

# from the EDITOR

## Should CEE be Discontinued?

One of our readers questions "the wisdom of publishing **Chemical Engineering Education**." His reason (see Letters section) is that "the magazine is so obviously a quality publication, it will no doubt attract all of the papers pertaining to chemical engineering education, whereas previously many of these papers would have been published in . . . journals such as *Chemical Engineering Progress*." Consequently, he argues, the practitioners in industry will be less aware of educational trends, thereby widening the industry-academic gap.

The editor has replied as follows:

"My immediate answer is that practically all the papers we have published so far would not have been published by CEP. . . . And, I believe that most faculty members would state that CEE fills an important professional need that other journals do not. Furthermore, I do not believe that abolishing CEE would enhance your quite desirable objective of getting educational matters before the practicing engineer.

It seems to me that what is needed, instead of abolishing CEE, is to make it easier for practicing engineers to see it. One way to accomplish this would be to ask the AIChE to co-sponsor CEE and have it sent at modest cost to engineers in industry. Another way, which we have considered, would be to send one or more copies to each local section to be read by their officers and to be used in career guidance work.

I certainly do agree with you that we should do all that we can to alleviate the industry-academic gap and keep graduate engineers abreast of new developments in education. In fact, I have often suggested that the articles we have published dealing with newer developments in education would be much more understandable to the practicing engineer than many of those in the technical journals. What do you think of the idea of promoting **Chemical Engineering Education** among industry as a medium of communication and of continuing education?"

In keeping with the above suggestion, CEE is planning to publish reports of various AIChE committees that deal with educational matters. We begin in this issue with a report from the Educational and Accreditation Committee; future issues will contain reports from the AIChE Educational Projects, Continuing Education and Career Guidance Committees. Through the publication of these reports and of our educational articles, we hope that we can encourage engineers in industry to read CEE. While financial support from the AIChE is very unlikely at this time, we would welcome AIChE assistance in bringing

the possibility of subscription to CEE to the attention of its members.

On the other hand the larger question raised by the reader who wrote us cannot be ignored. With CEE now beginning its fourth year of publication at the University of Florida, we feel that it is time to ask ourselves and our readers whether they feel that CEE should be discontinued. We are encouraged that many of the departments that have responded to Professor Churchill's letter soliciting bulk subscriptions have greatly increased the number of copies ordered—although many others have not replied. We are also pleased that, thanks to the yeoman efforts of Professor Jim Weber and his Publication Board, our advertising income may exceed expectations; we should have the financial resources to publish this year. However we would be pleased to see comments from our readers on whether we should continue publication, and, if so, on how we can better serve both the professor and the engineer in industry. R.W.F.

### NOTE TO DEPARTMENT CHAIRMAN AND SUBSCRIBERS

Renewals for 1971 of individual (at \$10/yr.) and bulk subscriptions (at \$4/yr. with \$25 minimum for six) should be sent to R. B. Bennett, Bus. Mgr. CEE, Department of Chemical Engineering, University of Florida, Gainesville, Florida 32601.

# from our READERS

## Discontinue CEE

Sir: I have recently been wondering about the wisdom of publishing **CHEMICAL ENGINEERING EDUCATION** and would like to solicit your viewpoints on the subject. You and the others who have been associated with the publication in recent years have done an outstanding job and certainly deserve the highest of compliments.

It is not the quality of the magazine that causes my concern, but rather its impact on the chemical engineering profession. As a matter of fact, it is my impression that the improved quality of the magazine may be its greatest weakness. Now that the magazine is so obviously a quality publication, it will no doubt attract all of the papers pertaining to chemical engineering education, whereas previously many of these papers would have been published in other chemical engineering journals such as **Chemical Engineering Progress**. I feel that the real strength in any profession lies in its interest in education of new members of that profession. Therefore, it is of utmost importance that the practitioners of that profession be constantly aware of educational trends and changes. Recent presentations by Max Peters and committee activities, such as the Wilke Committee, sub-

stantiate this proposition and also indicate concern throughout the AIChE for this ever-increasing gap.

