

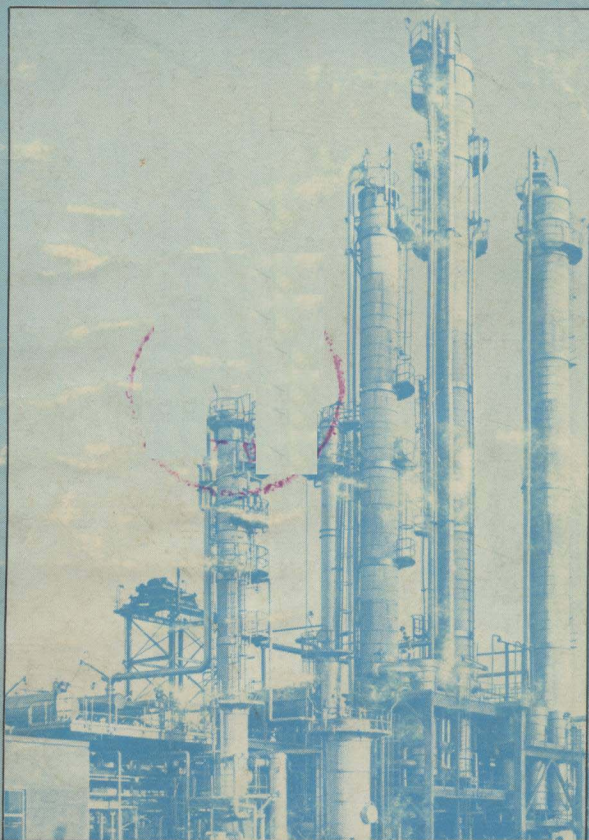
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1979

SUPPLEMENTARY DISCUSSION VOLUME TO

DISTILLATION 1979

A symposium organised by the Institution of Chemical Engineers in conjunction with the Fluid Separation Processes Group and the EFCE Working Party on Distillation, Absorption and Extraction.

London, April, 1979

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The supplementary Discussion Volume forms part of, and should be read in
 conjunction with the main proceedings of "Distillation 1979". The three
 papers further constitute Symposium Series No. 56 of The Institution of
 Chemical Engineers.

FOREWORD

This volume is a record of some of the discussion stimulated by papers presented at Distillation '79. It has been compiled from the written questions of delegates attending the symposium, and from the formal replies received from authors. Some questions had not been answered at the time of going to press, so these have been excluded from this volume.

It is arranged in session order, and although the questions and answers are not in strict paper order as presented at the symposium, there are cross references to the papers in the two-volume proceedings.

Included in this volume is the final paper of the symposium by K. E. Porter and J. D. Jenkins. It also appears in the hardback edition of the two volume proceedings published concurrently with this discussion volume.

SESSION 1 - VAPOUR/LIQUID EQUILIBRIA

PAPER: THERMODYNAMICS OF MULTICOMPONENT LIQUID-LIQUID-
VAPOR EQUILIBRIA FOR DISTILLATION -COLUMN DESIGN. 1.3/41

AUTHORS: G.Maurer* and J.M.Prausnitz**. *Universitat Karlsruhe,
W.Germany. ** University of California, Berkeley, U.S.A.

QUESTION BY: T.GANNON, Glaxo Operations (U.K.)Ltd. I would like to ask Mr. Maurer to expand on how the total pressures in the two phase region were calculated in their example.

ANSWER: I think Mr. Gannon refers to the examples given in Table 1 and figure 1. The total pressures 519, 619 and 504 mm Hg were calculated under the assumption that the liquid compositions (a), (d) and (e) are stable (one liquid phase only). Equation 1, written explicitly for each component together with the statement that the sum over all mole fractions in the vapor phase is equal one, was used in an iterative procedure to calculate the total pressure P. For these calculations the fugacity of the pure components were approximated by their vapor pressures yielding that at a given temperature and liquid composition for every component the right side of equation 1 is a constant. In the first approximation an ideal gas vapor phase was assumed ($f_i^V = 1$) and the mole fractions x_i and the total pressure calculated. For that composition of the vapor phase at the given temperature and at the calculated pressure, the fugacity coefficients were now calculated from the virial equation of state truncated after the second virial coefficient. Second virial coefficients were calculated from the method of Hayden and O'Connell. With the newly calculated fugacity coefficient the calculation was repeated yielding a slightly different number for x_i and P. The iteration was stopped when there was only an unimportant difference between the results of two consecutive calculations.

