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STEADY STATE REQUIREMENT FOR TECHNOLOGY TRANSITION

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ABSTRACT

Much has been said in recent years regarding the transitioning of new results from the research laboratory to operating practice, a step of key importance in the acquisition of new technology. However, relatively little has been said concerning the steady state requirements necessary to sustain a timely flow of new results. It is the purpose of this talk to outline several features that are necessary to ensure a continuing transition.

After that elusive title, I think it's important to tell you that when Don Thompson contacted me about coming to this meeting, he basically said to say what's on my mind, and that's essentially what you're going to hear. I'll certainly weave in that theme and I hope that it sets the stage for the papers that follow and the Poster Session.

Today I'd like to talk briefly about the place of quantitative NDE in the materials reliability or life management process as a key element to decision-making, and I'd also like to suggest how research and its useful product, technology, enters the realm of practical application as part of that decision-making process. My premises are these: first, that today's complex problems, such as systems reliability and life management, can be solved when the decision-making system is understood, is functional, and has been validated. My second premise is that useful technology transitions because the user wants it and because it is easy to use. The third point; that a coherent research base is necessarily a continuing part of the process, but maintains its vitality by being an integral part of a system which solves problems. Now those are the premises I will touch on at the end of what I have to say this morning. I could stop there, but really would like to tell you a story.

In the summer of 1967 at Hill Air Force Base in Utah, a Minuteman missile was disassembled that had been in the silo for seven years, and underwent a periodic inspection--x-ray, ultrasonic, etc. A technician removed the igniter from the front most of the first stage, looked inside with a hand-held mirror, and discovered some cracks about an inch and a half long in each one of the star valleys in that grain design. These cracks were located in a position difficult to see and easily missed by x-ray. They were, however, quite predictable in their location by looking at the stress analysis. The existence of real, field-generated defects led to a momentary paralysis of the system which was designed to anticipate and react to just such a problem. The planning for the occurrence had been elaborate. (I had been part of that planning.) It was based on generalities and abstractions, and it was tied to a level of understanding that was, unfortunately, over ten years old. The following questions were immediately generated: How serious was it whether there was a crack in that location; if it is, how critical is it? Can the size and location be assessed for an OK or not-OK decision? If it's currently OK, will it grow to a critical size? How many of the missiles in the thousand-missile force were so affected? How may the force be conveniently and quickly inspected when the missiles are out in the silos? How can a conclusive demonstration of an OK or not-OK condition be conducted, and at what cost? Finally, why weren't we ready for this?

These and many other questions couldn't be answered with the data and methods we had at the time--we couldn't answer the specific question, for instance, of how does a crack, which is burning, behave? Does the combination pressurize inside the crack and mechanically force it open so that it splits like a watermelon? Or, does it burn the tip of the crack and blunt it, and therefore just burn normally to the case, being quite predictable through the burning rate laws? We couldn't figure out how to inspect the missile in the silo for this condition. The cost of opening up the silo at that time was around $25,000 and disassembly was another $20,000. We couldn't even do a decent cost estimate. A massive, highly technical and complex program was undertaken to get answers to these questions.

I selected this example because I believe it's somewhat foreign in specific content to most of the audience in this session, and thereby might provide some stimulation. I also selected it because I was part of the technical community which had formed some ten years earlier to create a resurgent technology base in the highly complex area of solid rocket structure integrity. We had garnered the resources and had established a research center or two with long-range ambitions. We had spread programs throughout the nation in universities and research institutes to work a number of intriguing problems, sustained the program for nearly ten years, and met at least annually to exchange information. We found that (with a real need for answers in a situation which could have been predicted) to get the answers we had to generate a whole new set of oriented and integrated programs. Now you may see a parallel with this particular community.
I have a personal conviction and determination not to let that happen to me again if I can help it. I'm not saying that nothing of value had been produced in the technology programs in the previous years, but the key element which has stayed with me through the years is the realization that a lot of scattered work on obvious pieces of a perceived general need are of little value without integration into a workable decision-making system. I almost feel like saying that again, because it sticks in my mind. A second or subordinate realization which should have been obvious, and I still don’t know why it wasn’t, was that our early program thinking should have presupposed the occurrence of defects and we should have asked the questions: How critical? How to measure? How to decide? It wasn't that those questions hadn't been asked, it's just that they hadn't really been worked conscientiously as though they were related to real problems.

