

References

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DISCUSSION

F. F. Ling³

I have read the authors' papers with the greatest interest. As usual, this paper contains much valuable experimental information. There are two points which I would like very much to call attention to, however.

The first has to do with the statement in the third paragraph, "... The early theory of Blok [3] in 1937 has been put on a rigorous basis ...". Blok's theory is on a sound foundation, i.e., as much as Jaeger [4]. In other words, it is the straight application of the heat equation.

The second point is that, while there is nothing wrong with equations (3)–(6), it is not necessary to use two sets of equations. (Incidentally, Archard [5] used Blok's work to give the summary paper for the kind of calculations which the authors did.) In other words, there is one equation for the heat partition calculation which is valid for all Peclet numbers.^{4,5}

I have done a calculation for the case of $P_H = 1.7 \text{ GN/m}^2$, $v = 1 \text{ m/s}$ and $T.C. = .07$. ΔT at the center from Fig. 3, as near as I can gather, is 110°C . Pursuing the calculation, and using the average value of P_H , I got 115°C . This, of course, led to a calculated temperature (i.e., with bulk temperature added) at the center of 159°C .

P. M. Ku⁶

The authors have shown that their observed variations of the maximum surface temperature rise, ΔT , with respect to load and sliding speed can be accounted for by the known theoretical behavior of flash temperature. Considering the difficulties of the temperature measurements, the results are certainly gratifying.

The reported effects of surface roughness on ΔT are most welcome and are indeed qualitatively expected. As surface roughness is increased, one expects the friction coefficient, f , to increase, and likewise

the frictional power loss, $\phi = fWV$. It is therefore pertinent to ask whether the surface roughness effect shown in Fig. 8, for example, could be quantitatively related to the changes in friction coefficient.

As a matter of some interest, this discussor and associates have examined the behaviors of the quasi-steady surface temperature, T_s (i. e., Blok's "bulk temperature") and the conduction-inlet oil temperature, T_0 , with respect to the frictional power loss, ϕ , of several sliding-rolling systems [13, 14, 15].⁷ It has been found that

$$T_s - T_j = C\phi^n$$

and

$$T_0 - T_j = C_0\phi^n$$

where T_j = oil-jet temperature (which is akin to the bath temperature within the context of this paper), and C , C_0 , and n are fitting constants. The above relationships appear to hold quite well regardless of disk material, oil type, surface topography, and most operating variables, as long as the actual values of $\phi = fWV$ are used. However, the constants C and C_0 are quite sensitive to system design and the oil flow rate, i.e., they are dependent on the details of the heat transfer process.

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G. Paul,⁸ A. Cameron,⁹ and E. P. Shuttleworth¹⁰

We have read this paper with interest as it is very similar to work

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⁴ Ling, F. F., and E. A. Saibel, "Thermal Aspects of Galling of Dry Metallic Surfaces in Sliding Contact," *Wear*, Vol. 1, 1957, pp.80-91.

⁵ F. F. Ling, *Surface Mechanics*, Wiley-Interscience, Vol. 68, 1973.

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⁷ Numbers 13-15 in brackets designate Additional References at end of discussion.

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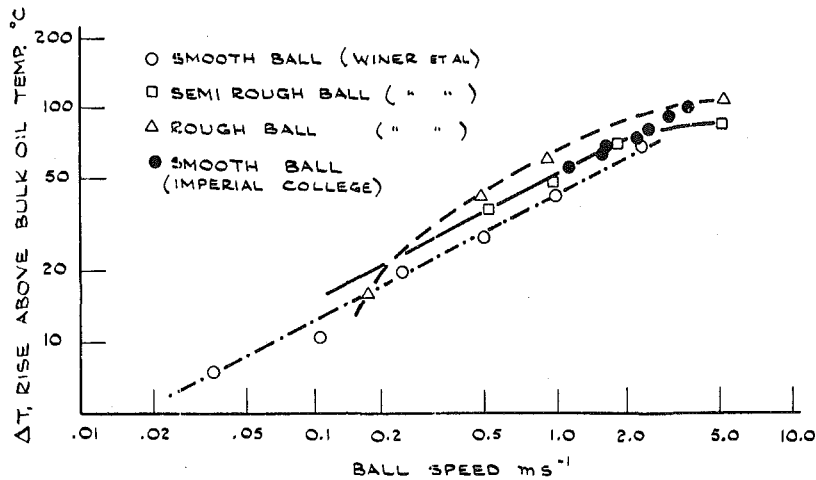


Fig. 13 Ball surface temperature versus sliding speed

that we have been doing. It is found in the paper that the experimental results for the variation of temperature rise with velocity do not correlate with predictions made from Archard's model. This is explained as being due to a variation in traction coefficient with sliding speed and yet, as stated in the text, Table 1 shows that for Hertz pressures above 1.5 GN/m^2 the traction coefficient is essentially constant. We do not understand this apparent contradiction.

