Reply by Authors to R. L. Glick

M. K. Razdan*
Exxon Research and Engineering Company, Linden, New Jersey
and
K. K. Kuo†
The Pennsylvania State University, University Park, Pennsylvania

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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*Senior Research Engineer, Corporate Research Sciences Laboratories. Member AIAA.

†Professor, Department of Mechanical Engineering. Fellow AIAA.

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

Merrill K. King*
Atlantic Research Corporation, Alexandria, Virginia

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 non-crossflow 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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*Chief Scientist, Research and Technology Division. Member AIAA.

crossflow burning rate on oxidizer particle size results from the importance of the O/F flame.)

Examining Eqs. (17-19) and Table 1 of Ref. 1 (and utilizing the knowledge that the base burning rate for the 75/25 AP/PBAA reference propellant is approximately 0.71 cm/s at 7.5 MPa,[6] the pressure used by the authors in generation of Figs. 2-9), one can indeed confirm that the authors have no gas-flame feedback in the zero-crossflow case and arrive at a proper prediction of zero-crossflow rate only by judicious selection of surface/subsurface heat release parameters. Application of Eq. (19) with the constants of Table 1 yields a surface temperature T_{ps} of 1130 K under these conditions. Equation (18), with $T_{ps}=1130$, $T_{pi}=298$, $C_s=0.3$ (Table 1) then yields $-\lambda_s(\partial T_p/\partial r)=250\ \rho_s r_b$. Substitution of this value along with $C_p=C_s$ and $Q_s=-250$ (again from Table 1) into Eq. (17) then yields $\lambda(\partial T/\partial r)=-250\,\rho_s r_b+250\,\rho_s r_b=0$. That is, the zero-crossflow gas-phase feedback is calculated to be zero, consistent with the authors' assumptions regarding control of the O/F flame processes, but at odds (as outlined above) with the generally accepted picture of composite propellant combustion. In addition, this model as a result requires that the surface/subsurface heat release be pressure dependent to yield the observed zero-crossflow burning rate vs pressure dependence. Thus, it appears that the model of Razdan and Kuo is seriously deficient in its treatment of the gas-phase combustion process.

The other area of major difficulty with this work lies in the use of a one-dimensional-core-potential-flow plus boundary-layer approach at and near the head end of the grain port. As has been shown experimentally by Yamada and Goto[7] and Dunlap et al.,[8] and analytically by Beddini,[9] among others, the entire flow near the head end of a perforated grain port must be highly two-dimensional, precluding use of such an analysis. Only at a distance a considerable number of diameters downstream of the motor head end, where the ratio of blowing velocity to crossflow velocity has dropped below a critical value as the crossflow velocity builds up is such an analysis applicable. In the upstream regions the highly two-dimensional nature of the flow results in near-wall turbulence intensities being quite small compared to those that would be predicted with the analysis of Razdan and Kuo. As a result, as discussed by Beddini, erosive burning effects in the upstream regions of perforated grains should be considerably less than predicted by this analysis.

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M. K. Razdan*
Exxon Research and Engineering Company, Linden, New Jersey
and
K. K. Kuo†
The Pennsylvania State University, University Park, Pennsylvania

THE analysis presented in Ref. 1 is restricted to the *turbulent boundary-layer* conditions over a composite propellant surface. This has been clearly acknowledged in the paper. The question about the model lacking the capability to yield zero-crossflow burning rate seems to be irrelevant. It is obvious that if there is no gas flow parallel to the propellant surface, there will be no boundary layer, and in fact no erosive burning problem.

Furthermore, mechanisms of gas-to-solid heat transfer are different in zero-crossflow (strand burning) and crossflow (erosive burning) conditions. In zero-crossflow situation, three flame zones are usually assumed (see Beckstead et al.,[2] for example) to be associated with the burning of a composite propellant. Under crossflow situation, this picture of various flame zones is drastically changed as the combustion gases are swept along the propellant surface. Within the boundary layer in the crossflow case, many flamelets will be established by the local fluid mechanics that controls the mixing of oxidizer and fuel gases and, therefore, the local stoichiometric ratio. The reactions within the flamelets are assumed to be diffusion controlled and the overall reaction rate is calculated by the eddy break-up model. (Relevance of the eddy break-up model to the present analysis was fully discussed by the authors in Ref. 3.) The heat release from the flamelets will be distributed by local transport processes, and therefore, the gas-to-solid heat flux is calculated by solving the energy equation applicable within the boundary layer. Under these conditions, the calculated heat flux thus gives the *total* burning rate of the propellant.

Our model is based on clearly stated physical conditions, namely: a gas flow exists over a propellant surface, the flow forms a boundary layer over the surface, and the boundary layer formed is turbulent. The main reason we have studied erosive burning in turbulent boundary-layer flow is that the flowfield developed over most of the solid propellant grain in a practical rocket motor is turbulent due to the presence of very high axial gas-flow velocities. It was not our intention to develop a model which can predict burning rates under both crossflow and zero-crossflow conditions. The model is not intended to extend to the zero-crossflow condition. King's presumption that the model is applicable to zero-crossflow condition misleads him into believing that all the heat necessary for preheating and vaporizing the propellant must be supplied by surface/subsurface heat release and/or a collapsed AP monopropellant flame, and that the model requires surface/subsurface heat release to be pressure dependent. No such assumptions have been made in the present analysis.

Regarding the nature of the flowfield near the head end of a rocket motor, we would like to say the following: Fig. 1 is a schematic diagram, and $x=0$ in this figure does not represent the head end of a grain port but a point from where the boundary layer starts to develop. Whether or not the potential

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*Senior Research Engineer, Corporate Research Sciences Laboratories. Member AIAA.

†Professor, Department of Mechanical Engineering. Fellow AIAA.