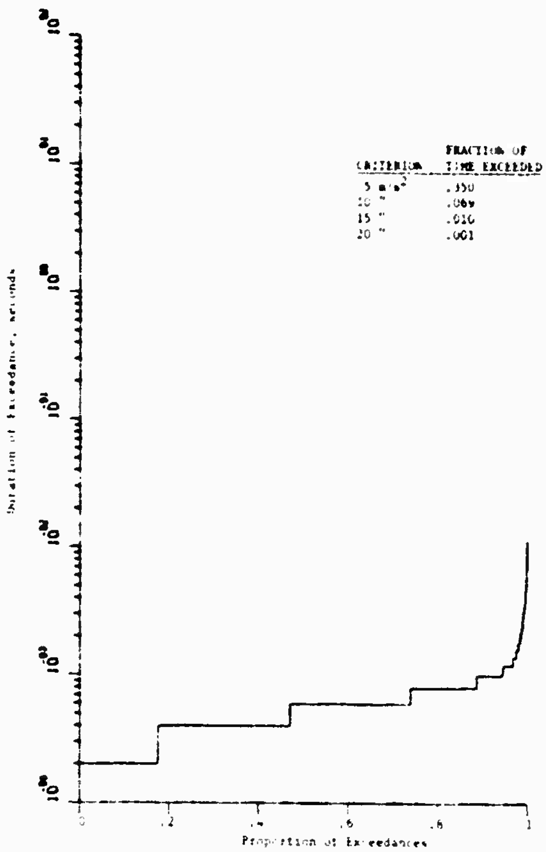

Figure 12. Exceedances of 5 m/s^2 Acceleration Criterion During Rapid Descent (Antenna Pointing Laterally).

CONCLUSIONS

This program has indicated the potential severity of the vibration-induced Doppler problem in communication by airborne terminals. The purpose of this program was to improve the tracking characteristics of a particular modem which was not originally designed for airborne use. The program results are being used to adjust the modem tracking loop characteristics to minimize the vibration-induced Doppler effects. Further actions which may be taken include various means of reducing the antenna vibration. The simultaneous measurement of all four motion parameters — displacement, velocity, acceleration, and jerk — was found to be a difficult instrumentation problem. The solution to this problem in the present program was to measure only the acceleration and to operate on the spectra by functions of the frequency to obtain the other parameters.

As indicated by the results of this program, the vibration-induced Doppler effects are substantial in the SHF range, near 8 GHz. They will be even more severe in the Ka band (36 GHz), for which terminals are already under development(8). These terminals should be designed from the beginning with vibration-induced Doppler effects in mind.

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FATIGUE DAMAGE EQUIVALENCE OF FIELD AND SIMULATED VIBRATIONAL ENVIRONMENTS

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(U) A method is developed for comparing the fatigue damage equivalence of field and laboratory simulated vibration environments. It involves the use of a model hardware specimen which is instrumented to record typical strain-time histories that occur in a given environment. The device is utilized to acquire data from both the field and corresponding laboratory simulation. The resulting strain-time histories are analyzed for fatigue damage potential. This analysis is based on the use of fatigue life gages. A mission ratio is defined for each pair of strain histories so that the degree of simulation achieved can be expressed in terms of the number of equivalent missions experienced. The technique is applied to OH-58A Helicopter, M-35 Truck, and M-113 Armored Personnel Carrier vehicle environments and their simulations. It is found that a typical uniaxial test simulation provides a rather poor duplication of the actual multi-axial field environments.

INTRODUCTION

The development of better laboratory test specifications is a continuing goal for the vibrations engineer. His objective must be to simulate a field environment in such a way that high probability of in-service failure in a typical specimen will result in a similar failure in the test environment. Depending on the nature of the specimen and its operational complexity, he may choose to derive a test specification based on some Military Standard as a guide, or may be required to derive an elaborate test scheme based on currently acquired field data which is representative of the intended environment. In either case, there are many factors which include characteristics of the dynamic environment, as well as the geometric and material design of the specimen itself, that can influence the actual degree of simulation that results. Unfortunately, a judgement on the degree of simulation achieved can be only subjective, since no practical means of direct measurement of vibrational equivalence is available.

It is generally accepted that standard specifications for vibrations tests often lead to severe overtests if they are applied indiscriminately. In such cases more accurate field data must be acquired, and revised specifications must be derived from these data. Refs. 1-3 report typical procedures that some test engineers have developed. Such tests assume that if the vibrational responses of a specimen are duplicated at several important specimen locations, then all failure mechanisms are faithfully excited at all points within the specimen. Most engineers recognize that this assumption is violated to some degree or another during all tests, but heretofore, no determination has been made of just how sensitive fatigue damage can be to this lack of proper simulation.

In view of the above comments, it is obvious that a technique for direct measurement of fatigue damage potential for two vibrational environments would be a very practical tool for evaluation of the validity of vibration specifications. Therefore, the purpose of this study has been to evaluate one particular technique which can be used for such a comparison. The general approach to the development of this technique is as follows:

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a. Develop an instrumented specimen capable of measuring characteristics of a dynamic environment.

b. Acquire field data with the instrumented specimen for typical helicopter, truck, and tracked vehicle environments.

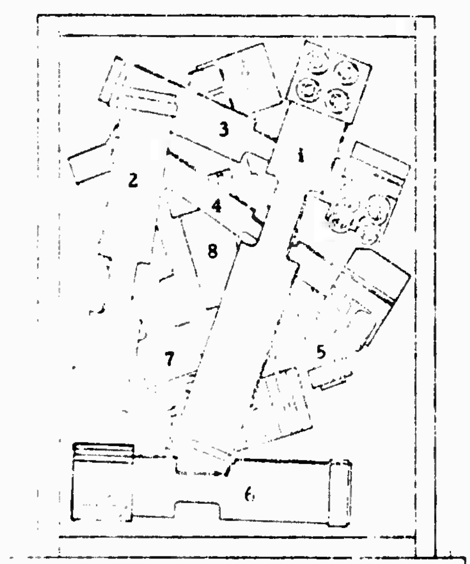
c. Acquire laboratory data with the instrumented specimen from tests that are designed to simulate the above field environments.

d. Develop a means of comparing the fatigue damage potential of the corresponding field and laboratory environments.

At the outset it was recognized that a number of possible paths could be followed to carry out the stated approach. In particular, the use of several different dynamic parameters and measuring devices for them could be considered. After studying the alternatives, we chose the approach described herein as one that is particularly straight-forward and one that can be implemented with a minimum of sophisticated support hardware. Further, the method of final fatigue prediction is based on devices which avoid many of the arguments that can be posed against the use of a particular fatigue damage accumulation theory. Only a brief summary of the work is presented in this paper; however, complete details can be obtained from Ref. 4.

CONCEPT OF MODEL HARDWARE SPECIMEN

It was recognized that the technique to be developed must be applicable to completely arbitrary items of hardware, although it was considered appropriate to focus attention on typical electronic items. Nevertheless, the basis for damage comparison is to be fatigue, which, of course, is a mechanical phenomenon that is dependent on the specific vibrational responses in a given item. Unfortunately, an arbitrary mechanical hardware item must be represented as a complex, multi-degree of freedom structure, in which many vibrational modes are present, and mode shapes may be oriented in virtually any direction in space. Likewise, various materials may be utilized in which a wide range of damping may be encountered. Therefore, a model hardware specimen (MHS) was designed to accommodate the above ideas as much as possible.



FREQUENCY (Hz)	BEAM NUMBER	DAMPING RATIO
10.6	1	0.0168
27.2	2	0.0136
36.9	3	0.0017
48.2	4	0.0027
75.9	5	0.0021
104.6	6	0.0016
132.4	7	0.0017
155.9	8	0.0025

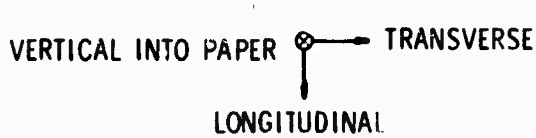


Figure 1. Diagram of Model Hardware Specimen

The MHS is designed geometrically to fit typical radio equipment mounting panels. Actually, it has the same physical size as an AN/ARC-115 Radio and can be mounted with dzus fasteners. Internally, the specimen is equipped with eight cantilever beams which represent typical modes of the internal components. A diagram of the device is shown in Figure 1, along with the natural frequencies and damping ratios for the fundamental modes of the various beams. These frequencies were selected from a set of random numbers to incorporate the concept of an arbitrary specimen. The frequency range was limited to 200 Hz, since this was the extent of important frequencies in the applications to be considered. Beams 1 and 2 were made of polyvinylchloride plastic while Beams 3-8 were made of aluminum. A total of eight beams could be readily incorporated into the volume of the box. The spacewise orientation of the beams was also selected at random.

Each beam is instrumented with strain gages at a necked-down section where the strain field can be considered uniformly distributed across the beam width. Thus, strain-time histories can be recorded on analog instrumentation tape during operation. Likewise, the box was instrumented with two triaxial accelerometers, one at each of the upper corners of the front face of the box, or in some environments near the support bracket. These accelerations are considered a measurement of the input to the MHS.

In view of the above description of the MHS, its use in acquiring either field or laboratory data should now be obvious. That is, it is subjected to a given vibrational environment while strain and acceleration time histories are recorded as representative parameters which describe the given environment.

Helicopter flight tests of the MHS were conducted at the U. S. Army Aviation Test Board at Ft. Rucker, Alabama. Instrumentation for these tests was provided by the Applied Mechanics Division of White Sands Missile Range, N. M. A photograph of the device mounted on the instrument panel of an OH-58A helicopter is shown in Figure 2. The MHS was mounted with six dzus fasteners in the same manner as typical avionics equipment. Location of the two triaxial accelerometers can be seen clearly in Figure 2. Acceleration and strain gage signals were amplified and recorded on analog tape. A voice signal was also put on the edge track of the tape for annotation by the flight crew.

Two flights were made with this equipment configuration, one without actual gunfire and the other with several bursts from a .30 Cal. mini-gun. In each case, however, a typical operational sequence was used which included engine start, take-off, hover, climbs, dives, etc. For these initial field tests, only Beams 3-8 were employed. Later, it was determined that two lower frequency beams could be incorporated into the box. Therefore, the helicopter data acquisition resulted in two analog tapes, one for nongunfire, and the other including gunfire maneuvers. Each tape contained multiple runs of various maneuvers, in the form of six strain-time histories, six acceleration histories, and a voice channel. Both tapes were later edited into continuous records, whereby all start and stop transients were eliminated.

Ground vehicle field data were acquired by transporting the MHS first in a M-35 truck and then in a M-113 Armored Personnel Carrier. These tests were conducted on the vehicle courses at Aberdeen Proving Ground. Typical installation locations of the MHS in each of these vehicles, as well as types of courses traversed, will be given in detail in the results section. The ground vehicle vibration data were recorded using a telemeter system. For these tests, all eight beams were incorporated into the specimen.

The MHS was mounted to the test vehicle in one of three ways; secure, loosely stowed, and cushioned. When mounted securely, the MHS was attached with six dzus fasteners to a heavy bracket which in turn was bolted rigidly to the vehicle structure. When loosely stowed, the MHS was resting freely on the vehicle structure and had a system of guide wires attached. These wires would permit the MHS to move freely in the vertical direction and yet provide restriction in both horizontal directions. The wires could also prevent overturning of the MHS. In the cushioned configuration, the MHS was resting on, and held down with thick soft foam rubber, so that there would be no direct mechanical coupling between the vehicle structure and the MHS. In all three configurations, a triaxial accelerometer was attached to the vehicle structure beside the MHS in order to record input accelerations and another triaxial accelerometer was attached to the rear of the MHS to record its accelerations.

Recording of the data was continuous, so that this time much of the analog tape editing problem was eliminated. Multiple runs at various speeds were conducted over the Munson Course. Thus, the results of these field tests

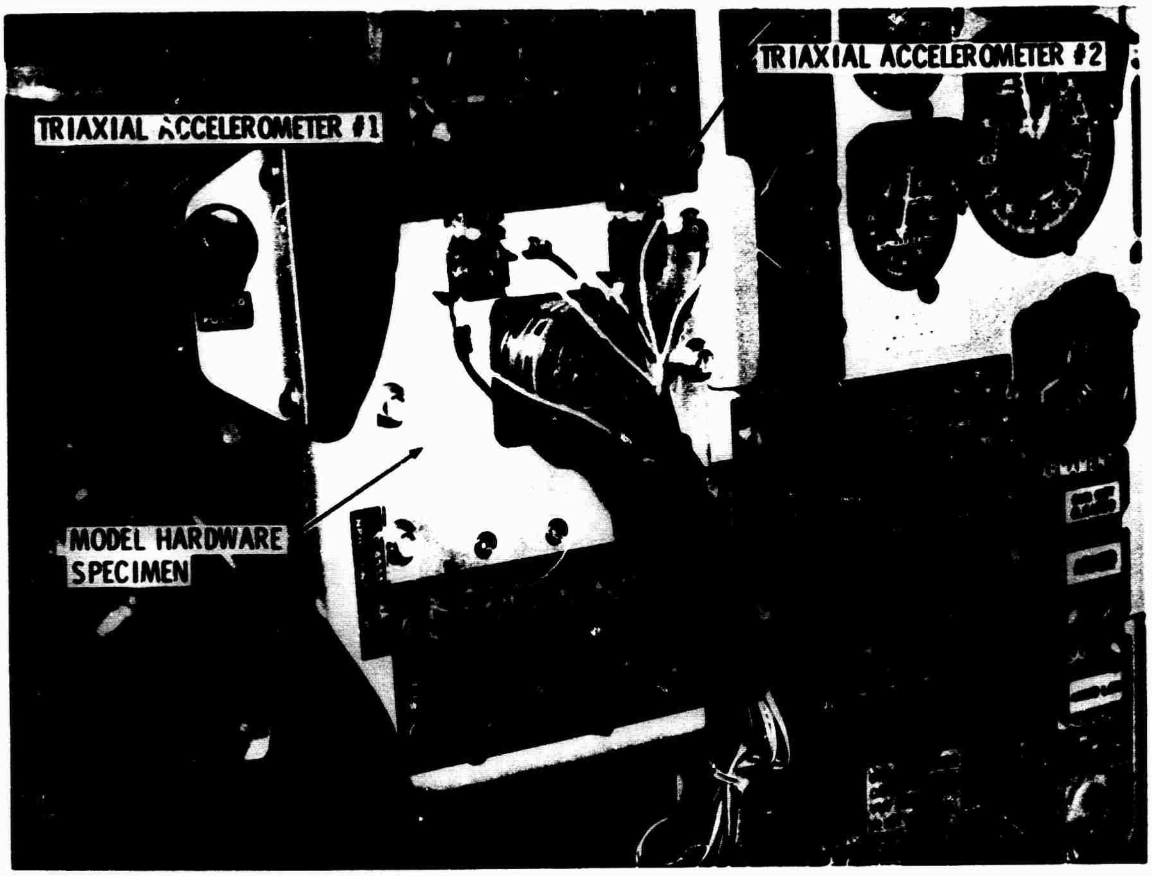


Figure 2. MHS Installed in Pilots Instrument Panel, OH-58A Helicopter

included several analog tapes, each of which included eight strain-time histories, six accelerations time histories, and a voice history.

