Expressing the usual autocorrelation function $R(\tau)$ in similar form and substituting leads to equation (22).

An analogous derivation is found in (8) for the form

$$P*Z^2{T, t, u} = \varepsilon[x(t)x(u)x(t + \tau)x(u + \tau)]$$

For $t = u = 0$

$$P*Z^2(0, 0) = \varepsilon[x(t)x(u)x(t + \tau)x(u + \tau)]$$

As shown in [8], this results in equation (24).

APPENDIX 2

The Spectrum $\sigma^2$ in Equation (29)

From (13), if $R(\tau)$, $R_1(\tau)$ and $R_2(\tau)$ are autocorrelation functions and

$$R(\tau) = R_1(\tau)R_2(\tau)$$

then for the corresponding spectra $S(\omega)$, $S_1(\omega)$, and $S_2(\omega)$

$$S(\omega) - \int_0^\infty S_1(\omega - \xi)S_2(\omega) = (\varepsilon)\delta$$

Setting $R(\tau) = R_2(\tau)$ and designating the spectrum corresponding to $R_1$ as $\sigma_1^2$, and that for $R_2$ as $\sigma_2^2$, results in equation (32).

If the spectrum $\sigma_1$ is given by equation (34), then

$$\sigma_1(\omega) = \frac{1}{4\pi} (I(\xi))_{\xi=0}$$

and $I(\xi)$ has discontinuities at several $\xi_i$. These, and the values of $I(\xi)$ and $\sigma_2(\omega)$ are given in Table 1, which leads to equation (35).

Table 1

<table>
<thead>
<tr>
<th>Interval</th>
<th>$\xi(0)$</th>
<th>$\xi(1)$</th>
<th>$\xi(2)$</th>
<th>$I(\xi)$</th>
<th>Form of $\sigma^2(\omega)$ over the $i$th interval</th>
</tr>
</thead>
<tbody>
<tr>
<td>$0 \leq \omega \leq 2(\omega_0 - b)$</td>
<td>0</td>
<td>$\omega_0 - b$</td>
<td>$\omega_0 - b$</td>
<td>$I(\xi)$</td>
<td>$\frac{1}{4\pi} (I(\xi))_{\xi=0}$</td>
</tr>
<tr>
<td>$2(\omega_0 - b) \leq \omega \leq 2\omega_0$</td>
<td>$\omega_0 - b$</td>
<td>$\omega - (\omega_0 - b)$</td>
<td>$\omega - (\omega_0 - b)$</td>
<td>$I(\xi)$</td>
<td>$\frac{1}{4\pi} (I(\xi))_{\xi=0}$</td>
</tr>
<tr>
<td>$2\omega_0 \leq \omega \leq 2(\omega_0 + b)$</td>
<td>$\omega - (\omega_0 + b)$</td>
<td>$\omega + b$</td>
<td>$\omega + b$</td>
<td>$I(\xi)$</td>
<td>$\frac{1}{4\pi} (I(\xi))_{\xi=0}$</td>
</tr>
<tr>
<td>$2(\omega_0 + b) \leq \omega$</td>
<td>$0$</td>
<td>$\omega + b$</td>
<td>$\omega + b$</td>
<td>$I(\xi)$</td>
<td>$\frac{1}{4\pi} (I(\xi))_{\xi=0}$</td>
</tr>
</tbody>
</table>

DISCUSSION

D. Berthe

I was very much interested in Dr Tallian’s remarkable analysis of the influence of surface roughness in pure rolling in an elastohydrodynamic contact. As we have performed a somewhat similar, if simpler, analysis, also in the two-dimensional case, I propose to outline very briefly what we have done and discuss at greater length the many points of agreement and the few points of disagreement between the two theories.

In our case surface roughness is represented by a sinusoidal expression $r = a \sin [2\pi(x/l) + \phi]$. We have supposed that (1) asperity heights are small when compared to oil film thickness and (2) pressure fluctuations introduced by asperities do not influence the shape of the contact, or more precisely the mean contact line.

