In summary, the recent Notes[1,2] have 1) developed some frequency relations that were presented in the original paper, 2) proposed mode coupling arguments with an analytical model in which mode coupling is impossible, 3) rediscovered resonance and apparently confused concurrent resonance in two modes with coupling between modes, 4) in the process explored a model whose boundary conditions are completely different from the problem to which the analysis was ostensibly directed, and 5) has apparently confused the previously reported peaks in quasi-steady pressure with occurrence of increased amplitude of pressure oscillations.

References

[1] Temkin, S., "Mode Coupling in Solid-Propellant Rocket Motors," *AIAA Journal*, Vol. 6, No. 3, March 1968, pp. 560–561.

[2] Temkin, S., "Large-Amplitude Transverse Instability in Rocket Motors," *AIAA Journal*, Vol. 6, No. 6, June 1968, pp. 1202–1204.

[3] Crump, J. E. and Price, E. W., " 'Catastrophic' Changes in Burning Rate of Solid Propellants During Combustion Instability," *ARS Journal*, Vol. 30, No. 7, July 1960, pp. 707–707.

Reply by Author to E. W. Price

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In the preceding Comment, Price has raised several objections to my recent Technical Notes[1,2] on large amplitude instability in rocket motors. His most important claims are that the Notes contribute nothing original to the literature and that they are not clearly relevant to the phenomenon I sought to study. It is my opinion that these claims are not valid, and that most of the objections raised are inapplicable.

Inasmuch as originality is questioned, it is appropriate to state here that the striking findings of Crump and Price[3] were fully acknowledged in both of my notes. In fact, the notes were motivated by their experimental results. However, one must differentiate between the findings of Crump and Price and those presented in Refs. 1 and 2. Crump and Price found experimentally that when the catastrophic pressure peaks occurred, the frequency of the transverse modes was an integer multiple of an unstable longitudinal mode. To show these results explicitly they plotted, in Fig. 4 of Ref. 3, the frequencies of the first two transverse modes of oscillation (as given by the well-known formula $f_{ms} = a_0\alpha_{ms}/2R$) vs the cavity radius. Of the many points shown on these plots, only four are experimental points; they give the frequencies at which catastrophic pressure peaks actually occurred. All of the other points (which I incorrectly identified as data points) merely show some frequencies at which the longitudinal and transverse modes would be related harmonically. With reference to the experimental points, Crump and Price stated that such striking results could be explained only by means of a nonlinear mechanism. They did not state why those specific integers of multiplicity were found, nor did they present any analysis showing that large pressure peaks would indeed occur under the experimentally found conditions.

In Ref. 2, I used a very simple analytical model to show that the empirical equation I had presented in Ref. 1 was indeed a condition for high-amplitude oscillations in cylindrical cavities. I considered there the simplest case where such large-amplitude oscillations would arise; namely, outgoing waves in a cylinder driven at one end by a piston oscillating (at a frequency ω) along the cylinder axis, and with a velocity that depended only on the radial coordinate. The case when there are reflected waves and where the piston velocity depends also on the angular coordinate gives basically the same result, i.e., the acoustic pressure is (except for a constant) given by

$$P\alpha \sum_{m,s} \frac{\cos(m\theta)\,\sin(\omega t) J_m(\pi\alpha_{ms}r/R)}{\sin k_x L[\omega^2 - (\pi\alpha_{ms}a_0/R)^2]^{1/2}} \cos k_x(x - L) \qquad (1)$$