We now will describe the data acquisition procedures associated with the laboratory test. However, it is first appropriate to comment on the specifications developed for it. Of course, the use of a typical laboratory simulation was desired. Therefore, a uniaxial test was selected in which the entire test would be conducted along a single axis that was considered to be the most severe in terms of its likelihood of producing large response. The vertical axis (see Fig. 1) was selected. Excitation along other axes was considered to be accounted for by cross-coupling in the system. Since acceleration time histories were available, the signal along the appropriate axis was used as the excitation, and was played directly into an equalized electrodynamic shaker system. This procedure was considered superior to the development of some presumably corresponding swept sine or random test.

In order to run the laboratory test it was first necessary to edit the field data into a more useable form. Parts of several runs along with their respective calibration signals were dubbed onto a single reel of tape using the carrier dub method. Only the acceleration channels were dubbed, so that the strain signals from the MHS lab test could later be recorded on the blank channels as the test was conducted.

A diagram of the equipment used in the lab test is shown in Figure 3. By removing certain record amplifier modules from the tape recorder, it was possible to play back and record simultaneously on a single tape recorder. The shaker drive signal was the vertical input acceleration signal from the triaxial accelerometer mounted to the vehicle structure. This signal passed through the equalizer filters and shaker power amplifier to the electrodynamic shaker. The MHS was mounted to the shaker in the same manner as it was mounted to the test vehicle in each respective configuration.

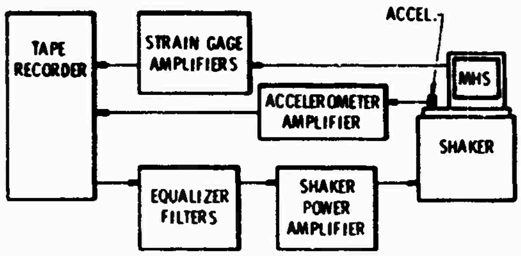


Figure 3. Uniaxial Lab Test Set Up

In view of the above description, it is apparent that the data acquisition program resulted in acceleration and strain-time histories for both a field and laboratory environment, for each of the three environments investigated. We now consider how these data must be analyzed to compare the fatigue damage potential of the respective field and laboratory environments.

FATIGUE DATA REDUCTION PROCEDURE

Various theories are available for prediction of fatigue damage accumulation when strain-time histories are available as in the present case. However, no one of these theories is directly suitable to all the different types of strain histories that resulted. Two typical types are shown in Figure 4. The upper trace shows a beam responding in basically its fundamental natural mode only, and the amplitude of the vibration fluctuates from time to time, depending on the particular maneuver or terrain being traversed. Thus, basically a sine wave of nonstationary amplitude results. The lower trace shows a more complex response, in which several frequency components are present in addition to that for the natural mode of the beam. Such complex strain histories are not so amenable for application of fatigue accumulation theories, and some alternate, more universal method was required. The use of Micro-Measurements fatigue life gages was found to satisfy our requirements very well.

Micro-Measurements, Inc. fatigue gages [5-7] can be used to produce an electrical resistance change which is a function of the number of strain reversals experienced at given strain levels, regardless of the complexity of the strain-time history. Calibration curves are available (Fig. 5) in terms of resistance change versus number of cycles for a family of sine wave strain histories, each at a constant amplitude. However, the latter curves can be

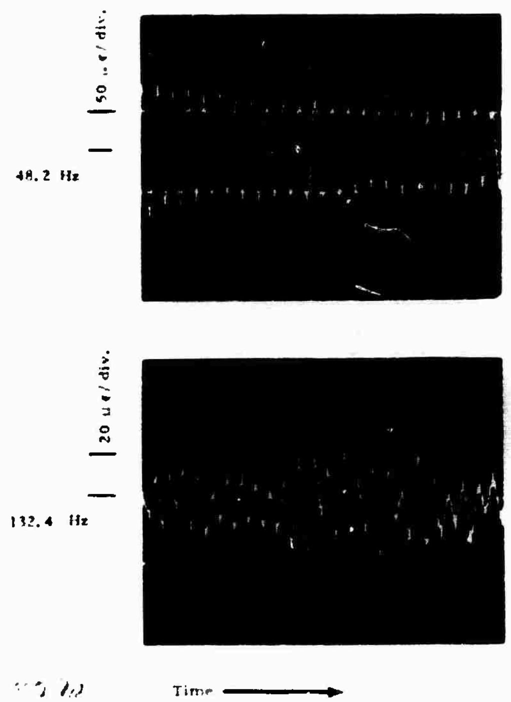


Figure 4. Typical Beam Responses

used graphically for a fatigue computation also if the strain histories are sufficiently narrow-band, such as the upper trace of Figure 4. Therefore, it was decided to predict fatigue for the simpler data through the use of a peak counting digital computer scheme, which was simple and fast, while that for the more complex data were predicted through the use of a more direct application of the fatigue gages. This is shown schematically in Figure 6. A preliminary scan allowed determination of which method was best for a given strain-time history.

The digital computer method required computerizing the fatigue gage calibration curve by means of an interpolation program. The input to this program then is the number of strain cycles at given levels. These data were computed by direct counting of the peaks in the strain-time histories. This was accomplished by the use of an amplitude modulation circuit, which provided the envelop of the time history, and then the use of a probability density analyzer. The output of this data reduction scheme was the desired count of peaks at incremental levels of 10 microstrain, which

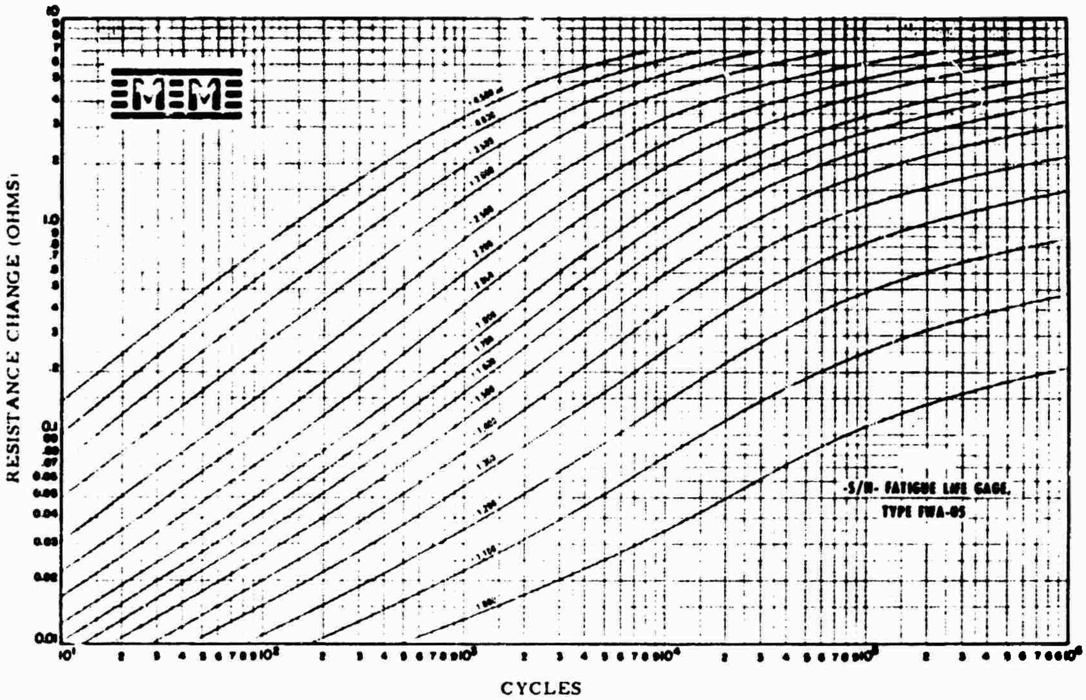


Figure 5. Sinewave Calibration Curves for Fatigue Life Gages

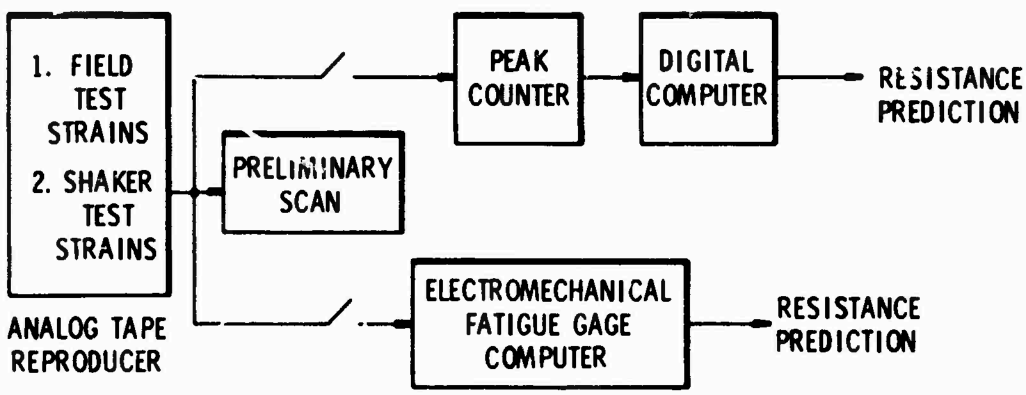


Figure 6. Procedure for Environment Duplication

then formed the input to the digital computer program. The program then summed the resistance accumulation for each time history.

The electromechanical computer method of fatigue prediction is shown in Figure 7. It consists of a device whereby the recorded strain-time history is very nearly duplicated in another aluminum beam specimen. The beam is instrumented with strain gages and a fatigue gage. The tape recorded strain history is used to drive an electrohydraulic shaker,

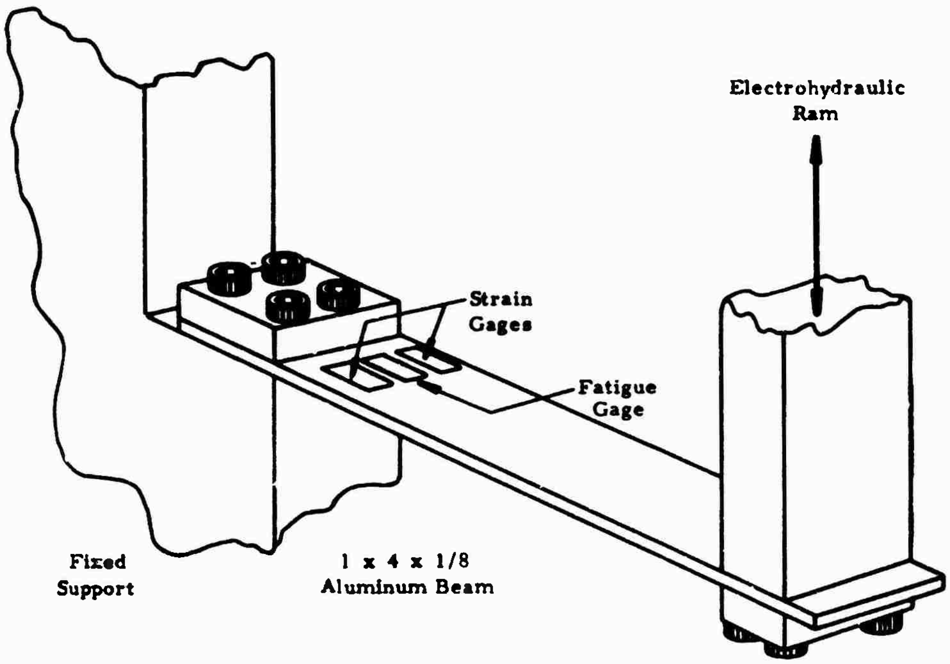


Figure 7. Diagram of Electromechanical Fatigue Gage Computer

which is controlled by automatic feedback of the strain signal from the beam. The control system was very accurate for virtually instantaneous control from 0 to 20 Hz. Thus, the tape recorded strain histories had to be reproduced at a speed factor reduction (and corresponding increase in test time) in order to utilize this method. It was time-consuming as a result. The fatigue gage resistance was measured accurately before and after a mission sequence, to produce the equivalent fatigue suffered for the given strain history.

It is appropriate to ask at this point why the fatigue gages were not employed directly on the beams in the model specimen during flight and the laboratory test, rather than contend with the more complex method described above. The reason was that the fatigue gages have a relatively narrow useful strain range, and there was no good assurance that strain levels would not exceed this range. By employing the methods described above, good signals were virtually certain, and the gain levels of these signals can be adjusted before application to the fatigue gages if necessary.

Finally, we are now in position to define a means of obtaining a numerical measure of the fatigue damage comparison between two different strain environments. This may be done by referring to the fatigue life gage calibration curve in Figure 5. For the moment, let us postulate that regardless of the complexity of a given strain-time history, an equivalent sine experience can be established for it. For example, suppose that for Beam No. 1, a resistance of 0.10-ohm resulted after the application of the field data strain-time history for one mission, regardless of the actual time length of that mission. From Figure 5, it can be seen that this is equivalent to a sine wave strain of, say, 1800 peak microstrain for 250 cycles. Now the laboratory strain-time history for the corresponding beam is applied to a fatigue gage also for one mission sequence. To continue the example, suppose that this resulted in 0.15 ohm resistance change. From Figure 5, it can be seen that this corresponds to 460 cycles at 1800 microstrain sine amplitude. We therefore can define a mission ratio as

$$MR = \frac{(\text{Lab Cycles at } 1800\mu\epsilon)}{(\text{Field Cycles at } 1800\mu\epsilon)} = \frac{460}{250} = 1.84$$

Thus, for this example, that beam would have experienced an overtest in the lab by the above factor. Obviously, $MR = 1.0$ is a perfect correspondence and $MR < 1$ is an undertest. This factor must be determined for each beam in the box in order to determine an overall indication of the similarity of the two environments.

The above scheme for development of a mission ratio requires the establishment of some equivalent sine-wave experience for a given complex time history. Results were obtained for all three environments investigated which show that this condition can be satisfied in each case.

OH-58A HELICOPTER ENVIRONMENT

Results will now be presented for data taken in the MHS for both the field and the laboratory environments. Recall that for this particular test series, only beams of 36.9 Hz and higher were incorporated into the specimen.

The mission sequence that resulted after editing the original data is shown in Table I for the nongunfire run. It can be seen that a 33.3-minute mission resulted. The various maneuvers are self-explanatory. Given test numbers within the sequence refer to an original flight plan which was used by the pilot. These test numbers were retained since they are incorporated into the voice track on the analog tape. A similar mission sequence was used for the gunfire mission. However, the sequence was much shorter (9.1 minutes), and included a number of gunbursts at 4000 rounds per minute.

