where α_R is the Rosseland mean absorption coefficient. The expression derived by Cess applies only to planar surfaces and relies upon Cheng's application of the Marshak condition. Both Cess' and Deissler's [14] versions of equation (31) apply only if the gas has constant absorption coefficients. Equation (31) is not so restricted.

Since the gas temperature at the wall differs little from T_w in this limit, it follows that

$$T_{(0)} - T_w \cong C\left(\frac{2-\epsilon}{\epsilon}\right)\frac{1}{3\tau_l\alpha_R}\vec{n}\cdot\mathrm{grad}\,T \qquad (32)$$

from which the slip coefficient may be identified.

$$\kappa = \left(\frac{2-\epsilon}{\epsilon}\right)\frac{C_1}{3\alpha_R}. \qquad (33)$$

Since this is exactly the form which applies in kinetic theory, the analogy of emissivity with thermal accommodation coefficients is further confirmed. For a black wall and Marshak's condition, $C_1 = 2$, equation (33) is identically Probstein's result. The agreement of his prediction with that of a P_1-approximation to which Marshak's boundary condition was applied is thus to be expected. Temperature slip is always present in RGD when molecular transport phenomena are ignored; thus no special treatment is necessary if one is consistent in the use of equation (21).

CONCLUSIONS

The Mark boundary condition of neutron transport theory may be extended to non-black surfaces by a method of images. The method is restricted neither to grey, non-scattering gases nor to LTE and may be applied to general geometries. Only the Mark condition is consistent both with the governing equations and with the physical restrictions imposed by the differential approximation. It has been noted that temperature jump conditions in the Rosseland limit are a consistent result of more general interactions of radiation with material boundaries in completely general situations. Finally, a definite relationship between the concepts of emissivity and thermal accommodation coefficient has been noted.

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MORE ON GENERALIZING THE DEFINITIONS OF "HEAT" AND "ENTROPY"

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NOMENCLATURE

c_i,	concentration of species i, per unit volume;
$\hat{C}_p$,	$\partial\hat{H}/\partial T)_{p,\text{composition}}$;
$\hat{E}$,	specific energy;
$\hat{H}$,	specific enthalpy;
H_i	partial enthalpy for species i;
J_i,	flux of i relative to v;
p,	pressure;
q,	$\varepsilon - v\cdot\pi$;
Q,	net energy addition to system by heat transports;

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R_i,	rate of production of i, per unit volume;
$\hat{S}$,	specific entropy;
t,	time;
T,	temperature;
$\hat{U}$,	specific internal energy;
v,	velocity;
$\hat{V}$,	specific volume;
W,	net energy removal from system by work transports;
ε,	energy flux relative to v;
π,	stress;
ρ,	density;
τ,	viscous stress.

INTRODUCTION

THE STUDENT of heat, mass and momentum transfer, and of thermodynamics, may well become puzzled by the confusion in terminology. In one of his classes, heat describes a "transport" (and only across a system "boundary" at that), while in his next class heat can be a "generation". When teaching Thermodynamics we insist that there is no such thing as a heat "source", as a part of our effort to destroy the false caloric-theory misconceptions. Yet, when teaching heat transfer we introduce heat "sources" as if it were "obvious" that they exist, as simple manifestations of the release of "latent heat". Thus, we appeal to—and hence reinforce—caloric theory misconceptions. If the student presses us for an explanation to resolve the conflict, we become pedantic. The student, unable to resolve the dilemma alone, consciously or subconsciously adopts the same illogical attitude as his teachers. Thus, the seeming paradox is transmitted from generation to generation; clearly, the time has come to face up to it.

Although it is well that Tribus [1] has forced the issue into the open, lest some wrong conclusions be drawn from his article the following points need to be made:

1. The concept of "heat *transport*" in *not* erroneous in *all* classical-type developments of thermodynamics.
2. The viewpoint of "heat *source*", as commonly conceived by readers of this journal, is *not* justifiable (unless the caloric theory is resurrected).
3. The definition of "heat" does *not* necessarily underlie that of "entropy" in all classical-type developments.
4. Information theory, alone, is *not* the only known approach that can resolve our difficulties with "heat" and "entropy".

