

Heat Transfer to Air-Solids Suspensions in Turbulent Flow

S1R: The correlation proposed by Sadek [*Ind. Eng. Chem. Process Des. Develop.*, 11, 133 (1972)] for heat transfer data on air-solids suspensions has two serious flaws. First, the idea of a correlation based on use of a multiplier of the gas-only heat transfer coefficient to account for the effect of solids in the gas could not be valid unless the multiplier for a given solids loading was independent of Reynolds number. However, h_0 varies as the 0.8 power of Re in turbulent flow, whereas for catalyst suspensions at W_s/W_g of 2.0 or higher, h varied as the 0.66 power of Re not only in turbulent flow but also in transition and viscous flow (Danziger, 1963). The absolute value of the h/h_0 ratio is thus Reynolds-dependent, so the proposed correlation cannot be correct.

Second, and ultimately more important, the author apparently did not check the reliability of the data he used. The following comments are made on the sources.

Mickley and Trilling (1949), in one of the earliest investigations of the heat transfer characteristics of fluidized beds, used an apparatus design with an enlarged cross section above the heat transfer tube to serve as a disengaging section. The highest velocity runs were assumed by Sadek to be examples of transport. However, an enlarged cross section disengages solids by reducing gas velocity and providing flow area for gravity return of the solids along the wall. The returning solids have lost so much of their original aerating gas that the wall flow has at least several times the density of the bed. Because of that density difference, the downward wall flow persists for a considerable distance; the authors' observation that the solid fell back down the column could not have been made unless the downflow was at the wall where they could see it. The upward gas flow occurs in the core where resistance is lower. A limited comparison of heat transfer in dense-phase upward transport with heat transfer in fluidized beds (Danziger, 1963) indicated that the bed, h , might be an order of magnitude higher than the transport, h , at the same conditions. The obvious physical difference is that in beds the solids move down along the wall and in transport the solids perforce move up along the wall; why this should have so great an effect on h is not yet known. The relatively high values of h for the runs in question are typical of downflow at the walls rather than upflow. Besides these fluid flow and heat transfer considerations, the combinations of airflow and mixture density that were reported appear to rule out the possibility of transport. Unless unequivocal experimental evidence is available showing that so much solid can be transported in stable flow with so little air in a tube of the size used, it cannot be accepted that any of the Mickley and Trilling data are representative of true transport.

In Sadek's Figure 2, the four top values of $\Delta h/h_0$ from the data of Farbar and Morley (1957) are easily identifiable. From a calculation of the values of nd^2D for those runs, it appears that the points have not been plotted at the correct abscissas. Of the quantities needed for the calculation, two require making an assumption in order to evaluate. One, the operating pressure, has little possible leeway; airflow was induced by an industrial vacuum cleaner at the downstream end, and the test section was near enough to the upstream inlet to have been just slightly below atmospheric pressure. The calculated air density of 0.07 lb/ft³ is certainly about as close as we can get without knowing the barometer and humidity readings at the time of each run. The second assumption is required to

evaluate the area-mean diameter of the catalyst; Farbar and Morley did not give a size distribution for the 11% by weight smaller than 12 μ . Assuming a minimum size of 9 μ , the mean for the catalyst is 33 μ . On the basis of those assumptions, and using the authors' values for the other quantities, the calculated abscissa for each of the four points is only about 63% of the value used by Sadek. It does not appear possible to account for the discrepancy except as an error in calculation or as an unrealistic assumption of submicron fines in the catalyst. The result is that the Farbar and Morley points are much farther from the Sadek correlation line than shown in his Figure 2. A comparison of predicted and actual values of h is given below.

