

# from our READERS

Editor:

I read your editorial in the latest issue of **Chemical Engineering Education** and found it quite interesting. . . . While I fully agree with everything you said in your editorial I think it should be broadened by leaving out the word "chemical" throughout. Engineering has developed with one root in science, other roots in the engineering sciences, and its trunk is a multi-channel communication cable with branches in operation and equipment design, in laboratories, pilot plants, and processing plants. Its leaves and fruits are products and goods of our consumer economy. Without its roots, it will die; without its trunk, the leaves and fruits will not develop; and without its fruits it has no reason for existence. This is essentially quoted, in part paraphrased, from your first paragraph. I could go on and continue this way throughout.

My point is that we must now begin to recognize the subservience of the adjective to the noun. I think it is time — perhaps past time — for us to accept the fact that we are engineers first and then, oh yes, chemical or electrical or civil or other kinds of engineers including, today, many many with strong interdisciplinary interests and commitments. I think there is *need* for a strong national unified engineering profession, an organization something like the American Association for the Advancement of Science, a journal structure to further breakdown the barrier resistances to communications among various kinds of engineering — the word "kinds" here used in its classical sense.

It seems to me that there is little today which truly characterizes any particular stripe of engineer. I think it is necessary for us to maintain the departments as foci around which engineering schools function but I think it is high time that we recognize and foster, administratively and in the practice of our profession, the idea that we are all members of one profession and that the interactions of engineers of all kinds with scientists, political scientists, economists, humanists, and those from the health professions are NECESSARY if we as a profession are to contribute to the important problems of our era.

I wish we could subscript the adjective the way we use subscripts in our journal articles. We would then have

ENGINEERING<sub>Chemical</sub> or Eng<sub>Chem</sub> and  
ENGINEERING<sub>Electrical</sub> or Eng<sub>Elec</sub>, etc.

as meaningful designations for our professional identities.

H. E. Hoelscher, *Dean*  
University of Pittsburgh

Editor:

I would like to make some comments on Dr. Griskey's opinion "Students, Faculty and Professionalism," particularly his derogatory implications about the University of California at Berkeley.

Berkeley is no paradise for students, particularly for lower division undergraduates; no large computer dominated state school can be. But to imply that the "Berkeley syndrome" is characterized by "staff members that do not care" is to do a great disservice to the many faculty members who are always open, concerned, and interested in the students well being.

As a graduate student in the Chemical Engineering Department, I have had the opportunity to observe the conscientious efforts of many professors seeking student opinions and ideas. Many of the busiest professors — measured in terms of research publications, administrative duties, etc. — are among the most concerned and most helpful.

A *much more important* facet of the Berkeley which I know is that many staff members make an honest attempt to understand the opinions and ideas of their students. We have several men in our department who are quite separated from their students in age, background, life style, and philosophy but yet make conscientious attempts to get an honest view of the student mind.

They are aware that their "professionalism" has not been a final solution to their society's local or global problems. They are also aware that the codes and paths which they have followed with integrity and distinction are not the only ones possible and in fact may not be the best ones for today's young. They often disagree strongly with the actions and ideas expressed to them. But their disagreement is based on their view of the merits of the ideas and actions not on the length of hair or the style of dress associated with them. These men do not view the students as "characters" with "all kinds of weird customs" as does Dr. Griskey, but rather as human beings who are reacting to their own boundary conditions and are trying to solve the problems they see.

Not all nor even most of Berkeley's faculty meet this description. But there are a sufficient number of these journeyman to make life at Berkeley an exciting and stimulating experience.

I would like to cite a recent action by individuals on the Berkeley faculty as one example of the changing attitudes here. This fall, during the turmoil precipitated by the Regents' action against course in which Eldridge Cleaver was to participate, many concerned faculty members and administrators held open meetings in various places all over campus to discuss the nature and the background of the situation. The tone of these meetings was frank and informal; Nobel Prize winners were challenged by first quarter freshmen. This aggressive attempt at communication on the part of these men and the students who helped plan the program goes well beyond Dr. Griskey's "open door" policy.