1. HEAT TRANSPORTS

Basically, the strict viewpoint of heat *transport* adopted by virtually all classical viewpoints *is* correct. However, the association of heat transports with *boundaries* is the cause of the difficulties. These difficulties are associated not only with the "new" problems, such as heat transfer from the *system* of spin states at negative T to its *environment* at positive T, and such as heat transfer between the *system* of electrons and the *system* of ions in a plasma, but also with the "old" simple conduction problems without a "source". Again, what student does not look warily at the old artifice, employed by teachers of heat transfer: getting around the tie of heat transport to a boundary, by cleverly relocating the boundary to make any interior point land on the surface as needed![†] The boundary concept has traditionally been viewed as a simple surface (perhaps moving) in three-dimensional physical space, which separates a system from its surroundings. This viewpoint is a concrete help in teaching the concepts of system and surroundings, and it has been, of course, appropriate in the majority of problems. Unfortunately, this boundary concept does not enjoy the generality of the system concept; not every system and its environment can be neatly separated by such a surface (as emphasized by the plasma and by the negative-T problems). Nevertheless, since the simple boundary concept is not crucial to the definition of system, we may speak of heat transports between systems, even though the systems are not "separated by a neat surface" but, rather, "occupy the same region in space." Such systems are said to be *coextensive* by Hatsopoulos and Keenan [2]. This heat transport between coextensive systems is readily incorporated into classical viewpoints, by freeing the definition of "system" from the concept of a simple boundary surface.

The definition of system

Much of our desire to retain the boundary concept stems from our view of the system as a real object, with the boundary separating this real object from the remainder of the universe. However, our systems are not real, but are only models. These models consist of a collection of states, described in terms of the thermodynamic properties selected to define the model. (See Hatsopoulos and Keenan [2], section 31.2, first paragraph, and section 31.16; too, different models for the same "object" must be consistent with one another, sections 31.2 through 31.9, 4.5.) From this viewpoint, it is misleading to say, as in [1], that a system may be defined as any one of the following: (a) a region of space, or (b) a subset of particles mixed with others, or (c) a subset of possible states of another system. Rather, it would be better to make this classification with the following wording: (a) a set of states to model a region in space; (b) a set of states to model a collection of particles, perhaps mixed with others; (c) a subset of the states which define another system. Thus, it is emphasized that these are not three different kinds of system, but that every system is merely a collection of states, which serve as a model.

Sometimes, the analysis of a phenomenon is enhanced by considering two (or more) coextensive models—for examples, (1) the constituents in an ideal-gas mixture [class (b)], (2) the ion system and electron system in a plasma

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[†] Paraphrasing a statement of Dean Tribus.

[class (b)], (3) the negative-T nuclear-spin system and its coextensive lattice system [class (c)], (4) the translational, rotational and vibrational systems of an ideal gas [class (c)]. In any case, processes undergone by different *systems* may reflect influences of one upon the other; during all such interactions between systems there is a transfer of energy between them. Certain of these interactions may be termed "heat interactions", in the classical spirit. In turn, the transport of energy associated with such an interaction may be termed a "heat transport" of energy. (See Hatsopoulos and Keenan [2], Section 34.3, for a definition of "heat interaction". In addition it should be mentioned that, with their definition of "heat interaction", there may be transports of energy which are coincident with temperature differences and yet there is no heat interaction—[2], Section 6.9. Thus, just as the simple boundary concept is not general relative to the concepts of system and surroundings, the concept of heat transport (heat interaction) is not general relative to transport of energy (interaction). For example, see Bornhorst and Hatsopoulos [3], wherein transports across phase interfaces are analyzed via irreversible thermodynamics. The important point here is that—as discussed in part 3 of the present article—"entropy" is more fundamental than "heat".)

Proposed: make "heat" an adjective

Notice that the word "heat" is being used here as an adjective: *heat* interaction, *heat* transport. Use of this word as a noun is unnecessary, and it is undesirable inasmuch as such usage perpetuates the mistaken caloric theory concept of heat as *something*—viz. an extensive property, or a transitory "energy in motion". Furthermore, the usage of "heat" as an adjective is pedagogically helpful in many ways. For example, given a system, the symbol Q is defined as the net amount of energy carried into the system by heat transports. Q is not the "heat addition"—i.e. the addition of energy in a transitory form called "heat"—but rather it is the addition of energy by the mode of transport called heat transport. Too, recall that it has been common to say that this "transitory energy" has meaning only *between* a system and its surroundings—that is, to say that the transitory energy is "heat" only while passing between system and surroundings (and this is the reason for having to "cleverly relocate the boundary . . ."). However, with the viewpoint of "heat" as an adjective and the definition of Q given here, all these "oddities", and the conceptual difficulties associated with them, disappear.

Also, the concept of heat transport is applicable to other extensive properties besides energy. For example, the heat transport of entropy past a surface $\mathscr{S}$ equals that of the energy, divided by the surface temperature T (for example, see Gurtin [4]), while the heat transport of availability equals $[1 - T_0/T]$ times that of the energy.

In analogy to "heat", the word "work" could also be used as an adjective: work interactions (e.g. see [2], section 32.5), and work transports (of energy, availability, momentum, . . .). The genesis of the usage of the word "heat" and "work" as adjectives lies in the definitions of the functions $Q(t)$ and $W(t)$ for a system, as advocated by Obert [5]. Of course, $Q(t)$ and $W(t)$ are *not* properties of the system.

