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# LETTERS TO THE EDITORS

## COMMENT ON "PREDICTIONS OF VIGOROUS IGNITION DYNAMICS FOR A PACKED BED OF SOLID PROPELLANT GRAINS"

(*Received* 19 *August* 1976 *and in revised form* 8 *October* 1976)

THE NUMERICAL results of Krier and Gokhale [1] reveal serious deficiencies in the analysis of a two-phase flow. The Appendix of the article presents computed values that are inconsistent with a physical conception of the process being modeled.

Predicted gas temperatures are unrealistic in the first two inches of the bed. For the one time step shown in the printed appendix the gas temperature of 8000*R* is about 1.5 times the adiabatic flame temperature of M30 propellant of about 5400*R*. Using the authors' input data and allowing an ideal gas simplification, the temperature of the gas introduced by the combustion should be

$$\frac{E_w}{C_v} = \frac{EM_w(\gamma-1)}{R} = 5470R$$

where $E$ is the chemical energy released in burning, $M$ the gas molecular weight, $R$ the universal gas constant, and $\gamma$ the ratio of specific heats.

In the actual code operation only 90% of the chemical energy goes into the gas and the predicted temperature should be less than 5000*R*. Although not shown in the cited article, the predicted gas temperatures for this case exceeded 20000*R* later in the calculation. With no external compression of the chamber such temperatures are unrealistic.

At the front of the compression wave in the bed interior the predicted gas temperatures and heat transfer violate thermodynamic principles. The initial physical condition is a quiescent gas in thermal equilibrium with solid particles. Hot gas entering at the aft portion forms a compression front driving gas and particles forward. In the forward portion of the bed the gas should be heated by the combination of compression by particle compaction and mixing with the hot combustion gas. As the gas temperatures rise, heat is transferred by convection (only mode allowed) to the particles.

What the code predicts however is a cooling of the gas from 550*R* to about 250*R* while the solid phase is being simultaneously heated from 550 to 560*R*. Heat transfer from a cold gas to a hot particle is inadmissable. Although the printed output in the Appendix shows only one time step, the full results show the minimum temperature region propagating through the bed but never any particle cooling. Neither the low gas temperature nor the particle heating can be justified by quantitative arguments.

Particle temperatures at the aft end of the bed are shown below the ignition temperature. A self-sustaining combustion of the solid propellant requires a heat feedback from the flame which means that the solid phase temperature cannot decrease. The surface temperature of the burning solid must be greater than the ignition temperature and heat transfer from the solid to gas is not allowed.

Porosity in the bed center is computed as 0.250 when the initial porosity of the "packed" bed is 0.470. Such a compression cannot be computed with a model which assumes the bed is always fluidized with no particle interaction. As the bed becomes "more packed" the propagation of disturbances proceeds through the bed as though it were true solid. Propagation rates are probably inversely proportional to porosity. Resistance to particle motion increases as packing increases. The drag function must account for such increased friction. The authors have used 0.250 as an arbitrary lower limit to compaction. They have not recognized that the model is probably not valid below porosities of about 0.40. Instead of merely overriding the computation of porosity, the computation should have stopped altogether. Imposing a lower limit on porosity has the effect of creating arbitrary gradients that affect the coupled equations. It effectively converts solid to gas without combustion.

A minor error was made in computing $DP/DX$ in that the printed value should be divided by the chamber length, in this case 8 in. The input value for energy of M30 propellant is incorrect, a value of 1132 kcal/kg is more appropriate. The authors have acknowledged these minor errors and will submit an appropriate correction.

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### REFERENCE

1. H. Krier and S. S. Gokhale, Predictions of vigorous ignition dynamics for a packed bed of solid propellant grains, *Int. J. Heat Mass Transfer* **19**, 915–923 (1976).

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## REPLY TO COMMENT BY C. W. NELSON

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THE COMMENT recently prepared by Nelson [1] regarding the paper by Krier and Gokhale [2] brings out some interesting points regarding the predictions presented in [2]. But at the same time some hasty conclusions were arrived at, possibly due to a lack of understanding of the basic theme of the work.

The first item deals with the fact Nelson thinks that during the unsteady compression process in the closed chamber (while an ignitor source is issuing hot gases) the predicted gas temperatures cannot exceed the adiabatic flame temperature of the propellant or ignitor gases. Of course this is not so, since one can show from the simplest thermodynamic energy balance that in filling process into a closed vessel, the temperature in the chamber may increase by almost the ratio of specific heats, i.e. $\gamma \cdot T_0$, where $T_0$ is the temperature of the hot ignitor gases flowing into the vessel. Also in our unsteady flow problem, because of the finite delay time to ignite success portions of the propellant, as the hot propellant gases are generated from the burning propellant, they are compressed and expanded locally depending upon the total (time-dependent) flow and local pressure gradients. One might think of an unsteady piston compression and expansion of the gases which may do local work and heat up the gases even more. And also we never reported such excessively high temperatures as mentioned in the Comment [1].

