Comments regarding the relative fatigue strength of different regions of the bearing balls were made by Tallian and Mahncke, Hopkins, and Rounds. The discussers differ among each other regarding the interpretation of our data, there being some agreement and some disagreement with the authors. In this case, as well as in others, it was our primary purpose to demonstrate the effect of small amounts of water in reducing the fatigue strength of bearing steel. We believe the results in Table 3 demonstrate this effect.

We appreciate the comments of Parker and Zaretsky concerning the constancy of the stress-life relationship we found for a high stress region. This constancy tends to enhance the validity of rolling-contact fatigue experiments at elevated stress levels. We did not recalculate Hertz stress based on the running track's altered surface geometry.

The authors express their appreciation to all the discussers for their detailed and penetrating comments.

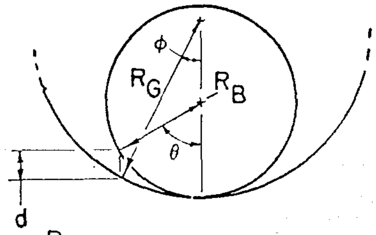
Contact Conformity Effects on Spinning Torque and Friction[1]

SUBHASH K. BATRA[2] The authors must be complimented for their keen insight in isolating an important factor contributing to friction losses in ball bearings. They have presented valuable experimental results by studying the influence of a component of motion in the bearing assembly, that of ball spin, that is often overlooked. In the process, of course, they have raised some intriguing questions about the elastohydrodynamic effects due to the spin motion. But first a few comments about the data obtained under unlubricated conditions may be in order.

In Fig. 3 the authors present experimentally measured spinning torque M_s as a function of total load. These data are compared with the torques predicted (for $f_s = 4$) by the formula developed by Poritsky et al., in reference [4]. This formula was developed for Hertzian contact conditions and assumed a flat contact ellipse. Under the loads imposed and in view of the calculations shown in Table 1 (based on the geometry in the undeformed state) for low values of C it seems reasonable to expect that the area of contact will wrap around the ball over an arc. As a consequence even the pressure distribution may depart from the Hertzian profile. It may, therefore, be more useful to first derive a value for f_s from $C \rightarrow \infty$ case, where Poritsky's assumptions are valid, and then use it to predict M_s for the other values of C. Such calculations provide an estimate of the influence of geometry on the analysis of reference [4], assuming that coefficient of kinetic friction will remain constant throughout the contact zone.

The spinning torque M_s versus load under lubricated conditions is shown in Fig. 7 of this paper. It is clear that the slope of the curve for C as 60 percent is the largest, that for 51 and 55 percent is smaller, and that for $C \rightarrow \infty$ is the least. It seems to suggest that perhaps geometry is playing a significant role in this elastohydrodynamic situation; for it is conceivable that there is a critical value of conformity where M_s versus load curve will have the maximum slope (if indeed it remains linear). It would therefore seem reasonable to explore a wider spectrum of conformity values to confirm or deny such a conjecture.

The calculated value of f_s in the lubricated case (where full film is assumed to exist) is at best a measure of the average shear stress acting over the contact area; its estimation can also be affected by the geometrical influences due to conformity levels.



$$c = \frac{R_G}{2R_B} \times 100 \text{ (percent conformity)}$$

$$\frac{d}{R_B} = (1 - \cos\theta) - \frac{c}{50}\left(1 - \sqrt{1 - \left(\frac{c}{50}\sin\theta\right)^2}\right)$$

Fig. 1

Its physical significance, however, is not very clear; for it is highly dependent on the lubricant properties, geometry of contact and other physical variables. As far as the discounting of viscous drag effect in accounting for the measured spin torques is concerned the authors may have made a hasty judgment. From the estimated Hertz stress it would seem that elastohydrodynamic effects are definitely present; but the analysis used in estimating the viscous drag does not take this into account. It is possible that a more comprehensive elastohydrodynamic analysis may explain the experimental data more adequately.