TABLE I

NONGUNFIRE MISSION SEQUENCE FOR OH-58A HELICOPTER

Min.	Event
0	Start level flight
0.3	Level off, continue level flight
2.7	Test 9. Left turn 90° and continue level flight
5.1	Test 11. Right turn 90°
5.2	Now turning
5.3	Continue turning
5.4	Now level off
7.0	Test 13, acceleration to minimum cruising speed
8.1	Still going Test 13
8.2	Cruising at minimum speed
9.4	Still at Test 13, cruising at minimum speed
10.7	Test 14, descent at 500 ft/min for 1 minute
11.2	Now descending
12.2	Test 18, acceleration to maximum permissible speed
12.3	Now accelerating
13.9	We are now going at maximum speed, close to maximum speed
15.0	Start of gun run
15.6	Now climbing
16.0	Now leveling off
16.9	Leveled off
19.1	Beginning second gun run, turn left
19.4	Diving
19.6	Still diving
19.7	Turning left
19.8	Still turning left
20.7	Climbing
20.8	Still climbing
22.7	At maximum speed
23.2	Maximum cruising speed
25.2	Test 32, decelerate to normal cruise
25.7	Test 36, climb at 500 FPM
26.6	Level flight
29.3	Test 37, level flight
29.5	Descending
31.1	Hovering
32.5	Test 43, hover
33.3	End data.

Average strain level variations with time are given in Figure 8 for the nongunfire mission. These results are based on an analysis of field data only. The times that appear correspond to those given in Table I. Similar data were obtained for the gunfire mission. It is obvious that only certain beams respond at certain times, which indicates the frequency contend of the input motion. Also, the strain levels can be seen to vary considerably with time. This strong time dependence was a major factor in selecting a laboratory test that included the use of the field accelerations as the excitation signals for the test system.

Some results for fatigue gage resistance change for the gunfire and nongunfire missions are shown in Figure 9. These results were obtained with the electromechanical computer. For the several beams identified, resistance changes were noted after the application of 1, 2, and then 3 missions for a given beam. Amplification factors for the strain signals were adjusted according to the values given on the figures. Again, this adjustment assured that a reasonably measurable resistance change would occur for the given mission time. For each set of three values for a given beam, the points were shifted along the horizontal axis of the figure, until they matched up with a given sine-wave calibration curve. It can be seen that all plotted data conveniently fit onto the 1800 $\mu\epsilon$ curve. As pointed out in an earlier section, this result allows establishment of

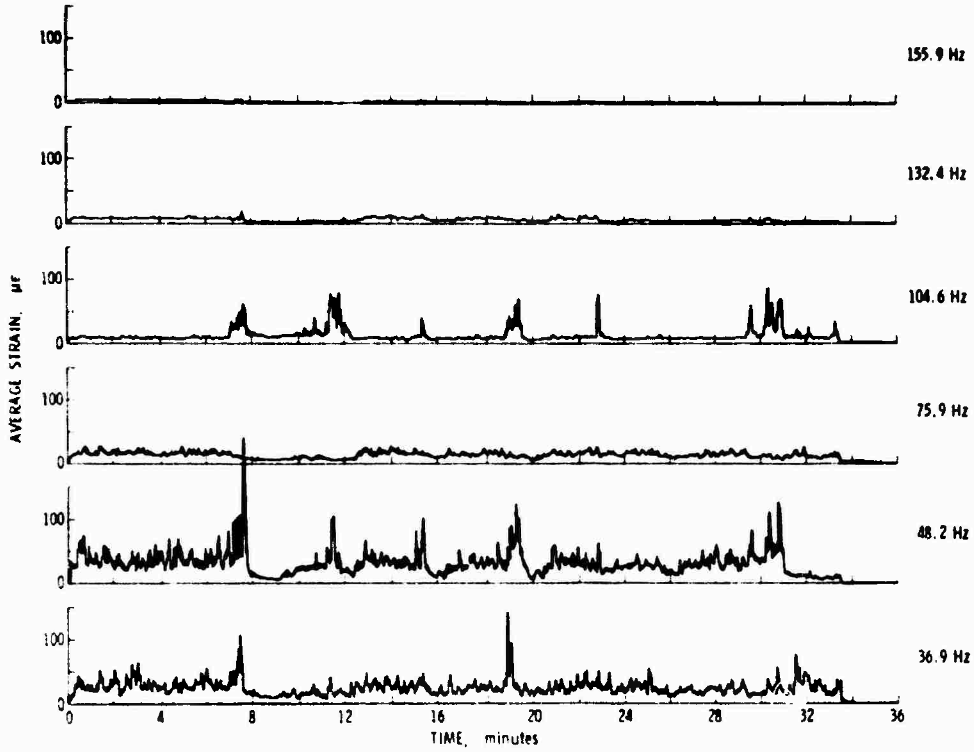


Figure 8. Strain Level Variation with Time for Nongunfire Mission in OH-58A Helicopter - Field Data

the 1800 με level as an equivalent fatigue experience for the corresponding number of cycles for each point. Thus, use of the previously-defined mission ratio is possible for this case.

Finally, a list of all results for one mission is given in Table II for both nongunfire and gunfire, and for both the field and laboratory data. Part A gives the maximum strain that occurred at any time in the field data (with no amplification). Further, it gives the amplification factor that was used in analyzing the data for fatigue potential (i.e., determining electrical resistance change in a fatigue life gage). Part B gives values of resistance change that occurred for the indicated beams after application of one mission. In general, corresponding values for field and lab data should be fairly near to each other. All values not identified by an asterisk were obtained by the digital computer method. In two cases, resistance changes were computed by both the digital computer method and the electromechanical computer method as a check. The values are very nearly the same for these cases. Finally, mission ratios were computed where possible and are given in Part C. The values can generally be seen to be both higher and lower than unity, which indicates that not too good a simulation was achieved. In fact, since both low and high values result, it appears that a good simulation is, in fact, impossible for the uniaxial form of laboratory test that was employed. It should be noted that results are not presented for all beams in Table II, since some beams experienced only very little response.

M-35 TRUCK AND M-113 ARMORED PERSONNEL CARRIER ENVIRONMENTS

Conditions under which field data were acquired for the M-35 truck are presented in Table III. The conditions were chosen as typical for hardware that must be transported in

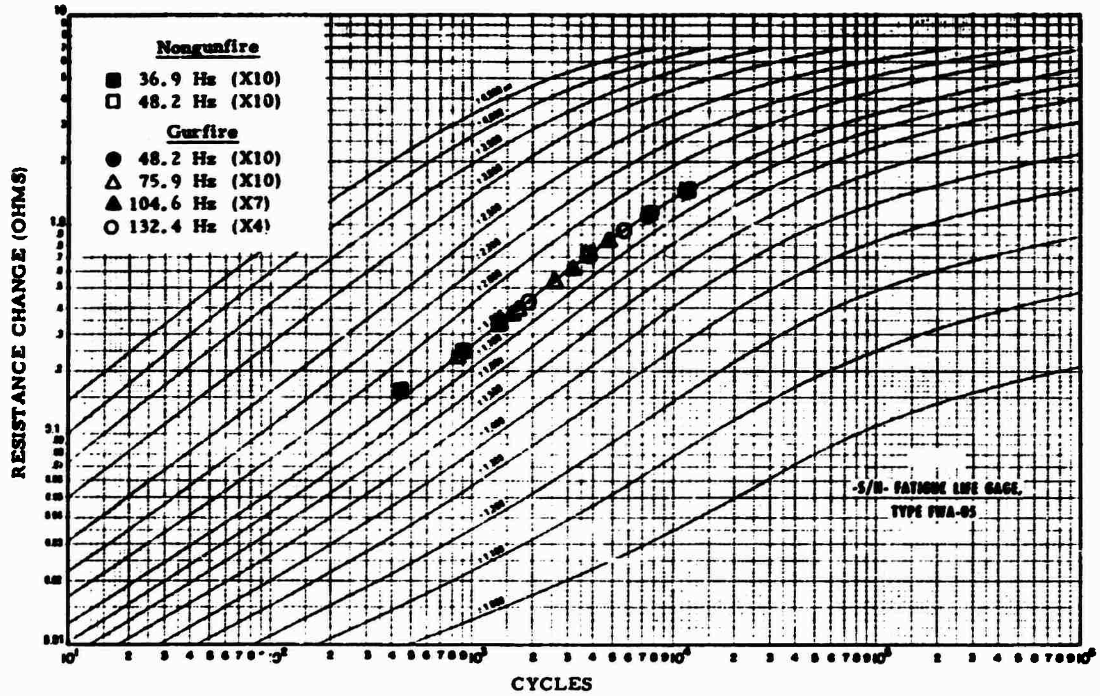


Figure 9. Multi-Mission Results for OH-58A Helicopter

this vehicle. In each case a total of five laps were made around the Munson Course. However, in order to save analysis time, a mission was defined as two laps which provided a total time of about 13 minutes. This can be seen from Figure 10, in which are presented typical average strain levels for a given run condition. These data are useful for showing the nonstationarity of the strain responses as different terrains are traversed. Multi-mission data were also acquired to again establish the validity of employing the 1800 $\mu\epsilon$ sine curve as a basis for mission ratio prediction, similar to the data presented in Figure 9 for the helicopter.

For this environment, a further check was made on the fidelity of duplication of the spectral characteristics of the field environment. That is, Figure 11 shows samples of acceleration power spectral densities for a corresponding maneuver for both the field and laboratory tests. The PSD's are essentially identical for the field and lab accelerations along the vertical axis, which verifies that the environment was properly duplicated along that axis. Of course, one must recall that a uniaxial test was employed so that duplication along the longitudinal and transverse axis was not achieved. Similar PSD results were obtained for a variety of the data conditions and found to be similarly valid.

Final results for field and laboratory tests are given in Table IV. It is obvious from the mission ratio results that a wide disparity exists between the simulated and field environment. Thus, a poor simulation has been achieved. At this point it is pertinent to emphasize that although such results may be rather surprising, they do not detract from the successful completion of the program objective, that is to develop a method for making such measurements. They do, however, indicate that fatigue is a failure mechanism that is very sensitive to errors in simulation, and what at the start seemed to be a good laboratory vibration simulation technique, has been shown to be quite poor by the method that was developed.

Field conditions for data acquisition in the M-113 armored personnel carrier environment are given in Table V. Typical average strains and PSD's for field and laboratory data will not be presented for the sake of brevity.

TABLE II

RESULTS FOR MODEL HARDWARE SPECIMEN IN HELICOPTER VIBRATION ENVIRONMENT

Run Identification	Frequency (Hz)					
	36.9	48.2	75.9	104.6	132.4	155.9
A. Peak Microstrain and Amplification Factor for Analysis						
Nongunfire (Field)	320 X10	390 X10	250	300 X10	240	210
Gunfire (Field)	140 X12	375 X10	300 X10	450 X7	880 X4	266 X10
B. Resistance Value for One Mission (ohms)						
Nongunfire (Field)		0.76				
Nongunfire (Lab)	0.53	Low				
Nongunfire (Field)*	0.16	0.68		0.39		
Nongunfire (Lab)*	0.47	Low		0.58		
Gunfire (Field)*	0.04	0.73	0.24	0.37	0.43	0.19
Gunfire (Lab)*	0.51	0.28	0.15	0.30	0.20	0.63
C. Mission Ratio						
Nongunfire	3.6	Low		1.7		
Gunfire	36	0.29	0.52	0.76	0.36	4.9

* Data obtained using electromechanical computer.

Final data for this environment is presented in Table VI. Careful scrutiny of these data lead one to the same comments made about that for the M-35 truck environment.

CONCLUSIONS

This study demonstrates the utility of a model hardware specimen for comparing the equivalence of two vibrational environments. It is recognized that strictly-speaking, the use of such a model device is not actually necessary, but an actual item such as a radio could have been strain-gaged at appropriate points and subjected to the same procedure. However, the parameters (natural frequencies and beam orientations) for the model were chosen at random, so that it represents a completely arbitrary item. Furthermore, because of the simple nature of the beam design, measurement of maximum strains were assured.

The results for equivalence comparisons between field and a typical laboratory simulation may be rather surprising to some. For all three environments investigated, rather poor simulations appear to have been achieved. Obviously, the fatigue mechanism is extremely sensitive to differences in the total excitational conditions. Differences which result from a uniaxial simulation are far too great to allow a more reasonable duplication of the field environment. It is apparent that cross-coupling in the structure has a marked influence on the final fatigue results. Therefore, basically one must conclude that a multidimensional excitation cannot readily be duplicated in a complex structure by means of only a uniaxial excitation. This conclusion is particularly supported by the fact that some beams experience a gross undertest while others experience a gross overtest.

TABLE III

DATA ACQUISITION CONDITIONS FOR M-35 TRUCK

	Road Type
Munson Area	- Courses used for M-35 truck test were spaced bump, radial washboard, two-inch washboard and belgian block, as well as smooth paved and improved gravel between courses.
	MHS Mounting Conditions
Secure	- MHS attached to frame with six Dzus fasteners and frame rigidly bolted to vehicle.
Loose	- MHS free to move vertically but restrained horizontally.
	Run Number Identification
1.	M-35 truck with 1000-lb payload, Munson area, MHS secured in forward cargo bed area.
2.	M-35 truck with 1000-lb payload, Munson area, MHS secured in aft cargo bed area.
2a.	Repeat of Run No. 2.
3.	Repeat of Run No. 1.
4.	M-35 truck with 1000-lb payload, Munson area, MHS loose in forward cargo bed area.
5.	M-35 truck with 1000-lb payload, Munson area, MHS loose in aft cargo bed area. This run not completed, judged too severe for MHS by test personnel.

In view of the above conclusion it is further apparent that the whole concept of laboratory simulation may now require closer scrutiny to place confidence limits on the results of a given test. That is, the implication of inherently wide confidence levels associated with laboratory simulations is apparent, until further demonstration of the equivalence of given tests is assured. The final result from this study may be to indicate that the heretofore use of subjective judgements in asserting vibrational equivalence requires a new very careful investigation.