The data on commercial operation with cracking catalyst (Danziger, 1963) were not used by Sadek. To extend the comparison of catalyst data, the writer evaluated nd^2D at the two lowest and the two highest values of W_s/W_g (at normal operation) for each of the two tube diameters reported in that paper, using 40 μ as the area-mean diameter. The average absolute deviations of the actual values of h from those predicted by use of the Sadek correlation, and the direction of deviation of the four individual points in this check of catalyst data, are as follows: Farbar and Morley, 57%, all plus; 1 $\frac{1}{2}$ -in. o.d. tubes, 34%, two plus, two minus; 1 $\frac{7}{8}$ -in. o.d. tubes, 49%, all minus. For the same points in the same order, comparison with the writer's correlation gives 26%, all plus; 5.4%, all minus; and 5.5%, two plus, two minus. Use of 33 μ instead of 40 μ as mean diameter of the commercial catalyst would change the average deviations to 30% and 57%, and use of 50 μ would change them to 39% and 36%; there is therefore no basis for attributing the disagreement to use of an incorrect diameter. However, the disagreement can certainly be attributed at least in part to the manner in which solids loading is treated. Farbar and Morley's finding that h varied as $(W_s/W_g)^{0.45}$ at constant Reynolds is well documented and has been confirmed by others. Sadek's equation proposing that Δh varies as $(W_s/W_g)^{1.19}$ cannot correctly predict the effect of change of solids loading at constant Re except by coincidence.

Farbar and Depew (1963) did not specify which of the four thermocouples in their adiabatic section was considered to be measuring the outlet temperature used in calculating the *LMTD*. However, in a related paper (Depew and Farbar, 1963), the same authors calculated equilibrium mixture temperature at the outlet for use in calculating *MTD*. It may be presumed that the same general philosophy was followed in the design of the apparatus and in the calculations for this paper, and that the final temperature at the downstream end of their adiabatic section was taken as their outlet (equilibrium mixture) temperature. But the driving force for heat transfer is the temperature difference between the wall and the gas, the continuous phase; use of an adiabatic or equilibrium mixing temperature results in a false *MTD* and h . Unfortunately, the data given in the paper are insufficient for estimating the magnitude of the error thus introduced; it is probably very slight for the 30 μ beads, but it could be substantial for the 200 μ beads (estimated $\Delta t_g/\Delta t_0$ in the heat transfer section about 0.9 for the small size and 0.1 for the large, at the minimum velocity used). And, since the error is greater the larger the particle, use of these data could result in a false estimate of the effect of particle diameter on h .

Jepson et al. (1963) reported values of h for air and for

air-solids mixtures. The values reported for air-only runs range from 14–20% lower than would be predicted by the Colburn equation. The Farbar and Morley calibration data, by contrast, show an average deviation from the Colburn equation of $1\frac{1}{2}\%$, a maximum deviation of 3%. Their data on the air-solids system have much greater variation, but that is to be expected with a difficult two-phase system. The Jepson results on the simple air-only system are indicative of difficulties with their apparatus and/or instrumentation; the built-in errors make their data on the two-phase system of very doubtful utility.

Wilkinson and Norman (1967) gave no hint of the pressure or temperature levels of their experimental runs. Specific information on those variables is required for the calculation of gas density, which occurs in Sadek's proposed correlation as the 1.19 power. Sadek has not noted his source of the data. In the apparatus used, the test section was heated electrically, but no formal cooling of the recycled air and solids was provided. Heat loss, mainly from the unlagged portion of the system, would presumably cool the separated phases different amounts, and even after recombining and passing through the acceleration section, the phases might well have been at

different temperatures when entering the test section. In the test section, of course, the air temperature increased more than the solids temperature. However, the authors used a "mean bulk suspension mixing temperature" over the entire heated length for evaluating the *MTD*. There was thus an error introduced in not using air temperature in and out, and another error if the terminal temperature differences were of a ratio requiring a logarithmic rather than an arithmetic mean. Here again, from the data in the paper there is no way to evaluate the magnitude of the possible error. Interestingly, if correction of the errors would have only a minor effect on the reported results, the latter would confirm that the h/h_0 ratio is *not* independent of *Re*.

The last three papers discussed above reported some values of h for air-solids mixtures that were less than, or only equal to, h_0 . Sadek's Figure 2 is based on $\Delta h/h_0$ (the relative increase in h), using a logarithmic scale, so zero or negative values of Δh cannot be plotted. In effect, a selective discard of some of the data indicated as being included has resulted.