Finally, I would like to suggest that **Chemical Engineering Education** publish a column giving student views on the issues raised by Dr. Griskey's article, particularly his view of the campus situation.

Thomas A. Massaro, Student  
University of California, Berkeley  
Letters (Continued on page 29)

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EDITOR'S NOTE: In reply to a letter from the editor, Dean Hoelscher stated that he was not trying to limit chemical engineering, but to express the hope that we will soon realize that engineering must become a single unified profession lest we "fragment ourselves out of the business."

If and when a single type of microflow element does not dominate the transfer situation — or does not lead to a desired functional form of average flux — one can turn to mixed populations of different types of elements. Danckwerts noted the possibility in his 1951 paper. The example in Figure 10 corresponds to two populations of the sort made especially convenient by tables of Laplace transforms; the two are distinguished by different mean element lifetimes ( $\tau_1 = 1/s_1$ ,  $\tau_2 = 1/s_2$ ).

$$\bar{j}(\tau_1, p_1) = \sum p_i \int_0^{\infty} j_i(t) \psi_i(t; \tau_i) dt, \quad \sum p_i = 1$$

Example: 
$$\bar{j}(\tau_1, \tau_2, p) = p \int_0^{\infty} j_1(t) e^{-t/\tau_1} d(t/\tau_1) + (1-p) \int_0^{\infty} j_2(t) e^{-t/\tau_2} d(t/\tau_2)$$

Fig. 10.—Mixed Populations.

At this point I have sketched a crude but serviceable conception of turbulent fluid interfaces, which I believe is more detailed and realistic — and pedagogically attractive — than any available heretofore, and I have delineated for the first time the general strategy for modeling turbulent action by means of populations of microflow elements. This strategy should work equally well, incidentally, in treating some turbulent reaction systems, for instance. So far as flow and transfer are concerned, the strategy suffers from the lack of experimental data on the population dynamics of local flows at chaotically agitated fluid interfaces. Just as importantly, it suffers from the lack of development of mathematical models of microflow elements with “real convection.” The remainder of the lecture is devoted to the latter, that is, to some relevant solutions of the convective diffusion equation.

**EDITOR'S NOTE:** The remainder of the lecture will be published in the Spring Issue of CEE.

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#### LETTERS

(Continued from page 5)

Editor:

The Spring 1968 issue of CEE was very interesting. I am particularly glad to see evidence of a forum for opinions like those of Lenz (Industry Needs Scientific Engineers Not Engineering Scientists).

During my several years as teacher and department chairman, I felt chemical engineering educators in ASEE were talking only to themselves in a positive feedback situation leading to a runaway reaction on teaching engineering science. Lenz's points are very valid from my experience in industry in research and now in operations, and from the statements of colleagues and competitors. The quotes from Fulton and Souders present points that all teachers should ponder.

Perhaps recitation of a coalescence of some recent experiences in recruiting will help teachers comprehend what a number of industrialists are trying to say. Fundamentals seem to be taught as an end unto themselves, not as tools to be used in the true engineering sense, because most of the teachers' time, research and study are focused on fundamental phenomena. Unfortunately the student lacks the experience to differentiate between the teacher's environment and the things that really need to be done in industry. Consequently, the new graduate is unprepared to face the situation when he learns that the very important problems of industry and society are usually interdisciplinary. Too frequently he retreats from situations offering real opportunity for growth and prosperity to the security of organizations with large sections of people working in the same discipline. It is truly a shame that

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The following problem on transport phenomena were contributed by Professor Ray Fahien, University of Florida.

hollow rear. When flow is stopped, the radial profile quickly disappears. Estimated from the Einstein relation,\* this time of disappearance should be in the order of 20 sec in the present system. After flow is restarted, the radial concentration profile reappears. The student can witness the formation of the axial concentration gradient by noting the presence of a non-uniform (laminar) velocity profile and a small radial variation in concentration both in front of and behind the tracer patch. The fact that the radial concentration gradient has decayed to a fraction of its initial value, while remaining the cause of the axial spreading frequently seems paradoxical to a student who has not seen the dispersion phenomenon. However, actual observation of the dispersing tracer during its journey helps resolve this paradox.