In Fig. 13 we have replotted some of the results presented in this paper to compare them with some made at Imperial College. We have subtracted 10°C from the rough ball temperature to get the contact centre temperature. It was stated that over the range of operating conditions the rough ball had a significantly higher temperature than the other two which were essentially the same. The plot does not appear to support this statement. It can be seen that our results fall nicely in the range given in the paper. The oil was a similar type and the reservoir temperature was about 45°C . The ball was optically smooth. It is reassuring that using this difficult technique there is such close agreement between the results.

The high frequency temperature variations are very interesting. We do not quite understand what causes a significant reduction in temperature after an asperity contact. If it were purely an elastohydrodynamic effect leading to a lower pressure it would be present whether or not there had actually been an asperity contact immediately before it. In this case there would be temperature falls at all values of λ . In practice the temperature troughs shown an even more marked sensitivity to the load than the peaks. Have the authors considered whether or not the emissivity of these troughs is significantly altered, perhaps by debris deposition or the formation of some form of oxide layer?

Authors' Closure

The authors would like to thank the discussers for their valuable discussion.

The authors agree with Professor Ling that Blok's theory is on a sound foundation. The comment in the paper was to indicate that we felt Jaeger's approach was more systematic. While the equations (3) and (4) can be replaced with a single equation, it was found convenient to represent the trends in the two regimes separately. The sample calculations reported by Dr. Ling are certainly encouraging.

The probable reason for the effect of surface roughness on ΔT is

the corresponding change in friction coefficient as suggested by Mr. Ku. However, since the friction coefficients were the experimentally measured values, no attempt was made to include the variation of TC with roughness in the expressions for ΔT . The authors would like to note that the traction coefficient increased appreciably (up to 50 percent) for λ ratios less than 1 where the asperity interactions are rather severe.

The range .70 to 1.39 m/s for the sliding velocity at 1.51 GN/m^2 peak Hertz pressure in Table 1, is indeed a small fraction of the range indicated in Fig. 13, and therefore the constant value for TC reported in Table 1 should not be misunderstood. In fact, over the full range of speed indicated in Fig. 13, measurements (not reported in the present paper) show an appreciable decrease in traction coefficient with sliding speed.

Paul, et al. in their discussion have indicated that ΔT is not substantially different for different roughness balls with reference to Fig. 13 drawn by them. The authors would like to point out that the dashed line in Fig. 13 for the rough ball (the basis for their conclusion) was plotted incorrectly by them. The ΔT values for the rough ball (at $P_H = 1.05 \text{ GN/m}^2$) used in Fig. 13 drawn by the discussers are up to 40°C in error. This kind of plotting error invalidates the conclusion made by the discussers. Keeping in mind that the ΔT values are plotted on a log scale in Fig. 13, the correct values of ΔT for rough ball obviously

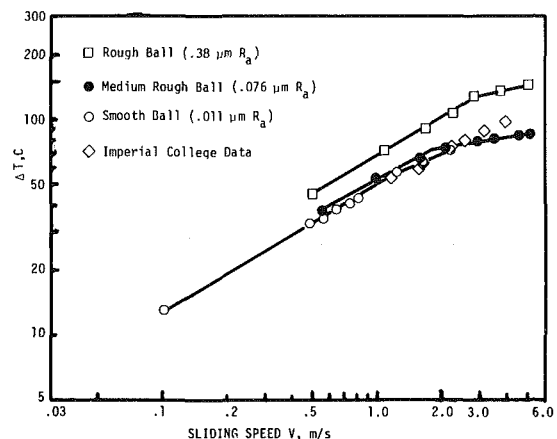


Fig. 14 Ball surface temperature rise at contact center versus sliding speed ($P_H = 1.05 \text{ GN/m}^2$)

indicate the effect of surface roughness. Fig. 14 has been prepared using the correct values for ΔT , which shows clearly the effect of surface roughness. Also an earlier figure, Fig. 8 in the paper, indicates that ΔT increases substantially with surface roughness. In fact, in a later work the authors have made correlations of ΔT with the c.l.a. value of surface roughness, and the results are seen to be in excellent agreement with surface roughness factors of the form $1/(1 - .8\sigma)$, where σ is the composite roughness in μm .

Further, it was determined that emissivity fluctuations were not present in the time scale corresponding to the temperature fluctuations. The reasons for this have already been cited in the paper. Similarly, the emissivity of troughs does not change in the time scale of measurements. In the case of a thick elastohydrodynamic film, the

local pressure variations are negligible and therefore no temperature fluctuations are observed. However, when the surface asperities touch one another, large local pressure variations and local traction variations do exist and therefore give rise to temperature fluctuations. Calculations of the portion of load carried by fluid pockets were made¹¹ based on surface statistics, which clearly indicated that a significant load is carried by the asperities at sufficiently low λ values.

¹¹ Nagaraj, H. S., Sanborn, D. M., and Winer, W. O., "Asperity Interactions in Partial EHD Contacts," to be published.