This discussion of "heat" as an adjective has been somewhat, a digression from the main point—that classical-type approaches can readily incorporate heat interactions between systems which occupy the same space. Indeed the tie between heat transports and the "simple boundary surface" needs to be broken. However, as has been pointed out, this can be accomplished without recourse to the information theory approach. Also, it is important to note: Advocating that a heat *transport* may exist without a simple boundary surface *should not* be construed as purporting the existence of "heat sources"—the next topic for discussion.

2. HEAT GENERATION

Tribus [1] discusses the "heating" of inert species by reactive species during chemical reaction. He uses this as one example to emphasize that we must provide for the possibility of heat *transports* from one system to another, both occupying the same space (from the "reactive species" to the "inert species", in this example).

However, it needs to be emphasized further that there is not a "generation of 'thermal energy'", as a result of such reactions. The "heated" system does not have "heat generation" *within* the system, but it is "heated" by a heat *transport* from a coextensive system. Nevertheless, the concept of "heat source" or "heat generation" which is so common to the subject of heat transfer is almost always—if not always—one of "generation within the system", such as "the generation of 'thermal energy' from 'nuclear energy', or from 'mechanical energy', or from 'chemical energy, or . . .". A viewpoint of "generation within" is a caloric theory viewpoint—release of "latent heat"—no matter what words we use in an effort to cover up. One should speak only of heat *transports* if he is to be compatible with any modern approach to thermodynamics—information theory included.

But, then, consider a case where coextensive systems are not appropriate and yet there is a "heating"—say, a temperature rise—as a result of, say, a chemical reaction. It is not always proper to define distinct, coextensive systems for inert species and reactive species. It is proper for ideal gas mixtures, such as in a typical combustion, where the energy of interaction between the species is negligible; then, separate systems can properly be employed since distinct values of the energy can be associated with each. (Hatsopoulous and Keenan [2], Sections 31.16, 36.10; the intent of their usage of the word "isolated" is that a distinct energy can be assigned.) However, in those instances where coextensive systems are not appropriate, there appears to be a dilemma: (*i*) the view that the inert species are heated by transports from the reactive species is inappropriate because separate systems cannot be defined for the inert and the reactive, (*ii*) the heating should not be viewed as arising from a source because this would be a reversion to the caloric theory viewpoint of "generation within". Fortunately, though, the dilemma is only apparent; the temperature rise can simply be viewed as *reflecting changes of other thermodynamic variables*, of the system which represents all the species, together. Thus (see Bird, Stewart and Lightfoot [6], Table 18.3–1) by combining

(*i*) the general energy equation

$$\rho \frac{D\hat{E}}{Dt} = -\nabla\cdot\varepsilon = -\nabla\cdot q - \nabla\cdot[v\cdot\pi]$$

where q is defined, as indicated, to be the total energy flux (ε) minus the energy transported with momentum ($v\cdot\pi$)†

(*ii*) the relations $\hat{U} \equiv \hat{E} - v^2/2, \hat{H} \equiv \hat{U} + p/\rho$,

(*iii*) the general differential relation between thermodynamic properties of simple variable-composition states

$$\rho\, \mathrm{d}\hat{H} = \rho\hat{C}_p\, \mathrm{d}T + \left[1 - \frac{T}{\hat{V}}\frac{\partial\hat{V}}{\partial T}\right]\mathrm{d}p + \Sigma H_i\rho\, \mathrm{d}[c_i\hat{V}]$$

the following equation for the temperature change results:

$$\rho\hat{C}_p \frac{DT}{Dt} = -\nabla\cdot q - \tau:\nabla v + \frac{T}{\hat{V}}\frac{\partial V}{\partial T}\frac{Dp}{Dt} + \Sigma H_i[\nabla\cdot J_i - R_i].$$

There is no *need* to associate the lingo "heat generation" with any of the terms on the right. Since the phrase "heat generation" is at odds with the thermodynamic concept of heat *transport*, it is harmful to use such terminology and thereby reinforce the misconceptions of caloric theory—especially since there is no *need* to. Indeed, in thermodynamics, when a constant-composition fluid is compressed adiabatically and reversibly we certainly do not say that the temperature rise $\{DT/Dt = [T/\hat{C}_p][(\partial\hat{V}/\partial T)_p]\, Dp/Dt\}$ reflects a "heat generation", but that it reflects a change in the thermodynamic variable p (or $\hat{V}$). In fact, even if the fluid is stirred adiabatically, we do not (or, at least, should not) say that the temperature rise ($DT/Dt = -[\hat{V}/\hat{C}_p][\tau:\nabla v]$) reflects "heat generation" but rather that it reflects a change in the thermodynamic variable S, as a result of the irreversible production ($-[1/T][\tau:\nabla v]$) of entropy.