QUESTION BY: T.MAGNUSSEN, Instituttet for Kemiteknik, Denmark.

On page 1.3/46 you state that you are not concerned with calculating tie lines, and you wish only to determine whether or not two liquid phases exist. This you may accomplish without going through your short-cut method (or any other method) for calculating the phase compositions. If $\delta\Delta G/\delta n_i^I = 0$ ($i = 1, 2, \dots, n$) and the matrix

$\begin{pmatrix} \delta^2 \Delta G \\ \delta n_i^I \delta n_j^I \end{pmatrix}$ is a positive definite, you have one homogeneous phase. If not you have two liquid phases:

$$(\Delta G = RT \sum_{i=1}^N [n_i^I \ln a_i^I + (n_i - n_i^I) \ln a_i^{II}])$$

(The derivatives are all evaluated at $n_i = n_i^I$ corresponding to the actual overall composition.)

Do you agree that this rigorous approach requires no more computer time than your short-cut method?

ANSWER:

No we do not.

The main reason for this answer is that Mr. Magnussen's criteria whether or not a liquid mixture which is thermodynamically not unstable, is metastable or stable, is incorrect.

To decide whether or not a liquid mixture is unstable, metastable or stable is a question of thermodynamic stability and equilibrium. That problem is discussed extensively in many textbooks - for example cf. M. Modell and R.C. Reid, "Thermodynamics and its applications", Prentice Hall, Englewood Cliffs N.J., 1974. Therefore, there is no need to repeat the spacious derivations here. It seems to be more appropriate to demonstrate the failure of Mr. Magnussen's proposal by considering a simple example. Let us assume that the total excess Gibbs energy G^E of a binary liquid mixture can be expressed by

$$G^E/RT = A_{12} \cdot \frac{n_1 \cdot n_2}{n_1 + n_2} \quad \text{with } A_{12} = 1$$

The criterion for thermodynamic stability yields that this mixture exhibits no miscibility gap (cf. J.M. Prausnitz "Molecular thermodynamics of fluid phase equilibria", Prentice Hall, Englewood Cliffs, N.J., 1969, p.236.)

For the mixture under consideration one gets the isothermal and isobaric Gibbs energy of mixing:

$$\frac{\Delta G}{RT} = n_1 \cdot \ln x_1 + n_2 \cdot \ln x_2 + n_1 \cdot n_2 / (n_1 + n_2) \quad \text{with } x_i = n_i / (n_1 + n_2)$$

$$\text{and} \quad \frac{1}{RT} \cdot \left(\frac{\partial \Delta G}{\partial n_i} \right)_{P, T, n_{j \neq i}} = \ln x_i + (1 - x_i)^2 \quad i = 1, 2.$$

It is easily recognisable that $\left(\frac{\partial \Delta G}{\partial n_i} \right)_{P, T, n_{j \neq i}} < 0$ whenever $x_i < 1$.

Following Mr. Magnussen, one would conclude that the binary is metastable in the whole range of concentration whereas it is stable. This demonstrates that Mr. Magnussen's proposal to decide, whether or not a thermodynamically not unstable is really stable, fails.

PAPER: EQUATION OF STATE CALCULATIONS OF VAPOUR LIQUID
EQUILIBRIA IN MIXTURE CONTAINING LIGHT ORGANIC
ACIDS. 1.2/31

AUTHORS: M.Baumgartner, W.Rupp and H.Wenzel*.
* Universitat Erlangen-Nuernberg

QUESTION BY: U.ONKEN, University of Dortmund. 1.The equation (11) for the association constant does not contain fugacity coefficients,(the same applies to eq.(12) and eq. (13))... because , equilibrium calculations yielded "identical" results, within experimental error, when equilibrium constants were considered as independant of pressure. How large were the differences in the results for the two cases (calculation with and without fugacity coefficients) for pressures of 10 to 16 bar ?