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SIR: Danziger finds flaw with two aspects of Sadek's (1972) article on the heat transfer to air-solids suspensions. First, he criticizes the lack of dependence of the ratio ($\Delta h/h_0$) in Sadek's correlation on the particle-free gas Reynolds number and claims that data are available which not only confirm that dependence but also allow its exact determination. Second, Danziger questions the reliability of all the data gathered from different sources and used by Sadek in developing his correlation and then proceeds to review the sources of these data in an attempt to point out where Sadek has erred.

A series of distressing misconceptions are evident in Danziger's arguments and he presents no sound reason for refuting Sadek's conclusions or for rejecting the data Sadek has used.

First, consider Danziger's first point. The experimental data upon which Danziger bases his conclusion that the heat transfer coefficient, h , varies as the 0.66 power of the Reynolds number were gathered in tubes having three different diameters (Danziger, 1963). The largest tubes were $1\frac{7}{8}$ -in. o.d.; these tubes were used to gather data points clustering around a Reynolds number of 10,000 (between 8990 and 10,440). The intermediate tubes had a $1\frac{1}{2}$ -in. o.d.; these tubes were used to gather two data points at Reynolds numbers of 178 and 267 (these constitute the extent of Danziger's "viscous" and "transition" flow data) and another cluster of data points at a Reynolds number of 5000 (between 3870 and 5410). The third set of data points used by Danziger was gathered by Farbar and Morley (1957) in a 17-mm i.d. tube at Reynolds numbers ranging between 13,500 and 27,000.

It is absurd to use these data alone as a basis for determining the dependence of the heat transfer coefficient on the Reynolds number. First, Danziger's conclusion was heavily influenced by the two points at the low Reynolds numbers without any consideration of the possibility that the dependence on the Reynolds number may be different at low and high Reynolds numbers. If these two points are eliminated, there remains no justification for Danziger's conclusion.

Second, these data were gathered while varying pipe diameters and Reynolds numbers simultaneously without any regard to possible pipe diameter dependencies. Third, Danziger offers no reassurance that Farbar and Morley's particles were identical to the ones used in his own tests. Failing to recognize these limitations and failing to recognize that h , the heat transfer coefficient in the presence of particles, must approach h_0 , the heat transfer coefficient in particle-free flow at the same superficial velocity as the solids loading is reduced—i.e., that ultimately h must vary as the Reynolds number raised to the 0.8 power, leave little confidence in Danziger's conclusion.

To detect a dependence of the relative increases in heat transfer coefficient ($\Delta h/h_0$) on the Reynolds number raised to, say, the 0.14 power, as may have been Danziger's intention to suggest, while using data whose accuracy is on the order of $\pm 15\%$, it is necessary to vary the Reynolds number over at least a sixfold range $[(1 + 2 \times 0.15)^{1/0.14} = 6.5]$, this, while maintaining the other variables within a range where their behavior is known and predictable. If the accuracy level of the data is, say $\pm 20\%$, the Reynolds number must cover over an order of magnitude for establishing the relationship suggested by Danziger. Contrary to Danziger's claim, further data are unquestionably needed to establish the dependence of the heat transfer coefficient of particle-bearing airstreams in turbulent flow on the Reynolds numbers.

With reference to Danziger's review of the literature, the following comments are pertinent:

Mickley and Trilling (1949) reported downward flow of particles from the disengagement section into the straight section of their apparatus. Within the core of the straight section, it is easy to show that particles falling from the disengagement section in all of Mickley and Trilling's higher velocity runs will reverse direction and flow upward within a distance of about 1 ft (for largest particles) or less (for smaller ones). This distance was estimated by assuming, conservatively, that the particles fell into the top of the straight section at their free-fall velocity; in fact, their

downward velocity is lower. The length of the straight portion of Mickley and Trilling's apparatus above the actual test section was clearly greater than 1 ft. In the core, therefore, excessive downward flow will occur only near the exit portion of the straight section. Particles falling near the wall from the disengagement section will experience two forces directing them toward the core: inertia from their radial velocity along the sloping wall of the disengagement section and the lift force associated with the presence of a spherical particle within a flowing gas having a nonuniform velocity (Rouhainen and Stachiewicz, 1969). In an upflowing gas, this force directs particles falling near the wall toward the center. This force is large for particles falling within the boundary layer with its high velocity gradients, and may be on the same order of magnitude as the weight of the particle, sufficient to propel the particles toward the core within a very short period of time where they will then be carried upward by the gas.