It is a straightforward matter to derive an alternative to Equation (1) to describe a step-input tracer rather than a pulse tracer. In practice, however, our experience indicates that step inputs of about 4%  $\text{KMnO}_4$  solution do not fit the predicted results as well as pulse inputs in a horizontal tube. The discrepancy would appear to result from small density differences between water and  $\text{KMnO}_4$  solution. The use of pulse tracers obviates this difficulty to a large extent.

Finally, Taylor showed that the characteristic Gaussian pattern did not appear until the following inequality was satisfied:

$$L/u_0 \ll \frac{a^2}{3.8^2 D}$$

An order of magnitude estimate is required for  $D$  initially, to estimate how long flow must proceed before the axial concentration profile will become Gaussian. Using the calculated value of the molecular diffusivity, it must finally be verified that the above inequality was, in fact, obeyed.

For the results shown in Fig. 1, the molecular diffusion coefficient was calculated to be  $0.7 \times 10^{-5} \text{ cm}^2/\text{sec}$  which compares favourably with Taylor's value of  $0.80 \times 10^{-5} \text{ cm}^2/\text{sec}$ .

### ACKNOWLEDGMENT

The author wishes to acknowledge the assistance of Messrs. J. Buchanan and V. Arunachalam in setting up and developing the apparatus.

\* $D = \bar{x}^2 / 2\tau$ , where  $\bar{x}^2$  is mean square displacement and  $\tau$  the time over which the displacement occurs. We may set  $\bar{x}^2 \approx a^2$  for present purposes.

A nuclear engineer is interested in predicting the temperature buildup in a nuclear reactor in which an annular fuel element is cooled by maintaining the inner and outer walls at a temperature  $T_0$ . The fuel element is initially at  $T_0$  also. At time  $t = 0$ , the nuclear reaction is permitted to take place and heat is liberated in the annulus at a rate (assume constant) of  $S_N (\text{Btu}/\text{ft}^3\text{-hr})$ .

a. Show how his problem is analogous to the momentum transport problem of unsteady state flow in an annulus of radii  $R_1$  and  $R_2$ , of an incompressible fluid of density  $\rho$  and viscosity  $\mu$ , with a velocity in the  $z$  direction of  $v_z$  and under a pressure drop (including gravity) of  $(p_0 - p_L - \rho g L) / L$ .

b. Write expressions for the total heat transport  $Q$  Btu/hr from the reactor walls and for the analogous momentum quantity. Repeat for the average velocity  $V$  and the analogous energy quantity.

c. Show how a knowledge of  $V(t)$  can be used to obtain  $Q(t)$ .

d. Show that this analogy can also be used in more complicated systems such as those in which several cooling tubes penetrate a cylindrical fuel element even though an analytical solution is not possible. Derive the general relation between  $V$  and  $Q$  and outline a procedure whereby experimental data on  $V$  can be used to obtain  $Q$ . State which dimensionless variables should or should not be made the same in each system.

### LETTERS

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the near-endless font of tax dollars diverting engineering teachers into science research and graduates into massive science-oriented programs is costing industry so much of the basic engineering talent needed for the expansion and profits to pay the taxes and clean up our environment. If more "scientific engineers" were trained, the outlook for our companies, plants and cities would be healthier.

Rex T. Ellington, Mgr.  
Sinclair Oil Corp.

Editor: The article by Dr. Sleicher entitled "Humanities and Social Science In Engineering Curricula" in the Spring, 1968 edition of Chemical Engineering Education was read with interest. Having been exposed to some 18 years of industrial experience with two major United States corporations, the need for development of "values"



is readily apparent to me.

