TO THE EDITOR:

Yu and Douglas (1975) have published, for application to a continuous crystallizer, a procedure for estimating the amplitude of a limit cycle that has been used in several earlier publications by Douglas and his co-workers. Acknowledging that the procedure gives qualitatively wrong conclusions about the character of the system in some cases, the authors explain that this problem arises because the procedure uses truncated Taylor expansions of certain functions appearing in the description of the system and that these truncated expansions do not give a correct topological description of the system in the large. Among the references they give to support this argument appears a reference to my paper of 1972.

This letter is written to point out that my paper does not support the position of the authors on this question, but explicitly opposes it. This is made clear by quoting one sentence, referring to a paper by Gaitonde and Douglas (1969): "The authors have overlooked the fact that the truncated expansion has significance only in the neighborhood of the origin of the expansion, so that its behavior in the large is irrelevant."

The real cause of the difficulty is the fallacy in the procedure used in this and earlier publications. It involves introducing a factor which is treated as arbitrarily small and also as being equal to unity. The failure of the method is demonstrated by its application to the system

$$\frac{dx}{dt} = 2\alpha x - y + 3x^2 - 3xy + \frac{1}{3}x^3$$

$$\frac{dy}{dt} = x$$

Here there is no question of Taylor expansions, and the only singular point is at the origin. The characteristic values of the linearized system are $\alpha \pm i\beta$, where $\beta = (1 - \alpha^2)^{1/2}$. After the transformation $x = u$, $y = \alpha u + \beta v$, the system is in the correct canonical form, and the procedure in question may be applied. It is worth noting that in the first approximation the procedure permits no contribution from the second-degree powers and products. The calculated value of the amplitude of the limit cycle in the first approximation is $(-8\alpha)^{1/2}$, which leads to the conclusion that no small limit cycle exists when α has a small positive value. The expressions given by Beek (1972), on the other hand, give the qualitatively different result $\alpha^{1/2} + \alpha + 0(\alpha^{3/2})$.

The validity of these results may be tested empirically by numerical calculation. The equations were integrated between two successive intersections of the trajectory in the phase plane with the positive x axis. The table shows x_o, the initial value of x, and $x_1 - x_o$, the corresponding change in x in the integration. A very small value of α, 4×10^{-6}, was used to provide a check on the accuracy of the analytically calculated amplitude, which is 2.004×10^{-3} in this case.

$10^3 x_o$	$10^8 (x_1 - x_o)$
1.004	1.8906
2.004	−0.0092
3.004	−9.0494

These numerical results show that the system actually has a small limit cycle when α has a small positive value.

I am glad to acknowledge the generous permission of the staff of the Zeeman Laboratory of the University of Amsterdam to use the Laboratory's C.D.C. 6400 for the calculations.

LITERATURE CITED

Yu, K. M., and J. M. Douglas, "Self-generated Oscillations in Continuous Crystallizers: Part 1. Analytical Predictions of the Oscillating Output," *AIChE J.*, **21**, 917 (1975).

Beek, John, "Small Oscillations in Undistributed Autonomous Systems," *AIChE J.*, **18**, 228 (1972).

Gaitonde, N. Y., and J. M. Douglas, "The Use of Positive Feedback Control Systems to Improve Reactor Performance," *AIChE J.*, **15**, 902 (1969).

JOHN BEEK
5620 Harbord Drive
Oakland, California 94618

TO THE EDITOR:

We believe that there is a fundamental error at an early stage in a paper by G. Grossman ["Stresses and Friction Forces in Moving Packed Beds," *AIChE Journal* **21**, 720 (1975)] that completely negates the subsequent conclusions. The error appears in the footnotes to pages 723 and 724. The former, which states that "The column and flow are clearly symmetrical with respect to the y-axis," implies a sign convention for shear stress that introduces the singularity mentioned in the second footnote. In fact the shear stress distribution is antisymmetric about the y-axis and $\partial\tau/\partial x$ cannot be taken as zero below the Rankine zone. This then invalidates the centre-line boundary conditions, viz. (16) $\sigma = y/(1 + \sin\delta)$ and (18) $\sigma_y = y$. A sufficient boundary condition for the method of characteristics is merely the other part of (16), i.e. $\psi = 0$.

The analogy with Poiseuille flow between parallel planes is helpful in this context. Again the flow is clearly symmetrical, but the strain rate, $\partial v/\partial x$, and hence the shear stress, is antisymmetric and $\partial\tau/\partial x \neq 0$ on the y-axis.

R. M. HORNE,
R. M. NEDDERMAN
Dept. of Chemical Engineering
University of Cambridge
Cambridge, England

TO THE EDITOR:

I have checked the points raised in the letter from Professors Horne and Nedderman and I agree completely. There is an error in the center line boundary condition (16) and (18) stating that at $x = 0$ $\sigma y = y$. While this boundary condition is valid in zone 1 along the line OB, it does not hold below. This error results from the wrong impression that along the center line where $\partial\tau/\partial x$ changes sign in the presently used coordinate system, one can use $\partial\sigma y/\partial y = 1$ for symmetry arguments which is equivalent to setting $\partial\tau/\partial x = 0$ in equation (12). $\partial\tau/\partial x$ itself *was not* assumed zero on the center line as shown in Figure (3c) and equation (23).

I also agree that in equation (16) $\psi = 0$ on the center line is a sufficient boundary condition for the method of characteristics. This is in fact the way equations (14) and (15) had been solved, without the additional condition $\sigma = y/(1 + \sin\delta)$. What surprises me is the good agreement between this solution and the one obtained by the integral method (Fig. 5) which now turns out to be incorrect. I intend to revise the integral solution and compare it again with the exact one by the method of characteristics.

DR. G. GROSSMAN
Technion—Israel Institute
of Technology
Haifa, Israel

ERRATA

The article "Constitutive Equation for Vapor Drift Velocity in Two-phase Annular Flow" by Mamoru Ishii, T. C. Chawla, and N. Zuber (*AIChE J.*, Vol. 22, No. 2, 283-289, 1976) has errors in Figures 1 and 2. The numbers of Equations in the Figures should read, respectively, Eqs. (28), (29), (30), (32), (33), and (35) instead of Eqs. (21), (22), (23), (25), (26), and (28).