Danziger's concern about flow stability in Mickley and Trilling's experiments under transport conditions is perhaps exaggerated. In these tests, Mickley and Trilling reported solids volume fractions of about 0.02–0.075 (one point was reported with a solids volume fraction of 0.0912). These are comparable to the volume fractions of the two low-velocity data points upon which Danziger relied so heavily in developing his correlation.

The solids volume fractions commonly used in transport are appreciably lower than those encountered in fluidized bed. This explains the difference in magnitude between transport and fluidized bed heat transfer coefficient. It is not surprising that this was not recognized by Danziger in his comparison between the two phenomena: Danziger based his comparison on an extrapolation of his own relationship which relies on an excessively low dependence of the heat transfer coefficient on the solids loading ratio. In his correspondence, Danziger states that the heat transfer coefficient, h , is dependent on the solids loading ratio raised to the 0.45 power (at solids loading ratios greater than 2) and claims that this variation is well documented. It is unfortunate that Danziger did not recognize that in deriving this relationship, Farbar and Morley (1957) implicitly combined the contribution due to the presence of the particles together with the basic convective heat transfer. This implicit combination, resulting from relating h itself to the solids loading ratio, leads to an averaging of the exponent on the solids loading ratio in an expression of the form $h \propto (W_s/W_g)^n$. At low solids loadings, h is essentially independent of the solids content and is approximately equal to h_0 . The exponent, n , is then close to zero. As the solids loading ratio is increased at constant gas flow the rate of change of h with solids content also increases and the value of n rises. At very high loadings, the value of n approaches an asymptotic value. This occurs when the overall heat transfer coefficient is much greater than the basic convective value. Use of a single exponent on the solids loading ratio to express the heat transfer coefficient over a broad range of solids loadings is therefore incorrect and grossly misleading. The effect of averaging of the exponent can be reduced by correlating the increase in heat transfer to the solids loading—i.e., by relating $\Delta h/h_0$ to the solids loading ratio. A glance at Farbar and Morley's plot of h/h_0 against the solids loading ratio (Figure 5, Farbar and Morley, 1957) indicates that the exponent, m , on the solids loading ratio in a relationship of the form: $(h/h_0 - 1) \propto (W_s/W_g)^m$ is slightly greater than unity as evidenced by their graph curving upward on their linear scales. Furthermore, all data published in the

