Reply by Author to Ernst D. Geissler

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GEISSLER'S comments are quite appropriate, and they indicate that such generalizations as were drawn in Ref. 1 must not be accepted without careful study for the specific application. In this regard, some information is available from the detailed study of a three-stage Nova-type booster vehicle made recently by Lockheed Missiles and Space Company.

The vehicle considered was 45 ft in diameter and 394 ft in length; the eight F-1 engines provided an initial thrust-to-weight ratio of 1.25. Vehicle dynamics analysis was based on an optimum ascent trajectory under a synthetic wind profile derived from the 99% design-wind criteria of NASA.[2] A simple autopilot with gain programmed to yield approximately constant natural frequency, but without angle-of-attack control, was found adequate for the requirements. Vehicle parameters for this design are summarized as follows:

First bending frequency	0.85 cps
Airframe-autopilot natural frequency f_0	0.12 cps
Maximum useful F-1 gimbal angle	5°
Damping (attitude rate gain)	critical

Under the design conditions, control was effected with a gimbal deflection not exceeding 2°, the vehicle angle of attack reaching a maximum of 11°. The effect of variations in the design parameters was studied, and stability analyses were made incorporating the first two body bending modes and first two slosh modes for each of the six tanks.

The following conclusions are quoted from Ref. 3:

> The [Nova] configuration is well controllable in the presence of synthetic wind profiles derived from the NASA 99-percent design-wind critiera.... Approximately 50 percent of the control capability is used, neglecting slosh and bending dynamics. Loss of an engine at maximum dynamic pressure results in a marginal tumbling situation with the design wind profile. This condition can be alleviated with an angle-of-attack sensor restoring controllability to a comfortable margin.
>
> For this vehicle, divergence begins below [an airframe-autopilot natural frequency] of 0.5 rad/sec. This fact suggests the vehicle can be satisfactorily controlled as long as the airframe-autopilot natural frequency exceeds 0.5 rad/sec... [In this case] approximately 46 percent of available control capability is used in NASA 99-percent design criteria winds.
>
> Rigid-body structural bending moments are not greatly influenced by the selection of control-system parameters except insofar as angle-of-attack is controlled. Variation of ± 50 percent about the nominal in the control gain [a_1] (engine deflection per unit vehicle attitude angular rate) has a negligible effect on the maximum bending moments....
>
> Control-system bending-slosh interaction stability can be assumed using a conventional control system... Tank baffles are not necessary either on the [first or second] stages except possibly for a baffle at the top dome of the second-stage LOX tank. The third-stage tanks require little damping to stabilize the slosh motion....
>
> Bending stability during first- and second-stage operation is assured by placing the rate sensor in the second-stage intertank area, combined with suitable electrical filtering and gain programming in the control computer. The problem is such that modest reduction of the first bending-mode frequency [may be] acceptable....
>
> Direct angle-of-attack control... is not necessary for this vehicle... except in the unlikely case that the most critical control engine is lost just before approaching the peak velocity of the NASA 99-percent design wind near the maximum dynamic pressure region of the trajectory. Nonetheless, angle-of-attack control has distinct advantages if gains can be programmed properly. Trajectory drift is reduced, thereby minimizing trajectory corrections required in upper stages, and air loads are minimized.

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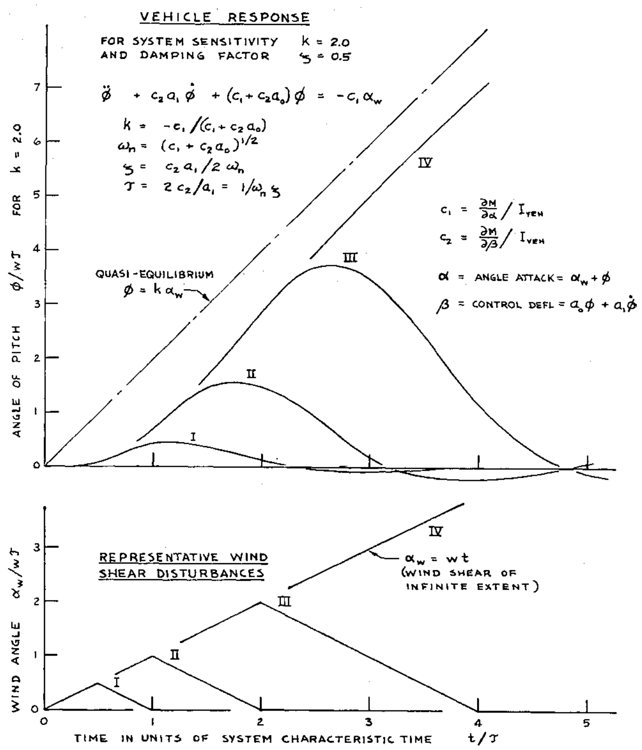

Fig. 1 Response of simplified vehicle-autopilot system to triangular-shaped wind inputs of various sizes

When comparing these results with the performance of smaller booster vehicles, allowance must be made for the increased control capability obtained by gimballing all rocket engines. It therefore seems fair to agree with Geissler that the control problem really has not been alleviated by the increase in size. On the other hand, there is no indication that vehicle size is limited by control considerations, as once may have been supposed. In fact, the comfortable margins indicated in this study together with the disappearance of design penalties associated with the propellant slosh problem are most encouraging for even larger booster designs.