where $k_x{}^2 = (\omega/a_0)^2 - (\pi\alpha_{ms}/R)^2$. Clearly, large amplitudes of oscillation will result under some conditions of interest. The case $k_xL = N\pi$, $N = 1, 2, \ldots$ gives longitudinal resonance and leads to wave amplitudes that are limited either by dissipation effects or by shock-wave formation, and therefore cannot result in very large amplitude oscillations. The other possibility for large-amplitude waves is provided by cutoff conditions, i.e., if the piston frequency is $\omega = \pi\alpha_{ms}a_0/R$. (For a given frequency, this condition may occur if R varies, such as in a radially burning solid-propellant rocket motor.) At this frequency, the oscillations will be purely transverse ($k_x \to 0$). One can, in fact, say that the transverse oscillations grow at the expense of the longitudinal modes. Also, the amplitudes of the transverse waves would, at this frequency, be very large since, as Maslen and Moore[4] have shown, the transverse waves are not limited by shock formation as in the longitudinal-wave case. One would therefore expect very large pressure peaks if the foregoing condition is satisfied. In particular, if the piston frequency is given by $\omega = n\pi a_0/L$, n an integer (i.e., the nth resonant frequency of a cavity of length L with no transverse waves) one finds that the condition for large amplitudes reduces to $n = \alpha_{ms}(L/R)$ (Eq. 1 of Ref. 2). In fact, if one substitutes in this equation the values of L and R from the experimental catastrophic pressure peaks found by Crump and Price, one finds exact agreement between the calculated value of n and the integers of multiplicity found experimentally. Consider, for example, test number 690 (Fig. 4 of Ref. 3). The value of the cavity diameter at the instant when large pressure peaks occurred in the first transverse mode was found to be approximately equal to 0.64 in., and the (fixed) length was 10 in. With these values of R and L, and with $\alpha_{10} = 0.586$, one finds $n = 18$; this is the value found experimentally by Crump and Price. Similar results are found for the other experimental points. Surely, one cannot discard as irrelevant analytical predictions that agree with experimental observations.

Price also has raised several objections to the model I considered in Ref. 2, and to my interpretation of the phenomena involved. The model, in fact, does not take into account burning of the propellant, mean gas flow, and other effects that were present in the actual experiments, but considers, as done by other authors, oscillations in a "cold" cavity. This approach is not entirely unrealistic since the cavity of a rocket motor acts as an acoustic resonator with unstable modes of oscillation identically equal to those predicted by acoustic theory for cold cavities. In the actual case, these modes are excited by the energy released during burning. The manner in which this energy transfer takes place is a most serious problem and is not considered in my Notes. However, the characteristic modes of oscillation of a cavity can be excited by many other means, such as a piston (as done in my Note), a gas jet, heat addition, etc. The model used in my Note is, therefore, a useful one since it provides excitation of acoustic modes in a cylindrical cavity with very simple mathematical arguments. Clearly, such simple analysis cannot answer all important questions that arise with regard to the actual experiments of Crump and Price. An example is provided by the fact that the analysis, although predicting very large amplitudes of oscillation under certain conditions, does not predict a net buildup of the mean

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pressure in the cavity. More important, the analysis does not indicate how such high frequencies may be excited in the actual case. One possible conjecture is that the more unstable, lower-order longitudinal modes scatter energy to the higher-order modes through the nonlinear convective terms in the gas equations of motion.

Another objection is that my Notes take no account of the presence of transverse and longitudinal modes before the pressure peaks occurred. Although it is true that no specific mention of this was made, it is clear that the solution allows for longitudinal and transverse modes to coexist simultaneously without large pressure oscillations unless the conditions for large amplitudes are satisfied.

Price also comments that mode coupling cannot occur under linear theory. This is completely correct if by a mode is meant the set of oscillations implied by an equation such as Eq. 1 for given values of m and s. However, it is clear that in my Notes the word "mode" was used to identify the various types of gas oscillations in the cavity (i.e., longitudinal and transverse). Furthermore, from Eq. 1, it is clear that for a given frequency, these types of oscillation are not independent of each other, i.e., they are "coupled."

In conclusion, the recent Notes[1,2] have made use of a cold-cavity model to derive some conditions for large amplitudes of oscillation in cylindrical cavities. These conditions do not appear elsewhere in the literature, and give exact agreement with experimental results obtained in solid-propellant rocket-motor research.

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[1] Temkin, S., "Mode Coupling in Solid-Propellant Rocket Motors," *AIAA Journal*, Vol. 6, No. 3, March 1968, pp. 560–561.

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[3] Crump, J. E. and Price, E. W., " 'Catastrophic' Changes in Burning Rate of Solid Propellants During Combustion Instability," *ARS Journal*, Vol. 30, No. 7, July 1960, pp. 705–707.

[4] Maslen, S. H. and Moore, F. K., "On Strong Transverse Waves without Shocks in a Circular Cylinder," *Journal of the Aeronautical Sciences*, Vol. 23, No. 6, June 1956, pp. 583–593.