I am sure that some industrial engineers will read *Chemical Engineering Education*, but I suspect that it is so small as to be insignificant. The proposition that the publication should be discontinued, after you and others have invested so much work and effort into it, will probably not receive enthusiastic support. An alternative solution might be to select some of the more significant papers from *Chemical Engineering Education* and have them reprinted in *Chemical Engineering Progress*. Although I am personally opposed to duplicate publications in this fashion, I feel that it might represent a solution to a problem that now exists.

Roy Foresti, Jr., Chairman  
The Catholic University

#### Havens-Starling comment on Lee

Sir: The paper "Transport Phenomena Equations of Change" by V. J. Lee which appeared in *Chemical Engineering Education*, Summer 1969, pp. 126-128, elicits a reply.

First, because of several typographical errors, the development is rather hard to follow. We call attention to the repeated jumbling of the Greek letter  $\rho$  for density and the letter, p, for pressure in Equations 7, 9, 15, 16, 17, 19 and 20. In the following discussion we have rewritten several of Lee's equations, with appropriate corrections for typographical errors, and have identified them with the same equation number used in his paper. Equations not taken from the paper are labeled consecutively with capital letters.

Of more concern than typographical errors is the misinterpretation of the fundamental, thermodynamic definition of heat. For example, Lee states that multiplication of his Equation 19)

$$\rho \frac{DU}{Dt} = \left[ -\underline{\underline{\tau}}: \nabla \underline{\underline{v}} - \nabla \cdot \underline{\underline{q}} \right] - p(\nabla \cdot \underline{\underline{v}}) \quad (19)$$

$$\delta U = \delta Q - p \delta V \quad (20)$$

which "is the familiar first law of thermodynamics for a closed system . . ." He defines  $\delta Q$  implicitly from Equation (19) as follows

$$\delta Q = \left[ -\underline{\underline{\tau}}: \nabla \underline{\underline{v}} - \nabla \cdot \underline{\underline{q}} \right] \delta V \Delta T \quad (A)$$

by  $\delta V \Delta T$  yields

wherein "the terms in the square bracket are rate of heat generation due to friction and rate of energy transfer to the system mainly as heat . . ." Lee's definition for  $\delta Q$  is not consistent with the thermodynamic definition of heat. The concept of heat has meaning only at the boundary of a system; it is a *transfer* quantity. Lee's misuse of the term "heat" can be seen more clearly from his total energy balance Equation (16), for a *closed* system:

$$-\frac{D}{Dt} \left[ \rho \delta V (1/2 v^2 + \Phi + U) \right] = \int \underline{\underline{v}} \cdot (\underline{\underline{n}} p) dS + \int \underline{\underline{v}} \cdot (\underline{\underline{n}} \cdot \underline{\underline{\tau}}) dS + \int \underline{\underline{q}} \cdot \underline{\underline{n}} dS \quad (16)$$

A more introspective (for reasons to be discussed later) form of Equation (16) is

$$-\frac{d}{dt} \iiint_{\delta V} \left[ \frac{\rho v^2}{2} + \rho \Phi + \rho U \right] dV = \iint \underline{\underline{v}} \cdot (\underline{\underline{n}} p) dS + \iint \underline{\underline{v}} \cdot (\underline{\underline{n}} \cdot \underline{\underline{\tau}}) dS + \iint \underline{\underline{q}} \cdot \underline{\underline{n}} dS \quad (B)$$

where on the left-hand side,  $\rho v$ ,  $\Phi$  and  $U$  are the local density, mass average velocity, potential energy and internal energy respectively, instead of "mean values" for the volume element,  $\delta V$ , as implied in Lee's Equation (16). Equation (B) is a statement of the first law of thermodynamics for a closed, nonreacting, nondiffusing system in which the nonequilibrium stress tensor  $\underline{\underline{\sigma}}$  (sometimes described as a dynamic pressure) has been expressed as

$$\underline{\underline{\sigma}} = - \left[ p \underline{\underline{I}} + \underline{\underline{\tau}} \right] \quad (C)$$

where  $p$  is the hydrostatic pressure,  $\underline{\underline{I}}$  is the unit second order tensor and  $\underline{\underline{\tau}}$  is a second order tensor involving velocity gradients. According to thermodynamic principles,  $\underline{\underline{q}}$  in Equation (16) or (B) must denote the heat flux vector at the *boundary* of the system. Therefore, Lee's contention that  $\underline{\underline{q}}$  must "denote the rate of energy dissipation per unit surface area of all forms of energy including heat flux as a major form" is incorrect.