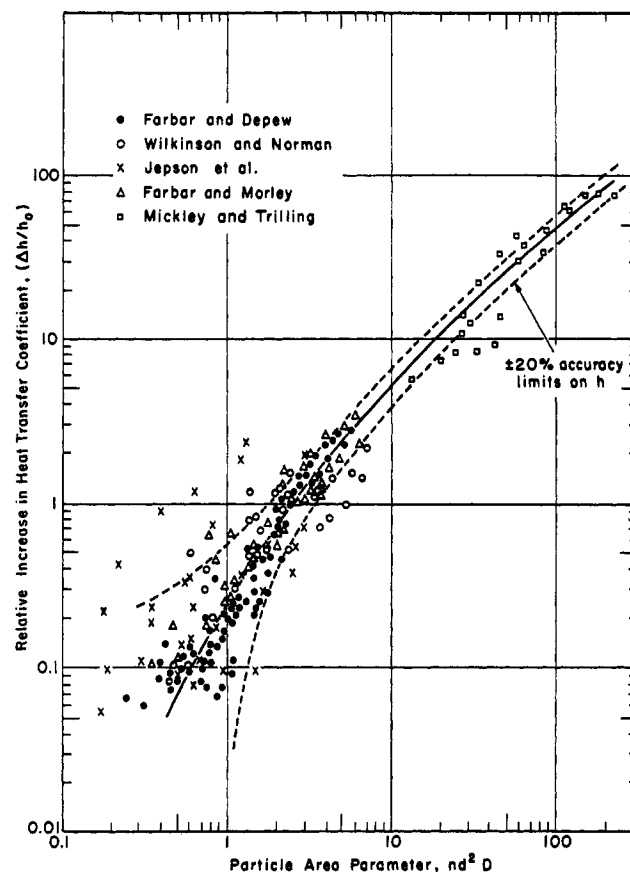


Figure 1. Correlation between increase in heat transfer coefficient and particle area parameter

open literature, including Danziger's own data, confirm that the exponent m is closer to unity than it is to 0.45. It appears that Danziger has taken Farbar and Morley's results out of context and drawn an erroneous conclusion by failing to recognize such a simple point.

Sadek (1972), in plotting Farbar and Morley's data used an incorrect mean particle size. Instead of 30μ , a mean particle diameter of 20μ was used. When this error is corrected, it is observed that Farbar and Morley's data fall along Farbar and Depew's (1963) data. In pointing out this error, Danziger helps support the view that the parameter nd^2D correlates very well the heat transfer to air-solids suspensions by reducing the variance of the correlation using different sources of data. A plot of the corrected data is shown in Figure 1. The curve through the data represents a visual fit and is an improvement over the two-constant relationships suggested previously by Sadek. The dashed lines represent $\pm 20\%$ deviations from the mean on the heat transfer coefficient, h .

Danziger "presumes" that Farbar and Depew (1963) used a "mixture temperature" at the outlet in calculating their logarithmic mean temperature difference (*LMTD*). Danziger derives his concern from another article published by Depew and Farbar (1963) in which these authors evaluate Tien's (1961) homogeneous fluid model. In this latter article, Depew and Farbar describe in detail their definition of this unconventional temperature driving force. In Farbar and Depew's article, on the other hand, beyond the author's explicit statement of their use of a logarithmic terminal temperature difference, they make no comment of any unconventional temperature definition. It is therefore

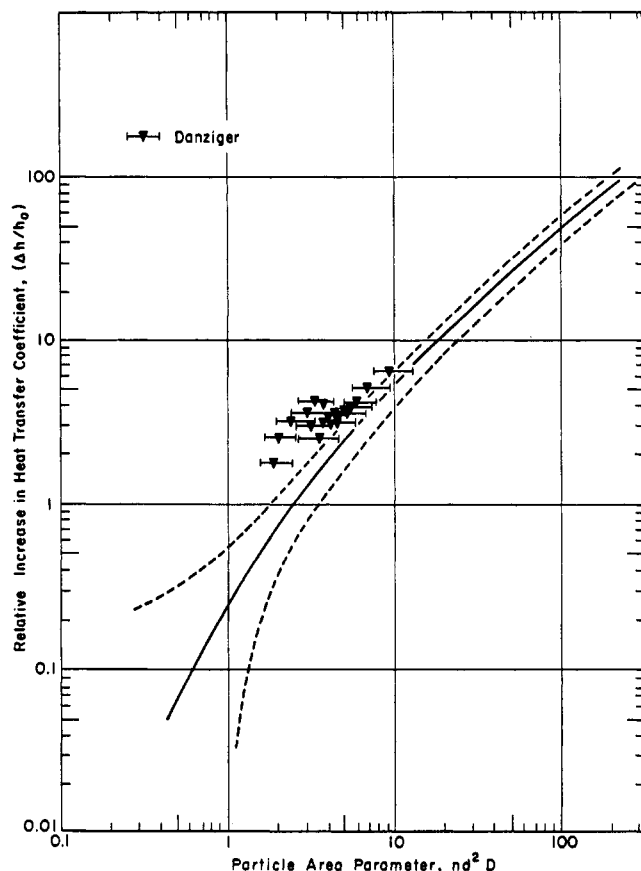


Figure 2. Comparison between correlation of Figure 1 and Danziger's data

Points calculated for a mean particle diameter of 40 μ ; lower limit represents a mean particle diameter of 50 μ , upper limit a diameter of 30 μ

