# Comments on 31st Wright Brothers Lecture

*Editor's Note:* Mr. Harper also presented the 31st Wright Brothers Lecture at a meeting of the Los Angeles Section of the AIAA on April 3, 1968. Following this presentation, representatives of the local aircraft industry made the following comments which, we believe, add to the significance of the Lecture in that they are thoughtful responses from the industry most concerned with the prospects for aeronautical research and development.

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As I read and reread the very excellent 31st Wright Brothers Lecture by Mr. Harper, I was struck with the simplicity, validity, and importance of his over-all message. Distilled to the simplest possible form, this message is as follows: 1) Take the great capability represented by aeronautics research personnel and equipment and apply it to the right problem areas. 2) Maximize the output and improve problem solution efficiency by analytical techniques wherever possible. 3) Recognize in doing all this that the most important problems may not be in individual areas, but may result from the attempt to integrate a number of individual areas.

These messages are not only fundamentally sound, but fully cognizant of current real-world problems. All of us can benefit from thinking about these messages and applying them in our daily activities.

Mr. Harper's paper points strongly to the enlarging future of aeronautics and the role that research and development must play in translating this future into specific accomplishments. As a comment on this paper and to add some thoughts, I would like to focus attention on the need for improved research and development facilities. Mr. Harper has clearly identified the needs for improvements in theoretical methods, but the same general comment can be made in the facilities area.

An analysis of future technical trends, based on designs already on the preliminary design drafting table, reveals some significant facts affecting facilities planning. Almost all classes of future aeronautical vehicles can be expected to be of substantially increased gross weight and linear dimensions as compared to those currently in operation. This is as true of rotary wing machines as fixed wing, and also applies to both commercial and military types.

Although the future trends in operating altitude and maximum speed are not as uniform as in the case of weight and size, there are numerous important vehicles of the future which will operate at higher speeds and higher altitudes, thus impacting development test facilities.

Perhaps one of the most important future trends in all types of air vehicles is that towards much higher levels of installed thrust and power. These increases not only affect the obvious propulsion test facilities, but have very important side effects in the areas of acoustics and mechanical drive systems.

In addition to the design and performance trends of future aeronautical vehicles, trends in the environment in which these vehicles will be developed also impact on our facilities requirements. Some of these trends include increasing acceptance of fixed-price developments subject to guarantees and warranties of progressively increasing depth. The substantial cost and time spans characterizing many of our major developments require increasing emphasis on minimization of development risk well before first flight. In addition to the direct influence of these factors on specific laboratory and test facilities, one of the areas receiving the greatest impetus is that of system simulation as a development tool.

Both the government and private industry must share the responsibility for the development of the necessary new facilities. The government should focus on large and expensive facilities outside of the reach of industry, facilities that can be regarded as national assets. Analysis of our technical needs vs capability leads to the conclusion that government interests at this time should be directed primarily at a high Reynolds number transonic wind tunnel and towards increased size and Reynolds number in a hyperthermal aerodynamic and structural test facility.

On the industry side, a broad spectrum of new facilities will be needed to develop future aeronautical vehicles. These include high Reynolds number transonic wind tunnels of moderate size, large low-speed wind tunnels, and more sophisticated flight simulators; facilities for static, fatigue, hyperthermal, acoustical, and mechanical system testing; and equipment for subsystem development, landing gear testing, and total mission simulation. Throughout many of these industry facilities must run the common thread of system integration test capability.

Although many facilities exist, they will not be adequate for the class of vehicles and missions we face, particularly in the development environment of our times. I wish to suggest that these new facilities must receive careful attention and planning if we are to realize many of the benefits predicted for the future of aeronautics by Mr. Harper.

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I appreciate the opportunity to publicly congratulate Mr. Harper for a needed and useful treatise. His subject is of great concern to most of us, and I hope his paper is widely read.

It is most gratifying to see air transportation recognized as a major force, making significant, catalytic contributions to worldwide social and economic evolution. It is further gratifying to hear a leader in the aeronautical research community speak forcefully of the need for our research planners to join efforts with others outside the immediate, relatively limited sphere of aeronautical technology to better understand the potential implications and ramifications of our research and development. I think most of us believe—whether we consciously identify the belief or not—that our work has import to something far broader and more meaningful than the details of the specific design to which we, as engineers, may be committed.