The second part of the comment deals with the predictions of the extremely low gas temperatures at later portions in the bed. We agree that these values are sometimes unrealistically low. They are the consequence of the large gas velocities and the resultant high kinetic energy of the gases. The complicated relations coupling the gas phase to the particle phase are due in part to viscous drag and convective heat-transfer interaction, which have been extrapolated from low speed, low pressure steady state correlations over inert particles. It may be that these correlations are not valid for the flow conditions at hand. Subsequent work to that reported in [2] has shown us that our predictions, as expected, are quite sensitive to the semi-empirical constants that one uses in these consituitive relations. For example, a 25% increase in the drag coefficient calculated from the Ergun relation reduces the gas velocities enough to prevent the predictions of such low temperatures as presented in the Appendix of [2].

Regarding the situation of having particle temperatures increase while at the same time allowing the surrounding suspending gas temperatures to decrease should really be no cause for alarm. Our model [2] utilizes an independent solid-phase energy equation. When written in operator-form one can see that the substantial derivative of the particle energy can change for reasons other than simply a convective heat transfer from the gas. But Nelson's comment here does raise an important issue, namely when deriving the conservation equations we (and most other investigators) assume that the solid-phase, although dispersed, represents a continuum. The net result is that the particles in many ways act like a gaseous medium. Thus a rapid deceleration of the particles locally could result in an increase in particle temperature. It is probably for this reason that one phase increases in temperature while the other phase, obeying its own conservation equation might decrease. It seems natural that additional work needs to be carried out to adequately explain such phenomena to the satisfaction of all.

A final query made by Nelson was in regard to the predicted bed porosity, $\phi$, during the transients. Here, solids loadings, defined as $(1-\phi)$ were predicted to be as large as 75% in some portion of the bed. Nelson is correct that such high loadings would result in a normal axial stress which would resist this and further compaction of the propellant grains. Since we had not included a constituitive relation for the solid mechanics of an aggregate under dynamic loading, we had arbitrarily cut off our calculations below this porosity value. However, Nelson is probably not correct when he states that the cut-off should be no lower than $\phi = 0.40$.

It must be remembered that the grains near the ignitor have been burning at high rates long enough to reduce their volume, so that the compaction of these smaller grains into the larger, some unignited grains, can result in a greater solids loading. Of course, for the problem presented, very little of the grain has burned away to change the grain sizes appreciably. And so the comment made by Nelson is a good one, in the sense that the model should have included a particle–particle interaction to prevent the high compactions we reported.

Finally, the errata as pointed out in the last paragraph of [1] is appreciated.

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# THE EFFECT OF SURFACE THERMAL CONDUCTIVITY ON DROPWISE CONDENSATION HEAT TRANSFER

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THE FACT that, during dropwise condensation, some parts of the condensing surface are essentially adiabatic (those covered by large drops) while other parts carry an extremely high heat flux, should give rise to an effective thermal resistance—the so-called "constriction resistance". Hannemann and Mikic have recently put forward a theory [1] for the constriction resistance which indicates that the effective vapour-side heat-transfer coefficient for dropwise condensation depends on the thermal conductivity of the condenser material. In support of their theoretical result these authors cite their own [2] and two earlier experimental studies [3, 4], while explanations are offered for conflicting evidence that the thermal conductivity has insignificant effect [5–7]. These comparisons warrant more detailed consideration.

A significant difference between those earlier measurements which, for metallic surfaces, indicate a dependence of heat-transfer coefficient on condenser material [3, 4] and that which suggests the contrary [7], is the fact that in the latter, the steel surface was thinly copper-plated (plating thickness 12 μm) to ensure effectiveness of the promoter. Leaving aside for the moment the question of the relative accuracy of these data, and in view of the well-known difficulty in establishing ideal dropwise condensation on most non-copper-containing surfaces, it is possible that the observed dependence on condenser material [3, 4] might have been due to variations in promoter effectiveness on the different materials rather than to their thermal conductivities.

On the question of accuracy of the earlier measurements, Tanner *et al.* [3] and Aksan and Rose [7] both measured the condensing surface temperature by extrapolation from temperatures indicated by thermocouples located at different distances from the condensing surface. The probable error in the surface temperature, arising from uncertainty in the positions to which the observed temperatures relate, has been analysed by Wilcox and Rohsenow [8] and, for fixed positions of the thermocouples, shown to be systematic.