reasonable to assume that Farbar and Depew refer to actual temperature measurements—i.e., gas temperatures. Furthermore, contrary to Danziger's implications, had these authors used a "mixture temperature" driving force, the errors resulting from reducing these data would have been quite small. Even though solid and gas temperature changes may be quite different from one another in a set of experiments, the effect of this difference on the driving force is generally small and decreases with an increasing driving force. Consider, for example, an extreme case when the solids temperature change is only one tenth the gas temperature change within the test section—i.e., $\Delta T_s/\Delta T_g = 0.1$. When the heat capacity of the solids is much greater than the heat capacity of the flowing gas, we may assume that the "mixture temperature change", ΔT_m , is approximately equal to ΔT_s . If the wall temperature is constant at, say, 180°F and if the inlet temperature (of both gas and solids) is 80°F while the gas warms up 40°F (i.e., leaves at 120°F), then the solids leave the unit at 84°F and the exit "mixture temperature" is also 84°F. The driving forces calculated for this hypothetical case are as follows:

The gas-based $LMTD = 78^\circ\text{F}$

"Mixture" temperature difference driving force at exit
= 96°F

"Mixture" $LMTD = 98^\circ\text{F}$

Under this extreme case there is a maximum deviation of about 26% (based on the gas $LMTD$). When $(\Delta T_s/\Delta T_g)$ is higher, when the gas has a finite heat capacity relative to the

solids and when higher overall driving forces are used, the error arising from poor definition of the heat transfer driving force—if that error does exist—is reduced to insignificance.

Danziger also questions Jepson et al.'s (1963) results because their air-only runs deviate 14–20% from the Colburn equation. Danziger compares this deviation to Farbar and Morley's calibration data which showed an average deviation from the Colburn equation of "1½% with a maximum deviation of 3%." Danziger must be aware that a deviation of 14–20% from the Colburn equation is quite common in heat transfer work. This may be checked by referring, for example, to McAdams (1954). In fact, the Colburn relationship does not pretend to predict values of the heat transfer coefficient with accuracies higher than about 5%: its proportionality constant, for example, is composed of only two significant figures (0.023). When all the uncertainties in a system are considered, a deviation of 14–20% from the Colburn relationship is not unreasonable. This reason cannot—or should not—be used as the basis for rejecting the data of Jepson et al. Farbar and Morley's calibration data are, on the other hand, surprisingly good in their agreement with the Colburn equation, especially so, since the correction that they made for the wall resistance and condensing resistance was, in some cases, as high as 41% of the resistance of the suspension to heat transfer. It is surprising that Danziger can accept and quote the apparent high accuracy of these data with no comments.

Wilkinson and Norman (1967) state that their data were gathered at high temperatures, up to 1000°F. From the diagram of their apparatus, it can be inferred that the pressure in their tests was probably close to atmospheric pressure. In reducing Wilkinson and Norman's data, Sadek assumed that the gas density was 0.035 lb/ft³. This is admittedly an approximation and as Danziger points out, the scatter in reducing Wilkinson and Norman's data can probably be attributed to this approximation.

To compare Danziger's data to Sadek's correlation, these data are shown plotted on Figure 2. These data were reduced while assuming that the gas pressure was atmospheric. Since Danziger did not report a particle size, a mean diameter of 40 μ was assumed with lower and upper limits of 30 and 50 μ . Since Danziger reported no particle density, a specific gravity of 2.45 was assumed. Danziger's data reduced in this manner fall above the correlation based on the other published data. At this point it is fit to examine the reasons for this difference. The particle sizes in Danziger's tests were ill defined, their specific gravity was not known, the gas density was uncertain. The equipment used in gathering Danziger's data was of an industrial scale with multiple tubes; mass flow rates of solids were not measured but were calculated based on a heat balance, the uniform distribution of gas and solids is uncertain, and entrance effects have not been demonstrated to be negligible. The choice between Danziger's data and the other published data is clear.