2. Did you apply your method to systems with more than two components i.e. 3-, 4- component systems, possibly with two associating components? Systems of this type are of industrial interest (e.g. containing formic and acetic acid).

ANSWER: 1. The difference in the results was such that in Fig. 12 of the paper where the system acetic acid - n-butane is shown, no noticeable shift of the solid line could be observed.

2. So far we have not applied the method to multi-component systems or to systems containing more than one acid.

QUESTION BY: J.A.LILES, I.C.I., Runcorn. It has been shown that prediction of VLE from heat of mixing or other enthalpy data can fail. This is often true where empirical parameters have been fitted and there is a correlation between parameter values. Have you tried enthalpy or heat of mixing calculations using the parameters fitted for VLE by your method?

ANSWER: Yes, recently we made a preliminary test for the method to predict heat of mixing data in the systems acetic acid - benzene and acetic acid - heptane. We used the parameters we had previously taken to describe the vapour-liquid-equilibrium. The experimental values for the maximum heat of mixing values are at 16°C 97 cal for acetic acid - benzene and at 20°C 250 cal for acetic acid - heptane. The calculations yielded 138 cal and 336 cal respectively, which means that the calculated absolute values are still more than 30% too high, but the ratio of the two values agree well with experiment.

PAPER: SELECTION OF SOLVENTS FOR EXTRACTIVE DISTILLATION
USING PREDICTED AND CORRELATED VLE DATA. 1.3/23

AUTHORS: B.Kolbe, J.Gmehling and U.Onken, University
of Dortmund, Germany.

QUESTION BY: R.G.GARDNER, University of Strathclyde.
Following the remark by the chairman, Dr.D.
Zudkevitch, that close boiling binary mixtures sometimes indicated
substantial changes of relative volatility over a short temperature range,
Dr.Gardner commented that while this may happen, it is not usually
sufficient to enable the system to be readily separated by ordinary
distillations.

Where two dissimilar components have similar
vapour pressure/temperature relationships, extractive distillation offers
an attractive method of easing the separation if a third component
(solvent) can be chosen which will differentially modify the liquid
phase activity coefficients of the original components, and hence
increase the value of relative volatility.

ANSWER: The authors replied that they were in agreement
with Dr. Gardner.

PAPER: THE PREDICTION OF VAPOUR-LIQUID EQUILIBRIA
FROM HEAT OF MIXING DATA. 1.1/1

AUTHOR: S.Wilkinson, British Petroleum Company Ltd.

QUESTION BY: P.K. MUKHOPADHYAY, Engineers India Ltd., New Delhi.

1. How did the author tackle the problem of
multiple roots for NRTL or Wilson equations?

2. Would the author recommend use of heat of mixing
for systems with two liquid phases and a vapour phase?

ANSWER: 1. In an attempt to overcome the problems of
multiple roots, we used a variety of search routines and starting points.
However, as demonstrated in the paper, there appears to be no direct link
between the quality of the fit to the heat of the mixing data, and the
accuracy of the VLE predictions. At present, we see no way of overcoming
the problem.

2. In my experience there is very little data
available on the heats of mixing of partially miscible systems. However,
if it were available, I would anticipate similar problems as are
encountered in one phase mixtures.

QUESTION BY: C.J.D.FELL, University of New South Wales.
Did you find strong cross correlation of the

Wilson and NRTL coefficients as suggested recently by Eckert? This could explain the poor ability of coefficients obtained by fitting heat of mixing data to predict vapour liquid equilibrium data.

ANSWER: Cross correlation of the Wilson and NRTL equation coefficients is not something which we have investigated. However, strong cross correlation would presumably force the equations to act as if there was only one variable parameter and thereby produce a certain amount of inflexibility in their properties.