The fundamental question raised by Geissler as to whether the minimum required control frequency f_0 is proportional to $1/L^2$ or $1/L$ depends on whether the vehicle finds the wind disturbance to be transient in nature, as a gust, or infinite in extent, as a wind shear. In the first case, the ratio of characteristic length of the vehicle to that of the environmental disturbance raises the scaling factor from $1/L$ to $1/L^2$. The actual conditions lie somewhere between these oversimplified forms, and the question remains as to which is more realistic for boosters of the near future.

The problem can be approached by considering the simplified dynamic response of an inertially stabilized vehicle, as illustrated in Fig. 1 in nondimensional form, under transient wind disturbances of idealized triangular shape. If the total angle of attack $(\alpha_w + \phi)$ is compared with the input wind angle $(\alpha_w)_{\max}$, summation for the several cases in Fig. 1 indicates that the peak value of $(\alpha_w + \phi)/(\alpha_w)_{\max}$ diminishes progressively as the system time constant is increased. This size effect, favorable to larger vehicles, would be superimposed on the relation $f_n = k/L$ of dynamically similar devices.

The Nova vehicle described in the foregoing has a characteristic time constant τ of about 2 sec and passes through the critical wind spike in approximately 8 sec or 4τ. The wind loading thus corresponds approximately to condition III in Fig. 1, in which case, as Geissler predicts, scaling laws would be established adequately by quasi-steady state considerations.

Perhaps for vehicles larger than Nova the effects may appear more transient in nature, in which case the control frequency will scale somewhat more favorably than $1/L$. This simplified apprasisal must, of course, be modified by system considerations and trade-offs.

References

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[3] "Lockheed Nova study program, progress report no. 3," Lockheed Aircraft Corp., LMSC-895215 (July 3, 1962); confidential.

Comment on "Response of Nonlinear Flat Panel to Periodic and Randomly Varying Loadings"

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Lin[1] presents an interesting analysis of the dynamic response of a flat plate when the nonlinear effects of the membrane forces are significant. Unfortunately, the author has neglected to reference several previous publications presenting results pertinent to this problem, some of which are listed here.[2] Also, the author's treatment of the random-excitation case deserves some comment.

The equivalent linearization technique used seems to be of questionable value in this case, since an exact solution for the mean-square response (as well as other statistical measures) of the randomly excited cubic system is available.[3, 4] It is true that the exact solution is limited to the case of white-noise excitation; however, the limitations of the equivalent linearization method imply much the same restriction.[5] That is, the "true" linear frequency and the equivalent linear frequency cannot be far separated, and the spectrum of the excitation normally may be considered constant over this restricted frequency range. In any case, it is not clear from the article why the author feels that the results of this approximate technique are valid for the case of nonwhite excitation.

The statements concerning the "effective transfer function" also may be questioned. Not all people who are working on the problem of nonlinear panel response would agree that physical occurrence of the upper branch of the nonlinear response curve is as unusual as the author implies.[6] Certainly, if the response of a panel does lie on the upper branch, the effect of damping may not be considered unimportant.

References

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Author's Reply to Comment by R. J. Herzberg

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Herzberg has listed several recent papers that the author is accused of having neglected in Ref. 1. However, the present author regrets that Herzberg has overlooked the fact that, except for Ref. 2, the other additional references were not available at the time Ref. 1 was submitted for publication. Moreover, the author had no desire to compile a complete bibliography. For example, on the method of equivalent linearization, reference was made to the work by Caughey, which generally is recognized to be one of the earliest, without tracing back to the original idea of Kryloff and Bogolinoff.

The author disagrees with Herzberg that equivalent linearization implies the same restriction as required in the "exact" solution, which, incidentally, may be traced to the work of Kramers in 1940. He also disagrees that the equivalent linearization technique is of little value. The assumption of white-noise excitation is a very strong one and is, strictly speaking, physically unrealistic. Nevertheless, this strong assumption is the basis for treating the response of a mechanical system as a Markoff random vector process in the phase plane. Thus the mathematically exact solution is founded on a physical idealization of an extreme nature. Even when the excitation is a *truncated* white noise, this solution is at best an approximation, since there will be no justification to regard the response as Markoffian.

In Ref. 1 a trial-and-error procedure was suggested so that the method of equivalent linearization might be applied to excitations of varying power spectra. The convergence of this procedure requires only that the forcing power spectra be varying slowly in the frequency range of interest, which is considerably less restrictive than requiring the excitation to be strictly white.

One must not forget that, unless the excitation is strictly white, a truly exact solution for the nonlinear response is not known at the present time. Therefore, it seems strange that Herzberg questions the validity of the linearization method but accepts the so-called exact solution under a condition on which the solution is not based. In contrast, the author believes that the two methods help to substantiate one another when a more realistic excitation is considered.

Unfortunately, the question concerning the effective transfer function also is misunderstood. The author clearly stated in Ref. 1 that the upper branch response *can* be produced under controlled experiment; namely, by gradually sweeping the excitation frequency, as was reported in Ref. 5. However, if the frequency of excitation is *fixed,* then the response of the lowest energy level should prevail.

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