I'd like to generalize a moment and look at Fig. 1. I've grown fond of this slide because I spent a lot of time trying to simplify the structural design process in which the elements are described, and the information flow is directed toward reaching a conclusion; i.e., a structural safety margin. This flow chart was developed in concert with a lot of design engineers. I wanted to understand the fundamental elements of the design process and relate them to structural failure. It ought to be obvious, but what we have attempted to do is have, as a central idea, the analysis—a stress analysis. We're interested in how loads are converted into stresses, taking into account the geometry and the material properties. Material properties here have been split simply into "response" and "limit"—for example, the ultimate properties. There are generally small deformation properties such as modulus, some sort of fracture property, or an ultimate stress. The stress analysis provides a distribution of stresses and strains in the particular object and when one compares the output with a failure criterion that says if you exceed this value you get failure and if you stay within this value you don't, it includes, of course, all of the probabilistic qualifications and concern with statistical distributions of properties. A major job of the designer is to conduct a strength analysis and come up with a margin of safety.

When one assumes pre-existing defects, an analytical predictive process may be used if a quantitative characterization of the defect is available. Such an analysis is generally based on the idea that whatever flaws are present are very small and are quite reproducible; therefore, we don't even pay any attention to them. Let me get into the fracture analysis realm where, assuming defects are there, we are concerned with the characterization of those defects. By this I mean knowing where the flaw is, how big it is, how it is oriented, and examining how it interrelates with the stress field. We are then able to deal with the question—under geometrical, load, and environmental conditions—"Will the crack grow?" Will it grow to an unacceptable limit?

This particular conceptual overview of the design process, integrating the existence of defects, has some new terms, or has some terms that I still have to describe. These terms essentially form the basis upon which the Minuteman service life analysis is now conducted and has been conducted for the last ten years. Actually, it took about two years to react to the defect problem and this conception was the sort of thing which eventually popped out of that reaction.

Let me breathe a little life into this story by describing some of these terms and then I'll come back to the conclusions. Figure 2 shows that the first thing necessary is to define the loads. Loads are hard to define, especially in a dynamic environment. Missiles are handled a lot. They go in and out of silos, they get inspected, and then they are used. A motor fires for about a minute, so it's used in a severe environment for only about a minute, but it sees a lot of time sitting around with its own body forces acting, usually vertically in the silo. Determining loads and, as we like to say, stresses which must be calculated from the loads is the initial problem. The concept of a structural test vehicle is to define the load and to validate analysis is a rather important concept.
This, by the way, is a fair drawing of what the Minuteman first stage looks like. As many of you know, the grain is contoured down the center perforation to get a certain burning profile and a certain pressure-time behavior and, thereby, the thrust-time behavior. It has four nozzles, and you can see the immensity of the problem of trying to inspect for a crack which is up in this region, radially outward from one the the star valleys, and thirty feet up the port, since you can't get access to the front end. Its standing vertically and there are four nozzles on the base, which requires two right-angle turns to get an inspection device up in there. Coming up with a nondestructive device that would extend up and look down the valley in the front end was almost impossible. But that was only the first challenge; to see how one could inspect in the silo from the inside. I won't dwell on this much more, other than to say that the definition of loads is one element and in Fig. 3 you get yet another element on the chart, that is, the concept of "over-test." I find quite frequently, in serving on panels that try to assess the viability of a system, that if one projects that things are getting serious and that we really ought to take these things out of the field and put new ones in, the question always asked is, "Have you ever failed one?" Usually the answer is no. Thus, the idea of defining a limit test. This is kind of a healthy empirical approach to life. Defining a failure mode by purposely breaking something with a somewhat exaggerated, but realistic load is an important part of the process. It's fairly expensive, but this is the sort of thing one does. One purposely breaks the motor by such techniques as plugging the nozzle and pressurizing with inert gas. The intention here is to simulate the loading environment which takes place on ignition, which is one of the more important loading environments.

In order to validate the analysis and to get some statistical data, some sort of smaller test object needs to be created. Figure 5 shows an analog motor. It is essentially a small cylinder containing propellant with a certain kind of notch. One can then have a predictable corner condition, do some calculations, and then break the motor, the point being that you can break lots of these so that you can get some sort of statistical data base. Figure 6 shows how one attempts to reproduce the particular stress field, the corner condition which one might find in a motor, and conduct the same kind of stress analysis of this condition that one does in the motor. Then by performing the transformation one can conclude that, under the loading conditions and with these particular materials, one can predict the growth of the flaw.
Figure 6. Model for reproduction of corner stress condition.