ACKNOWLEDGEMENTS

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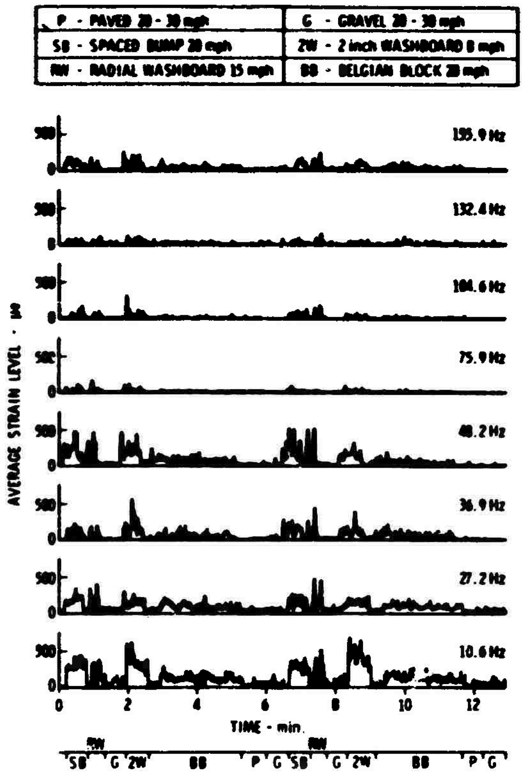

Figure 10. Average Strain Level Variation with Time Run No. 1 MHS Secured in Forward Cargo Area M-35 Truck - Field Data

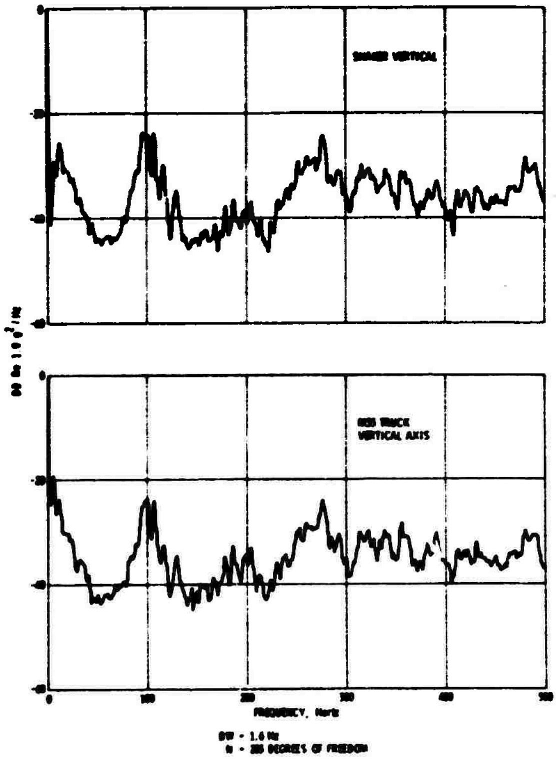

Figure 11. PSD of M-35 Truck on Belgian Block

TABLE IV

RESULTS FOR M-35 TRUCK

Run No.	10.6	27.2	36.9	48.2	75.9	104.6	132.4	155.9
A. Feak Microstrain and Amplification Factor for Analysis								
1 (Field)	1290	850	1050	930	905	565	325	545
1 (Lab)	1050	930	525	465	265	1210	145	
	X3	X4	X4	X4	X5	X3	X10	X5
2a (Field)	1210	1090	1530	1090	385	425	445	545
2a (Lab	850	465	605	465	265	345	185	
	X3	X4	X2	X4	X10	X10	X10	X5
3 (Field)	1130	850	1130	810	365	385	305	425
	X3	X4	X4	X4	X5	X3	X10	X5
4 (Field)	1530	3980	1770	1170	825	1490	1690	1610
4 (Lab)	1050	1250	730	505	865	845	725	
	X3	X1	X2	X3	X5	X3	X1.34	X1.21

TABLE IV (Contd.)

Run No.	Frequency (Hz)							
	10.6	27.2	36.9	48.2	75.9	104.6	132.4	155.9
B. Resistance Values for One Mission (ohms)								
1 (Field)	0.214	0.113	0.279	0.462	0.113	0.010	0.427	0.127
1 (Lab)	0.085	0.180	0.069	0.041	0.010	0.319	0.010	
1* (Field)	0.213	0.161	0.426					
2a (Field)	0.139	0.320	0.177	0.531	0.703	0.492	1.594	0.226
2a (Lab	0.036	0.014	0.010	0.014	0.010	0.197	0.031	
2a* (Field)	0.150							
2a* (Lab)	0.049							
3 (Field)	0.144	0.126	0.510	0.351	0.026	0.010	0.422	0.097
4 (Field)	0.413	0.080	0.308	0.379	0.928	0.872	0.056	0.032
4 (Lab)	0.081	0.010	0.010	0.010	0.570	0.150	0.046	
4* (Field)	0.504	0.072	0.283	0.350	0.857	0.803	0.040	0.016
4* (Lab)	0.105		0.012					
C. Mission Ratio								
1	0.272	1.953	0.143	0.033	0.023	178.6	0.004	
2a	0.136	0.009	0.012	0.005	0.002	0.298	0.003	
2a*	0.199							
4	0.107	0.037	0.006	0.004	0.500	0.089	0.740	
4	0.120		0.009					

Data obtained using electromechanical computer.

TABLE V
DATA ACQUISITION CONDITIONS FOR M-113 ARMORED PERSONNEL CARRIER

Road Types	
Paved	- Three mile straightaway with banked turnaround loops at each end.
Cross Country No. 3	- Rough course of native soil.
Cross Country Course A	- Hilly cross country course.
MHS Mounting Conditions	
Secure	- MHS attached to frame with six Dzus fasteners and frame rigidly bolted to vehicle.
Cushion	- MHS resting on thick foam.
Run Number Identification	
6.	M113 tracked vehicle, paved road, 4 to 36 mph in 2 mph increments, MHS secured aft end of right sponson.
7.	Repeat of Run No. 6.
8.	M113 tracked vehicle, cross country course No. 3, normal speeds, MHS secured aft end of right sponson.
9.	M113 tracked vehicle, paved road 4-36 mph in 2 mph increments, MHS cushioned aft end of right sponson.
10.	M113 tracked vehicle, paved road, acceleration to max. speed, MHS secured aft end of right sponson.
11.	M113 tracked vehicle, cross country course A, normal speeds, MHS secured aft end of right sponson.

TABLE VI

RESULTS FOR M-113 ARMORED PERSONNEL CARRIER

Run No.	Frequency (Hz)							
	10.6	27.2	36.9	48.2	75.9	104.6	132.4	115.9
A. Peak Microstrain and Amplification Factor for Analysis								
6 (Field)	225	705	1330	1450	1850	3700	2660	
6 (Lab)	165	725	1250	1530	1410	2100	1050	
	X10	X5	X3	X2	X2	X1	X1	
8 (Field)	650	805	930	425	625			
8 (Lab)	610	865	1130	665	405			
	X5	X5	X3	X5	X5			
9 (Field)	1290	585	505	245	Low	Low	245	
	X3	X7	X7	X10			X10	
11 (Field)	370	410	1010	1170	1570	1490		
	X10	X10	X4	X3	X2	X3		
B. Resistance Values for One Mission (ohms)								
6 (Field)	0.068	0.14	0.559	0.297	3.470	0.800	0.205	
6 (Lab)	0.035	0.11	0.516	0.653	0.734	0.288	Low	
8 (Field)	0.039	0.471	0.099	0.025	0.702			
8 (Lab)	0.211	0.714	0.296	0.568	0.069			
9 (Field)	0.135	0.126	0.119	0.074			0.042	
11 (Field)	0.067	0.427	0.366	0.785	1.168	1.696		
C. Mission Ratio								
6	0.361	0.715	0.888	2.845	0.036	0.252	Low	
8	11.61	1.768	4.582	88.71	0.042			

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AN EVALUATION OF SHOCK RESPONSE TECHNIQUES FOR A SHIPBOARD GAS TURBINE

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The response of a large gas turbine engine as tested on the Navy Floating Shock Platform was calculated by the transient response and shock spectrum methods. Four methods of combining the modal contributions were used for comparison with the transient response method. Both flexible and rigid rotors were modeled in order to evaluate the required degree of detail in modeling. Accelerations, deflections, bearing loads and mount loads were obtained.

INTRODUCTION

Equipment intended for application on naval ships must often demonstrate combat durability. Heavy weight equipment are tested on the Navy Floating Shock Platform (FSP) by mounting the test equipment on the specially built barge, instrumenting and shocking with underwater explosions [1]. As relatively light weight power sources, large gas turbines can be particularly sensitive to shock if improperly designed. Primary potential problem areas associated with shock on a gas turbine are; the engine structure, engine mounts, accessory mounts, bearings and severe rotor tip rubs. A detailed shock analysis during the engine development program can indicate problem areas and through modifications minimize the required post test redesign. Analytical tools that are currently available allow complex engine systems to be modeled with hundreds of degrees of freedom for study by transient response or shock spectrum methods. The former, more involved analysis, produces responses versus time whereas the latter simpler method provides only approximations to the largest responses. With large analytical models, the detail required to produce desired results becomes an important factor. The study herein presented was made to compare the results of the transient response method with those of the shock spectrum method as applied to a shipboard gas turbine, to give some indication of the factors dictating if rotors can be modeled as rigid bodies and to provide an example of shock analysis on a large shipboard gas turbine.

CONCLUSIONS

Transient response and shock spectrum analyses both have a place in shock studies. The transient analysis allows piecewise linear solutions and provides additional appreciation of the response by yielding the entire time response rather than an approximation to the maximum values. However, the simplicity of the shock spectrum method is very attractive. Based on the system studied here, the recommended shock spectrum response method of combining modal contributions is the second modified root summation square approach, whereby, the two largest modal contributions are added to the square root of the sum of the squares of all other modal contributions. Although this method is not as accurate as the first modified summation method, it is generally conservative and has a small and more constant discrepancy relative to the transient solution. The first modified summation method was found to be unconservative. When a conservative design approach is justified, a response obtained from direct summation of modal contributions can be applied in design with complete confidence that the transient response for the same model will be less.

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A square pulse of 0.005 sec duration and of 65.5 g's amplitude is recommended for simulation of the FSP design shock spectrum when a transient response method of analysis is preformed.

The precise degree of detail required in a model is difficult to establish. If the lower natural frequencies of the system contain bending in a rotor, and if the rotor is represented as a rigid body, then significant errors in quantities directly related to the rotor can result. It is recommended that all expected modes of vibration that contain significant amounts of energy be allowed to occur by the model.

DESCRIPTION OF THE SYSTEM ANALYZED

Shown in Figure 1 is a schematic of the engine system model used in this study. The power turbine and gas generator rotors which are hidden in Figure 1 are shown in Figure 2. The mount points of the engine case to the substructure are indicated in both figures. The 404 lb power turbine, 470 lb gas generator and 3785 lb case are coaxial and interconnected by linear radial springs representing the bearing stiffnesses. Thrust bearings at the compressor end of the gas generator and at the turbine end of the power turbine are modeled by axial linear springs. Mass and inertia properties are lumped at discrete points and interconnected by flexible beams. As shown in Figure 3, the engine is mounted to the rigid 15,000 lb substructure at the proper locations to provide a statically determinant system. The forward bottom mount is pinned taking vertical and transverse loads. The rear bottom mount is pinned taking fore-and-aft and transverse loads. The vertical side mount beams are pinned at both ends taking only vertical loads. The rear mount side flex bars are rigidly attached to the case at the forward end and pinned at the rear connection taking vertical and transverse loads. The substructure is centrally mounted to the FSP by four beams to simulate a deck mounting. The deck beams are connected to the FSP with a pin such that only transverse moments are released. The beams are choosen to provide a fundamental natural frequency of 29.5 cps. The total length of this system is 18 ft. 4 in. Two models are considered, one with flexible rotors and case resulting in 98 degrees of freedom and another simpler model with rigid rotors and case and effectively 22 degrees of freedom. Both models are three dimensional and in general possess 6 degrees of freedom at each node.

The system excitation for the transient response analysis is provided by a square pulse in the vertical direction whose corresponding shock spectrum is very similar to the vertical FSP design shock spectrum [1] The excitation for the shock spectrum analysis was provided by the shock spectrum for this square pulse rather than the design shock spectrum of the FSP. A comparison of the vertical FSP design shock spectrum and that of the square pulse is shown in Figure 4. The square pulse amplitude was 65.5 g's for a duration of 0.005 sec.

ANALYSIS

Eigenvalues and eigenvectors were obtained for both the rigid rotor model and the flexible rotor model with a finite element beam program. Gyroscopic stiffening was not included in the analysis. A transient response analysis was performed on both models with a program using the method outlined in the Appendix. System accelerations, and deflections, bearing loads, mount loads and relative deflections at potential tip rub locations were obtained. The flexible rotor model was also analyzed on a shock spectrum program based on the analysis presented in the Appendix. The above mentioned quantities were obtained by the shock spectrum method using four methods of summation of the modal contributions: (1) the direct summation of modal contributions, (2) the root summation square approach, (3) the first modified root summation square approach, and (4) the second modified root summation square approach. The analytical form of these methods are presented in the Appendix. The root summation square method is considered in [2], and the first modified root summation square method is recommended for specific applications in [3], [4] and [5].

PRESENTATION OF RESULTS

Shown in Table I are various flexible rotor accelerations, deflections and loads as calculated by the transient response method and by the various modal summation methods to the shock spectrum method.

Table II contains a comparison of the same quantities in Table I for the rigid rotor and flexible rotor models resulting from the transient response analysis. Figure 5 shows a comparison of the time response of the acceleration at the engine c.g. for the two models and Figure 6 shows a comparison

