ments) longitudinal PIO's involving attitude control only are essentially impossible.† However, in variable-stability flight testing and in ground-based simulation studies where the general practice is to hold Z_w constant and vary ζ_{sp}, ω_{sp}, stick force and displacement per g, etc., artificial relationships between $\zeta_{sp}\omega_{sp}$ and $1/T_{\theta_2}$ can lead to PIO's of the simple type under consideration here. For example, Fig. 2 of A'Harrah's paper,[1] reproduced here (also as Fig. 2), shows the cross-hatched PIO "limits" for the lower left region of the ζ,ω plane. The data were obtained for a fixed value of Z_w corresponding to $1/T_{\theta_2} = 3.22\ \mathrm{sec}^{-1}$ for all the conditions tested. The theoretical boundary for zero ζ_{sp}' as given by Eq. (5) is superimposed on the original plot. It may be seen that there is general agreement between the predicted possible "simple" PIO region to the left of the boundary and the observed region. The fact that the experimental region for very light stick-force gradients lies somewhat to the right of the theoretical boundary is evidence of additional dynamics—in this case probably nonlinear effects due to the high breakout force to stick gradient, 1.2 lb/1.0 lb/g.

Although the PIO region of Fig. 2 is thus shown to be dependent on artificial relationships between $1/T_{\theta_2}$ and $\zeta_{sp}\omega_{sp}$, there *are* flight test examples of PIO's in precisely the same region (e.g., Refs. 5 and 6). The degree to which all such situations are dependent on control system contributions, linear or nonlinear, is not exactly known. Nevertheless it appears to be true that longitudinal PIO's were nonexistent (or not reported) until the advent of modern hydraulically powered elevator actuation systems. The PIO reported in Ref. 6 can in fact be traced directly to the *linear* (lag) contribution of the hydraulic system, which was measured and reported. It appears therefore that, except for very unusual configurations, longitudinal PIO's can be sustained only for conditions in which control system dynamics are a contributing cause.

As a final observation it must be emphasized that the foregoing is but one example of improper simulation due to inadequate consideration of numerator dynamics; there are many other examples in the pertinent literature (and the writer's own organization is to blame for at least one of these). The specific importance of the numerator dynamics associated with aileron control of bank angle has been theoretically and experimentally established[3] and is not likely to be forgotten. But the short shrift usually given to the proper simulation

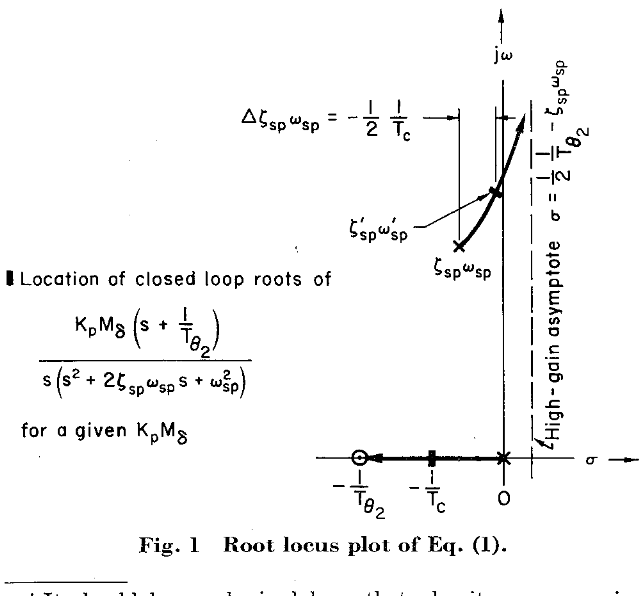

Fig. 1 Root locus plot of Eq. (1).

† It should be emphasized here that, despite accompanying vertical accelerations, attitude cues will be those the pilot primarily uses in attempting to get out of a PIO situation, so the foregoing considerations are valid. Acceleration inputs introduced by the pilot's arm bobweight effect acting through control system friction could be a nonlinear destabilizing influence.

