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Reply by Authors to R. H. Smith

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THE objective of the comment by R. H. Smith seems to point out that the controllability limits as given by Refs. 1 and 2 disagree widely because of the difference of practice, and that predictions of the limits of manual control made by Ref. 1 are not capable of yielding the limits of Ref. 2. It is noted at the beginning that we shall confine subsequent discussions to second-order controlled elements with positive static stability, namely, with positive stiffness, unless otherwise stated.

Replying to the first part of the comment, we agree that practice is an essential factor for explaining the discrepancy, although it should be remembered that no system input is present in the experiment of Ref. 1, whereas random inputs are present in that of Ref. 2. As mentioned in Ref. 1, we conducted experiments for obtaining the difference between the controllability limits at one trial and at three trials and concluded that the results indicated, evidently, the effect of practicing.

The controllability limit of a human operator may depend on many factors such as practice, his naiveté, or experience as an airplane pilot, and so forth. Consequently, it is considered indispensable when presenting data on the controllability limit to prescribe conditions fully under which experiments have been conducted. We believe that our data show *a* controllability limit under the prescribed test program of Ref. 1, namely, a controllability limit at one trial and at three trials. We note that our data agree very well with those obtained by NASA in Ref. 3.

On the other hand, we find in Ref. 2 that the two controllability limits, the establishment of which required nearly 900 trial runs, show clearly the effect of heavy practice. This is *a* controllability limit established by Smith. It seems probable that a more sophisticated technique would further improve the controllability limit established by him. We note here that unpublished data obtained recently at National Aerospace Laboratory of Japan also show that the controllability limit can be improved by heavy practice far beyond that of Ref. 1.

In replying to the second part of the comment, we agree that a transfer function, which is more complicated than Eq. (3) of Ref. 1 must be employed in predicting the limits of Ref. 2. The purpose of writing Ref. 1 was this: we assumed that the transfer function of a human pilot was given by Eq. (3) and that the pilot could conduct a self-adaptive control. Then, we made a speculation to find how our prediction could correlate with our experimental data. During the prediction, we were satisfied with obtaining a rough idea of the controllability limit by taking $T_L = \infty$, although it was possible to take an assigned value for T_L. We believe that Eq. (3) is an appropriate transfer function of a human pilot who is trying to find a controllability limit on a "first-encounter basis."

Received July 30, 1965.

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A brief mention is made here on an operator's behavior under heavy practice. Since there exists a periodicity in transient response of the controlled element with positive static stability even if the response may diverge, the operator is aware of the frequency and time to double amplitude of the response during a series of the practice. There exist indications that have led us to believe that, as the practice proceeds, the operator can improve the controllability limit by taking the response characteristics into consideration and employing a second-or-more higher-order lead equalization or the so-called "quasi-precognitive" technique. Consequently, we may conclude that the operator under heavy practice is employing a more sophisticated technique than that expressed by Eq. (3).

The second-order lead equalization technique may probably be favorably employed only within some region of the static stability and damping. For example, too small or too large values of positive static stability may be unfavorable for an operator who wants to employ the improved technique. On the other hand, this technique may not be so favorably applicable to controlled elements with negative static stability as it is to those with positive static stability, since the transient response in the former is aperiodically divergent.

The authors wish to thank Smith for the comment and agree with him in stating that the effect of practice on the controllability limit of a human operator is a very important subject and in hoping that more research will be conducted to clarify its mechanics in the future.

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Combustion Instability in Gas Rockets

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ROCKET motors burning premixed gaseous propellants have been used by several investigators[1–3] to study the longitudinal mode of high-frequency combustion instability. The experimental results of these investigators differ in one important aspect. Zucrow and Osborn[2] and Tsuji and Takeno[3] have observed one region of unstable operation located around the stoichiometric mixture ratio. Pelmas et al.[1] have found two unstable regions, one on either side of stoichiometric.§ This apparent difference in the observations of these investigators has resulted in some uncertainty

Received July 16, 1965. Research sponsored by Air Force Office of Scientific Research, Office of Aerospace Research, U. S. Air Force, Contract No. AF49(638)-1268.

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§ An unstable region is a region in the equivalence ratio-combustion chamber length plane or equivalence ratio-combustion pressure plane where combustion pressure oscillations are observed.

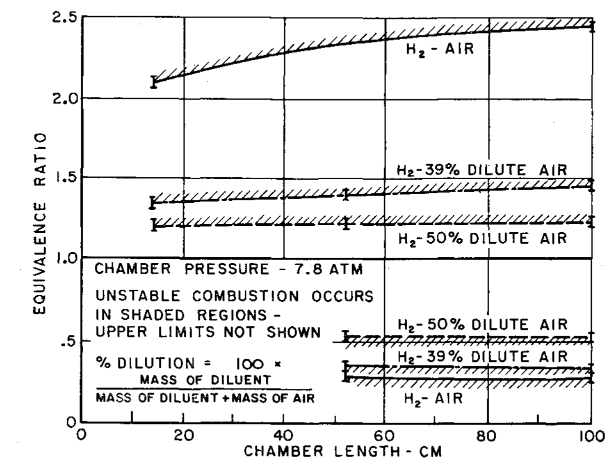

Fig. 1 Longitudinal mode instability regions for hydrogen-dilute air mixtures.

in the interpretation of instability data obtained from gas rockets.