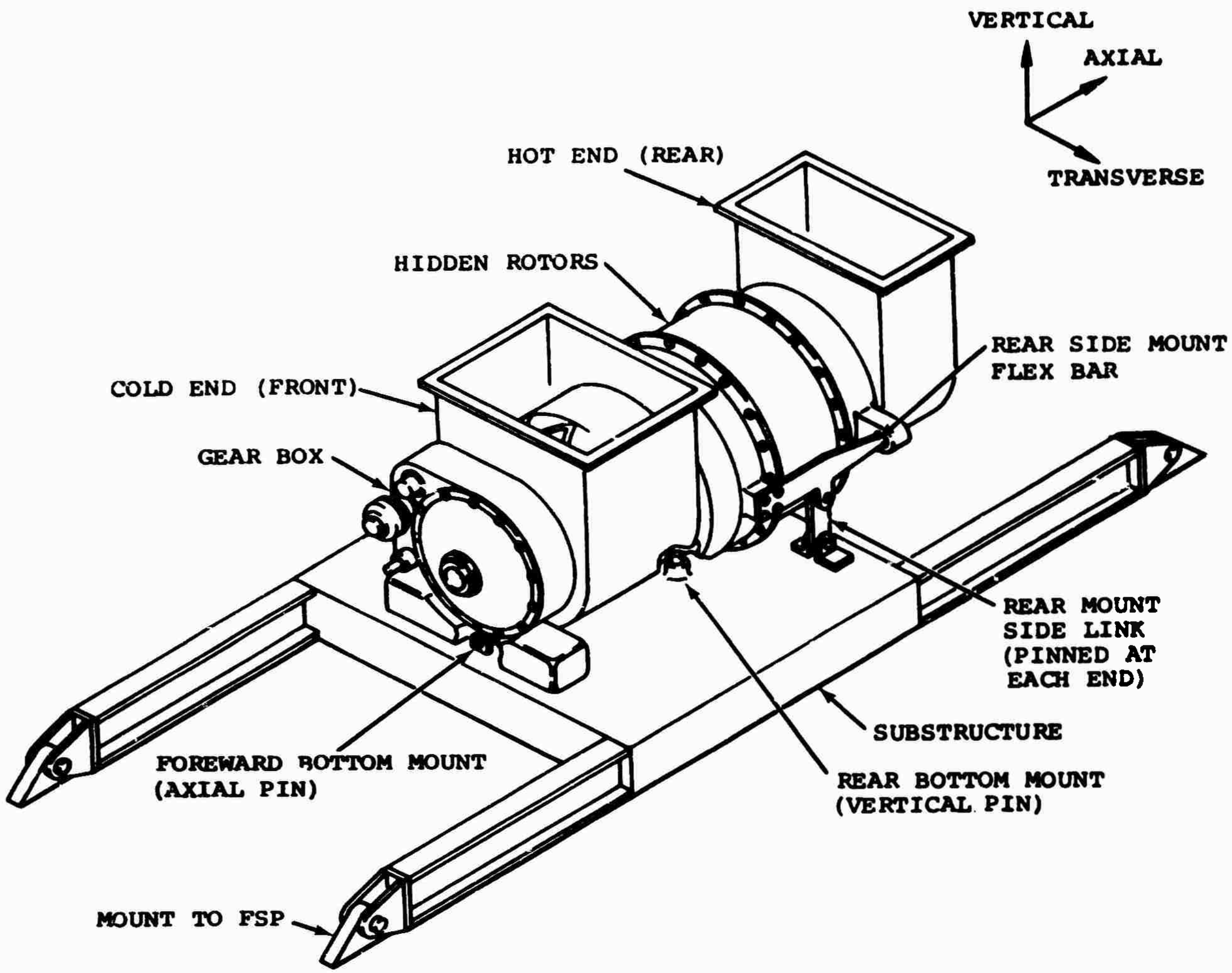
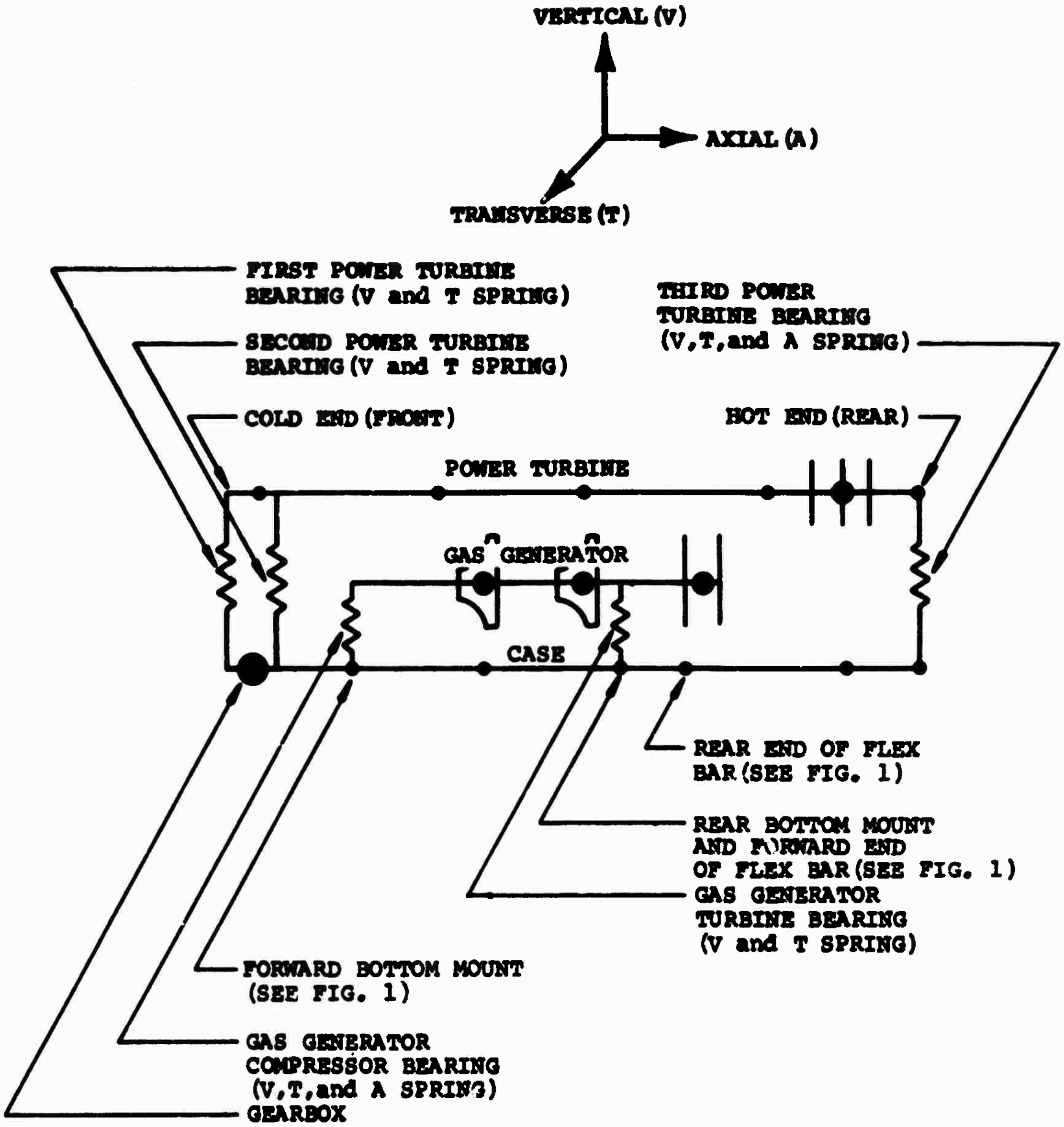


Fig. 1 - A large gas turbine mounted in the FSP



NOTE: V,T and A REFER TO THE COORDINATE DIRECTIONS

Fig. 2 - A schematic of the concentric beam portion of the large gas turbine engine model

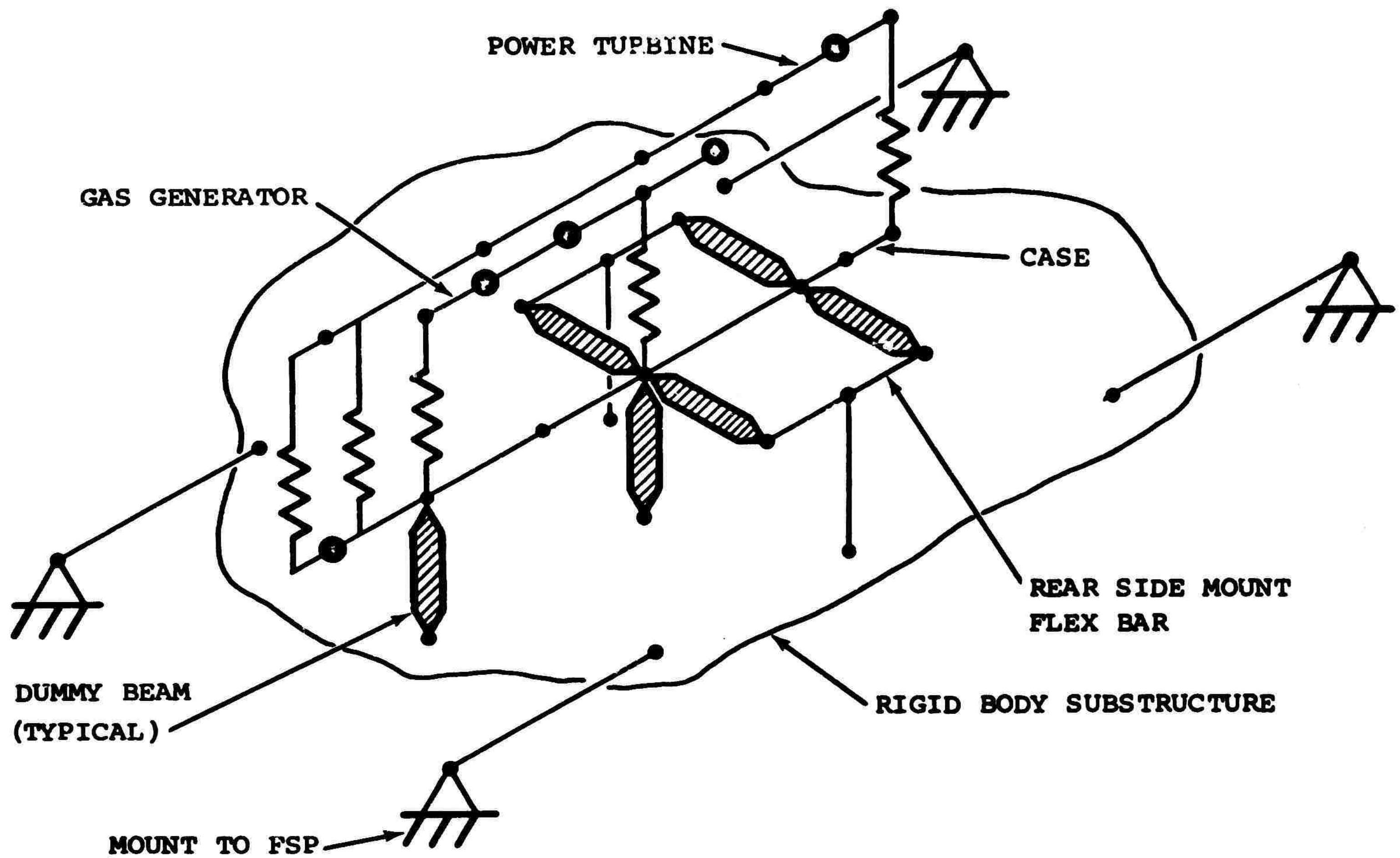


Fig. 3 - A complete schematic of the large gas turbine engine model

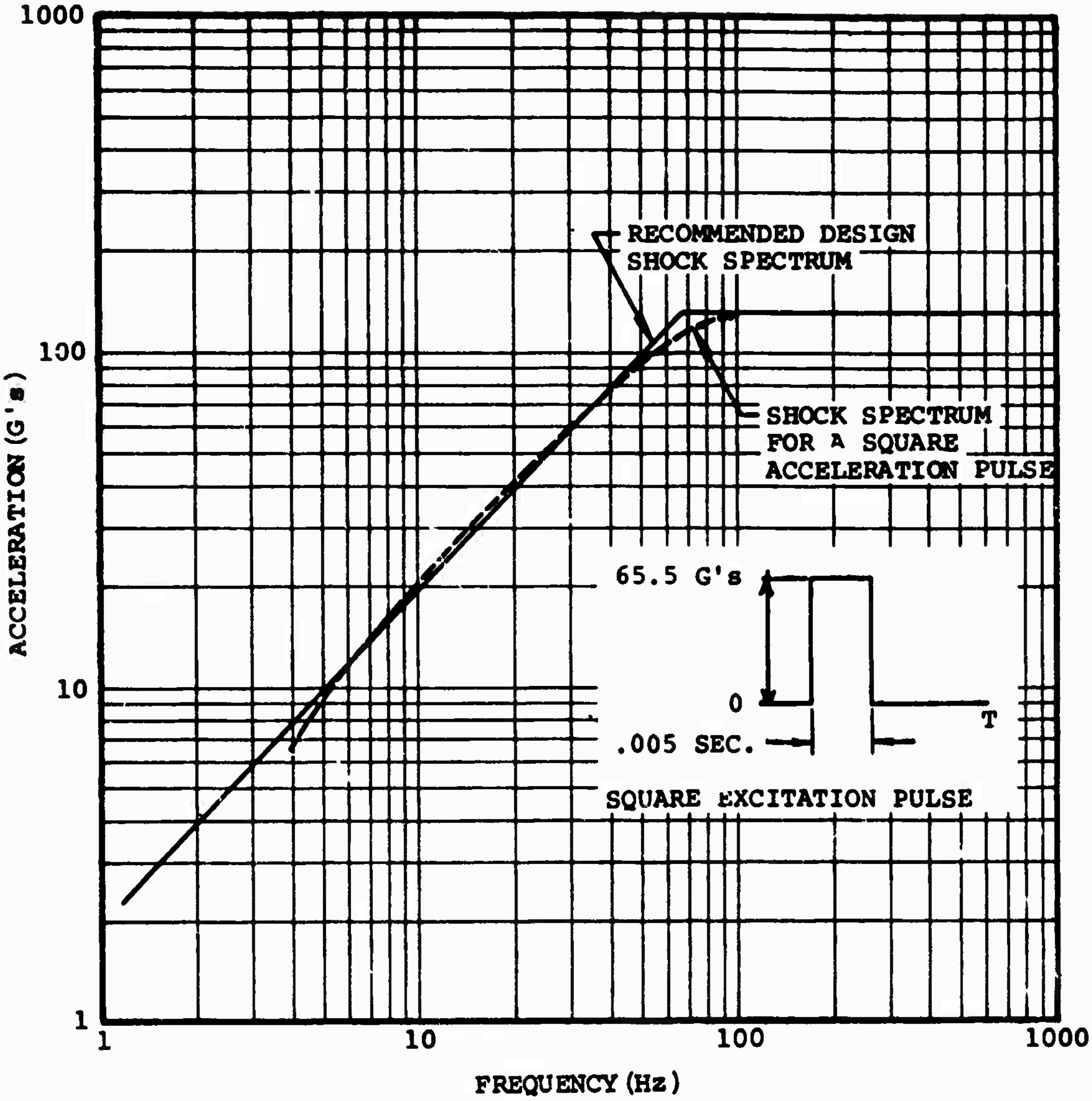


Fig. 4 - Shock spectrum

TABLE I

SELECTED MAXIMUM RESPONSES OF THE FLEXIBLE ROTOR SYSTEM BY THE TRANSIENT RESPONSE METHOD AND CORRESPONDING PERCENT ERROR OF THE SHOCK SPECTRUM METHODS

Location	Transient Response		Shock Spectrum % Errors			
	Time (sec)	Value	Modified Root Σ Square I	Modified Root Σ Square II	Root Σ Square	Σ Of Model Contributions
Accelerations (g's)						
Gearbox	0.009	62.4	-3.8	+3.4	-23.9	+8.7
Engine c.g.	0.027	62.5	+1.4	+5.6	-12.6	+10.2
Substructure	0.011	58.7	+1.2	+2.7	-15.7	+4.4
Gas Generator 2nd Compressor	0.010	75.3	-5.0	+3.7	-23.2	+6.6
Deflections (in.)						
Gearbox	0.010	0.616	0.0	+2.6	-15.7	+2.9
Engine c.g.	0.011	0.670	+0.9	+1.9	-9.1	+2.2
Substructure	0.011	0.624	+0.6	+1.0	-12.0	+1.1
Gas Generator 2nd Compressor	0.011	0.707	+0.3	+1.7	-10.9	+2.3
Bearing Loads (lbs)						
Power Turbine Brgs						
1st	0.08	49,153	+0.1	+1.9	-29.2	+2.5
2nd	0.08	76,678	0.0	+2.2	-29.2	+2.8
3rd	0.08	301,267	+0.9	+3.6	-27.7	+5.2
Gas Generator Brgs						
Compressor	0.009	7,537	-1.4	+8.5	-22.3	+17.4
Turbine	0.148	29,453	-5.5	+4.1	-26.0	+11.9
Vertical Mount Loads (lbs)						
Forward	0.009	127,530	-12.9	+4.3	-38.4	+10.5
Rear Mount	0.096	165,860	-4.7	+3.4	-23.2	+6.7
Average % Error			-1.9	+3.4	-21.3	+6.4
Standard Deviation of This Selection			3.9	1.9	8.4	4.6

TABLE II

SELECTED MAXIMUM RESPONSES OF THE FLEXIBLE AND RIGID ROTOR MODELS

	Location	Flexible Rotors		Rigid Rotors	
		Time	Value	Time	Value
Accelerations	Gearbox	0.009	-62.4	0.010	-62.1
	Engine c.g. on Case	0.027	62.5	0.027	66.0
	Substructure	0.011	-58.7	0.011	-58.7
	Gas Generator Turbine Bearing	0.010	-75.3	0.010	-78.9
Deflections	Gearbox	0.010	0.616	0.010	0.616
	Engine c.g. on Case	0.011	0.670	0.011	0.673
	Substructure	0.011	0.624	0.011	0.626
	Gas Generator Turbine Bearing	0.011	0.707	0.011	0.718
Bearing Loads	Power Turbine Bearings				
	1st	0.08	49,153	0.008	2,238
	2nd	0.08	76,678	0.008	5,430
	3rd	0.08	301,267	0.008	164,895*
	Gas Generator Bearings				
	Compressor	0.009	7,537	0.009	10,124*
	Turbine	0.148	29,453	0.146	23,713*
Vertical Mount Loads	Forward Mount	0.009	127,530	0.009	129,142
	Rear Mount	0.096	165,860	0.092	-116,943*

*Relative maximum occurred at approximately the same time as the flexible rotor model.

