Technical Comments

Comment on "Thermodynamic Performance Evaluation of a Hydroduct Using a Thermite Fuel"

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IN a recent paper, Hacker and Lieberman[1] discuss the feasibility of using a hydroduct as a power source for an underwater vehicle. They conclude, among other things that 1) the hydroduct does not develop sufficient thrust to overcome drag effects and 2) the cruising velocity of the hydroduct is determined by the launch velocity. . . .

Basing the opinion on the graphs and calculations presented by the authors, it is believed that neither of these conclusions is valid. Considering the first conclusion, a cigarette grain with a burning area of 10 in.[2] is given with the reasoning that this was the best design possible for the available space. It is not stated what duration was expected but a cigarette burning grain is not the best solution in cases of marginal thrust and it could be easily modified to supply more thrust (heat) for shorter periods, even while retaining its basic end-burning character.

An estimate was made (based on the burning area and on various sources in Ref. 2) of the size of the present vehicle and using the value of 450-lb drag at a velocity of 200 fps an approximate diameter of 0.45-ft and about 4-ft length was obtained for the vehicle in question. Now this is fairly small for a vehicle carrying a payload but the important point is that by increasing the dimensions of the vehicle the drag increases approximately by the second power of the dimensional increase but the volume, and consequently the energy storage capacity, increases by the third power. The authors' conclusion neglects these scale effects. A similar calculation performed for a standard torpedo (21-in. diam) will show that even with its greater drag at this velocity (about 12,000 lb) there is enough volume to give it a meaningful range.

Considering the second conclusion, the sample design calculation does not show in any way that the thrust is a function of the launch velocity (as stated at the end of Sec. II of the paper) except possibly the initial thrust of an accelerating or decelerating vehicle; neither does cruise velocity depend on the launch velocity but on drag and thrust and their dependence on the velocity, assuming of course that the vehicle does attain terminal velocity and that it is launched at some velocity which is higher than the minimum at which the hydroduct will operate at all.

Let us consider Fig. 2; barring severe transients in operation any vehicle will accelerate as long as there is a net thrust, namely, thrust minus drag has a positive value. Terminal velocity is achieved when thrust equals drag.

There is no technical justification for fixing the launch velocity at 250 fps particularly that the calculations are performed at 200 fps; furthermore, based on the calculations presented in the paper there is no reason for the drop in thrust around 250 fps but on the other hand, at $V = 0$, the thrust should be nearly zero: see Eq. 3 and the strong dependence of the burning rate and consequently the thrust on the pressure. Finally, considering the dependence of the drag on V^2 and cavitation the drag curve is all wrong. (Since no other definition is given it is assumed that drag is defined by the classical equation $\frac{1}{2}\rho V^2 C_D S$ or $\frac{1}{2}\rho V^2 C_F S$.) It appears that Fig. 2 in the paper should look as shown in Fig. 1 here.

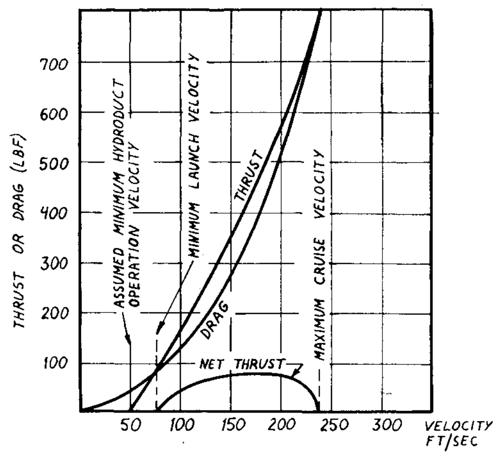

Fig. 1 Thrust and drag vs velocity.

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References

[1] Hacker, D. S. and Lieberman, P., "Thermodynamic Performance Evaluation of a Hydroduct Using a Thermite Fuel," *Journal of Hydronautics*, Vol. 3, No. 3, July 1969, pp. 139–144.

[2] Greiner, L., ed., *Underwater Missile Propulsion*, 1st ed., Compass Publications, Arlington, Va., 1967.

Reply by Author to A. K. G. Lorber

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IN response to Lorber's[1] comments on our recent paper, let us say that while it is quite true that other burning configurations are possible, the selection of the end burning cigarette grain is only typical of a heat source in which one could study this modified ramjet propulsion system. The combustion process is nonspecific with respect to the thermodynamic performance of the device. This fact may have been obscured by the sample calculation. Therefore, we concur with the respondee's first conclusion that effects of scaling have not been considered in this study.