The average pressure $p_{av}$ and the maximum amplitude of the pressure variation $\Delta p$ about the average pressure can be shown to be

$$p_{av} = p_s \left[1 + \frac{6}{h_m^2} \frac{a^2}{l^2} + \ldots\right]$$

$$\Delta p = \exp \left(\frac{a}{l} \frac{d}{dx} \frac{3aI}{2xh_m} \right)$$

where $p_s$ is the pressure calculated for the corresponding smooth contact operating under identical conditions, $a$ is the standard deviation of the roughness, $h_m$ is the minimum film thickness, and $a$ is the pressure-viscosity coefficient; $p_{av}$, $\Delta p$ are seen to be linked to the smooth contact pressure $p_s$. For $p_s$ given in reference [15] and for $a/h_m = 0.4$, $l/b = 0.1$, where $b$ is the hertzian half-width, curves $p_{av}$ and $p_{av} \pm \Delta p$ are given in Fig. 6.

Our results are within 20 percent of the author’s for the example tested. Pressure fluctuations are also maximum, as in the author’s case near the contact exit. We differ, however, with the author in the following points. We believe that the statistical methods used in both studies are only applicable if a large number of asperities are found at one time in the contact (i.e., $b/l > 10$). Hence the pressure fluctuations, which are proportional to the wavelength $l$, cannot in our view be extended to the case for which $b \approx l$, and the values found in this case would be larger than the values that can be expected to exist in the real system.

Further, the pressure distribution in a rough and deformed contact with both rolling and sliding is given, if the asperity is taken as rigid, after integration of [16].

Fig. 6 Pressure variations in an elastohydrodynamic rough contact

2 Laboratoire de Mécanique des Contacts, Institut National des Sciences Appliquées de Lyon, Villeurbanne, France.
The author is to be congratulated upon his continuing efforts to bring the practically important, yet extremely difficult, problem of the elastohydrodynamic lubrication of rough surfaces within the scope of quantitative analysis.

I have no objection to the author's analysis, but it appears to me that his conclusion, which describes the large amplitudes of pressure ripple that he has calculated as an "approximate lower bound," is open to misinterpretation.

With data on pressure gradients and film profiles available to the author [12, 14], the calculations of the maximum value of the quantity $\gamma_1 \exp \alpha \rho$ are admitted as being very imprecise. Accordingly, the author deliberately underestimates this quantity and refers to his final values of the pressure ripple as "lower bounds." This is not a point of substance, however, and no doubt with appropriate computations reliable values of the maxima of $\gamma_1 \exp \alpha \rho$ could be found. I am really concerned in this contribution with the probable influence upon the magnitude of the pressure ripple of the basic idealizations in the author's analysis, in particular: (a) the restriction to transverse ridges of the same profile in the rolling direction and (b) the assumption of a Newtonian fluid having an exponential viscosity-pressure relationship.

Purly longitudinal ridges give rise to no pressure ripple, so that a real surface, comprising random irregularities in both longitudinal and transverse directions, will produce a pressure ripple whose amplitude is smaller than that due to a purely transverse ripple.

Most investigators are agreed that real fluids under EHD conditions show effective viscosities that lie below the exponential relationship assumed in the theory. The reason is not yet fully understood, but recently Adams and Hirst [18] have put forward a convincing argument that lubricating oils become non-Newtonian in the sense that their effective viscosity falls, when the shear stress reaches sufficiently high values. The peak shear stresses in an EHD contact coincide in position with the peak values of $\gamma_1 \exp \alpha \rho$; hence any reduction in the effective viscosity in these locations will directly reduce the magnitude of the pressure ripple.

Finally, we note that the author, quite reasonably, neglects both frictional heating and asperity deformation. It is clear that both these effects, if they are significant, will act to reduce the pressure ripple.

It seems to me, therefore, that it would be more appropriate to regard the analysis in this paper as giving an upper bound to the magnitude of the pressure ripple. The extent to which the magnitude of the ripple is reduced by the effects that the author ignores is at present unknown and provides items for further research.

