DISCUSSION

A. A. Orning

The author's data may be compared with earlier data this discussor obtained by photographic measurement of burning times of particles falling freely in an externally heated furnace [2]. Results of such comparisons are given in Table 5. The experimental burning times were for the total period during which the burning was intense enough to record on photographic film. Equations (3), (12), and (13) were used to obtain calculated residue burning times, and equation (2) with $K_w = 60$ was used to obtain the volatile matter burning time. The total time calculated for burning the residue plus volatile matter is given in Table 5 as the calculated burning time. The calculated values range from 2.5 to 3.8 times the experimental values. Since the calculated values are based upon equations which the author has found to fit his experimental values, differences in experimental procedures must be considered in order to find a logical reason for the great differences in burning times.

The earlier photographic measurements were made with freely falling particles. It was found that burning was about 85 percent complete or better. A possible explanation is that the burning rate, in terms of percent of original combustible per unit time, fell off rapidly at the end. The photographic evidence, in fact, showed that the temperature of the burning particle fell off gradually until what was interpreted as loss of ignition occurred. The temperature then fell very rapidly. It was also observed that if a burning particle hit the furnace wall, it would stick to the wall and there burn completely but at a lower rate than would a freely falling particle. Forced circulation was used to give a downward flow of air in the furnace. Continued burning was caused by the relative velocity between the air and the particle on the furnace wall. The rate of burning, and possibly heat transfer to the wall, were such that the temperature fell too low to be recorded photographically, but it was high enough to be observed visually as combustion continued to completion.

In the presently reported experiments, the coal particles were cemented to fine silica threads which, in turn, were supported between two horizontal heating coils. It appears that the silica thread does not interfere with complete burning, but it must contribute to heat transfer from the burning particle to the surrounding air. Natural convection through open spirals of the heating coils also must have some effect upon the relative velocity between the burning particle and the air. Alternate conclusions appear reasonable. Either (a) the net effect of heat loss through the silica thread and the relative velocity between the suspended particle and the rising air current are such that the burning rate is reduced; or (b) the device used to detect continued burning of the particle is much more sensitive than the photographic technique, extending the measured time into a period of slow burning. During this period the particle would promptly lose ignition if it were placed in a position where heat loss could occur by radiation to cold furnace walls.

The equations used to fit the experimental data gave some justification in theory, but there is question as to the degree of the assumptions used in the application. In particular, it cannot be assumed that the swelling of the coal particle is isotropic. Where evidence exists, particularly under conditions comparable to those of the author's experiments, it has been found that the residues, after burning off the volatile matter, have an irregularly shaped hollow eggshell-like structure. Since all of the mass is in the external shells, these have been loosely called "cenospheres." Nusselt's equations should be modified; the integration should not be carried to zero radius, as applied to such structures. Nevertheless, the empirical equations do compare to Nusselt's equations.

Further evaluation of the empirical results may be based upon an estimate of heat release rates. For a typical Pittsburgh Seam coal, the premixed fuel and air at 20 percent excess air contain 83.4 Btu per cu ft of mixture. For particles of 0.030 mm size (mean screen opening for 200 by 300-mesh size) the calculated burning time is 0.042 sec. The ratio of the Btu per cu ft to the burning time, with proper conversion of time units, is the heat release rate, 7.1 million Btu per cu ft per hr. This estimate should be corrected for depletion of oxygen during combustion. For a reaction somewhere between zero and first order with respect to the partial pressure of oxygen, the corresponding increase in burning time should be less than fivefold with 20 percent excess air. A twofold increase in particle size, which would exceed the 300-mesh screen opening, would give a fourfold increase in burning time.

Table 5 Coal particle burning times

<table>
<thead>
<tr>
<th>Particle size, mm</th>
<th>Burning time, sec</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pittsburgh Seam coal</td>
<td>Calc.‡</td>
</tr>
<tr>
<td>0.101</td>
<td>0.070</td>
</tr>
<tr>
<td>0.127</td>
<td>0.080</td>
</tr>
<tr>
<td>0.163</td>
<td>0.10</td>
</tr>
<tr>
<td>0.214</td>
<td>0.13</td>
</tr>
</tbody>
</table>

‡ Determined by discussor's photographic technique.
§ Calculated by author's equations.