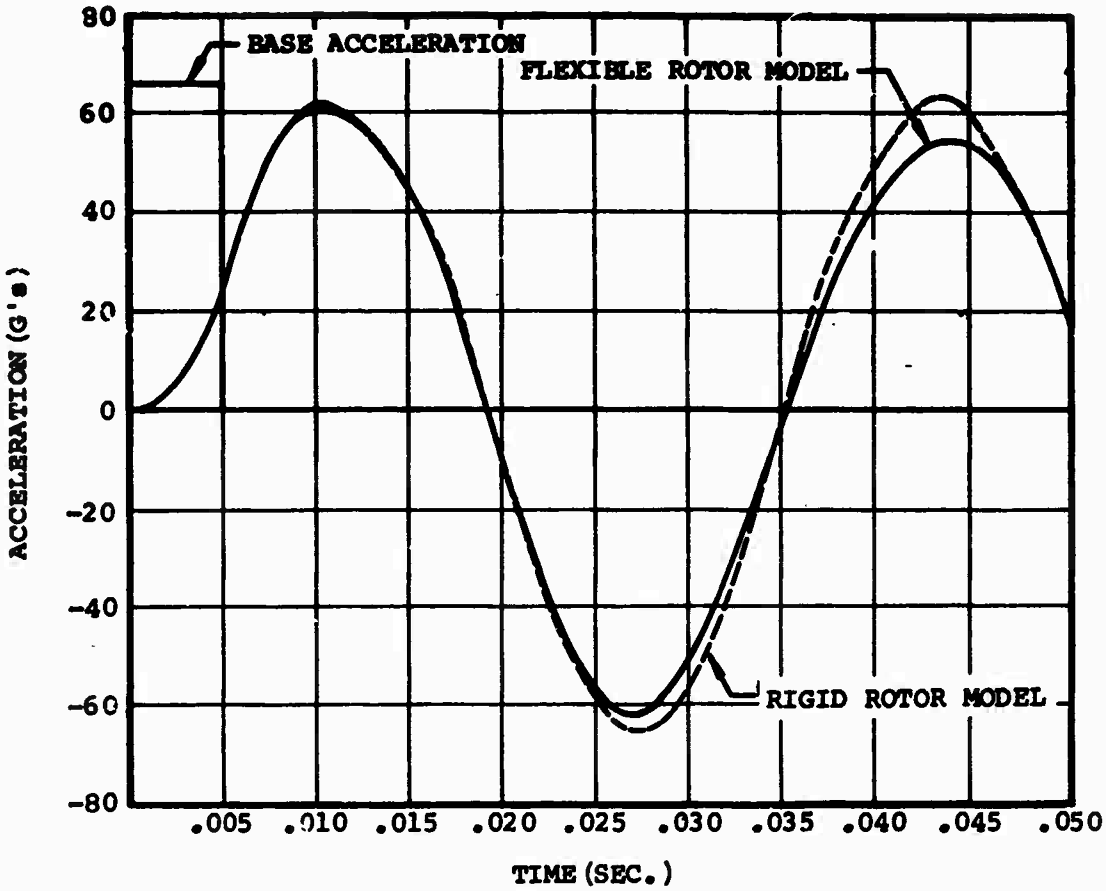


Fig. 5 - Engine C.G. acceleration

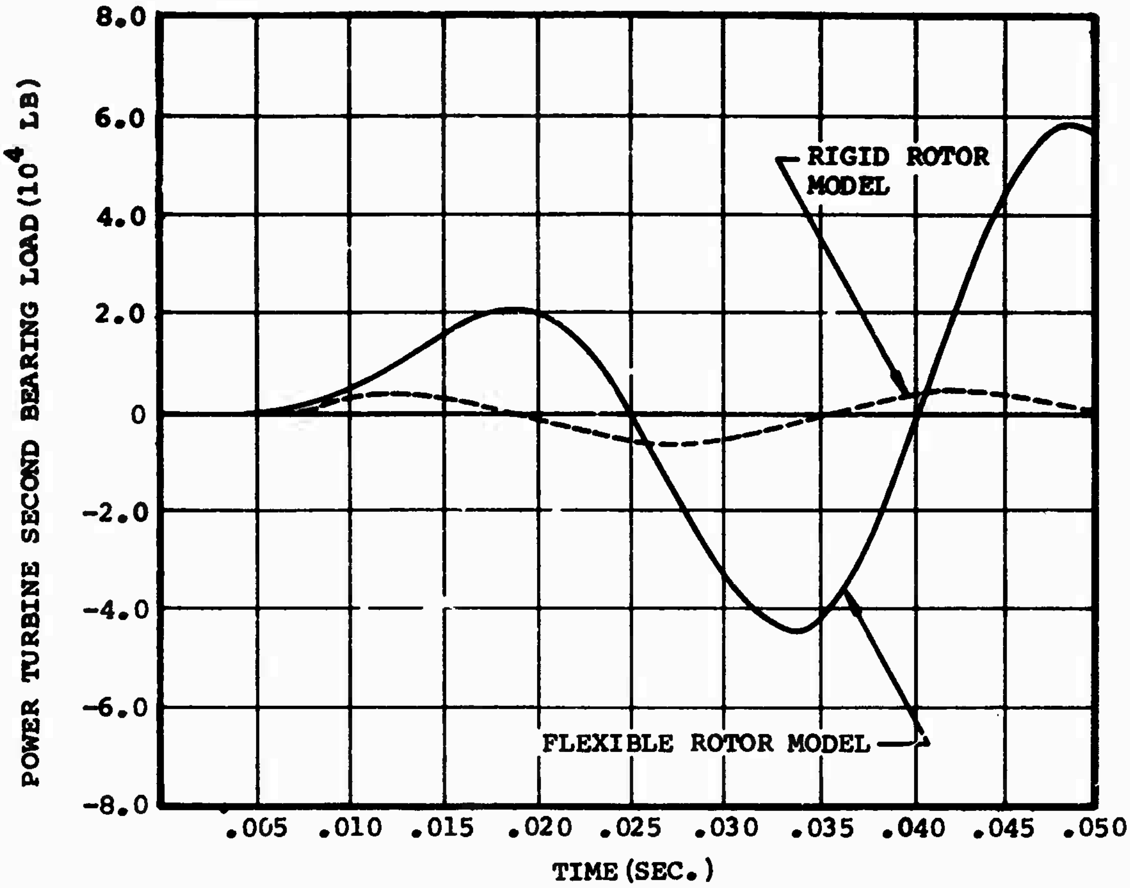


Fig. 6 - Power turbine second bearing load

of the time response of the second bearing load of the power turbine rotor for the two models. Both of these figures were obtained from the transient response analysis.

Rotor deflections are sufficiently large to ensure that rotor tip rubs will occur. Shown in Figure 7 is a time response of the relative deflection between the case and the various rotors where tip rub is a potential problem. The symbols on the plot are located to indicate the time at which tip rub will occur for the indicated rotor stage.

DISCUSSION

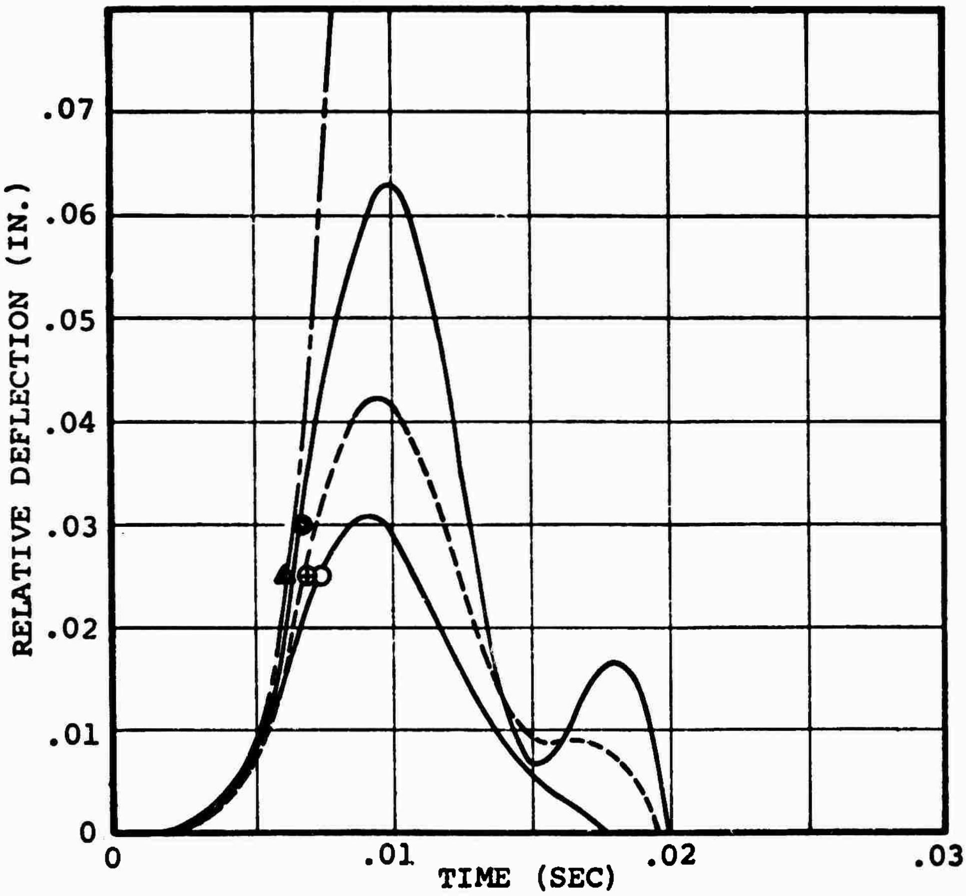
The excitation used in this study was the square pulse indicated in Figure 4. As compared to the recommended design shock spectrum for the FSP, the shock spectrum of the square pulse generally is in good agreement. From 100 Hz on up the agreement is very good, but at 67 Hz there is a disagreement of 13 percent. A transient response analysis that is intended to simulate the recommended shock spectrum will give low results if a mode of about 67 Hz is present and if this mode is participating significantly in the response of the degrees of freedom in question. If this is a problem, and use of the transient response technique is essential, modal weighting factors can be used. Variations between these two shock spectrum are not important in the comparison of transient response analysis and the shock spectrum analysis or in the comparison of models.

The shock spectrum analysis of both models used 24 modes of vibration. Examination of selected degrees of freedom responses indicated that after the 13th mode, which was 131 Hz, the modal contributions were negligible.

The percent errors of the various summation methods of modal contributions as compared to the maximum transient response solution give an indication of the corresponding conservatism. These results must not be accepted as representative of all dynamic models but rather as an example of the model studied here. The quantities selected for Table I were chosen because of their engineering interest, not because of credence they added regarding the better method of shock spectrum analysis. The maximum values chosen occurred within the initial 0.15 seconds. It is felt that in reality there will be sufficient damping in the system to preclude the possibility of larger responses after this time. During the first 0.15 sec the lowest frequency goes through over four periods. With 2 percent damping, the vibration amplitude will be reduced to 57 percent of the original amplitude. The root summation square method as discussed is always unconservative by a significant amount. The first modified root summation square approach is more conservative, but for this example is, on the average, still unconservative. Although this method generally agrees well with the transient response method, significantly unconservative results can occur as shown by the -12.9 percent error on the forward mount load. The second modified root summation square approach is still more conservative, less accurate on the average but has a more consistent error. That is, the standard deviation is smaller. Although the quantities shown are always conservative for the second modified method, some quantities not tabulated here were slightly unconservative. The only way to be completely confident that all results are conservative is to use the last method, the summation of modal contributions. This study indicates that for the system studied here the better method is the second modified root summation square method. The approach combines a generally conservative result with close approximation to the transient response solution. However, the summation of modal contributions provides complete confidence that all results are conservative but not excessively so. These comments up to now have delt with analytical comparisons. No comparisons of the shock spectrum methods with test data are implied. It may be that other factors not included in the analysis may be sufficiently strong to warrant the use of other approaches.

The results shown in Table II demonstrate that the simple rigid rotor model was adequate for some quantities but that the flexible rotor model was necessary for others. Of the engineering quantities of general interest, the accelerations, deflections and mount loads agree very well for the two models. The gas generator bearing loads agree moderately well, and the power turbine loads are in gross disagreement. The power turbine rotor is very flexible and as a result, several of the lowest frequency modes contain a significant amount of power turbine rotor bending. The gas generator is much stiffer and so has bending only in the higher modes. The case is very stiff and has bending only in the very high modes. A general guideline is, as may be expected, if the lower natural frequencies of the system contain bending in a rotor, and if the

SYMBOL	CLEARANCE LOCATION	CLEARANCE (IN.)	TIME AT RUB (SEC.)
▲	POWER TURBINE	.030	.00608
○	GAS GEN. 1st COMP.	.025	.0074
⊕	GAS GEN. 2nd COMP.	.025	.00693
●	GAS GEN. TURBINE	.025	.00667



NOTE: FLEXIBLE ROTOR MODEL

Fig. 7 - Relative deflections at the rub locations

rotor is represented as a rigid body, then significant errors in quantities directly related to the rotor can result. The good agreement demonstrated on some quantities can not be relied on for all models that have significant similarities to the model studied here. Figure 5 indicates how well the results from the two models agree on the engine c.g. accelerations and Figure 6 indicates how gross the disagreement can be, for example, on the power turbine second bearing load.

The relative deflections at the potential tip rub locations shown in Figure 7 indicate that for the anticipated tip clearances, tip rubs can be expected beginning at 0.00608 seconds. This is not expected to curtail proper functioning of the engine even though performance may deteriorate because of increased tip clearances from wear. The effect of tip rub on case deflections, case accelerations and mount loads is expected to be small. The effect on bearing loads will be very significant since a great deal of dynamic radial load will be carried at the rub location. The first and second power turbine bearings are expected to increase in load even after rub since the rub location is sufficiently remote that it will not take load away from them. The third power turbine bearing load, however, should not increase greatly after rub since the rub location is adjacent to the bearing. The gas generator rubbing loads will be relieved by rub at the two compressors and the turbine.