Additional Reference


Patrick E. Fowles

This interesting and important paper represents a significant step forward in our understanding of elastohydrodynamic lubrication. Studies such as this aimed at determining the effects superposed on the ideal smooth surface conditions by realistic surface roughness at realistic specific film thicknesses, together with starvation studies, are beginning to provide the next level of knowledge required to properly apply EHL to practical applications.

The magnitudes of the pressure and traction ripples predicted by the author are of no great surprise to this discussor. Values such as these were predicted by the discussor's analysis of the collisions of individual asperities in sliding contacts [5], even for

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K. L. Johnson

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interactions in which the asperities did not overlap and could not therefore ever touch. Since both the author’s and the discussers work involved asperities in the form of transverse ridges, and since asperity collisions are essentially squeeze-film events both in sliding contacts and in the regions in which the author finds the pressure and traction ripples to be significant, the discussers results should compare in magnitude with the author’s, and it is encouraging that this is the case. It is particularly encouraging that the author predicts such large traction ripples, because similar predictions by the discussers evoked scepticism and disbelief at the time.

The magnitude of the mean traction increase over the smooth surface value with decreasing specific film thickness suggests that this effect should be measurable. Since this would constitute a good test of the analysis, has the author any such experimental data corroborating this roughness effect? Further in this vein, with pressure ripples in the entrance region and at the exit constriction of the same order of magnitude as the maximum Hertz pressure, might the author not expect microscopic cavitation effects resulting from the “negative” ripples? Does the author have any evidence of such effects from his laboratory’s optical EHL experience?

The assumption that a small sliding velocity does not introduce pressure rippling in the plateau region is obviously necessary to the determination of the traction ripples at this stage of the analysis. However, since the discusser’s results imply that even small sliding speeds do, in fact, produce quite large pressure ripples, it would seem that the addition of sliding to the analysis would be a logical next step. Has the author any predictions as to what we might expect from such an addition?

One of the questions raised in macroscopic EHL concerns the response of a lubricant to the very rapid pressure changes imposed on it as it passes through the contact. Many feel that the viscosity of the lubricant might not change with sufficient rapidity to keep up with the pressure, especially in the region of the pressure spike. This problem would seem to be much more severe when considering pressure rippling, when the predicted rate of change of pressure must be very high indeed. Under such conditions one might expect the lubricant to “integrate” the ripples so that, at the very least, the traction ripples will be severely reduced if not eliminated. Would the author care to comment on this?

Author’s Closure

The most far-reaching comment was made by Dr. Berthe. The author confesses insufficient familiarity with reference [16] of his discussion, which makes an adequate reply difficult. However, the last equation in Dr. Berthe’s discussion, giving a formula for the pressure gradient in pure rolling, is identical with equation (11) of the paper, and as such certainly satisfactory to this author. Dr. Berthe calls attention to the fact that the expression in brackets on the right side of equation (11) is non-random, and as such is zero only at a selected small number of coordinate points (3 or 4) within a Hertzian contact. Consequently, the gradient of the reduced pressure (and thus of the pressure itself) shown on the left side of this equation, even though it is a random variable, should not be zero except at these few locations, in view of the fact that the random film thickness $h$ appears as a negative power, which cannot be zero for finite film thickness values. From this, Dr. Berthe concludes that “ripples,” i.e., peaks and valleys of pressure at which the pressure gradient is momentarily zero, cannot occur in the contact except at three or four singular abscissa points.

Physically, it seems convincing that in the presence of transverse ridges and valleys of roughness, moving through the Hertzian contact, there should be pressure ripples. Consequently, Dr. Berthe’s point goes to the validity of equation (11) as a description of physical reality. Without attempting in this closure to analyze an alternate, more realistic expression, the following is submitted. Equation (2) assumes that $h$ is a non-random function of $x$ only, i.e., that the average film thickness (the spacing) of the two bodies in contact does not vary in time. This assumption, of course, is based on the postulate of short wavelengths of the asperities. If one relaxes the postulate, he obtains

$$H(x,t) = h(x,t) + r(x,t)$$

(D1)

where $h$ is now written as a function not only of space but of time. By definition $h$ must be of the form

$$h(x,t) = h_i(x) + h(t)$$

(D2)

signifying that $h_i$, the ensemble-average profile of the film, is time-independent, but that $h$, the spacing of the two bodies in contact, may vary randomly in time. In pure rolling one continues to have

$$r(x,t) = r(x - Ut)$$

(D3)

so that equation (5) remains valid. However, in place of equation (6) we now have

$$\frac{\partial}{\partial x} \left( H^2 \frac{\partial^2 g}{\partial x^2} \right) = 12 \eta \nabla U \frac{\partial h}{\partial x} + \frac{\partial h}{\partial t}$$