The first law of thermodynamics states that the *total* energy of a *closed* system can only be altered by heat and work transfers *across* the boundaries. Therefore, for Equation (16) or (B) to be valid, each term on the R.H.S. must be identifiable as either a heat or a work transfer; dissipation or production terms are meaningless in the total energy balance for a system in which relativistic effects are absent. It is clear from Equation (16) or (B) that the boundary of the system (taking the system to be the volume,  $\delta V$ ) has been arbitrarily taken at an interface where a presumably identifiable force (as calculable from  $\underline{\underline{\sigma}}$ ) results in a corresponding motion of the boundary. Thus, the sum of the first and second terms on the R. H. S. of Equation (16) or (B) represents a work transfer, so the second term should not be associated with heat (as Lee has suggested). The ultimate consequence of the work transfer involving the tensor  $\underline{\underline{\tau}}$  insofar as giving rise to "dissipation effects" in the system is of no concern to the *total* energy balance of Equation (16) or (B) since any phenomena taking place (even an infinitesimal distance) away from the boundaries of the system are completely irrelevant. Thus, whereas the first two terms on the R. H. S. of Equation (16) or (B) account for all mechanical work transfers, the third term accounts for *all* heat transfers. Many examples could be cited from the literature where indiscriminate identifications of heat and work transfers have precipitated erroneous conclusions.

Part of the difficulty in interpretation of the individual terms contained in the energy equation can be attributed to insufficient attention to the physical significance of terms involved in mathematical manipulations. During the limiting — or conversely the integrating — process of calculus, the location of the boundary of the system may be changing; consequently the identifi-

cation of different types of energy transfers may be undergoing change. For example, let us apply the Reynolds transport theorem to Equation (B). Note that Equation (B) must hold for any arbitrary volume element  $\delta V$ . Consider the limit of the resulting equation as the magnitude of  $\delta V$  approaches zero; thus

$$\lim_{\delta V \rightarrow 0} \left[ - \iiint_{\delta V} \frac{\partial \rho \left( \frac{v^2}{2} + \phi + u \right)}{\partial t} dS = \iint \rho \left( \frac{v^2}{2} + \phi + u \right) \underline{v} \cdot \underline{n} dS + \iint \underline{v} \cdot \underline{np} dS + \iint \underline{v} \cdot (\underline{n} \cdot \underline{\tau}) dS + \iint \underline{q} \cdot \underline{n} dS \right] \quad (D)$$

Quoting Gibbs,<sup>(1)</sup> "The surface integral of (a vector)  $\underline{A}$  for a closed surface bounding a space  $dV$  infinitely small in its dimensions is  $\nabla \cdot \underline{A} dV$ ." Therefore Equation (D) can be written as

$$- \frac{\partial \rho \left( \frac{v^2}{2} + \phi + u \right)}{\partial t} dV = \nabla \cdot \left[ \rho \left( \frac{v^2}{2} + \phi + u \right) \underline{v} \right] dV + \nabla \cdot \underline{pv} dV + \nabla \cdot (\underline{v} \cdot \underline{\tau}) dV + \nabla \cdot \underline{q} dV \quad (E)$$

or, equivalently (dividing by  $dV$ ),

$$\rho \frac{D}{Dt} \left[ \frac{v^2}{2} + \phi + u \right] = - \nabla \cdot \underline{pv} - \nabla \cdot (\underline{\tau} \cdot \underline{v}) - \nabla \cdot \underline{q} \quad (17)$$

Note that throughout the limiting process leading to Equation (17),  $\underline{q}$  is the heat flux vector at the surface of the diminishing system ( $\delta V$ ) under consideration. The term  $\nabla \cdot \underline{q} dV$  in Equation (E) should be interpreted as the integral of the heat flux vector over the surface of this infinitesimal volume element  $dV$ . This interpretation is consistent with the identification of heat in the first law of thermodynamics as a transfer quantity, (energy transfer due to a temperature gradient at the boundary) having meaning only at the boundary of the system under consideration.