With respect to the second comment, there is some question whether the thrust calculation obtained in Fig. 2 is correct. Lorber believes that the results should be a monatonic function of velocity. The criticism of this approach is that it assumes that operation is independent of the process that

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occurs in the combustion chamber. For example, excessive water rates at high forward velocities may quench the reacting grain while very low rates will result in superheating the liquid to produce choking in the nozzle which in turn limits the forward velocity of the vehicle. We point out that the water flow rate to the combustion chamber is not simply related to combustion efficiency and hence to thrust. The fact that the system must operate in a two phase regime to maximize the thrust should be clear from Fig. 5. At the higher launch velocities, the area in the two phase dome available for stable operation is reduced, thus limiting the choice of entry port geometry. Under these conditions the thrust developed by the vehicle can be shown to attain a maximum value as is shown in Fig. 2. If we could neglect the two-phase problem in the combustion process, Lorber's analysis would be correct.

As a concluding remark, the choice of 200 fps is quite an arbitrary choice of speed. We could have performed the same set of calculations for any arbitrary velocity. However, for the specific design criteria, e.g., size, geometry, etc., one does obtain approximately 250 fps as the maximum launch velocity. This value is again dependent upon the steam-water equilibrium diagram.

We also note an error in Eq. (2) which should read correctly: $T = (m/g_c)\,[(2Jg_c\Delta h + V_h^2)^{1/2} - V_h]$.

Reference

[1] Hacker, D. S. and Lieberman, P., "Thermodynamic Performance Evaluation of Hydroduct Using a Thermite Fuel," *Journal of Hydronautics*, Vol. 3, No. 3, July 1969, pp. 139–144.

Comment on "Analytical Prediction of the Incompressible Turbulent Boundary Layer with Arbitrary Pressure Distribution"

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SUBSEQUENT to the 1968 Stanford Conference (a confrontation?) on Computation of Turbulent Boundary Layers, improvements of the methods presented are being made by many of the participants. This paper is one of the results. The weighted-residuals (WR) method presented at the conference used an empirical τ integral lag equation, while this newer method used a mixing-length model for the τ distribution. Perhaps the authors would comment on the extent to which predictions are improved by the change of the τ model.

Much attention was given at the Stanford conference to the importance of bringing more information about the physics into the mathematical models. The WR technique is a moment method according to the morphological survey by W. C. Reynolds, and about such methods, he remarked: "information lost by the time averaging of the Navier-Stokes equations can never be regained by using multiple moments of the mean momentum equation. This information can only be regained through use of equations obtained by forming the moment before time averaging, i.e., by use of ··· the turbulent energy equation." Perhaps the authors would also care to comment on present and possible future WR methods in this regard.

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Reply by Author to A. G. Fabula

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IN reply to Fabula's comments, it is expedient to describe the relationship between the Method of Weighted Residuals (MWR) and the turbulent boundary-layer problem. The MWR is a technique for finding approximate solutions to differential equations. When applied to the turbulent boundary-layer equations there is a certain degree of freedom in the selection of 1) governing equations, 2) approximating function, 3) weighting functions, and 4) turbulence model. By proper selection of these four functions the MWR can duplicate identically many of the existing prediction methods currently in use (e.g., see Ref. 1).

The present method is similar to earlier works in terms of the form of the governing equations but differs in terms of the approximating function, weighting function, and turbulence model. The current selection of approximating and weighting functions represents a significant improvement over previous works, as reflected in the improved analytical behavior of the resulting equations.

The mixing-length model introduced in the current paper probably does not represent an improvement in predicted results per se. It was chosen because of the flexibility it lends to the method. It was recognized that existing global models, such as used in the earlier studies, restricted the solutions to second order, whereas detailed descriptions, such as the mixing-length model, impose no such limitations. Thus, in the present study, it was possible to take a cursory look at the expected convergence capability of the MWR, the results of which are discussed in the paper. A study is currently in progress to determine the reliability of various turbulence models, and it would be premature to make any conclusions regarding this at present.

The remark by W. C. Reynolds, referred to by Fabula, is certainly correct. To the author's knowledge, there is no current effort to solve such equations as the turbulent energy equation, by the MWR, in order to recover information lost by time averaging. These other equations can, in principle, be handled by the unified approach represented by the MWR—all that is required is the expenditure of effort.

Reference

[1] Abbott, D. E. et al., "Application of the Method of Weighted Residuals to the Turbulent Boundary Layer Equations," *Proceedings of the AFOSR-IFP-Stanford 1968 Conference on Turbulent Boundary Layer Prediction*, edited by S. J. Kline, D. J. Cockrell, and M. V. Morkovin, Stanford Press, pp. 16–29 and 46–53.

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