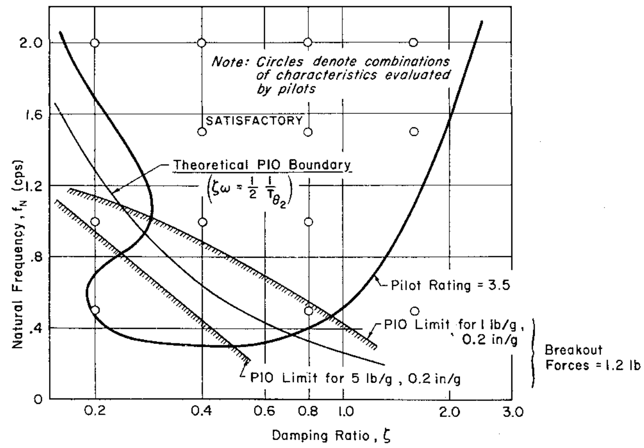

Fig. 2 Comparison of theoretical and observed PIO limits.

of numerators in general continues to give anomalous, misleading, and generally inapplicable handling quality results. It is the writer's hope that the fairly dramatic example exposed here will stimulate more widespread interest in simulating the complete numerator-denominator vehicle characteristics.

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Reply by Author to I. L. Ashkenas

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The following is a rebuttal to the critical review by Irving L. Ashkenas of the Pilot-Induced Oscillation (PIO) boundaries defined from the dynamic flight simulator research results reported in Ref. 1. The word rebuttal implies an argument which is indeed the intent, though let it be first understood that this author agrees with Ashkenas on the two points developed at length in his critique, namely 1) that for reasonably configured airplanes with negligible control system dynamics and negligible nonlinearities, PIO's involving attitude control only are impossible for pilots who perfectly synchronize their corrective control with the aircraft's attitude change; and 2) that the value of $1/\tau_{\theta_2}$ used in the simulation program is not numerically consistent with the level of short period damping for two of the evaluation points ($\zeta = 0.2$ at $f_n = 0.5$ and 1.0 cps). However, to state that on the basis

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of these two facts there existed a deficiency in the study technique and to imply that the resulting PIO boundaries are therefore invalid, is completely unjustified.

Point number one is true but pertinent to the argument only if the hypothesis "that perfectly synchronized pilots were used in the experiment (or more important, that all pilots will synchronize their inputs during the incipient PIO) and that they were controlling with the pitch attitude loop" is valid. Thus, to paraphrase an old adage, to wit, "the proof of the pudding is in the hypothesis," is considered apropos. Certainly some pilots are able to synchronize their control or, for that matter, adapt a stabilizing lead equalization form, which accounts for some pilots being less susceptible to PIO than others. However, to assume that all pilots will accomplish such adaption in the relatively short period prior to exceeding the design load factor during a PIO does not appear warranted based on any information with which this author is familiar.

Moreover, the assumption that pilots are always flying a pitch attitude loop when they experience a PIO is based on little more than the fact that Ashkenas emphatically, and twice, states this to be the case. There is no question that pilots use pitch attitude as an important cue, particularly in low-speed flight. However, for the flight conditions under discussion here, the aircraft attitude change is extremely small relative to changes in normal acceleration or rate of climb. It is therefore more logical to assume that the pilot is flying a normal acceleration or rate of climb loop during maneuvers conducive to PIO. For the simulator investigation, the pilots received attitude cues from a standard all-attitude indicator, acceleration cues from both the simulator motion and a signal displayed on a cathode ray tube, and instantaneous rate of climb (substitute for the relative wing tip motion with wing-man in formation flying) from a cockpit display meter. The pitch attitude change during a 1 g transient was less than 1°, thus rendering the all-attitude-indicator pitch indication useless to the pilot for the type of precision flying during which a PIO might be excited.