QUESTION BY: U.ONKEN, University of Dortmund, Germany .

I should like to remark that the experimental determination of vapour-liquid-equilibria will require about the same amount as is necessary for measuring heats of mixing for a system. Besides for both kinds of experimental investigation of mixtures, the main effort consists of purifying the components of the systems to be measured. Therefore, it appears to me, that one should preferably measure the data required directly, instead of calculating it from other types of data.

If VLE data for a system of organic components is not available, activity coefficients can be predicted in many cases by a precalculation method such as UNIFAC or ASOG. Especially with UNIFAC, predicted VLE data should be about as reliable as VLE data calculated from heats of mixing. In particular for preliminary design and in the assessment of process alternatives, predicted VLE are of great value. For detailed design, however, the vapour-liquid-equilibria of the key systems should be determined by experiment. When high temperature differences will occur in the distillation column e.g. with low pressure distillations, additional heat of mixing will be useful, as has been shown in the paper of Murthy and Zudkevitch.

ANSWER: If the results of our work had been different (i.e. that the predictions of VLE data from heats of mixing were reliable) we would then have investigated the possible time savings in experimental effort likely to be achieved. However, at the outset of the work, we understood from the literature that such savings could be substantial .

We would always prefer to use VLE data directly if possible, and are very interested in any methods of obtaining such data more rapidly.

As regards UNIFAC, we find it invaluable and look forward to its continued development. We would very much have liked to compare the predictions of VLE from heat of mixing data with those from UNIFAC. Unfortunately, because of the systems investigated, some of the group parameters were not available to us at the time of the investigation although I understand that this is remedied in the latest updates.

QUESTION BY: S. DATE, University of Aston, Birmingham.

Can the author comment on any new models being developed on prediction of VLE data?

ANSWER: From the literature, I understand that appreciable effort is being directed towards extending equation of state methods to represent mixtures of polar components.

PAPER: EFFECTS OF EQUILIBRIUM DATA CORRELATING EQUATIONS
ON THE DESIGN OF CONTINUOUS RECTIFICATION COLUMNS
FOR BINARY SEPARATIONS. 1.3/1.

AUTHOR: R.P Verhille, Janssen Pharmaceutica, Belgium.

QUESTION BY: M.J. DOLAN, Norton Chemical Process Products Ltd.,
Stoke-on-Trent. Was the effect of the choice of
the objective function used in obtaining the parameters of the VLE
constants taken into account when analysing the accuracy of the VLE
equations?

ANSWER: If you mean the effect of some presumed or pretended
difference in ability I attributed to, or admitted to exist between the
various correlating equations, to fully describe the Th.CVLE, the
answer is "no". The analysis concerns the global effect of the use of
each correlating equation, as a separate algebraic model, independently
of each other. In my opinion, there seems to be "a priori" no valuable
reason for allowing more credit to one correlating equation than to
another.

"A posteriori" indeed, it became clear on applying
the Redlich-Kister equal area test, as a global consistency test, to the
activity coefficients calculated with the related constants, that in this
respect, all correlating equations examined, behave similarly. Such
activity coefficients are in accordance with the Gibbs-Duhem equation,
restricted to constant temperature and constant total pressure, which
means an inconsistency according to the Gibbs "phase rule" for a binary
vapour-liquid equilibrium system.

QUESTION BY: T. GANNON, Glaxo Operations (U.K.) Ltd., Northumber-
land. In response to a question Mr. Verhille
indicated that he had taken account of the heat of mixing in his
calculations. If this is so, I wonder if he could expand this point,
explaining how he did it?

ANSWER: The way to take account of the heat of mixing in the
calculations is implicitly indicated by the unrestricted equations for
binary systems, for stepwise testing of the activity coefficients on
their thermodynamic consistency. This equation is given on page 1.3/4
of the "Distillation '79", Volume 1, Symposium Series no. 56. For any
liquid concentration x (mole fraction of acetone) considered, the
corresponding molar heat of mixing value ΔH at boiling point conditions
of the related liquid mixture, has to be accessible. At the same time,
one has to take account of the slope of the boiling point curve, dT/dx
for that liquid mixture.