In review, then, the preceding two paragraphs say that, although it is certainly justified for the heat transfer literature to violate the classical tie of "heat" to a boundary surface, to take the view of "heat generation within" is not correct (unless it were concurrently and explicitly acknowledged that the view is employing the caloric theory in these instances), nor is it needed, nor is it desirable.

3. ENTROPY

Tribus [1] argues—and we agree—that the concept of "heat" should not be primitive to the concept of "entropy", in the logical theoretical development. That is, the definition of the word "entropy" should not depend, logically, on the definition of the word "heat". On the other hand, this does not mean that "heat" cannot be defined before "entropy", as long as the definition of entropy does not refer to "heat" in any way—when two concepts can be defined without logical dependence upon one another, the order of presentation is a matter of *taste* (which can change). However, contradictory to the claim "An examination of all "*C*" type developments of thermodynamics reveals that the concept of heat is developed as primitive to the entropy concept", careful study of Hatsopoulos and Keenan shows that their definition of entropy does not *rely* on having defined "heat" before (which they did). Granted, the word "heat" appears often in the sections of their book wherein Hatsopoulos and Keenan define entropy ([2], sections 34.5, 13, 14, 18)—but not because it had to. Most importantly, what they elect to call a "heat reservoir" could as well have been called simply a "reservoir". Clearly, that Hatsopoulos and Keenan view "entropy" as more fundamental than "heat" is borne out by study of the aforementioned work of Bornhorst and Hatsopoulos [3]. In any case, *entropy can be defined without reference to "heat" by approaches other than the information theory method.*

4. CONCLUSION

Certainly, like all scientific subjects, Thermodynamics needs to change and evolve continuously. However, in our efforts to contribute to the evolution of this subject let us keep our minds open to the potential contributions of all approaches to the subject. Certainly the information theory approach has been and will continue to be a significant contributor to this evolution, thanks to the diligent efforts and fervent promotion by Dean Tribus and his co-workers. But information theory will not singlehandedly provide "all the answers". Let the signal contributions being made by Hatsopoulos and Keenan, the information theory approach, the mathematically-sophisticated approach of Truesdell–Noll–Coleman–Gurtin–Mizel–etc., and other approaches complement one another in the current stages of the evolution.

To conjecture, the evolution will consolidate into one logical theory.

(1) Macro- and micro-thermodynamics (including the concept of "information" as a crucial aspect), wherein micro- will not be more fundamental than macro-.

(2) Thermodynamics (with its complement, equilibrium

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† q, defined as $\varepsilon - v\cdot\pi$, cannot invariably be called the heat flux of energy, since the concept of heat transport is not always appropriate.

statistical mechanics) and transport phenomena (with its complement, nonequilibrium statistical mechanics).

Evidence for (1) is the degree of consolidation already attained, distinctly, by the Jaynes–Tribus and Hatsopoulos–Keenan approaches. Also, modes for incorporating the concept of "information" into the Hatsopoulos–Keenan approach are becoming apparent, thereby, providing a link for consolidation.

Evidence for (2) includes (a) the initial development of relationships for transport properties via information theory applied to steady-state processes; (b) the re-emergence of interest in "irreversible thermodynamics", among engineers, continuum mechanicians, and statistical mechanicians.

"Thermodynamics appears to be a renascent science on the threshold of a new era."†

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† Hatsopoulos and Keenan [2], p. *xlii*. (The *intent* of the frequent references to this work is not necessarily to indicate a preference for the methods of Hatsopoulos and Keenan over the Jaynes–Tribus approach, but to emphasize the existence of another approach which has great assets and potential.)

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NOTE ON THE TEMPERATURE PROFILE OF A LAMINAR WEDGE FLOW

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This note makes the point that if $n = \frac{1}{2}(3m - 1)$, then equation (167), p. 70, of Curle's book [1] has a particularly simple integral for $\theta(\eta)$ in terms of Falkner-skan function $f(\eta)$.

Falkner and Skan's relation of the momentum equation is

$$f''' + \tfrac{1}{2}(m + 1)\, ff'' + m(1 - f'^2) = 0.$$

Upon differentiating the above equation with respect to η, it becomes

$$f^{iv} + \tfrac{1}{2}(m + 1)\, ff''' - \frac{3m - 1}{2} f'f'' = 0.$$

It is easy to see that,

$$\theta(\eta) = 1 - \frac{f''(\eta)}{f''(0)}$$

is a solution of equation (167), if

$$n = \frac{3m - 1}{2}.$$