Author's Closure

Dr. Orning's comments on this paper are most pertinent and well made. The discrepancies that he points out on the calculated burning times, which are 2.5 to 3.8 times his experimental values, clearly represent a problem that must now be investigated.

So far as comparison of the two experimental techniques is concerned, there seems little that can be added to Dr. Orning's very careful analysis. I would just like to qualify a few minor points particularly in his third paragraph. So far as the natural convection through the open spirals of the heating coils is concerned, this I think has much less influence than might at first have been suspected. The flow patterns shown up by smoky volatiles show that the air currents come around the coils rather than passing directly between the spaces. This appears to be the main pattern of convective flow. This also seems to mean that the heat loss by this rising current is rather less important than Dr. Orning might have suggested. So far as the effect of heat loss through the silica thread and also by way of the relative velocity between the suspended particle and the rising air current are concerned any such heat loss is of course relatively immaterial so long as the particle temperature is maintained. In fact, it may well be that one reason for complete burnout of the particles is maintenance of the particle temperature by heat contained within the silica thread. As far as the Orning's particles in the furnace are concerned, this fact was not the case since he observed the particle temperature to fall off very rapidly toward the end of the combustion. Dr. Orning also raises a question of sensitivity of the detectors used. He used photographic film which I think he may be right in suggesting was less sensitive to detection of the final burnout than was my photocell. Dr. Orning also questions the assumption of isotropic swelling
of the coal particles. In this particular instance we have some direct photographic evidence to show that the swelling of the coal particles was in fact isotropic. As reported in the paper the swelling factor of all the coals, but for the anthracite, was constant with a figure of 1.5. It was also found from photographic evidence of the particles as they burned, that they retained their original shape to quite a remarkable degree. It was also found by taking values of the diameters from the photographic film that when their squares were plotted against time a straight line was obtained, all the way down to almost zero radius, and quite easily extrapolatable to zero radius. It seems fairly clear that the normal cenosphere formation as observed by Sinnati and his collaborators was not obtained in these particular experiments. Although the particles swelled, and the swelling factor was also found to vary with the degree of oxygen in the surrounding atmosphere, it also seems that the average density of the particles formed was pretty uniform right across the swollen particle. Since reaction was controlled solely by diffusion through a boundary-layer film, then any variation in inherent reactivity had no effect on the burning out, neither had slight random variations in density. Here then is a possible source of the discrepancies between Dr. Orning’s experimental results and his calculated values using my equations extrapolated down to his diameters. His particles would appear to have been true cenospheres whereas mine evidently were not.

A further complicating factor in this is the recent discovery notably reported in the two papers by Beér and Thring1 and by Lee, Thring, and Beér, that combustion of the very small particles of pulverized fuel size is a reaction system controlled by the chemistry of the system and not by diffusion. This now proves beyond doubt that extrapolation of my diffusion equations, or the original Nusselt diffusion equations, down to small p.f. sizes, say about 100 microns and under, is inapplicable. As a further point in all this it should be noted that the total burning out times observed by Beér in the One-Dimensional Flame Furnace, or Controlled Mixing History furnace, were substantially greater than the expected burning out times on the basis of extrapolation of the diffusion equations, even when allowing for the increased burning times due to oxygen depletion through the flame. This is the opposite to Dr. Orning’s observations.

As a final point, I am not sure that I can agree with Dr. Orning that the flame volumes are generally determined by the rate of mixing of the secondary air with the primary stream of coal and air, and not by the burning rate of individual particles. The most recent results, particularly from the International Flame Research Foundation would seem to indicate that while mixing plays a major part it is not the dominant effect, and the dominant effect is in fact the burning rate of individual particles. I would, however, entirely agree with Dr. Orning that there is still much need for further information about the pattern and rate of mixing of volumes rich in air with volumes rich in fuel. I would also like to thank Dr. Orning for his most careful and illuminating discussion.
