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LETTER TO THE EDITORS

COMMENTS ON "TURBULENT CONVECTIVE HEAT TRANSFER FROM ROUGH SURFACES"

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MY ATTENTION has been drawn to Lyall's comments [1] on a paper by Dalle Donne and Meyer [2]; these comments reveal differences in the interpretation of my paper [3] on heat transfer in channels bounded by both rough and smooth surfaces. The subject has progressed since my paper was written, but it may nevertheless be helpful to clarify the issues as I saw them at that time.

Very briefly, my proposal was to divide an annular passage into two regions, separated by a surface of zero shear, so that each region could be treated as a channel having uniform roughness. Data for either of these regions could then be applied, using the "effective diameter" concept, to passages of a different shape (e.g. those formed by the rods of a fuel element "cluster"). In order that the boundary conditions for heat transfer should be similar to those for momentum transfer (as they are in the above application), I further proposed that the temperature profile across the annular passage should be adjusted so as to represent an adiabatic surface co-incident with the surface of zero shear (i.e. $q = 0$ at $\tau = 0$). It is apparently this adjustment that is the source of the controversy.

Two explicit assumptions are made in my paper concerning the "transformation" of the temperature profile. One asserts that the effective conductivity of the fluid in the radial direction remains unchanged whilst the temperature profile is adjusted. The other requires that "fully-developed" conditions prevail, and hence the axial temperature gradient has the same value at all points in the channel. An implicit assumption (which, in retrospect, should perhaps have been made explicit) is that the distribution of mass velocity across the channel is unaffected by the proposed change in the temperature distribution. Given these assumptions, the "transformed" temperature profile for a specified wall heat flux is determined, apart from an arbitrary constant. It was my intention that the constant should be chosen so as to make the bulk temperature in the transformed region coincide with that used to evaluate the transformed Reynolds number, i.e. with the bulk temperature evaluated from the measured velocity and temperature in that region. As Lyall points out, the choice of arbitrary constant is of concern only in the determination of property values in the Stanton number, and does not affect the evaluation of the "transformed" temperature difference. Consequently the wall temperature derived from the transformed temperature profile will not coincide with the measured wall temperature. (My use of identical wall temperatures in Fig. 2 was for the purpose of illustrating the change in profile *shape* only.)

In conclusion, I find that my views are substantially in accord with those expressed by Lyall.

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