Now, it is only possible to obtain the correct
values of the molar heats of mixing as well as of the slopes of the
boiling point curve, when the thermodynamically consistent x , y , t data
are already at our disposal. Since $d(A G^E/RT)$ is an exact differential

and $\Delta G^E = 0$ for the pure components, integrating from $x=0$ to $x=1$ requires that the final value of

$$\int_0^1 (\ln \gamma_1 / \gamma_2 - \frac{\Delta H}{RT^2} \frac{dT}{dx}) dx, \text{ becomes zero}$$

Because of this interdependency, and starting from experimental VLE data, the method of successive adaptations is the only way to make the equilibrium data, heat of mixing data and shape of the boiling point curve converge simultaneously to the thermodynamically consistent set of data. A direct experimental determination of the real heat of mixing at boiling point conditions of the mixture is practically not feasible. Thus, heat of mixing values are measured, either at the indicated constant pressure, but at lower temperatures, or at higher pressures for the different boiling temperatures, in order to assure a liquid phase for the pure components at those temperatures. Heat of mixing values obtained from such determinations are undoubtedly not the correct ones. To obtain the heats of mixing it is erroneous to use the relationship

$$\Delta H/T^2 = \delta \left(\frac{\Delta G^E}{T} \right) / \delta T$$

which is only valid for constant concentrations and constant total pressure, because the Gibbs "phase rule" states that binary two-phase systems have only two degrees of freedom. Furthermore, as:

$$\Delta G^E = RT x_1 \ln \gamma_1$$

is calculated from the activity coefficients, it supposes that the ΔG^E values, or at least the changes of $(\Delta G^E/T)$ with temperature, are the correct ones.

This practically means that the activity coefficients and the boiling temperatures used should be thermodynamically consistent, but they are not, and one becomes involved in a vicious circle.

A new, still unrevealed approach has been elaborated to obtain the real heat of mixing values. This method neither incorporates any direct experimental determination of heats of mixing of liquids, nor calls for thermodynamically consistent activity coefficients. It requires simply the constant pressure heat capacity values of the pure component liquids and vapours and of the vapour mixtures, together with the integral iso-basic heat of vaporization curve of the binary system.

If there exists some detectable heat of mixing of the pure vapours, these heat of mixing values should be experimentally determined, but this can be done even with slightly superheated pure vapours.

I intend to fully expose this new approach in a paper which I hope to be able to publish in the near future. That paper will indicate how to elucidate quantitatively the net effects of the vapour and the liquid phase deviations from their ideal state, and of the heat mixing values in calculating the thermodynamically consistent set of VLE data.

QUESTION BY: D. ZUDKEVITCH, Allied Chemical Corporation,
New Jersey, U.S.A. Why mole fraction acetic
acid? Why not weight fraction?

ANSWER: Dr. Zudkevitch's comment on the possible use
of weight fractions instead of mole fractions to give a better
correlation of acetic acid vapour-liquid equilibrium data is valid if we
are only interested in straight forward graphical correlation. But if
we wish to handle multicomponent mixtures using, say, NRTL or Wilson or
UNIQUAC equations, then we must use the same basis as used for the
remaining components, viz. mole fractions. Furthermore, the whole weight
of the model used is its use of fundamental thermodynamics to constrain
the compositions in each phase through the true reaction equilibrium
constants. While it is possible to express the phase relationships in
terms of weight fractions rather than mole fractions through the use of
yet another set of concentration dependent "correction factors", this
would not in fact improve the model in any way, nor would it improve the
fit obtained. The apparent values of the other correction factors might
fortuitously be made smaller in magnitude, so much that it is conceivable
that they could be dispensed with, but this would be a mathematical
coincidence rather than a fundamental description of the behaviour of the
system. Further, extension to other cases would always be uncertain.