Considering the load factor or rate of climb loop in terms of the synchronized pilot model (the author does not subscribe to this approach) would show that the load factor loop could not be driven unstable by the pilot without the inclusion of the control system actuator lag but that the rate of climb loop could be. Further, if the pilot is allowed his normal reaction time and neuromuscular delay of 0.2 sec, then the pilot gains required to drive the load factor and rate of climb loops unstable, and the respective crossover frequencies are relatively insensitive to the actuator time lags over a reasonable range. Therefore, the conclusion could be made that actuator dynamics are not important for an unsynchronized pilot. That conclusion will not be drawn here. The point of import is that a mathematical describing function for the pilot can be obtained showing the airplane-pilot loop to be unstable in a manner consistent with the experimental results, and this pilot describing function is consistent with the forms used by Ashkenas et al. in their many works. Why the very singular, and to this author, very unrealistic synchronous form of the pilot transfer function should be used in an effort to ferret out a purported anomaly, is not at all understood.

The second point of Ashkenas' criticism shows the incompatibility between the numerator lead term ($s + 1/\tau_{\theta_2}$) in the θ/δ transfer function with the level of short period damping coefficients, when interpreted in terms of the physically realizable aerodynamic characteristics. It would certainly take an unusually configured aircraft or a negative gain on a pitch damper to achieve the exact numerical combinations used. However, before getting all wrapped up in the numbers game, a review of the results of varying the lead term in question, is in order.

Had the original experiment included variations of $1/\tau_{\theta_2}$ consistent with the levels of vehicle damping tested, then the lowest value of $1/\tau_{\theta_2}$ would have approached 1.0. Therefore, the influence of $1/\tau_{\theta_2}$ in the region from 1.0 (the lowest value needed for consistency) to 3.22 (the value used) is of interest. Investigations,[2,3] that include results from Ashkenas' own organization, have shown that the pilots are completely insensitive to variation of $1/\tau\theta_2$ in this range. These results appeared logical particularly for low-altitude, high-speed flight, since as stated previously, the vehicle rotation required to obtain a given load factor is extremely small, and the pilot will fly either a load factor loop or a rate of climb loop, neither of which is influenced by the numerator lead term (i.e., the aircraft's load factor or climb response to the pilot command is not altered by $s + 1/\tau_{\theta_2}$ in the pitch attitude transfer function). This was the reason that $1/\tau_{\theta_2}$ was not varied in the original study and it is still felt to be valid. There is no question that flight simulation must include proper consideration of the numerator terms, and proper consideration was given to them.

The final argument concerns the implication that the PIO is primarily a result of the control lags associated with fully powered control systems. Certainly, the advent of hydraulically powered systems introduced time lags, but they also introduced the need for artificial feel, "bob" weights, stick dampers, nonlinear springs, gear shifters, etc., etc. Fully powered systems took away the infinitely variable spring rate inherent in the unpowered systems and in its place a fixed spring rate, artificial feel bungee appeared. Thus, gone is the control that could be considered to give a reasonably constant level of stick force per g. In its place is a control which, if designed so that the pilot can get the airplane off the ground without pulling with both hands, a good sneeze with the pilot's hand on the stick will probably overstress the aircraft at high speeds. Incorporation of all kinds of gadgets to help alleviate this problem has been necessary, and on occasions these gadgets have had destabilizing effects on the pilot-aircraft-control loop.

Therefore, reduction in the force gradients and/or the destabilizing effects from bob weights, etc., are considered to be the primary causes of PIO tendencies stemming from fully powered systems and not the actuator time lag, per se. It is worth noting that the PIO discussed in Ref. 4 was not tied directly to the actuator lag contribution by the authors of the report, but evidently results from the synchronized-pilot, pitch-command hypothesis.

In conclusion, the PIO results presented in Ref. 1 are still considered to be valid guides that have some flight verification. Although no single criterion can be expected to cover all of the complex combinations of pilot-airframe-control system dynamics, particularly with extreme nonlinearities, it is felt that the proposed criteria represent a reasonable approach. It is recommended that future analytical work on PIO's include the more general form of the pilot transfer function rather than the very restrictive synchronous form, and that investigations include considerations of the load factor and rate of climb loops. The comparison of analytical results with experimental data should improve considerably.

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