# LETTERS TO THE EDITOR

**To the Editor:**

**Some further Comments on the reply by Cresswell and Dixon**

We appreciate the reply of Cresswell and Dixon very much and thank the authors for the mathematical thoroughness showing the validity of our assumption for—as it is said—only unsteady fixed bed operation. We agree that for the steady state without exothermic chemical reaction our assumption concerning the second derivatives becomes weaker with increasing Reynolds number. This leads to the maximum in fig. 2 of the preceding reply. In fact we have calculated the same maximum already in 1975 [1]. Fortunately these deviations are not so important for practical calculations since in the case of radial heat losses axial dispersion processes become of minor importance for the calculation of temperature profiles beyond say Re > 100.

However, beyond the excellence of the mathematical treatment there still remains an open question concerning the work of Cresswell and Dixon by purely physical arguments: Consider a calculated temperature profile from equs. (1) and (2) of the original paper. At a given rate of convection the slope of the profile calculated from the two phase model depends on $k_s$, $k_g$ and the heat transfer coefficient h. If we subsequently try an equivalent one-phase model the effective dispersion coefficient of this model has to incorporate besides $k_s + k_g$ an additional term which corrects the one phase model with respect to the heat transfer dispersion of the two-phase-model. It is exactly this term which we still miss in the theory of Cresswell and Dixon.

By our theory we offer such a term, however with some limitations concerning the steady state at larger Reynolds numbers.

Furthermore we do not agree with the statement that the theory of Cresswell and Dixon is particularly suitable for calculating steady states of non-isothermal and non-adiabatic catalytic fixed bed reactors. An exothermic chemical reaction usually also in the steady state provides S-shaped temperature profiles (similar to those in fixed beds with no chemical reactions but unsteady operation). Under these conditions our assumption concerning the second derivatives again is fullfilled very well. The usefulness of our approach has been shown in various publications.

DIETER VORTMEYER
Technical University
8000 München 2
Arcisstraβe 21
Germany

## LITERATURE CITED

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See R&D Note on page 508 of this issue.

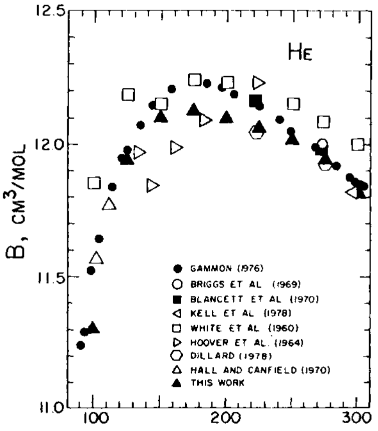
**To the Editor:**

Holste, *et al* (1980) have presented pure-component and interaction second virial coefficients for the system He—$CO_2$. Their results (see their Figure 5) for He show a lower $B_{He}$—T relationship (in the region of maximum $B_{He}$) than most previous investigations. Exceptions are the works of Hoover, *et al* (1964) and Dillard (1975), whose data lie below those of Holste, *et al.*

I wish to point out that the values of Dillard (1975) are incorrect. A more precise analysis of Dillard's experimental data appears in Dillard, *et al* (1978); these values are in excellent agreement with the results of Holste, *et al.* Table I and a revision of Figure 5 of Holste, *et al* appear below. These two data sets agree to within 0.02 cc/mol at (nominal) temperatures of 0 and −50°C. This substantiation of the results of Holste, *et al* indicates that the maximum of $B_{He}$ is, in fact, lower than that reported by previous investigators.

TABLE I COMPARISON OF HELIUM SECOND VIRIAL COEFFICIENTS

| Temperature, T (K) | Second Virial Coefficient $B_{He}$ (cc/mol) | |
|---|---|---|
| | Holste (1980) | Dillard (1978) |
| 223.15 | — | 12.04 |
| 225.00 | 12.06 | — |
| 273.15 | — | 11.93 |
| 273.16 | 11.95 | — |



ROBERT L. ROBINSON, JR.
School of Chemical Engineering
Oklahoma State University
Stillwater, OK 74078

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Dillard, D. D., M. Waxman, and R. L. Robinson, Jr., "Volumetric Data and Virial Coefficients for Helium, Krypton, and Helium-Krypton Mixtures," *J. Chem. Engr. Data*, *23*, 269 (1978).

Holste, J. C., M. Q. Watson, M. T. Bellomy, P. T. Eubank, and K. R. Hall, "Determination of Interaction Second Virial Coefficients; He—$CO_2$ System," *AIChE J.*, *26*, 954 (1980).

Hoover, A. E., F. B. Canfield, R. Kobayaski, and T. W. Leland, "Determination of Virial Coefficients by the Burnett Method," *J. Chem. Engr. Data*, *9*, 568 (1964).

**Reply:**

Speaking for my co-authors (J. C. Holste, M. Q. Watson, M. T. Bellomy, P. T. Eubank) and myself, we concur completely with the observation of Prof. Robinson. The plot in our paper is from an M.S. thesis which predates Dillard, Waxman and Robinson (1978) and its use is an oversight on my part.

My conclusion is that the helium second virial accuracy is probably well established now because both the higher and lower values have confirmation. I am somewhat embarrased by being on both branches through Holste *et al.* (1980) and Blancett, Hall and Canfield (1970).

KENNETH R. HALL
Chemical Engineering Department
Texas A & M University
College Station, TX 77843

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