LETTERS TO THE EDITOR

To the Editor:

A recent paper titled, "The Effect of pH on Continuous High-Temperature/Short-Time Sterilization of Liquid Foods," by K. R. Davey, S. H. Lin and D. G. Wood, published in *AIChE J.*, **24**, 537-540 (1978) presents a novel and potentially valuable way of increasing the usefulness of the Arrhenius equation, particularly to those involved in thermal process design of food systems. However, there are some serious errors or printing mistakes in the paper that should be pointed out:

1. In the text on page 539, thiamin is parenthesized as being Vitamin B-12, which is incorrect as thiamin is otherwise known as Vitamin B-1. Cobalamin is the other name for Vitamin B-12.

2. On page 539, equation (3) is written as $\ln A = a(\text{pH})^2 + b(\text{pH}) + c$, while in Table 1, it is written as $\ln A = a(\text{pH})^2 - b(\text{pH}) + c$. From the data presented, it appears that the latter is correct, i.e., the sign against b should be minus.

3. The Arrhenius plots of inactivation rate constant k versus reciprocal temperature (Fig. 1 and Fig. 2) are obviously wrong. Not only does it contradict the same data in Figs. 3 and 4, but it should have been obvious to the most casual reviewer that rate constants *increase* with increase in temperature, i.e., the slopes of the Arrhenius plot should be negative and not positive as shown in Figs. 1 and 2. It is surprising the authors obtained reasonable activation energies in Table 1, if indeed they used the data presented in the paper.

4. The coefficients a, b, c, listed in Table 1 also appear to be in error. When used in the equation shown in Table 1 for *Clostridium botulinum* in the spaghetti product, it predicts k values 50-60% lower than the experimental values. Also, the curves generated using these coefficients and the equation does not coincide with the predicted curves in Figs. 3 and 4. Statistical comparison of experimental vs. calculated data should have been presented to enable the reader to judge the usefulness of the model.

5. The citation to the work of Feliciotti and Esselen is incorrect. It should be *Food Technology*, *11*, 77-84 (Feb., 1957) not *Food Research* (which is another journal, now called *J. Food Science*).

6. Finally, the reader should not conclude that inactivation rates of food components and microorganisms is solely a function of pH and temperature, as the authors may have implied in their model. Feliciotti and Esselen (1957), among others, have shown that thiamin in actual foods is more resistant to thermal breakdown than is pure thiamin in buffered solutions. Also, k is affected by ionic strength, water activity, concentration of thiamin in the system and whether it exists in a "free" or "combined" form.

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To the Editor:

In a recent paper by Davey, Lin, and Wood [*AIChE J.*, **24**, 537 (1978)] there are a number of important errors which I would like to point out here. Fortunately, the authors' final result, given in their Figure 5, appears to be correct.

First, in Figure 2 the rate constant k is plotted vs the reciprocal of the absolute temperature T. According to this Figure, k increases as T decreases, which is physically unreasonable, and opposite to the predictions of their Equation (1). Secondly, the values presented in Table 1 for the calculation of k do not agree with the k values given in either Figure 2 or Figure 4. For example, from Figure 4 at a pH of 4 and a temperature of 110°C, $k \simeq 4.95$. From Figure 2 for the same conditions, $k \simeq 25.0$. The constants in Table 1 however, lead to the prediction that $k = 2.28$ or 2.6×10^8 depending on whether one uses the negative coefficient for b in Table 1 or the positive coefficient in Equation (3)! It should also be noted that the predicted curves in Figure (4) are inconsistent with Equations (2) and (3), which indicate that for two fixed temperatures T_1 and T_2, the quantity $\ln k_2 - \ln k_1$ where k_1 and k_2 are the corresponding rate constants, is independent of pH. Clearly, this is not the case in Figure (4).

These errors and inconsistencies severely detract from an otherwise interesting paper.

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Reply to the Comments of Drs. Gordon and Cheryan:

The author wishes to thank Drs. Gordan and Cheryan for their interest in our short note. Most of the comments pointed out by them were right. The plots in Figs. 1 and 2 were incorrectly presented, but Figs. 3 and 4 were correct. The mistakes made in Figs. 1 and 2 were due to mismatches of the value of k with the temperature. The right plots should look the two accompanying figures. As for the second point raised about the numerical values in Table 1 of our note, we found one activation energy was not right. This is ΔE for Cl. botulinum inactivation in spaghetti, tomato sauce and cheese. The correct value should read −73.66 Kcal/g·mole-°K instead of the original −74.29 Kcal/g·mole-°K. The rest of the tabulated values is correct. Dr. Cheryan also pointed out that the rate coefficient of microorganism inactivation is also dependent of several factors. We agree with that. But because of lack of experimental data, it is still difficult to quantify the influences of those factors on the rate coefficient. If sufficient experimental data are available, incorporation of those factors into a single rate coefficient may be similarly done.

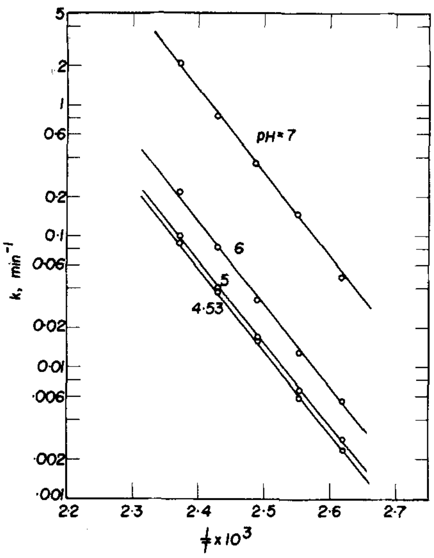

Figure 1.

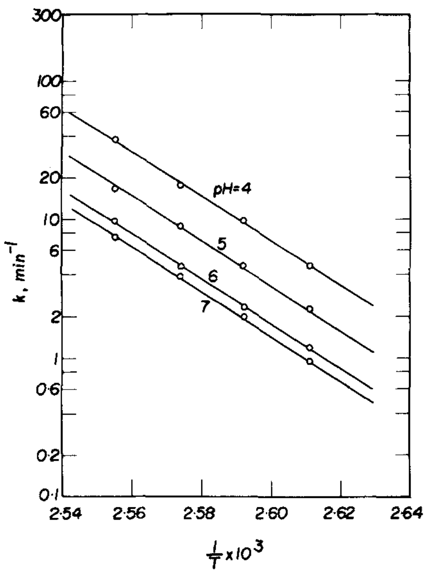

Figure 2.

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