Table 1 Gap between the ball and the groove in the meridional plane (R_B = 0.25 in.)

θ (degrees)	d (C = 51%) (μ-in.)	d (C = 55%) (μ-in.)	d (C = 60%) (μ-in.)	d ($C \rightarrow \infty$) (μ-in.)
0	0	0	0	0
5.0	18.7	86.77	159.01	951.3
10.0	75.58	349.88	640.43	3,798.06
20.0	313.99	1,446.1	2,633.0	15,076.8
30.0	754.7	3,442.6	6,211.4	33,493.0

Maximum deflection for 100,000 psi maximum Hertz pressure is of the order of 1 mil.

A. B. JONES[3] **AND T. A. HARRIS.**[4] The subject of friction torque in ball bearings must yet be regarded as a substantially unknown quantity insofar as prediction of bearing adequacy is required. Too little testing effort has been devoted to this topic by researchers and therefore this paper is a welcome sign that serious consideration is being given to better definition of this important factor affecting bearing performance. The authors are to be commended on their presentation with the anticipation that further material will follow.

The discussers, however, wish to assert that establishment of spinning friction coefficients and torque values is only a small portion of the overall problem. Rather, frictional phenomena in ball bearing contacts are the result of combined rolling, sliding and spinning actions. In establishing a meaningful method to predict bearing friction, it would appear that rolling-sliding phenomena are more significant than spinning owing to the ability to develop elastohydrodynamic lubricant films thereby. Fig. 10 of the paper gives an indication of this significance when the sharp decline in friction coefficient is noted in the area of 1.7 percent reciprocal conformity; i.e., $f = 0.59$ where $f = r/d$. It appears that only inside this region of conformity is it possible to establish a satisfactory lubricant wedge and hence generate a load-supporting lubricant film. Since the rolling-sliding interaction is much more effective in generating a lubricant film, it is here anticipated that

[1] By M. W. Dietrich, R. J. Parker, E. V. Zaretsky, and W. J. Anderson, published in the April, 1969, issue of the JOURNAL OF LUBRICATION TECHNOLOGY, TRANS. ASME, Series F, Vol. 91, No. 2, pp. 308–313.

[2] Battelle Memorial Institute, Columbus, Ohio.

[3] Consulting Engineer.

[4] Manager, Analytical Services Department, SKF Industries, Inc., King of Prussia, Pa. Mem. ASME.

spinning action is less significant than the former in affecting performance. It further appears to the discussers that notwithstanding the authors' calculation, that viscous drag accounted for only a small percentage of total at high contact stress. Figs. 8 and 10 seem to imply the contrary. Perhaps non-Newtonian effects are significant in this instance.

Moreover, the discussers do not mean to indicate that spinning friction plays an insignificant role in establishing bearing performance. The localized generation of heat thereby in the lubricated contact must tend to diminish the lubricant film thickness unless adequate measures are taken to dissipate the heat.

To be better able to predict ball bearing performance, it is necessary to establish some mathematical means to determine friction forces in rolling-sliding contacts as functions of conformity, load, speed and lubrication. This discusser does not believe some simple relationship will suffice in the long run, although such may be very useful as a preliminary estimator. A wedding of empirical and analytical methods will ultimately be required in the development of large scale digital computer programs to predict bearing adequacy in the more demanding applications of the future.

Furthermore, one must not lose sight of retainer-ball and retainer-ring frictional effects. These, too, require definition if superior bearings are to be developed. The writer ends this discussion with the hope that the authors will expand this initial effort to encompass testing in the directions indicated in the foregoing.

D. G. FLOM,[5] J. V. MULLIN,[5] AND D. K. SNEDIKER.[5] This paper attacks a very important aspect of ball bearing technology, namely, that of torque arising from ball spin. In analytical models to date, frictional losses from spin either have been assumed or have been introduced empirically to provide a correlation between experimental and theoretical results.

