[5] Tribus, M., "Comment on 'Fundamentals of boundary-layer heat transfer with streamwise temperature variations,'" J. Aerospace Sci. 29, 1482–1483 (1962).

## Reply by Author to M. W. Rubesin

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THE full text of assumption 5 in the conclusions of Rubesin's paper (Ref. 2 of the preceding comment) reads as follows: "The local heat-transfer coefficient determined on a plate having a constant surface temperature applies to a plate having a variable surface temperature when it is expressed by an equation based on the local flow and thermal boundary-layer thicknesses instead of the distance along the plate."

In the foregoing context, the concept of local heat-transfer coefficient embodies a physical model, which is fundamentally wrong. In addition to being inadequate to cope with the anomalies, it also misrepresents the real physical nature of the phenomenon. This is shown clearly and explained in my analysis of what happens inside the fluid by introducing a two-dimensional model.

In two recent papers (Refs. 1 and 4 of the preceding comment), I have developed a new approach to transient temperature analysis in structures in contact with a moving fluid in laminar or turbulent flow. The significance of this work is being obscured by comments that are focused on a side issue.

In order to restore true perspective and dispel some of the misconceptions evidenced by the comments, I should like to state more precisely what has been accomplished.

1) A correct formulation of transient temperature problems in a system that includes coupling between conduction in a solid and convection in an adjacent moving fluid leads to partial differential equations with integro-differential boundary conditions. They are impractical for use in structural analysis. However, the new Lagrangian variational procedure (Ref. 4 of the preceding comment) avoids this difficulty while retaining a correct representation of the physics. The problem is reduced to the solution of a system of ordinary differential equations with a few generalized coordinates. The treatment also brings to light some new and fundamental aspects of nonequilibrium thermodynamics for which Onsager's relations do not apply.

2) Additional simplifications of practical importance are provided by extending to convective phenomena the concept of "associated field." It is shown to be applicable in spite of the fact that the problem is not self-adjoint.

3) As a corollary, I have developed in Ref. 1 of the preceding comment a new approach, also based on variational procedures, for the conventional more restricted problem of boundary-layer heat transfer, including turbulence and nonparallel streamlines in two and three dimensions. The method combines simplicity with high accuracy and brings to light the significant parameters.

4) Without the use of new methods, a correct formulation of transient temperature problems with surface convection is so involved that it has been customary to represent the boundary condition by a local heat-transfer coefficient. The first portion of the first paper (Ref. 1 of the preceding comment) is devoted to an examination of the misconceptions involved in this procedure by analyzing very simple mathematical models that highlight the essential features and provide a deeper insight into the physics. It points to the key role played by the Peclet number as a measure of the distortion of the temperature field and its relation to a thermal flow reversal inside the fluid. This is in contrast with the customary use of the Reynolds and Prandtl numbers, which are not representative of the physics involved.

5) This physical analysis of convective phenomena is based on the classical procedure used by Leveque, which amounts to introducing a "conduction analogy." Since this in itself is an approximation, its validity and limitations were examined in the first paper (Ref. 1 of the preceding comment), with particular reference to transient phenomena.

6) It is shown that the unorthodox behavior of the convective heat transfer is inherent in the physics and does not result from spurious mathematical properties. Any attempt to camouflage it by mathematical juggling misrepresents its real nature.

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## Comment on "Fundamentals of Boundary-Layer Heat Transfer with Streamwise Temperature Variations"

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THE author of Ref. 1 replies to a comment by Tribus[2] and maintains that the latter's statements "are in gross contradiction with the facts." I wish to confirm that these statements are not in contradiction with the facts.

Negative and infinite "traditional" heat transfer coefficient mentioned in Ref. 1 already has been predicted in the last reference cited in Ref. 2. The writer has treated[3,4] the problem of sinusoidal heat flux and obtained another extension of Leveque's solution. The "traditional" coefficient is defined with the remark that "it is of little use since there is no connection with other relevant quantities of heat transfer."

### References

[1] Biot, M. A., "Fundamentals of boundary-layer heat transfer with streamwise temperature variations," J. Aerospace Sci. 29, 558–567, 582 (1962).

[2] Tribus, M., "Comment on 'Fundamentals of boundary-layer heat transfer with streamwise temperature variations,'" J. Aerospace Sci. 29, 1482–1483 (1962).

[3] Dzung, L. S., "Heat transfer in a round duct with sinusoidal heat flux distribution," *Proceedings, Second U.N. Conference on Atomic Energy* (United Nations, Geneva, 1958), Vol. 7, p. 657.

[4] Dzung, L. S., "Heat transfer in a flat duct with sinusoidal heat flux distribution," Ref. 3, p. 671.

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## Reply by Author to L. S. Dzung

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IT should be obvious, after reading my foregoing reply to Rubesin's comments, that much more is involved here than a passing reference to unorthodox and often misinterpreted analytical properties. The real question is why does this happen? Is it more than a spurious mathematical property, and what is its more intimate physical nature? This I have

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attempted to answer by discussing fundamental but unsophisticated mathematical models that highlight all essential features (Ref. 1 of Dzung's comment). I have exposed the statement (Ref. 2 of Dzung's comment) that this material is the same as found in the countless applications of routine classical procedures. In addition, the false impression has been created that Dzung's Ref. 1 deals only with this subject. It contains many other items, including a variational approach to boundary-layer heat transfer which is of drastic simplicity and remarkable accuracy. This is in contrast with the extremely elaborate procedure of Dzung's Refs. 3 and 4, which are more suitable for dealing with the Schroedinger equation than with engineering problems.