(D4)

where on the right-hand side $\partial h/\partial t$ is a random function. This equation can also be written

$$\frac{\partial}{\partial x} \left( H^2 \frac{\partial h}{\partial x} - 12 \eta \nabla U \frac{\partial h}{\partial x} \right) = 12 \eta \frac{\partial h}{\partial t}$$

(D5)

showing that the quantity in parentheses at the left side is no longer identical zero, as suggested by equation (6), but is itself a random variable. This means that $h$ can go through zero without the need for $h_i$ to do the same.

Dr. Berthe’s interesting equation giving the pressure gradient in the presence of sliding needs further study on the part of this author in order to determine whether it is compatible with the assumptions on which the present paper is based.

The author agrees with Dr. Berthe that inclusion into the analysis of asperity spacings that are of length comparable to the entire contact is inexact, particularly in view of the fact that the ripples are thought to be limited to the relatively narrow zones of the inlet and the outlet constriction. Accordingly, results depending on long asperity spacings should be viewed with reserve. It is for this reason that Fig. 1 is of interest; it shows that while the ripple magnitudes increase rapidly as the bandwidth of the roughness is increased, they have a minimum below which they do not fall, even for single-frequency roughness. Certainly the spacing of asperities corresponding to $\omega = 6000$ in., satisfies the criterion that it be short by comparison to the contact.

Dr. K. L. Johnson’s warning about possible misinterpretation of the paper, where the calculated $\text{var}^{1/2} p$ is called a lower bound, is well taken. It would have been much preferable to designate $\text{var}^{1/2} p$ simply as a low estimate.

In the same discussion, the point is made that asperity deformation has been neglected, and this will overstate the pressure ripple. This was recognized as the price of the attempted simplification. Of particular interest in this context is the fact that pressure ripples were found to be large at the upstream slope of the exit constriction. The author wishes to raise the question whether ripples at this location might not create asperity deformations that help the asperities “squeeze through” the exit constriction without touching each other. There is circumstantial evidence to
the effect that the exit constriction, at low film thickness to roughness ratio, fails to cause the noticeable asperity damage that one would expect from its height. Therefore, a “squeezing through” seems an attractive hypothesis.

Dr. Fowles' discussion addresses itself both to the pressure and traction ripple calculations. Regarding pressure ripples, the author agrees that even small sliding velocities are likely to cause pressure ripples in the plateau. To the extent that Dr. Berthe's equation giving the pressure gradient in the presence of sliding is applicable in the framework of the present theory, we now have the beginnings of an approach for the prediction of these ripples.

With respect to Dr. Fowles' apprehension about neglect of the finite response time of the lubricant, it is not immediately obvious that the pressure gradients caused by the ripple are much sharper than those predicted from classic elasto-hydrodynamic theory in the pressure spike region. The pressure ripple should be expected to be attenuated by response delay to the same degree as the pressure spike. Regarding traction rippling, the discussor is correct that the magnitude of traction increase predicted from this theory should permit experimental observation by comparing traction values for given operating conditions between surfaces with different degrees of roughness. Regrettably, no data specifically taken for purposes of such a comparison are known to this author. Data taken with artificially grooved disks and similar long-wave surface features do not qualify. However, it is entirely possible that a more diligent search of the literature will unearth traction measurements that answer the discusser's question. Failing this, they should not be difficult to perform in the film thickness to roughness ratio range above $h/a = 3$, where the contribution to traction attributable to direct asperity contacts is negligible.