Our comments have to do largely with the nature of the contact area and the sources of friction within this area. The average value of friction coefficient, $f_s = 4$, obtained from the unlubricated spin experiment is considerably higher than one might predict from other observations on the sliding of steel on steel. This suggests that either the Poritsky model is greatly oversimplified or that the confined contact area in spinning actually prevents immediate formation of (and lubrication by) surface films which can form more readily in linear sliding. Very probably there is a speed effect also. It would be interesting to learn what values of f_s would be obtained at speeds much lower than the 1050 rpm used in these experiments.

It is obvious that more elastic strain energy is induced in the ball when it is rotated for the 51 and 60 percent conformity than for the flat plate. In the latter case, the contact area is circular and rotation of the ball doesn't change the stress state in the ball at any point. Sliding is, therefore, the only mechanism of energy dissipation and this is more nearly in agreement with the Poritsky assumptions. Consider the initial contact area on the ball as depicted in Fig. 2 of this discussion. In the case of 51 and 60 percent conformity, the contact area is elliptical as shown in the figure and as the ball spins the loaded area A rotates to the unloaded position at A' while the unloaded area B' moves to the loaded condition at A. This cyclic loading and unloading of a portion of the contact surface represents deformation work which must be added to the energy dissipated in just sliding. Therefore one would expect the spin torque to be higher for the higher conformity as the data show.

In this connection, interpretation of the data might be simplified by considering first the effect of load and conformity on contact area and then using contact area as the sensitive parameter rather than conformity.

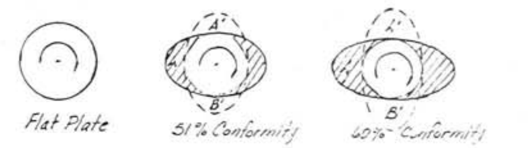


Fig. 2 Cyclic load regions on ball with varying conformity

In applying these results to real bearings, a question arises concerning the surface condition of the simulated race. A groove machined in a ball probably does not have the same orientation, with respect to the surface, of such structural variables as "fibers" (resulting from forging), carbides, and inclusions. The effects of these parameters on spinning friction are largely unknown.

B. W. KELLEY.[6] The authors have made clever use of the spin rig to judge the effect of ball and race conformity on the friction losses in angular-contact ball bearings. One difference which appears to exist between the test case and the actual problem lies in temperature inequalities which in themselves would have a marked effect on friction. In the test case the ball spins on a stationary groove or flat, as the case may be, whereas in the rotating ball bearings freshly cooled material is entering contact. This will tend to increase the elastohydrodynamic oil thickness and probably reduce the effect of conformity. Such an effect would be then added to the film thickness produced by the rolling velocity component of the balls.

Some sense of this might be obtained by examining the actual torque trace from the instant of startup on a specimen previously broken in, to the time at which friction (and thermal) stability are achieved.

ALAN R. LEVEILLE[7] The authors are to be congratulated for investigating an area of significant potential in bearing analysis and lubricant evaluation. I regret that the authors did not elaborate more fully on the overall direction in which they are heading and that they did not go into more detail concerning experimental results and techniques of analysis.

I believe that a better understanding of the subject paper requires that the following questions be discussed by the authors.

1 Is it intended that the friction coefficients extracted be applied to a full bearing where the kinematics of ball to raceway motion and oil film generation are seriously different?

2 The question of the influence of viscous drag effects could have been answered more directly by running constant load, variable speed tests. Why did the authors choose an unproven mathematical model to make the judgment that viscous effects were minor? Also, by what basis was a value of h_0 selected and how sensitive were the results to h_0 values?

3 The very high friction coefficients in the dry tests are stated by the authors to be due to extreme oxygen starvation and subsequent adhesive wear and localized welding. The work of

[5] General Electric Co., Space Sciences Laboratory, Schenectady, N. Y.

[6] Staff Engineer, Research Department, Caterpillar Tractor Co., Peoria, Ill.

[7] Senior Research Engineer, The Barden Corp., Danbury, Conn.