How many companies will **deliberately avoid** development of products primarily geared towards destruction of fellow human beings?

How many industries will **take the lead** in controlling pollution, even when the cost will reduce profits and dividends, at least for several years?

How many individual engineers will **consciously turn** the attention of management toward their worthy peers, even at the risk of being passed by themselves?

How many graduate students and their advisors would **refrain from early publication** of a research effort, to avoid destroying the efforts of another group or institution working in a similar field?

In other words, how many of us at any level of our society are more interested in others than in ourselves? Can courses in humanities change these basic patterns of human behavior? Or is a far more drastic, more unpopular and more "unsettling" change needed? And could it be that even 2000 years later, the needed change still begins and ends with the Person who said, "So whatever you wish that men would do to you, do so to them, for this is the law and the prophets."

Leigh E. Nelson  
Hastings, Minn.

Editor:

It is gratifying to find that there are others who assert the validity of a macroscopic derivation of the basic equations of irreversible thermodynamics. However, it is not immediately obvious why Professor Wallis considers his derivation more correct conceptually and more useful in practice than ours [CEE, 2, No. 3, 109-112 (1968)]. The question of using the idea of "lost work" as opposed to the "rate of entropy generation per unit volume" involves something more than a matter of taste despite the fact that  $lw \equiv TS_p$ . (It is not clear how Professor Wallis distinguishes between the system property, S, and entropy production,  $S_p$ .)

The assumptions inherent in Professor Wallis' Equations (1) and (2) are certainly not less tenable than the assumptions of the bilinear form of the entropy production and the restricted definitions of the fluxes and forces in the microscopic derivation according to Onsager. However, such assumptions must be examined for generality. Although at this point in the derivation there are no limitations imposed on the magnitude of the fluxes or forces, our derivation shows that lost work (or entropy production) can be treated as an exact differential only for the case of discontinuous or steady state systems in which no work is transferred at any stage of the process. Further extension to other processes can be made only as approximations to special cases.

Aside from these details, the critical point in Professor Wallis' derivation, as well as ours, is the utilization of the concept of an exact differential, the significance of which has been apparently overlooked in previous derivations based on the microscopic approach.

As a final point, Professor Wallis raises the question "of just why 's' should be a homogeneous function of the second degree in the fluxes or potentials." In an earlier paper [Sliepevich and Finn, *Ind. Eng. Chem. Fund. Quart.* 2, 249 (1963)], we attempted to show that the assumption of small fluxes or forces leads to an arbitrary

analytic function for which all terms of higher order than two can be neglected as a first approximation. However, such a series expansion raises some questions as to the method of combining the terms without making some a priori assumptions regarding symmetry. On the other hand, to the extent that lost work can be represented as a quadratic in either the fluxes and forces, it seems reasonable to conclude from our Equation (3), and the basic postulates following it, that lost work is a homogeneous function of the second degree in the fluxes or forces.

In summary, the principal difference between our derivation and the one proposed by Professor Wallis is that we have attempted to show how, and under what conditions, the functional form of lost work arises as a direct consequence of the mass, energy and entropy balances and the Gibbs' equation. We prefer this approach rather than simply asserting the form of the function.

C. M. Sliepevich  
University of Oklahoma

Editor:

I have noted with some dismay the continued claims by C. M. Sliepevich and co-workers to having achieved a derivation of the Onsager reciprocal relations of irreversible thermodynamics from macroscopic principles alone. In-as-much as it is well known that this cannot be done without the benefit of additional microscopic information such as time reversibility in the dynamics of molecular encounters, it is tempting to pass these claims off as being preposterous were it not for their reputation and their potential for misleading the uninitiated. As noted by the authors of the most recent publication, a negation of their macroscopic "derivation" was offered previously by F. C. Andrews, but this criticism has been inconclusive. The matter of whether lost work could be regarded under certain prescribed conditions as being path independent is treated correctly by Sliepevich et. al.