PAPER: APPLICATION OF A CUBIC EQUATION OF STATE TO VAPOUR-
LIQUID EQUILIBRIA OF SYSTEMS CONTAINING POLAR
COMPOUNDS. 1.2/1

AUTHOR: G.S. Soave, Snamprogetti, Milan, Italy.

QUESTION BY: U. ONKEN, University of Dortmund, Germany.
Did you apply your new equation to systems with data
for larger temperature and pressure ranges than given in your paper? For
the system ethanol/water e.g. VLE data are available from room tempera-
ture up to 200°C.

ANSWER: I tried some applications to experimental data
ranging on rather wide temperature fields, and I could see that no
dependence on temperature of the binary interaction parameters is
required to improve the reproduction of the data.

QUESTION BY: A. FREDENSLUND, Institutet for Kemiteknik, Denmark.
Does your R-K modification apply to the solubility
of gases in polar solvents? (For ex. H_2 + methanol).

ANSWER: Although the experimental data on the solubility of
inorganic or hydrocarbon gases in polar solvents are very scarce, I
could see that the equation applies well (this is one of its most
interesting features).

PAPER:

VAPOUR-LIQUID EQUILIBRIUM WITH ASSOCIATION IN
BOTH PHASES. A RECENT MODEL APPLIED TO ACETIC
ACID - WATER AND - ALCOHOLS. 1.1/17

AUTHORS:

J.D.Jenkins* and M.Gibson-Robinson**
* University of Aston, Birmingham
** B.P.Ltd., London

QUESTION BY:

U. ONKEN, University of Dortmund, Germany.

Association constants for the liquid and the vapour phase are given as functions of temperature (table 1). It strikes me that the constants of the temperature term are different by more than 2 for the liquid and the vapour phase. Since the constants are proportional to the heat of association one should conclude that heat of association is differing so much in the two phases.

ANSWER:

i) The values of the constants in our Table 1 differ from those of Baumgartner, Rupp and Wenzel since the units of pressure and the base of the logarithms used are both different.

ii) K_A is defined as p_{2A}/p_{1A}^2 , or $(y_{2A}/y_{1A}^2)P$

where 1 and 2 refer to monomer and dimer respectively, and A indicates pure acetic acid, p partial pressure, y vapour phase mole fraction, and P total pressure. Similarly

$$k_A = x_{2A}/x_{1A}^2$$

But the relationship $\frac{d \ln K}{dT} = - \frac{\Delta H}{RT^2}$ (1)

refers to the true thermodynamic equilibrium constants which are

$$K_A^T = (\phi_{2A}/\phi_{1A}^2) (y_{2A}/y_{1A}^2)P \quad \text{and} \quad k_A^T = (\gamma_{2A}/\gamma_{1A}^2) (x_{2A}/x_{1A}^2)$$

where ϕ indicates fugacity coefficients and γ activity coefficients. Integration of equation 1 gives $\ln K = A + H/RT$, but to identify this equation with those in our Table 1 is to overlook the fact that while

ϕ_{2A}/ϕ_{1A}^2 may be regarded as 1.0 with some confidence (since the usual treatment of association in the vapour phase for acetic acid assumes $\phi_{2A} = 1 = \phi_{1A}$), we cannot assume $\gamma_{2A} = 1.0 = \gamma_{1A}$, and hence, $k_A \neq k_A^T$, thus a comparison of the two values of B is not a comparison of the enthalpies of association in the two phases.

SESSION 2 - DISTILLATION FUNDAMENTALS

PAPER: ENHANCED MASS TRANSFER IN DISTILLATION BY USE
BAFFLED TRAYS. 2.3/65

AUTHORS: K.E.Connelly and G.G.Haselden, University of Leeds.

QUESTION BY: M.W.McEWAN, Koninklijke/Shell-Laboratorium, Holland.
The authors' tests with a distillation system were carried out on a single baffled tray, and compared with normal sieve tray efficiencies derived from other sources, presumably with several trays. There is a great danger in using efficiencies derived from a single tray where end effects and factors like condition of vapour liquid feed can give apparently high tray efficiencies. I suggest that the actual advantage of the baffles can only be derived from a direct comparison with a tray without baffles in the same test equipment.

