a square-section rib rather than a circular one, the effective heat-transfer perimeter for a well-bonded rib is $(s + 2e)$, for an un-bonded rib $(s - e)$.

s/e	$\frac{s-e}{s+2e} = \frac{s/e - 1}{s/e + 2}$
7	0·67
10	0·75
15	0·82

Although this is only a very approximate calculation it illustrates the obvious fact that heat transfer must be most affected by a poor bond for small s/e, particularly if one were to allow for the fact that heat-transfer coefficients over the rib itself are known to be large. This may be the main explanation of Sutherland's disagreement with the conclusion of our own work that an s/e of seven gives significantly better heat transfer than ten and fifteen.

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NOTE ON THE MEANING OF "HEAT"

(DISCUSSION OF A PAPER BY M. TRIBUS [1])

Tribus [1] raises the question of the proper definition of the concept "heat" and suggests that the usual definition as found in many textbooks of classical thermodynamics should be revised or generalized.

In classical thermodynamics, heat is defined as a special type of energy flux through the boundaries of "systems". Tribus distinguishes between the "C" system (as used in thermodynamics) as a region of space and the more general "Q" system, which includes: (a) the "C" system, (b) a subset of particles mixed with other particles, and (c) a subset of possible states of a "C" system. He argues that the limitation to "C" systems in thermodynamics is too restrictive. Now, type (b) is actually identical to type (a). It is only necessary to note that the region of space may be multiply connected or *unconnected*, e.g. the space including the particles of the subset but excluding other particles. The boundary of the system may be fixed or moving with respect to any spatial coordinates. In fact, in no book on thermodynamics, the restriction of simply connected or fixed space has been invoked in the definition of a system.

To illustrate his type (c), Tribus mentions two examples, namely the "relaxation" of the vibrational state and two-temperature plasma. Relaxation phenomena, namely the energy redistribution from one mode of motional state to another, are certainly not to be considered as heat transfer, in the sense used in this journal or as understood by any heat-transfer engineer. The transport phenomena of the two-temperature plasma refer to the transfer from the system ["C" or "Q", type (b)] of the plasma to another system, so that the legitimate usage of the term "heat" here is never contested.

Whether one chooses to consider certain energy flow phenomena as heat depends on the choice of the system boundary. Guggenheim [2] explained at pain how an electrical heater may or may not be considered as converting the kinetic energy of electrons into heat, and this is analogous to the "heating" of a ceramic moderator. Thus the energy flow between the molecules of H_2O and N_2 may or may not be considered as heat depending on the choice of the system ["Q" type (b)] boundary.

Tribus' last example is a paper by Gollnick [3] in which the "heat flux to the wall" is divided into a "conduction contribution and that due to thermal diffusion". This is also quite in order in the "thermodynamical" sense, provided the system boundary is taken at the wall.

The case of negative (absolute) temperatures is not in conflict with the classical definition. Once the system boundary is properly defined, the interaction of two systems at temperatures of opposite signs involves no principle distinction from the interaction of systems at temperatures of the same sign provided one remembers that negative temperatures are "hotter" than positive temperatures. The adoption of the parameter $\beta = 1/kT$ is irrelevant to the issue, since there is a unique relation between β and T.

Tribus also argues that the variance between the usage of "competent physicists" and textbooks of thermodynamics points to the "inadequacy in the way thermodynamic concepts are developed" and suggests that "the only known approach to thermodynamics which gives entropy a life of its own is that based upon information theory". I do not deny that information theory and statistical mechanics offer an alternative approach to classical thermodynamics. However, these approaches are rather mutually supplementary and not mutually exclusive. This is not the place to compare the relative merit of each approach, it suffices to say that the classical approach has the beauty of freedom from any special model, for instance, the particle structure of matter, and can be developed in a strictly logical way.

In conclusion, I do not find any necessity of revising the definition of "heat" in the sense of Carathéodory–Born and as given in many good textbooks of thermodynamics. On the other hand, I agree that there are many textbooks in which the term "heat" is only "sloppily" defined.

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