Recently the difference has been resolved in terms of a model in which the driving mechanism for instability is related to chemical kinetic factors.[4] In this model, the longitudinal stability limits are defined in terms of a critical value for a rate parameter E/RT (E = an over-all activation energy for the propellant combination, T = combustion temperature, and R = gas constant). For a given propellant combination E may be assumed constant, and, hence, the stability limits are defined in terms of a critical combustion temperature, T_{crit}. For $T < T_{\text{crit.}}$, the combustion is unstable, and for $T > T_{\text{crit.}}$, the combustion is stable.[4] It is concluded, therefore, that there will be two unstable regions, one on either side of stoichiometric, whenever the maximum combustion temperature for the propellant combination is greater than T_{crit}. When the maximum combustion temperature is less than T_{crit}, one unstable region, located around stoichiometric, will be observed. For propellant combinations for which two unstable regions are observed, it is possible to collapse the two regions to a single region through the addition of an inert diluent. The diluent reduces the over-all range of combustion temperatures, and thus causes the stability limits to shift toward stoichiometric. In the experiments of Fig. 1, increasing amounts of nitrogen were added to hydrogen-air, and, as predicted, the stability limits approach one another until finally the two regions coalesce. The combustion temperature at the stability limits is found experimentally to be approximately 1400°K for all degrees of dilution. For the case when the two unstable regions coalesce (60% dilute air), the maximum combustion temperature is somewhat less than 1400°K. Figure 2 shows similar results for methane-dilute oxygen mixtures. For methane-($0.4O_2 + 0.6N_2$) two

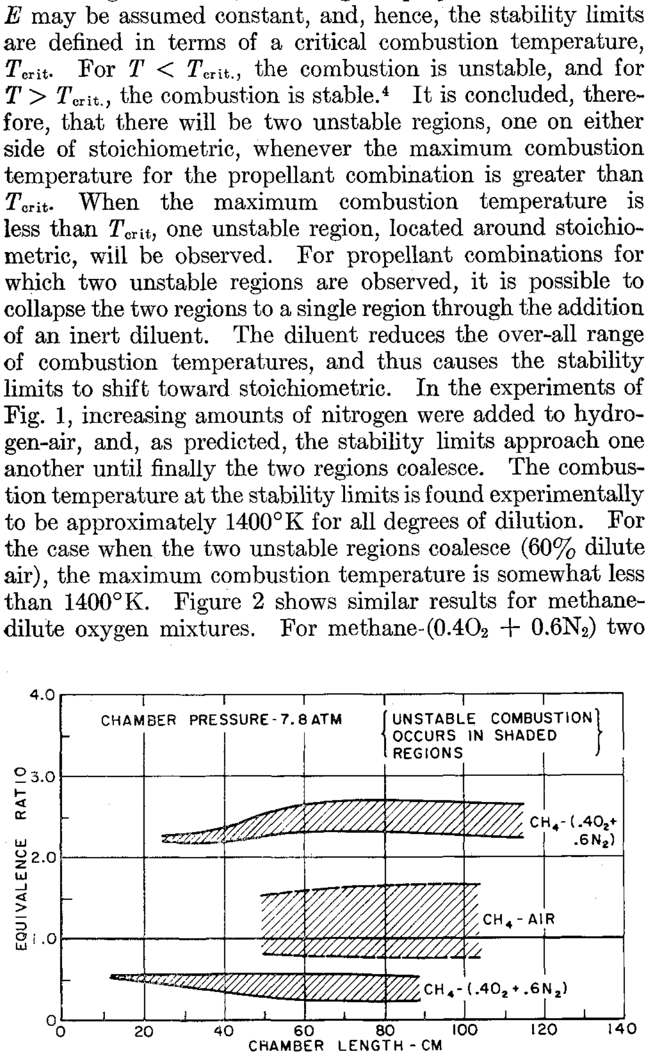

Fig. 2 Longitudinal mode instability regions for methane-dilute oxygen mixtures.

unstable regions are found, with a combustion temperature at the stability limits of approximately 2200°K. For methane-air one unstable region is observed, and a maximum combustion temperature somewhat less than 2200°K is measured. It is probable that one unstable region is characteristic of the combustion of gaseous paraffins and air, since the over-all activation energies and maximum combustion temperatures are approximately the same. One unstable region has been observed by Zucrow and Osborn[2] for methane-air, ethane-air, and propane-air.

In conclusion, it is seen that the observations of the various investigators with respect to the number and location of unstable regions are consistent, and that the observations can be explained in terms of an instability model in which the driving mechanism depends on chemical kinetic factors.

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Comment on "Proposal Concerning Laminar Wakes behind Bluff Bodies at Large Reynolds Numbers"

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THERE has been considerable interest in the determination of flow patterns at large Reynolds numbers, when the motion is completely steady. Under these circumstances, flows are required to satisfy the Navier-Stokes equation without regard to questions of the stability of the flow; features characteristic of turbulent motion are excluded from discussion. One configuration of general interest is that of a bluff body held in an infinite, steady stream. Batchelor[1] reviewed this problem and gave strong reasons for believing that the region behind the body is one where the streamlines are closed. This feature is observed in solutions and in visualization at low Reynolds numbers. Batchelor also discussed the form that this closed region, or bubble, would take as the Reynolds number tends to infinity. He concluded that the bubble remains finite in length. An essential part of his argument in support of this model was that the bubble contains an extensive region in which viscous forces are small (in the boundary-layer sense). This work of Batchelor was published shortly after a finite-difference solution of a related wake problem by Allen and Southwell[2] for Reynolds numbers of 10, 100, and 1000. More recently, Acrivos et al.[3,4] have published measurements for wakes behind two-dimensional bluff obstacles and have suggested that the steady laminar wake has a length increasing linearly with Reynolds number. The data on which the latter suggestion is based include measurements taken with a stabilizing plate in the wake region, and the steady wake flow in its presence cannot be identical with that in its absence, so that it may have influenced the variation of length with Reynolds number. The purpose of this note is to outline an alternative proposal, which has its

Received June 28, 1965. The work reported was supported in part by Contract No. AF 33(616)-5756.

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