ANSWER: In our earlier work (Ashley, M.J. and Haselden, G.G., 1973, Trans. Inst. Chem. Engrs., 58, p.188) we compared the performance of the same plate, with and without baffles. When we changed to the new apparatus, to handle larger vapour and liquid loadings (area based), we were forced to go to a smaller plate size. At this reduced size we believe that the results for a baffled tray are meaningful, whilst those for an unbaffled tray would not be - because weir and wall effects would dominate for an ordinary tray.

We were aware of the pitfalls quoted by Mr. McEwan, but did our best to avoid them.

QUESTION BY: F.E.RUSH, E.I.Du Pont De Nemours & Co., U.S.A.
Have you made any estimates of the relative costs of a column equipped with your high efficiency baffled trays to a column fitted with conventional sieve trays for a given separation and throughput?

ANSWER: We do not have reliable cost data available for baffle trays, but we are starting a costing exercise in collaboration with APV Spiro-Gills Ltd. Naturally the cost information must cover full installation costs, and not just that of fabricating the trays.

We expect that the weight of metal used per tray will not be increased by the use of baffles, because the form of construction allows thinner gauge material to be used. It is likely, nevertheless, that the plates will be somewhat more expensive due to more complex fabrication. Since, however, it will be possible to use fewer plates for a given separation, and taking into account the reduced costs associated with a shorter column shell etc., considerable overall savings are to be expected.

QUESTION BY:

DR.H.J.NEUBURG, Atomic Energy of Canada Ltd.

What are the reasons for the baffled tray point efficiency to be higher than the unbaffled tray point efficiency?

Do you have any practical proof on the validity of your tray efficiency enhancement factor model on baffled trays?

ANSWER:

The point efficiency is higher on a baffled tray because the dense froth region is maintained up to much higher vapour loadings, and the contact between vapour and liquid is far more uniform. On a normal sieve tray at high loadings a significant fraction of the vapour by-passes the liquid either in vapour jets or vapour voids.

We do have some justification of our liquid recirculation model, firstly from separate air water tests which established the practical rates of recirculation, and secondly from the relationship of point efficiencies to plate efficiencies in the present results. For a given vapour loading at low liquid rates the plate efficiency exceeds the point efficiency, whilst at the highest liquid rates the opposite applies. Further experimental work is in hand to provide quantitative tests.

QUESTION BY:

C.BARBE, Societe L'Air Liquide, France.

The baffle tray is a plain sieve tray plus baffles.

As an engineer I would like to know what is gained, and what is lost through the addition of baffles, mainly with respect to:

1. Pressure drop per theoretical plate
2. Maximum throughput (gas and liquid)
3. Flexibility
4. Column height (tray spacing)

ANSWER:

Some of the answers to these questions can be deduced from the full table of results presented in the paper. Qualitative answers are as follows:

1. Optimum exploitation of the baffle tray is likely to involve a % free area of about 15%. Such trays will give a lower pressure drop than conventional trays of 10% free area, whilst having much higher plate efficiencies. Hence the pressure drop per theoretical tray will be lower - say by 25%.

2. Because vapour flow through the baffled plate is more uniform, entrainment will be lower. Thus higher vapour loadings can be achieved at the same plate spacing - say by 20%. Also by using the newly developed baffle, with sloping secondary orifices, hydraulic gradient is avoided and large liquid loads can be handled.

3. A turn-down ratio of at least 3:1 is possible due to the way in which the baffles direct liquid flow on to the plate. To gain maximum advantage the inlet and the outlet weirs must be correctly designed.

4. Especially with the new baffle design, employing the inclined secondary orifices, froth height is low even at very high vapour flowrates. For normal liquids it is probable that the optimum tray spacing will be between 30 - 40 cm., but for cryogenic use it would be possible to design a baffle tray for 20 cm. spacing.