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.

References

[1] Razdan, M. K. and Kuo, K. K., "Turbulent Flow Analysis of Erosive Burning of Cylindrical Composite Solid Propellants," *AIAA Journal*, Vol. 20, Jan. 1982, pp. 122-128.

[2] Cohen, N. S. and Strand, L. D., "An Improved Model for the Combustion of AP Composite Propellants," AIAA Paper 81-1553, July 1981.

[3] Beckstead, M. W., Derr, R. L., and Price, C. F., "A Model of Composite Solid-Propellant Combustion Based on Multiple Flames," *AIAA Journal*, Vol. 8, Dec. 1970, pp. 2200-2207.

[4] King, M. K., "Experimental and Theoretical Study of the Effects of Pressure and Crossflow Velocity on Composite Propellant Burning Rate," *18th International Symposium on Combustion*, The Combustion Institute, Pittsburgh, Pa., 1981, p. 207.

[5] Renie, J. P., Condon, J. A., and Osborn, J. R., "Oxidizer Size Distribution Effects on Propellant Combustion," *AIAA Journal*, Vol. 17, Aug. 1979, pp. 877-883.

[6] Razdan, M. K. and Kuo, K. K., "Erosive Burning Study of Composite Solid Propellants by Turbulent Boundary Layer Approach," *AIAA Journal*, Vol. 17, Nov. 1979, pp. 1225-1233.

[7] Yamada, K., Goto, M., and Ishikawa, N., "Simulative Study on the Erosive Burning of Solid Propellants," *AIAA Journal*, Vol. 14, Sept. 1976, pp. 1170-1177.

[8] Dunlap, R., Willoughby, P. G., and Hermsen, R. W., "Flowfield in the Combustion Chamber of a Solid Propellant Rocket Motor," *AIAA Journal*, Vol. 12, Oct. 1974, pp. 1440-1442.

[9] Beddini, R. A., "Aerothermochemical Analysis of Erosive Burning in a Laboratory Solid-Rocket Motor," *AIAA Journal*, Vol. 18, Nov. 1980, pp. 1346-1353.

Reply by Authors to M. K. King

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

Received July 6, 1982.

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

†Professor, Department of Mechanical Engineering. Fellow AIAA.

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 regions exist. As far as the experimental evidence on the nature of the flowfield cited by King, it should be noted that these experiments were conducted under nonreactive "simulative" flow conditions. But 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.[4] and find no evidence or conclusion in regard to the nonexistence of the potential core. 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. 4. Yamada et al. further point out that the role of turbulence adjacent to a propellant surface is to enhance mixing rate of decomposed gases and increase the heat transfer rate. 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, Eqs. (27) in Ref. 1.

References

[1]Razdan, M. K. and Kuo, K. K., "Turbulent Flow Analysis of Erosive Burning of Cylindrical Composite Solid Propellants," *AIAA Journal*, Vol. 20, Jan. 1982, pp. 122-128.

[2]Beckstead, W. M., Derr, R. L., and Price, C. F., "A Model of Composite Solid Propellant Combustion Based on Multiple Flames," *AIAA Journal*, Vol. 8, Dec. 1970, pp. 2200-2207.

[3]Razdan, M. K. and Kuo, K. K., "Erosive Burning Study of Composite Solid Propellants by Turbulent Boundary-Layer Approach," *AIAA Journal*, Vol. 17, Nov. 1979, pp. 1225-1233.

[4]Yamada, K., Goto, M., and Ishikawa, N., "A Simulative Study on the Erosive Burning of Solid Rocket Motors," *AIAA Journal*, Vol. 14, Sept. 1976, pp. 1170-1177.

Comment on "Finite Elements for Initial Value Problems in Dynamics"

Cecil D. Bailey*
The Ohio State University, Columbus, Ohio

DR. Simkin's paper[1] is quite significant. In my opinion, it indicates that he has given additional thought to the problem of direct solutions to time dependent systems.[2,3] Others,[4-6] through application of Hamilton's Law of Varying Action (HLVA), have obtained gratifying and significant results. Dr. Simkins references certain of my papers and then invokes d'Alembert's Principle as his starting point.[1] But my starting point was never d'Alembert's Principle nor any other force or energy balance equation.