By subtracting the equation of change for potential energy and kinetic energy from Equation (17) and utilizing the identity

$$\nabla \cdot (\underline{\tau} \cdot \underline{v}) = \underline{v} \cdot (\nabla \cdot \underline{\tau}) + \underline{\tau} : \underline{\nabla v}$$

we arrive at Lee's Equation (19),

$$\rho \frac{Du}{Dt} = \left[ - \underline{\tau} : \underline{\nabla v} - \nabla \cdot \underline{q} \right] - p (\nabla \cdot \underline{v}) \quad (19)$$

To get an internal energy balance equation for a closed system  $\delta V$ , we integrate (19) to get

$$\frac{d}{dt} \iiint_{\delta V} \rho u dV = - \iint \underline{q} \cdot \underline{n} dS - \iiint_{\delta V} \underline{\tau} : \underline{\nabla v} dV - \iiint_{\delta V} p (\nabla \cdot \underline{v}) dV \quad (F)$$

Since the surface integral involving  $\underline{q}$  accounts for *all* heat transfer, the second term on the R. H. S. of Equation (F) cannot be associated with heat.

In view of the serious inconsistencies in the development of Section IV of the subject paper, the final statement, that "Equation 20 confirms the self-consistency of the derivation," is invalid.

Another point of criticism is directed at the starting point for Lee's development of the equations of change

<sup>1</sup>Gibbs, J. W., Scientific Papers, Vol. 2, Dover Publications, p 32.

\*Correction: change  $dS$  to  $dV$  on L.H.S.

for mass, momentum and internal energy. Lee implicitly defines the system as "the infinitesimal fluid element,  $\delta V$ , in motion." He further specifies that the velocity of the volume element,  $\delta V$ , is  $\underline{v}$ . This specification of velocity is ambiguous. First of all the volume element must be arbitrarily specifiable to pave the way for a valid development of the local equations of change from a balance equation as has been done, for example, in proceeding from Equations (8) to (9) and (16) to (17). Lee's specification requires consideration of the velocity  $\underline{v}$  as a mean value over the volume element  $\delta V$ . Similar difficulties arise with the density,  $\rho$ , the potential energy,  $\phi$ , and internal energy  $U$ . For this reason we resorted to rewriting Lee's Equation (16) as Equation (B). The latter equation is obtained by defining velocity, *first*, as the mass average velocity corresponding to the theory of continua. Then, and only then, the system to be considered can be defined by fixing its location at some time and specifying its boundary velocity for all times thereafter. If the system boundary is defined as moving with the local mass average velocity, the system is a Lagrangian system; in the absence of diffusion it becomes a closed system.

The purpose of this critique is to call attention to common misapplications of thermodynamics in analyzing continuous systems. In so doing, we hope that we do not discourage authors, such as Lee, in their efforts to achieve "formalistic simplicity" and "expose the conceptual continuity from the Newtonian equation of body motion to the continuum motion of fluids." With respect to these two objectives, Lee's efforts are to be commended.

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## GRADUATE RESEARCH ASSISTANTSHIP

Assistantships are available to students with a good degree in Chemistry, Metallurgy or Chemical Engineering to study in the fields of Fire Science, Materials Science or Chemical Engineering. These studies should lead to the degree of M.Sc. or Ph.D. in Chemical Engineering. The value of the assistantship will be up to \$4600 per annum. Apply to Chairman, Department of Chemical Engineering, University of New Brunswick, Fredericton, N. B., Canada