Although I completely agree with Mr. Harper that good, broadly based planning is indeed essential, I would urge caution to keep in mind that "plans" are not an end in themselves, and only become meaningful when they are made part of a vital, moving endeavor, continuously adjusted to accommodate changes in environment and the needs for which the plans were made. Planning is obviously never perfect; we simply do not have enough information ahead of time. As we gain experience and learn, the new information must be applied to our planning and the plans appropriately adjusted.

A case in point is the aircraft noise situation. Well before the commercial jet age began we were all familiar with jet noise from military aircraft experience. So we did indeed do some broad planning. We estimated the probable airport neighborhood noise levels. We examined the flight paths and the communities surrounding the major airports, to anticipate community noise exposure. We consulted long range planners and economists for forecast traffic growth, flight frequencies, and community expansion trends around the airports. We asked the psychologists "how noisy is noisy?" And we convinced ourselves that jet noise would be annoying; so we undertook research programs to reduce jet noise, and from the day commercial jet service began, the aircraft have been equipped with noise suppressors. As an industry we have spent more money on noise research than we could afford.

With the passage of time, there was indeed traffic growth. The fan noise coupled with sharply increased numbers of flights resulted in increased community annoyance, even though the sound levels of individual flights had been reduced. At the same time, more people became airport neighbors—there were no zoning laws to prevent them—and a lot more people became exposed to a lot more noise, and indeed, as was predicted, they did not like it.

Our planning for noise research has now escalated to the "scramble" stage. And we have lots of help from airport planners, community planners, government agencies at every level, lawmakers, civic groups, and just plain irate citizens. The noise problem is now a national issue.

The point of all this is that we did do our planning. It was as broadly based as Mr. Harper feels it should be. But we still have the problem. Broadly based or not, planning does not do it all. It still takes time, money, and an early start—in this case, for example, about 15 years ago. This is, and always has been, the essence of useful research—to have the courage to make big commitments, when the need is only dimly seen.

I believe, with Mr. Harper, there are many ways in which aeronautical research, as part of larger, broadly base objectives, can directly benefit mankind. There is every evidence that we will find many willing partners in the effort, and eager recipients of the results, in the United States and the rest of the world. It is indeed good to see the renaissance of aeronautical research within NASA.

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Mr. Harper has done an extremely competent job of elaborating the successes and failures of aeronautical research throughout the short history of aviation. The widening gap between theory and experimental research has been of increasing concern to many of us over the past decade. He has highlighted the manner in which this has occurred. The suggested positive actions for narrowing of this gap by applications of the systematic tools available to the scientist and engineer is indeed encouraging. There is no doubt that the creators of today's sophisticated air vehicles must share in the responsibility of solving certain of the problems created by this sophistication. Sonic boom and aircraft noise are but two of these problems.

The method of identifying future research and development by the systems analysis process leaves me with a certain uneasy feeling. The systems analysis process is able to identify the interactions of the socioeconomic factors with future technological developments in air transportation and thereby rationalize the areas for immediate concentration of specific research programs. Too often, as past experience has shown so well, the systems engineer and the economist become so enamored of their jobs and methodology that the final result only leads to the conclusion, "We must do some additional studying." The goals and objectives were lost somewhere between the fourth and fifth computer runs. And the understanding and insight disappears somewhat sooner.

I would like to point out an additional danger in the systems approach. As was mentioned in Mr. Harper's paper, we sometimes fall into a trap of accepting barriers (the heat barrier or the sonic barrier). If in systems analysis we accept certain apparent barriers and thereby concentrate our R&D effort, we not only fail to encourage the Stacks and Whittles, but actually make it almost impossible for them to obtain any support at all.

By these remarks, I don't want to degrade systems analysis as a powerful tool, especially if it is used with restraint to gain insight into the relative importance of various factors. The instance cited as to the relative value of cruise speed and descent time on the DOC at short stage lengths is a prime example of this. I do wonder just a little if the result is a product of methodology or the insight of the analyst.

Another thought occurs to me as I think about some of the points Mr. Harper has made. Perhaps we need to encourage the emergence of a group to really specialize in the interfaces function between the theorist and the experimenter. Such a group could help in identifying the findings of the theorist to the experimenter and, in turn, reflect the results of experimental investigation back to the theorist. It is only through these increased interactions that we can shorten or almost eliminate gaps such as between Hays' and Whitcomb's theory and experiment and achieve the benefits of the positive action steps that Mr. Harper has outlined