Figure 7 shows an elaborate test used to get the failure criterion. Shown is a sort of plane specimen, a cross-section in which we are applying biaxial loading which is the thermal stress condition pulling on the outside. There is a thermal mismatch between the propellant and the case. When the temperature changes what you get is a tendency when the temperature is low for the propellant to pull away from the wall to which it is bonded. What sets up is essentially a hoop stress condition around the centerport. Figure 8 shows the consequences of that condition; you grow a crack. These are the sorts of tests one runs to validate analyses, but you see, analysis was central to that flow diagram and one of the problems we had was that it is too easy to believe in analysis once it's completed.

Figure 8. Same as Fig. 7. Note crack growth.

Figure 9 shows what is an unfortunate combination of things. What it essentially says is that one finds, from over-testing to the various loading conditions, the kinds of failures which can take place. The thing that's unfortunate is that it doesn't indicate what the consequences of cracking might be. The consequences of cracking might be burning to the case wall if there is a crack there, or fire getting in and burning perhaps too large an area thus creating over-pressurization. Certain failure criteria based on the continuum mechanics approach to life had been reasonably well established. Unfortunately, this did not include a fracture mechanics approach which was so necessary.

Figure 7. Model test for determination of failure criterion.

Figure 9. Replacement criteria from "over-test" results.
Next, I'd like to get back to my initial thoughts. My purpose in running through these examples is to show that there is nothing mysterious about an organized program integration and to show how to introduce the analysis and experiment when a problem is to be solved. I guess the last thing I want to tell you about Fig. 1 is that somehow I've come out of the experience with the belief that it's alright to naively push ahead. Perhaps not naively, because I think that we sell our institution a little short. We push ahead doing the best we can in each element of a process which will lead to a real decision, taking into account what are the most likely causes of failure. I also think, in retrospect, that this is all so obvious, but for some reason we ran a program for ten years without all those obvious things becoming apparent and without our doing much more than making compromises on what would be realistic tests and realistic problems because they cost too much. Each piece of the technology underlying research has a place in the scheme of things and has a vital role in the final decisions and solutions of these problems.

...how to move into the technology transition subject which is shown in Fig. 10. The point I want to emphasize here is that this is a kind of technology flow chart. (I meant to write that word "need" a little bigger.) This flow is driven by need. It's nice to have something driven by opportunity, but if you really want to get some acceleration it ought to be driven by need. The point I want to emphasize here is the coherence in the upper-left corner; the coherent research and technology base. I don't know how to get that point across other than to say that I think in terms of the previous chart. I think in terms of the pieces of the problem. I think in terms of the interfaces and making sure that those interfaces really represent the flow of analytical information, i.e. that they represent the flow of numbers which are useful in an input/output sense. That coherent research and technology base essentially takes the previous system apart and asks the question, "Which are the defective elements in this system that produced a useless answer?", or "Is the system all there?" If I view Fig. 10 as a decision-making machine, then, if all the elements are there, I can turn the crank and an answer will come out. The answer might be poor, but at least the machine works. If I've established that basis and then moved back to ask in a kind of sensitivity analysis approach—which of the elements is most defective and is fouling up my answer most—then I'm prepared to deal with that framework as a constant, as an invariant. I can then deal with the elements in an upgrading fashion, perform a sensitivity analysis which isolates those elements which are most deficient, and concentrate resources there. That's essentially the picture.

The picture shows that new knowledge is required and that it's coherent in the sense that you know what to do with the knowledge in terms of the final decision.

If you've evolved the concept, conducted a feasibility demonstration, and done the system integration, you get into an element here which we have sort of taken as a new focus in our applications program in NDE, that is, the problem of transitioning to the field in a real way through field trials. We generally stop too soon with our programs. In our typical program we demonstrated the concept, made a few prototypes, then gave them to people and said, "Go to it." The real thing to do is to get these prototypes out there, get them used, and then deal with what are important reliability problems.

New devices are unreliable primarily because there are a lot of complicated electronics in some things. What we find out, unfortunately, is that the reliability of the device is, for quite a while, much worse than the reliability of the thing we are inspecting. Then, what drives it into widespread use? If we specify that people must use this integrated systems approach, then they need to be brought into the thing in a cultural sense. That is, people are generally suspicious of the new, are sure that it's going to give them trouble, don't know how to use it and aren't sure they want to.