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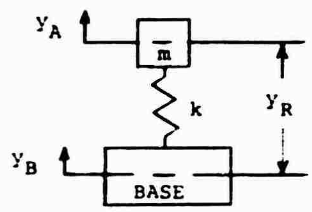
APPENDIX

ANALYTICAL METHOD

Consider the system:



where y_A is the absolute displacement of the mass, m, y_B is the displacement of the base, y_R is the displacement of the mass, m, relative to the base and k is the stiffness. The corresponding equation of motion is:

$$m\ddot{y}_A + ky_R = 0$$

where,

$$\ddot{y}_A = \ddot{y}_B + \ddot{y}_R \text{ and } (\ddot{y}) = \frac{d^2(y)}{dt^2}$$

so that,

$$m\ddot{y}_R + k\ddot{y}_R = -m\ddot{y}_B$$

Extending this to a system of equations for a multi-degree of freedom system on a common base gives:

$$[m]\{\ddot{y}_R\} + [k]\{y_R\} = -[m]\{\ddot{y}_B\} \qquad (1)$$

The matrix $\{\ddot{y}_B\}$ is populated for only those unknown displacements and/or rotations for which the base motion is specified. Introducing the modal transformation,

$$\{y_R\} = [\phi]\{\eta\} \qquad (2)$$

where $[\phi]$ are the eigenvectors of the system and $\{\eta\}$ are the modal coordinates, the equations of motion may be uncoupled by substituting Equation (2) into Equation (1) and premultiplying by $[\phi]^T$ giving:

$$[\phi]^T[m][\phi]\{\ddot{\eta}\} + [\phi]^T[k][\phi]\{\eta\} = -[\phi]^T[m]\{\ddot{y}_B\}.$$

The quantities $[\phi]^T\,[m][\phi]$ and $[\phi]^T\,[k][\phi]$ give the diagonal modal mass and modal stiffness matrices, $[M]$ and $[K]$, resulting in an uncoupled system of equations:

$$[M]\{\ddot{\eta}\} + [K]\{\eta\} = -[\phi]^T[m]\{\ddot{y}_B\}. \qquad (3)$$

As a result, the original coupled equations excited by $\{\ddot{y}_B\}$ and as shown in Equation (1), have been transformed to uncoupled equations excited by $\{\ddot{y}_B\}$. The transient response solution is obtained in closed form by describing $\{\ddot{y}_B\}$ analytically and solving the resulting linear second order differential equations. The resulting values of $\{\eta\}$, $\{\dot{\eta}\}$ and $\{\ddot{\eta}\}$ can be transferred back to $\{y_R\}$, $\{\dot{y}_R\}$ and $\{\ddot{y}_R\}$ with the application of Equation (2).

The shock spectrum solution is obtained by rewriting Equation (3) as:

$$[M]\{\ddot{\eta}\} + [K]\{\eta\} = -[\phi]^T[m]\{\beta\}\ddot{y}_B \qquad (4)$$

where $\ddot{y}_B$ is a scaler and $\{\beta\}$ is composed of ones for those degrees of freedom contributing to motion in the direction of y_B and zeros for the others.

Equation (4) may be written to give

$$[M]\{\ddot{\eta}\} + [K]\{\eta\} = \{\rho\}\ddot{y}_B \qquad (5)$$

where,

$$\{\rho\} = -[\phi]^T[m]\{\beta\}$$

The absolute values of the responses of $\{\eta\}$, $\{\dot{\eta}\}$, and $\{\ddot{\eta}\}$ are obtained by solving the equation:

$$[M]\{\ddot{S}\} + [K]\{S\} = \{-\ddot{y}_B\}$$

for $\{|S|\}$, $\{|\dot{S}|\}$ and $\{|\ddot{S}|\}$. These may be obtained from shock spectrum data. This is particularly useful when $\ddot{y}_B$ is not analytically defined as with the FSP. The absolute amplitude of the modal coordinate is given by:

$$|\eta_j| = \rho_j\,|S_j|$$

A modified version of Equation (2) may now be used to transform back to an approximation to $\{|y_R|\}$:

$$(y_{Ri})_{max} = \sum_{j=1,\ell}\phi_{ij}\,|\eta_j| \qquad (6)$$

where ℓ is the number of modes. This can be referred to as the summation of modal contributions. It leads to an approximation for the maximum response of y_i. It is always conservative since all modal contributions are considered to be in phase. Another method has been used where:

$$(y_{Ri})_{max} = \sqrt{\sum_{j=1,\ell}(\phi_{ij}\,|\eta_j|)^2} \qquad (7)$$

This is denoted the root summation square approach. Based on experience this method has been found generally anti-conservative (relative to transient solution maximum).

Another commonly used approach is in which:

$$(y_{Ri})_{max} = (\phi_{ij}|\eta_j|)_{max} + \sqrt{\sum_{j=1,\ell} (\phi_{ij}|\eta_j|)^2 - (\phi_{ij}|\eta_j|)^2_{max}} \qquad (8)$$

This is the largest modal contribution plus the summation of all the other modal contributions. This approach can be conservative or anti-conservative and can be called the first modified root summation square approach. Another method which is slightly more conservative can be expressed as:

$$(y_{Ri})_{max} = (\phi_{ij}|\eta_j|)_{max} + (\phi_{ij}|\eta_j|)_{semi-max} + \sqrt{\sum_{j=1,\ell} (\phi_{ij}|\eta_j|)^2 - (\phi_{ij}|\eta_j|)^2_{max} - (\phi_{ij}|\eta_j|)^2_{semi-max}} \qquad (9)$$

Here, the two largest modal contributions are added to the square root of the sum of the squares of all the other modal contributions. This can be called the second modified root summation square approach. The logic of this approach is based on the expectation that in a reasonable amount of time, the first two modal contributions will be in phase. At that same instant of time, the other contributions can be expected to add still more and are accounted for by the square root of the sum of the squares of their values.

The calculations for loads and/or stresses can be expressed as functions of the system displacements and rotations and are found by transformations of the form

$$\{L\} = [B]\{y\} \qquad (10)$$

where $\{L\}$ are the desired quantities and $[B]$ is the appropriate (stiffness) transfer matrix. The values of $\{y\}$ that are used must be at the same instant of time which is not the case for the results of the shock spectrum approach. Therefore, the transformation to obtain $\{L\}$ must be performed with the modal coordinates. A new modal transformation matrix is found to be

$$[\Phi] = [B][\phi]$$

and the new modal transformation matrix $[\Phi]$ is used in place of $[\phi]$ in Equations (6) through (9).

DISCUSSION

Mr. O'Hearne: (Martin Marietta Corp.) In a case like this my overall conclusion would be to do the transient analysis, it is not that big a deal, and you have nothing to wonder about except your modeling.

Mr. Manceau: That is basically right, there are some advantages to the shock spectrum analysis. First it computer time, second it is usually a little easier so there is a little less opportunity for error, and third we based our excitation on the recommended design shock spectrum even though there are some questions on its validity. It is fairly conservative, and so with that in mind the question is do we need to use as sophisticated a method as a complete transient response solution? In general, I have to agree with your comment and I don't think there is any real answer at this time.

Mr. Shell: (NRL) For some items that can't be tested on a floating shock platform there is a requirement that analysis be performed. The Navy's Dynamic Design Analysis Method might be used and I believe, that the first means of summing the modes, that is the first modified modal summation method, includes the largest response plus the square root of the sum of the squares of all of the other modes. However you recommend that on the basis of the fact that the two modes can get into phase with each other that they be used. Aside from the actual comparison with your solution, did you do anything to verify, or do you have any other rationale for saying, that these two modes would get into phase with each other?

Mr. Manceau: The frequencies are commonly such that in a reasonable amount of time both will be going through one period so there is a very high probability that they will be in phase; beyond that that method is more conservative than what I called the first modified method.

Mr. Butzel: (Boeing Company) If in fact you have two modes that nearly combine in phase wouldn't this indicate that the sum of the squares method would produce an estimate that is approximately in the ratio of 2 to 1.4 compared to summing all the modes together? Would this account for the generally low estimate that you would obtain from the squares method?

Mr. Manceau: The sum of the squares method places every mode 90 degree out of phase with every other mode and in doing that you will obtain a lower result.

THE DEVELOPMENT OF A WATER PARTICLE VELOCITY METER

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A method of making a direct measurement of the water particle velocity due to the shock wave of an underwater explosion is discussed in this paper. Measurements of water particle velocity produced by shock waves from tapered and compact charges are compared with computations of velocity based on pressure measurements. It is shown that the independent measurement of water particle velocity provides a check on the calibration of the piezoelectric gages used to measure pressure. Uses of the water particle velocity meter in applications where computations of particle velocity are unreliable are also discussed.

INTRODUCTION

The design of marine structures which are resistant to the shock of underwater explosions is facilitated when accurate computations of the response of the structures to underwater shock loading can be made. To verify a structural analysis technique or provide information necessary for the further development of the technique, experiments must be performed in which both the loading and structural response are measured. Water particle velocity as well as shock wave pressure is part of the specification of underwater shock wave loading. When the form of the wave propagation (plane, spherical, etc.) is not known or reflections are involved, the relationship between vector particle velocity and scalar pressure is not known well enough to determine the particle velocity loading from the pressure measurements alone. Under these conditions a direct measurement of particle velocity should be made to determine the particle velocity loading.

In September 1973 the Naval Ship Research and Development Center (NSRDC) conducted experiments in the Chesapeake Bay in support of the analytical predictions of shock response of submersibles being carried out by contractors of the Structural Mechanics Program of the Office of Naval Research (ONR) under the DNA ONR/NAVSEA Shock Hardening Program. The objective of the experiments was to determine the loading and response of a small scale submarine model subjected to the shock wave of an elongated charge consisting of a series of truncated cones (tapered charge). Because the relationship between water pressure and water particle velocity for tapered charge shock waves is not exactly known, the requirement was established by ONR contractors that the loading be determined by free field measurements of both pressure and particle velocity. The unavailability of a suitable water particle velocity meter previous to this application necessitated the use of a particle velocity meter developed at NSRDC especially for this project.

OBJECTIVE

The purpose of this paper is to give the characteristics of the water particle velocity meter recently developed at NSRDC and to demonstrate its effectiveness in measuring the particle velocity of shock waves resulting from the underwater explosion of compact and tapered charges.

APPROACH

The velocity meter normally used by NSRDC to measure structural velocity transients provides the basis for the water particle velocity meter design. This structural velocity meter consists of a coil wound tube with a spring mounted bar magnet inside. The structural velocity meter is adapted for use as a water particle velocity meter by sealing the tube with appropriately designed end caps and seismically mounting the resulting cylinder in the desired orientation underwater. When the cylinder is made neutrally buoyant, it moves with the surrounding water and a voltage proportional to the longitudinal relative velocity between the water and the magnet is provided as the output.

THE WATER PARTICLE VELOCITY METER

Fig. 1 is a schematic of the particle velocity meter developed at NSRDC. The cylindrical coil form is wound with two electrical coils connected in series opposition. The inertial reference is provided by a bar magnet aligned with the axis of the tube by means of helical springs. These springs join the ends of the magnet to a longitudinal brass bar attached to the inside surface of the tube. Aluminum end caps are provided to seal the ends of the tube and restrain the total longitudinal travel distance of the magnet to 1 inch. Each end cap has 3 pad eyes which provide points of suspension for seismically mounting the particle velocity meter. A small lead mass is cemented to the outside edge of each end cap on the side opposite the brass bar. These lead masses are of equal weight and are provided to adjust the buoyance of the particle velocity meter. They also tend to balance the brass bar. The lead masses are varied until the weight of the particle velocity meter in sea water is equal to the weight of the magnet in air. When this requirement is met, the particle velocity meter cylinder exclusive of the magnet is neutrally buoyant and will move with the surrounding water.

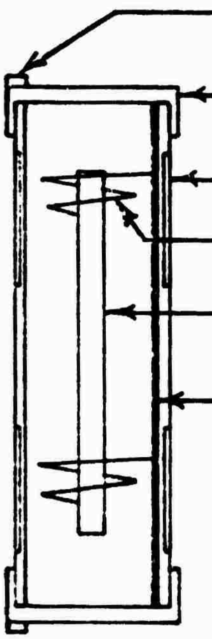

Fig. 1 - Water Particle Velocity Meter

The voltage sensitivity of the meter to longitudinal relative velocity between the coil form and magnet varies slightly with magnet position and is determined experimentally in air. Fig. 2 is a plot of the ratiu of the sensitivity to the peak sensitivity as a function of magnet displacement from one end of the cylinder. The sensitivity of the meter is taken as the peak. Deviation from the peak sensitivity greater than that shown in Fig. 2 is not permitted. The directional sensitivity of the meter was determined experimentally in air using a ballistic shock generator to separately provide longitudinal and cross-axis step input velocities. Fig. 3 shows the longitudinal and cross-axis meter response to longitudinal and cross-axis step inputs of 4 ft/sec The meter cross-axis insensitivity seen in Fig. 3 demonstrates good directional characteristics.

The use of the particle velocity meter requires a structure of fixed orientation in the water from which the meter is seismically mounted by rubber bands. The rubber bands must maintain the axis of the cylinder in the direction of the desired particle velocity component without much restraint of longitudinal motion. A longitudinal frequency of 3 Hz in air is used for this purpose. Fig. 4 is a picture of a horizontal particle velocity meter and a vertical particle velocity meter seismically mounted for use. For the small fast particle motions due to a shock wave, the neutrally buoyant cylinder moves with the water and the magnet remains fixed in space. The voltage output of the meter is proportional to the average water particle velocity over the length of the meter.

The steady state sinusoidal frequency response of the particle velocity meter in water has been investigated theoretically by considering the longitudinal envelopment of the meter by a plane free water pressure wave ignoring shock wave-structure interaction. The calculated ratio of the meter indicated velocity to the particle velocity at the coordinates occupied by the center of the meter meter is given versus frequency in Fig. 5. Frequencies at which the input is faithfully reproduced lie between the low frequency resonance due to the magnet seismic system and the high frequency cut off determined by the meter length. Fig. 5 gives high frequency characteristics which are optimistic because the shock wave-structure interaction ignored in the calculation tends to reduce the high frequency cut off frequency. The well known characteristics of the magnet spring mass system permit the low frequency resonance to be removed through seismic correction. However, high frequency extension of the meter response can be done with confidence only by a redesign of the meter using a shorter cylinder length.