Finally, Danziger raises a trivial point; he criticizes Sadek's plotting $(\Delta h/h_0)$ vs. nd^2D on a logarithmic scale; zero and negative values, he points out, cannot be plotted on such a scale. This is true, but few other choices are available. Plotting the data as h/h_0 on a logarithmic scale or $\Delta h/h_0$ on a linear scale may perhaps be more satisfactory in indicating the zero and negative values of $\Delta h/h_0$; either method, however, is an extremely poor method for showing the augmentation at low nd^2D and $\Delta h/h_0$ values. A plot of these data on a linear scale showing their higher end will render the lower end indiscernible. A linear plot will

weigh a line drawn through the data very heavily toward the higher end without any consideration of the behavior of the lower end. Examination of the data on Figure 1 shows that the experimental data available for positive $(\Delta h/h_0)$ fall reasonably well within the $\pm 20\%$ limits on h . Perhaps Danziger's concern about zero and negative values of $(\Delta h/h_0)$ is exaggerated.

Sadek's correlation (Figure 1) was developed in an attempt to establish the major operating parameters affecting heat transfer under transport and to provide some guidance, heretofore unavailable to engineers, for estimating transport line heat transfer rates. It is believed that a step was taken toward achieving that goal by developing the use of the parameter nd^2D . Undoubtedly, as in any area of learning, further information will serve to establish the quantitative dependencies more accurately.

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Note to Bruin-Prausnitz One-Parameter and Palmer-Smith Two-Parameter Local Composition Equation

SIR: Bruin-Prausnitz (1971) and Palmer-Smith (1972) equations have, in comparison with classical NRTL equation, the following advantage: Certain restraining conditions between constants of NRTL equation must be fulfilled if the expressions for three- and more-component systems are to be consistent with Renon-Prausnitz model.

NRTL equation characterizes the binary system by means of the following constants:

$$\tau_{ij} = (g_{ij} - g_{jj})/(RT) \quad (1)$$

$$\tau_{ji} = (g_{ij} - g_{ii})/(RT) \quad (2)$$

$$G_{ij} = \exp(-\alpha_{ij}\tau_{ij}) \quad (3)$$

$$G_{ji} = \exp(-\alpha_{ij}\tau_{ji}) \quad (4)$$

Three parameters $(\tau_{ij}, \tau_{ji}, \alpha_{ij})$ are necessary for the description of a binary system. According to Equations 1 and 2, we can write for the ternary system:

$$\tau_{ij} = (g_{ij} - g_{jj})/(RT) \quad (1)$$

$$\tau_{ji} = (g_{ij} - g_{ii})/(RT) \quad (2)$$

$$\tau_{ik} = (g_{ik} - g_{kk})/(RT) \quad (5)$$

$$\tau_{ki} = (g_{ik} - g_{ii})/(RT) \quad (6)$$

$$\tau_{jk} = (g_{jk} - g_{kk})/(RT) \quad (7)$$

$$\tau_{kj} = (g_{jk} - g_{jj})/(RT) \quad (8)$$

It can easily be shown from Equations 1, 2, 5-8 that Renon-Prausnitz NRTL constants are related in the ternary system

by the equation:

$$\tau_{jk} - \tau_{kj} = \tau_{ik} - \tau_{ki} - (\tau_{ij} - \tau_{ji}) \quad (9)$$

The extension to four- and more-component systems is straightforward and it can be shown that $0.5m(m-3) + 1$ restraining conditions similar to Equation 9 must be fulfilled in the m -component system.

It is evident from the preceding discussion that we cannot use NRTL constants evaluated from binary experimental data directly to the characterization of three- and more-component systems. The entire adjustment for the multi-component system must be made in such a way that the restraining conditions of Equation 9 are fulfilled.

Both Bruin-Prausnitz and Palmer-Smith equations fulfill the boundary condition of Equation 9 automatically as is seen if we insert for g_{ii} and g_{jj} values estimated from pure component data. It can be concluded: Bruin-Prausnitz and Palmer-Smith constants (evaluated from binary and pure component data) are free of restraining conditions and can be used directly to the characterization of three- and multi-component systems.

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