Reply by Authors to R. L. Glick

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WHETHER or not the potential flow region exists in real rocket motors will depend on factors such as initial conditions, grain geometry, and even igniter gas flow characteristics. However, in the case of high performance rocket motors, we believe that strong convective forces inside a rocket motor establish a boundary-layer flow over the propellant surface for which both developing boundary layer and the potential core region exist. Our model in Ref. 1 has been based on this physical picture. As far as experimental evidence on the nature of the flowfield cited by Glick, it should be noted that these experiments were conducted under nonreactive "simulative" flow conditions. However, the boundary layer in an actual grain port may be different due to differences in temperature, pressure, chemical reactions, etc.

Furthermore, we have reviewed the results of the simulative study by Yamada et al.[2] and find no evidence or conclusion in regard to the nonexistence of the potential core or the production of turbulence near the centerline. In boundary-layer flows, it is well known that turbulence is produced mainly by mean shear in the near-wall region. This was also observed in the experiments of Ref. 2. Yamada et al.[2] further point out that the role of turbulence adjacent to a propellant surface is to enhance mixing rate of decomosed gases and increase the heat transfer. This is precisely what our model predicts and is also part of the erosive burning mechanism suggested by our study. It may be noted that our model can take into account a nonzero freestream turbulence level (which may be present as a result of initial conditions such as igniter gas flow characteristics) and its spread within the boundary layer through the boundary conditions, Eq. (27) in Ref. 1.

Glick speculates that the flowfield *may* contain four subdomains. The argument presented in regard to the interaction between negative erosive burning and the initial flow domains is not convincing. It is true that our model does not predict negative erosion. The reason we do not consider negative erosion is that for most propellants now being used nowadays, this phenomenon is not observed. Some older propellants using polyurethane binder do exhibit negative erosive burning. However, according to Langelle,[3] this is believed to be due to the covering of the AP particles by the molten binder at the surface and is related to the plateau strand-burning effect exhibited by those propellants. Furthermore, experiments of Marklund and Lake[4] show that the type of flowfield ("subdomains" (i)-(iii) and large scale turbulent surges) is not likely to be responsible for negative erosive burning. Indeed, their experiments conducted under clearly established boundary layer conditions, with no large scale turbulent surges present, showed negative erosive burning on a polyurethane propellant. Therefore, we disagree with Glick's hypothesis that negative erosion is a result of the effects of initial flow "sub-domains" or turbulent surges.

In regard to the comment on the theoretical work of Ref. 5, we find no reasonable combustion modeling of the reaction processes in the model. Also, the results have not been compared with erosive burning experimental data.

Based on the comments made above, we disagree with Glick's statement that the flow model proposed in Ref. 1 is incorrect in the developing flow region. However, we would like to point out that the application of the model should be limited to situations where the propellant is burning under strong convective conditions such as those which exist in high performance rocket motors.

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Comment on "Turbulent Flow Analysis of Erosive Burning of Cylindrical Composite Solid Propellants"

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THERE appear to be at least two major problems associated with the paper by Razdan and Kuo.[1] First, the model does not include a realistic description of the controlling combustion processes of composite propellants under limting conditions of zero crossflow velocity, an important consideration when it is realized that for many cases of interest, erosive burning ratios are on the order of 1.5 or less. That is, while erosive contributions are often significant, they are not dominant in most cases, with the result that the noncrossflow effects must be properly described even with crossflow. This model deficiency results from the assumption that the heat feedback from an oxidizer/fuel gas flame is totally controlled by eddy breakup, with the result that in the absence of crossflow-induced turbulence no contribution to the propellant ablation is made from O/F gas-flame heat feedback. Thus, in the absence of crossflow, all heat necessary for preheating and vaporizing the propellant ingredients at the observed rate is assumed to be supplied by surface/subsurface heat release and/or a collapsed AP monopropellant flame, a scenario considered to be unrealistic by all modern modelers known to ths author. (The O/F flame in the models of Cohen,[2] Beckstead,[3] King,[4] Renie et al.,[5] etc., is calculated to make an important contribution to the surface heat balance at zero crossflow under all reasonable pressure conditions—in fact, the dependence of zero-

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