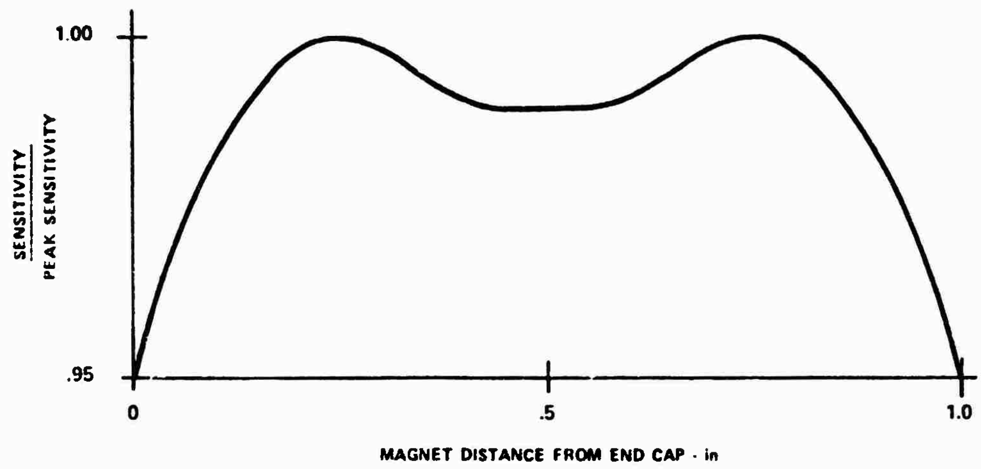

Fig. 2 - Sensitivity vs. Magnet Position

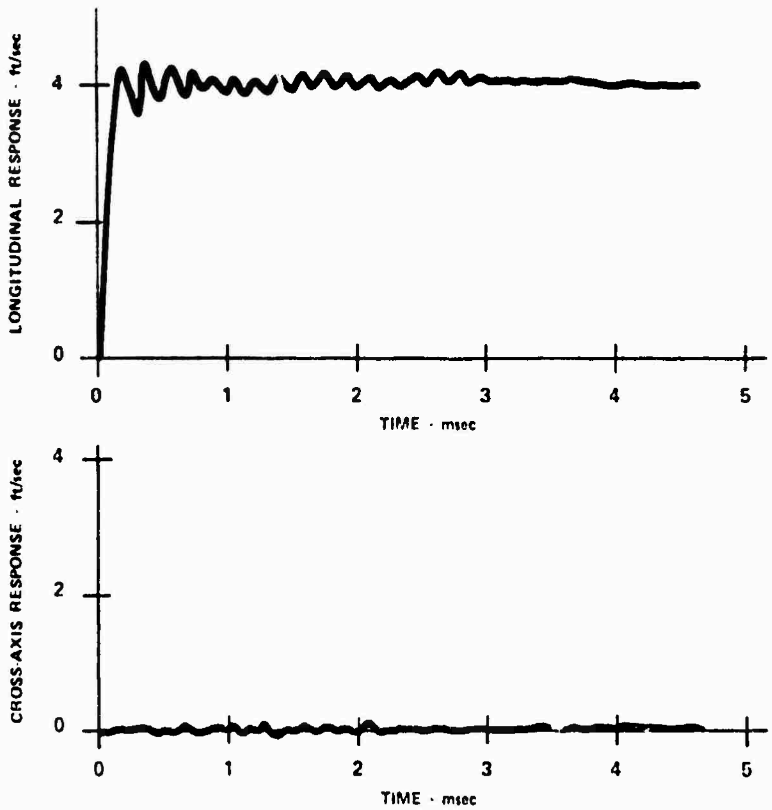

Fig. 3 - Longitudinal and Cross-Axis Step Response

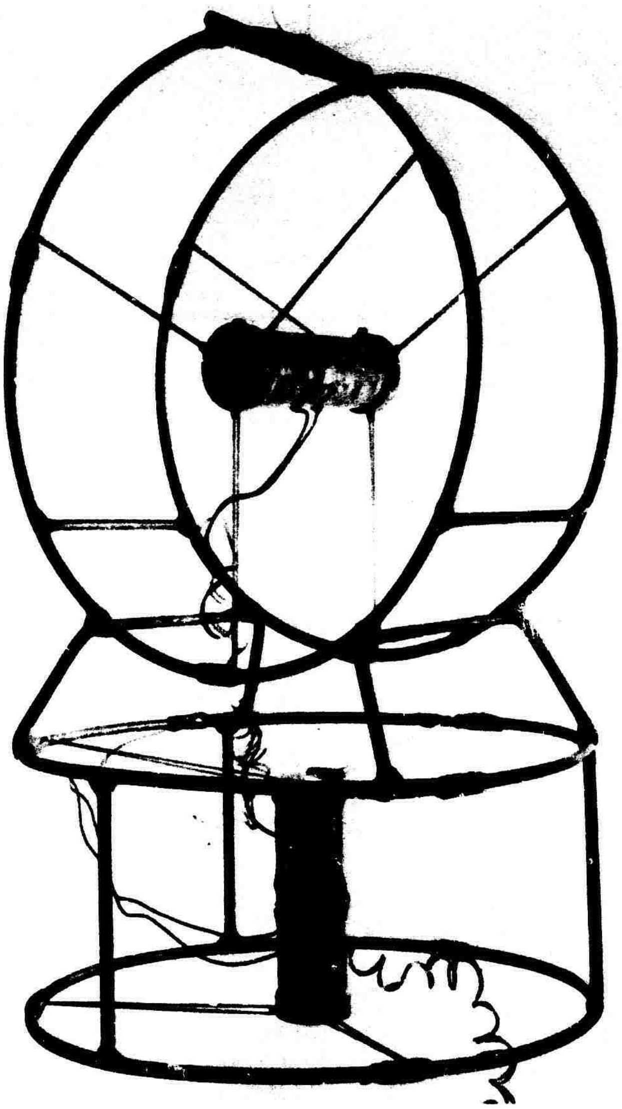

Fig. 4 - Horizontal and Vertical Meters

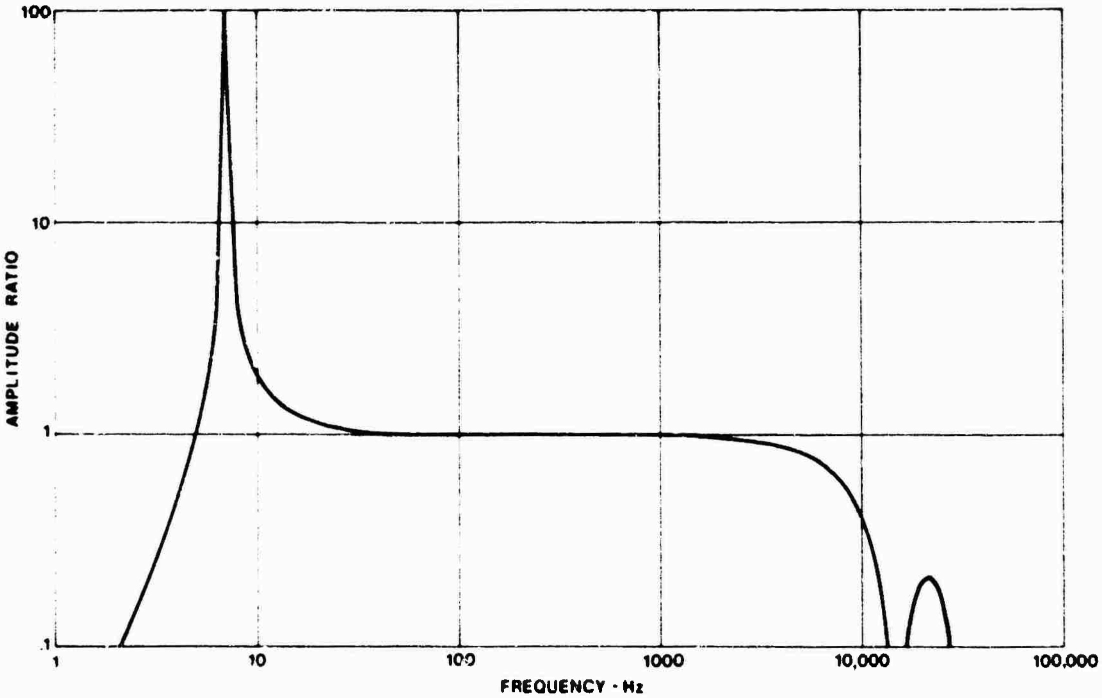

Fig. 5 - Frequency Response

Nominal particle velocity meter specifications are given in Table I.

TABLE I
Particle Velocity Meter Specifications

Sensitivity	35	mv/ft/sec
Max. Magnet Displacement	1	inch
Magnet Seismic Frequency	7	Hz
Length	4	inches
Diameter	1.25	inches
Total Mass	89	gm
Magnet Mass	14	gm
Resistance	20	ohms
Capacitance	350	pfd
Inductance	1.5	mh

MEASUREMENTS OF PARTICLE VELOCITY

The particle velocity and corresponding pressure in underwater shock waves produced by the explosion of compact and tapered charges have been measured using the NSRDC particle velocity meter and a commercial tourmaline pressure gage. A computation of particle velocity from measured pressure assuming a spherical wave has been made for comparison with the measured particle velocity. Fig. 6 is the shock wave pressure and the comparison of measured and computed radial particle velocity at 400 ft. distance from a compact charge. Since the compact charge produces a spherical wave, the velocity comparison shown in Fig. 6 provides a check for inconsistency between the particle velocity and pressure measurements. Because the particle velocity measurement is made using an untested meter, the good agreement seen in Fig. 6 is taken as a verification of the operation of the particle velocity meter.

Having demonstrated the operation of the particle velocity meter in the known spherical shock wave of a compact charge, the meter was used in the tapered charge experiments for which it was developed. The shock wave pressure 70 ft. from a tapered charge in the direction of the charge axis is given in Fig. 7. The corresponding vertical and horizontal particle velocity are given in Fig. 8. The comparison of the horizontal particle velocity computed from pressure with that measured using the meter shows that the spherical wave assumption applies and further strengthens confidence in the water particle velocity meter. The vertical partical velocity plot shown in Fig. 8 provides a measurement of the vertical kick off velocity of water particles at the arrival of the reflection from the surface. In general a water particle velocity vector is the resultant of incident and reflected waves from different directions. When reflections are involved, obtaining particle velocity from scalar pressure measurements by computation involves so many questionable assumptions that results cannot be considered reliable. As seen in Fig. 8, particle velocity meters allow the components of particle velocity to be measured directly and the resultant loading on underwater structures determined after reflections have occurred.

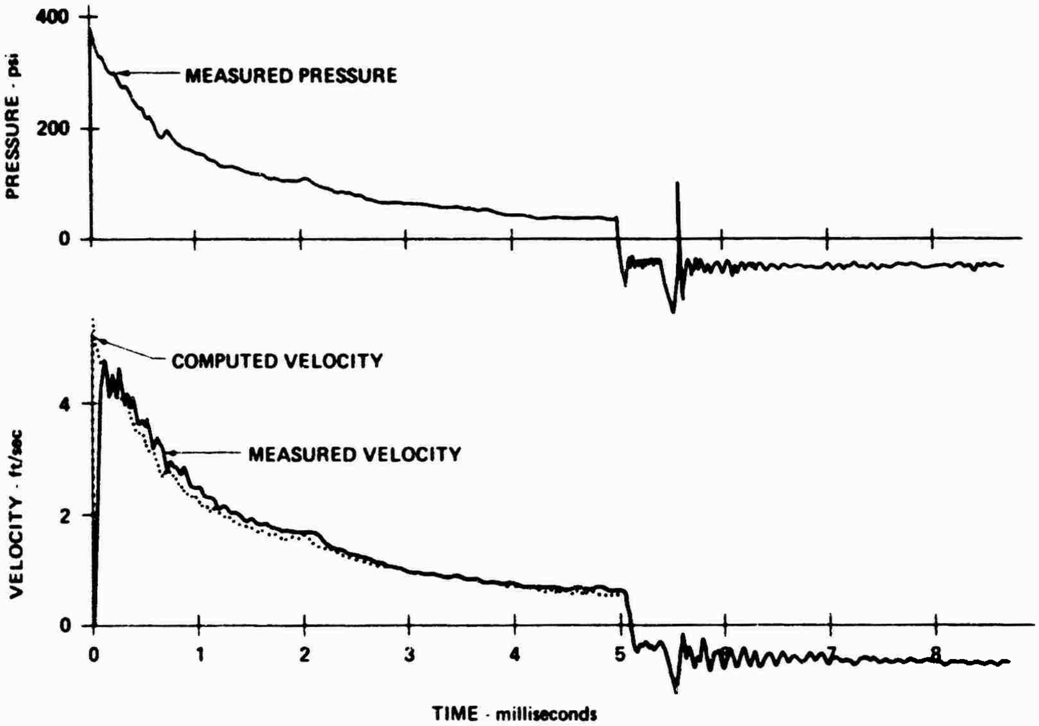

Fig. 6 - Pressure and Particle Velocity, Compact Charge

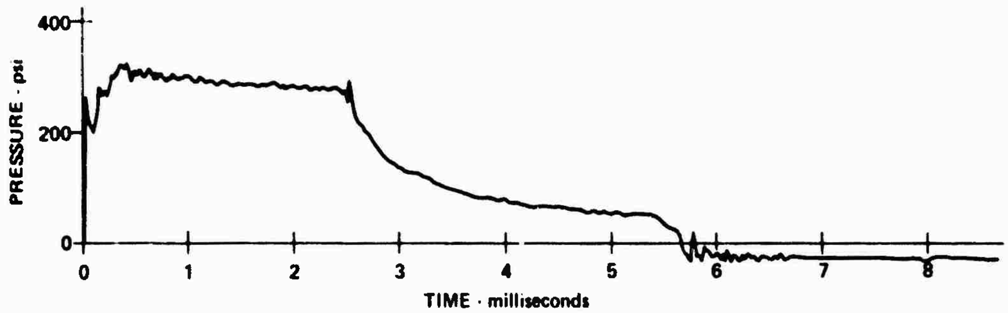

Fig. 7 - Shock Wave Pressure, Tapered Charge

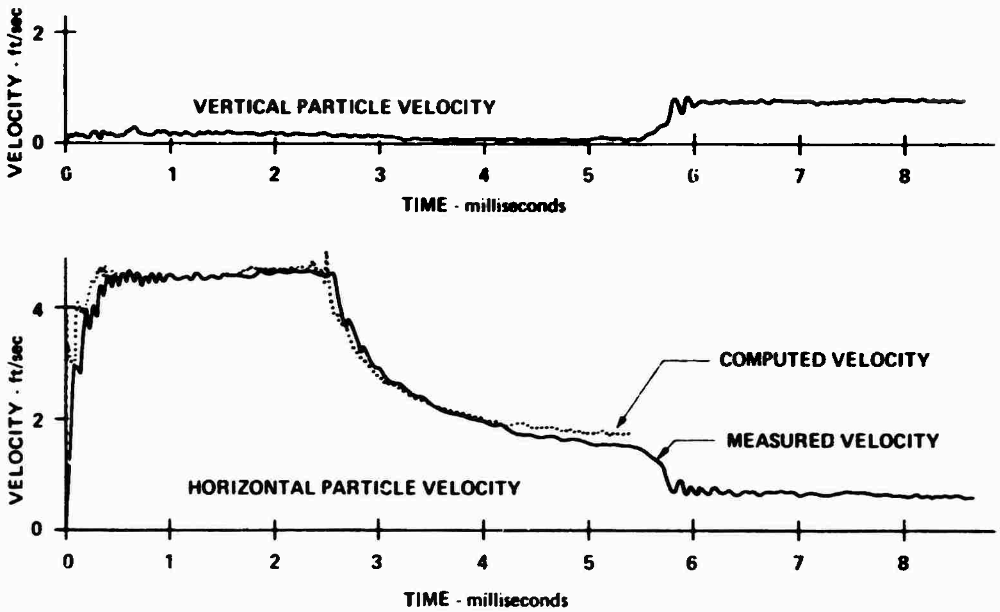

Fig. 8 - Particle Velocity, Tapered Charge

DISCUSSION

The experimental data from the water particle velocity meter shows that the approach taken in the design of the meters is satisfactory so long as the anticipated pressure is not high enough to damage the meter and frequency response requirements are not too severe. The data indicates that the rise time of the meter output to a plane step pressure wave is about 1.5 times the longitudinal transit time. For the meter discussed, this rise time is about .1 msec and is ample for the measurement of the long time constant shock waves produced by tapered charges. An additional application for the particle velocity meter is its use to provide an independent check of piezoelectric pressure gage sensitivities under shock conditions. Pressure gages are usually calibrated under quasi-static conditions and an independent evaluation of their performance under shock is necessary before full confidence can be placed in them.

ACKNOWLEDGMENTS

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