D'Alembert's Principle, as well as his ideas about virtual work, were presented about 240 years ago. At about the same time, however, other ideas and concepts which have influenced the development and application of the truths of mathematics to mechanics and physics were set forth. As implied in my Comment[3] on Dr. Simkins' previous paper,[2] and as obviously now recognized by Dr. Simkins[1] and others,[7] the problem lies in what we have all been taught about Hamilton's Principle. The confusion[8] which has been apparent for years and which has now been brought into the open literature,[9-11] can be traced directly to the foundational concepts of the Calculus of Variations.[12]

Examine the Fundamental Lemma of the Calculus of Variations:

"If

$$\int_{x_1}^{x_2} M\eta \, \mathrm{d}x = 0$$

for all functions η which vanish at x_1 and x_2 and poses a continous derivative on (x_1, x_2) then $M=0$ on (x_1, x_2)."

This Fundamental Lemma constitutes an unquestionable mathematical truth. It transcends even that branch of mathematics called the Calculus of Variations. Note that the function $M=0$ is both necessary and sufficient for the definite integral to vanish when the function η satisfies the prescribed conditions. Further note that the function M must be some sort of "balance" or "conservation" equation which must be known to vanish, a priori, because it cannot itself be determined from this fundamental mathematical truth. D'Alembert's Principle, $F-\dot{P}=0$, is a force balance equation, directly from Newton's Law, which qualifies as a function M. Also note that the Fundamental Lemma is exactly of the form obtained by application of the Galerkin method to any differential balance or conservation type of equation which is known, a priori, to vanish.

Dr. Simkins integrates d'Alembert's Generalized Principle of Virtual Work over an interval of time, $t_1 > t_2$,

$$\sum_{i=1}^{N} \int_{t_1}^{t_2} (F_i - \dot{P}_i)\delta r_i \mathrm{d}t = 0 \tag{1}$$

This equation is seen to be exactly of the form required by the Fundamental Lemma of the Calculus of Variations. Integrate by parts and one does, indeed, obtain a form of Hamilton's Law of Varying Action, which (except for one term) was presented by Hamilton 147 years ago,

$$\sum_{i=1}^{N} \left[\int_{t_1}^{t_2} (F_i\delta r_i + P_i\delta\dot{r}_i)\mathrm{d}t - P_i\delta r_i \Big|_{t_1}^{t_2} \right] = 0 \tag{2}$$

There should be no argument with what Dr. Simkins has done *except* for the foundational concepts associated with the Calculus of Variations. *If* the Fundamental Lemma requires that the function, η, vanish at x_1 and x_2 *before* integration by parts, logically the functions δr_i must vanish at t_1 and t_2 *after* integration by parts. Thus, one obtains Hamilton's *Principle*

$$\sum_{i=1}^{N} \int_{t_1}^{t_2} (F_i\delta r_i + P_i\delta\dot{r}_i)\mathrm{d}t = 0 \tag{3}$$

It is now clear that this equation and the concepts associated therewith have caused massive confusion with respect to direct solutions to the problems of mechanics and physics[7-15] in both the time and space domains.

If one wishes to associate "potential" functions with a physical system, d'Alembert's Generalized Principle of Virtual Work, integrated over time, again yields exactly HLVA,

$$\delta\int_{t_1}^{t_2} (T-V)\mathrm{d}t + \int_{t_1}^{t_2} \sum_{i=1}^{N} F_i\delta r_i \mathrm{d}t - \sum_{i=1}^{N} P_i\delta r_i \Big|_{t_1}^{t_2} = 0 \tag{4}$$

Received Jan. 4, 1982; revision received April 12, 1982.

*Professor, Member AIAA.