This paper (Dzung's Ref. 1) also prepared the ground for a companion paper that follows and deals with the more difficult problem of coupling between a solid and a moving fluid in transient heat flow.

## Spontaneous Ignitability of Nonhypergolic Propellants under Suitable Conditions

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Bipropellants used in rockets are classified as hypergolic (self-igniting) and nonhypergolic (non-self-igniting). Ignition usually is preceded by exothermic chemical reactions. If the heat generated is not enough to raise the temperature of the vapor or gaseous reaction mixture to the ignition temperature, the flame will not be produced. Ignition will not take place if the vapor is intrinsically nonignitable. In general, the former factor is responsible for the inability of several bipropellants to ignite spontaneously. It appears, therefore, that, if the chemical reaction preceding ignition can be accelerated, spontaneous ignition can occur. Suitable additives can be employed for this purpose. It is the purpose of this comment to report the role of such additives.

The nonhypergolic fuels used in the investigation were m-cresol, furfural, cyclohexanol, anisole, and triethanolamine, which were of laboratory grade. For oxidizer, red-fuming nitric acid (density 1.5 g/cm$^3$) containing 6% nitrogen oxides was used. These fuels did not ignite with it, but in few cases red-fuming nitric acid containing 5% potassium permanganate was used to make the fuel self-igniting. The ignition delay was measured by the cup-test method, as described earlier.[1] The fuel and the oxidizer were taken by volume. The volume of the oxidizer taken was 1.1 ml, and the volumes of m-cresol, furfural, cyclohexanol, anisole, and triethanolamine were taken as 0.6, 0.6, 0.6, 0.6, and 0.8 ml each time, respectively.

The results given in Table 1 show that the forementioned fuels become hypergolic when red-fuming nitric acid containing potassium permanganate is used. Similar behavior of potassium permanganate is known for the gasoline/red-fuming nitric acid system.[2] The mechanism of action of potassium permanganate is under investigation.

**Table 1 Average ignition delay using red-fuming nitric acid**

| Fuel | With $KMnO_4$, sec |
|---|---|
| m-cresol | 0.30 |
| furfural | 10.0 |
| cyclohexanol | 8.0 |
| anisole | erratic |
| triethanolamine | 3.0 |

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[1] Rastogi, R. P., Girdhar, H. L., and Munjal, N. L., "Ignition catalysts for rocket propellants with red-fuming nitric acid as oxidant," ARS J. **32**, 952 (1962).

[2] Warren, F. A., *Rocket Propellants* (Reinhold Publishing Corp., New York, 1958), p. 28.

## Comments on "Wing-Tail Interference as a Cause of 'Magnus' Effects on a Finned Missile"

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Having read Benton's paper[1] recently, the author finds that an additional description of the flow over a rotating wing is necessary in order to understand it. The author agrees with Benton's conclusions concerning wing-tail interference creating a side force but does not agree with his picture of the mechanism involved.

The angle of attack on a rotating wing varies linearly along the span of the wing according to $\alpha = \omega r/U$. Here $\omega$ is the spin rate, $r$ is the spanwise distance from the centerline of rotation, and $U$ is the freestream velocity. In turn, the spanwise lift distribution is not only a function of the stationary wing factors but also the variation of angle of attack along the span. The spin changes the lift distribution on the wing and makes it necessary to integrate along the span in order to determine the resultant lift force and its center of pressure. The forementioned alters the flow and force pattern present on a stationary wing and must be considered in analyzing the conditions existing on a rotating wing.

It also is interesting to note that when free spin conditions exist on a cruciform wing plus body ($\delta \neq 0$, $\alpha = 0$), such as in Benton's paper, the resulting rolling moment on each wing must be zero ($\int r dL = 0$). However, the lift force is not zero on all sections of the wing but varies from positive values (tends to increase spin) on inboard sections to negative values on outboard sections. Under free spin conditions the resultant lift on each wing, which must be located on the body centerline, is in the direction of the inboard lift. However, the resultant lift on a symmetrical configuration at $\alpha = 0°$ will be zero, for the lift on opposing wings will cancel.

From Benton's paper it is seen that, when the wing-tail configuration reaches $\alpha = 10.5°$, the upper tail fin (fin $d$) is clear of the wing vortices and is subject only to freestream conditions. In this case the lift (side) force on the fin can be computed from

$$L = q \frac{dC_L}{d\alpha} \frac{P_{WBT}}{U} \int_{r_1}^{r_2} r\, ds$$

Here $r$ is the span distance from the body axis, $r_1$ and $r_2$ are the span distances to the root and tip chord, and $ds$ is the incremental wing area. However, fin $b$ at the same time is immersed fully in the vortex pattern so that it is no longer subject to freestream conditions. Instead, its lift (side)

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