The actual error in the recent publication occurs as follows: having concluded that the lost work has the generic form

$$dg = X dx + Y dy \quad (1.1)$$

with dg an exact differential and X, Y the affinities or driving forces for transfer conjugate to x and y, the authors use the special case or "rate form" of (1.1) namely

$$\frac{dg}{d\theta} = \dot{g} = X\dot{x} + Y\dot{y} = X \frac{dx}{d\theta} + Y \frac{dy}{d\theta} \quad (1.2)$$

together with the postulate that g assumes the quadratic form

$$g = \alpha x^2 + \beta xy + \gamma y^2 \quad (2.1)$$

with dependence of  $\alpha$ ,  $\beta$ ,  $\gamma$  upon state variables permitted. The argument proceeds by using Euler's theorem to reexpress (2.1) as

$$\dot{g} = [\alpha x + \frac{1}{2}\beta y]\dot{x} + [\frac{1}{2}\beta x + \gamma y]\dot{y} \quad (2.2)$$

or

$$dg = \dot{g}d\theta = [\alpha\dot{x} + \frac{1}{2}\beta\dot{y}] \dot{x}d\theta + [\frac{1}{2}\beta\dot{x} + \gamma\dot{y}] \dot{y}d\theta \quad (2.3)$$

Then by assuming that  $dx \equiv \dot{x}d\theta$  and  $dy \equiv \dot{y}d\theta$  may be regarded as independent differentials in (2.3), the square bracket coefficients of (2.3) are compared with (1.1) to conclude that

$$X = \alpha\dot{x} + \frac{1}{2}\beta\dot{y} \quad ; \quad Y = \frac{1}{2}\beta\dot{x} + \gamma\dot{y}$$

from which

$$\dot{x} = \frac{\gamma X - \frac{1}{2}\beta Y}{(\alpha\gamma - \frac{1}{4}\beta^2)} \quad ; \quad \dot{y} = \frac{-\frac{1}{2}\beta Y + \alpha X}{(\alpha\gamma - \frac{1}{4}\beta^2)}$$

obey the reciprocity relation. This procedure is, of course, equivalent to identifying the square bracket coefficients

of  $\dot{x}$  and  $\dot{y}$  in (2.2) **individually** with  $X$  and  $Y$  in (1.2), and is clearly invalid for one could just as well have written (2.2) as

$$\dot{g} = [\alpha\dot{x} - \beta\dot{y}] \dot{x} + [\gamma\dot{y}] \dot{y} \quad (2.4)$$

and concluded by the same argument the asymmetrical form

$$X = \alpha\dot{x} + \beta\dot{y} \quad ; \quad Y = \gamma\dot{y}$$

The difficulty with the procedure is that in (2.1) there is but one independent variation, that of the time parameter  $\theta$ . The source of the difficulty may be traced

to the fact that (2.1) with  $\dot{g}$  positive definite is not a proper statement of the postulate of irreversible thermodynamics. Rather, it is necessary to proceed from (1.2)

with the postulate that the fluxes,  $\dot{x}$  and  $\dot{y}$  are linear in the affinities,  $X$  and  $Y$ , to deduce (2.1). Obviously the values of  $\alpha, \beta, \gamma$  are determined by the symmetrical portion of the phenomenological matrix alone, and no amount

of manipulating the  $\dot{g}$  forms can yield conclusions about the remainder of the phenomenological matrix. Although

we have the restriction that  $\dot{g}$  is positive definite, there

is nothing to say that  $\dot{g}$  be an "even function" of  $\dot{x}$  and  $\dot{y}$ .

Finally, your authors seem not to have recognized that if molecular models are even conceivable which violate microscopic reversibility and yet are compatible with the phenomenological approach, one need go no further to conclude that the macroscopic theory per se has no more inherent capability of predicting reciprocal relations than it has of predicting numerical values of transport coefficients, short of direct measurement.

Duane W. Condiff  
Carnegie-Mellon University

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