While I have drawn this "deficiencies and additional requirements" line back toward the start, it would really be legitimate to draw a line to almost any stage in this process because deficiencies exist in all stages. I think, since in this meeting we are talking about that coherent research and technology base, that the continual input of what the real problems are and what the real deficiencies are, based on that kind of systems picture, is what is necessary so that you know you are part of something which is producing a real and valuable answer.
I'd like to go back to Fig. 10 on the "Elements of Technology Transition for NDE" again to emphasize my starting points and conclude my remarks. If we're going to maintain this coherent research technology base as a kind of continual source to pump the system which is driven by needs, I think it's critically important, of course, that the resources be maintained. The resources to continually support a generalized subject are hard to come by. Therefore, I think the future vitality of this particular kind of activity--not from the standpoint of having meetings, but from the standpoint of maintaining the momentum and the coherence which has been built up--has a lot to do with constantly injecting this particular arrow, the feedback from the real problems or needs. Also, I think the rump session which Don Forney held the other night with several of you on the C-5 problem, in which researchers from various institutions were brought together and brainstormed a real problem, is the greatest way to do that. So, I'd advocate a couple of things for the future for, at least, your consideration. One is that you "design-in" rump sessions like that--they won't be brought to these meetings which are well-defined and important. Line up a few people to talk about the problems. Another way might be to hold a meeting in the places where the guys use these things and get out there and walk around--that was always an eye-opener to me--seeing the environment. Although, while people are polite to you, if you go in on a one-to-one basis you soon get the feeling that you're not trusted which either turns you off and you leave the field entirely, or you say I'm going to beat this problem and I'm going to work with these guys. That's the sort of thing that gives you such great stimulation with regard to continuing your work and I think that resources follow. It's one thing to put in front-end money to get a community going. It's another thing to sustain it, and I believe the thing that will sustain it is constantly getting involved with those real problems and understanding your part in the overall scheme of things. That's why I tried to display that design system with the consideration of defects.

I'd like to stop here because I'm out of time anyway. Thank you.
Larry DeVries (University of Utah): What was the outcome of the cracked Minuteman story?

F. Kelley (Air Force Materials Laboratory): That's such a fascinating story that it would have taken all morning to go into it. What we found out was that it was really a fail-safe thing and we could live with those cracks. The analysis which was done—the fracture mechanics analysis, which is kind of tricky on a viscoelastic material, as Larry well knows—indicated that propagation of the crack would go longitudinally rather than radially and we did some over-testing in which we carved out four-inch-deep cracks in each of the star valleys and fired the motors and they fired normally. Our calculations showed that the cracks should not go more than an inch and a half deep; under the worst stress field they would grow longitudinally to the end of the motor. We over-cooled the motor and split it, and it did indeed grow to the end. Therefore, what we have is essentially verification of the analysis that says you can live with those kinds of cracks. Now there have been other kinds of problems while dealing with unbonding about which you come to a different conclusion.

K. Salama (University of Houston): I was glad to see that you mentioned training and education as parts of the progress of NDE. Do you know of any educational training programs on the university level?

F. Kelley: Let me say, honestly, that I don't know of any university program that purposely sets out to teach the subject. If there is, it's probably known to this crowd here. I know that there are kinds of training courses; maybe Don Forney could answer better than I. I think that you may or may not have been exposed to it. Dr. Burte and I did a survey of the science base in materials processing over the past year to year and a half, and visited many, many campuses to talk about what we perceived as the real needs in terms of unsolved problems for the Air Force. Nondestructive evaluation was brought out as one of the most critical science needs. By implication or directly, in that environment one talks about what the educational program is doing for this need. If any of you have started a program in this area, I think it would be important to bring it out. We have a number of people from the universities represented here. Has anybody got one...there's one.

(Speaker not identified): There are a number of survey courses, but the only degree program in NDE that I know of is at Lowell Technological Institute; Steve Serabian's program. It goes to a Master's level.

F. Kelley: Do you know much about that course?

(Speaker not identified): Other than that it's in the Mechanical Engineering Department, Lowell Technological Institute is in Lowell, Massachusetts, near Boston, and it uses all the NDE measurements. It seems to emphasize radiography and ultrasonics, but I have never taken the course.

F. Kelley: I was wondering if anybody used a type of systems concept that integrates the measurement of flaws with the eventual decisions that must be made about them, and whether or not a course had been structured like that.

(Speaker not identified): I know of a number of survey courses at RPI that are in development—having been given on a one-time basis—but I don't know of any formalized course of instruction such as you describe. There are several two-year schools that do this kind of thing, but they are more or less aimed at generating specialists and technicians. An example is Hutcheson Vocational Institute in Hutcheson, Minnesota.

F. Kelley: It might be useful to get someone who is attending one of the sessions for teaching those courses to describe them.