Buckley, Swikert, and Johnson is referenced to substantiate the results. However, the work referenced was done at 10^{-7} Torr, at higher contact stresses, at higher sliding velocities, and it took 30 minutes before the friction coefficient exceeded 1.0 and welding occurred. Comments by the authors about the physical appearance of the tested specimens and evidence of oxygen starvation would be useful in supporting the statement made in the paper.

4 The test data of Fig. 7 shows in general a nonzero intercept of torque at zero load. Apart from a need to account for this, it becomes important in the subsequent analysis to subtract this term from the total torque when deducing a friction coefficient. Otherwise, at low loads the friction coefficient deduced will be higher than that which will be deduced at higher loads. That is, the trend in friction coefficients noted in the paper would occur. Would the authors comment on how they handled their data?

5 Fig. 7 results do not agree with the mathematical model of ball-spin on two accounts. First, is the nearly linear change in torque with load rather than $(\text{load})^{4/3}$. Secondly, is the inconsistent ranking of data according to curvature. The authors should comment on this behavior.

6 Have the authors considered the possible effect on their results due to radial runout of the spinning shaft where now a circular sliding of the contact area would be superimposed on the spinning process?

In summary, I hope that the questions asked will provide the authors with an opportunity to strengthen the value of their work.

Authors' Closure

The authors would like to express their appreciation for the interest and comments of the discussers.

With regard to Mr. Harris' comments, the authors fully acknowledge that spinning without rolling is only a single factor in determining losses in a full-scale thrust-loaded ball bearing. However, it must be realized that current ball bearing technology is based upon the analysis of Jones,[8] which in turn is based upon spinning torque calculated according to Poritsky, et al., [4]. Based upon [4], Jones assumes nonlubricated contact and a constant coefficient of friction at the inner and outer races. While the analysis of Jones is an extremely useful tool, elastohydrodynamic effects can and do play an extremely important role. The significance of the instant paper is that the coefficient of spinning friction under lubricated conditions can no longer be assumed constant, but rather a function of stress and contact conformity. A further extrapolation of this conclusion is that the concept of inner or outer-race control may no longer be absolutely valid. This, of course, suggests a reanalysis of the existing model of bearing kinematics and, hence, dynamic loading. As a condition precedent, combined rolling and spinning torque experiments should be conducted as suggested by Mr. Harris.

With regard to Mr. Kelley's comments, it was calculated that the temperature rise in the contact zone was approximately 13 F. The effect of such a small temperature rise was considered insignificant to experimental results.

The spinning torque data reported were obtained during short duration (30 second) tests. At the start of upper-ball rotation, the torque-time trace showed an oscillation about a steady state value. This oscillation was damped out within 3 or 4 seconds. These steady state values of torque were those reported in the instant paper. As long as an elastohydrodynamic lubricant film was present, the torque for a particular operating condition remained constant. However, when the film was sufficiently diminished due to side leakage, a boundary lubrication mode predominated and the torque increased rapidly.

With regard to the comments of Dr. Flom, et al., the high values of the coefficient of friction found in these experiments were of the same order of magnitude as those of [5]. It is speculated that in the unlubricated experiments, after the first few revolutions, the surfaces in contact are essentially nascent and free of contaminants that would tend to lower the coefficient of friction.

The elastic strain energy phenomena in the ball were not considered in these experiments. Since the mass of the ball was relatively large compared to the stressed volume, the energy involved could have been manifested as a slight temperature increase in the ball which would not have affected the experimental results.

Where elastohydrodynamic lubrication exists, fiber orientation, carbides, and inclusions should have an insignificant effect on spinning torque. However, where boundary lubrication is the predominant mode, their effect may be significant. From this, the conclusion of Dr. Flom, et al., that, "the effect of these parameters on spinning friction are largely unknown," is true. In fact, these may be the factors accounting for the scatter in the dry friction data reported.

Contrary to the implications of Mr. Batra, the Hertzian calculations take into consideration the effects of surface curvature. In addition, the major axis of the contact ellipse is extremely small relative to the ball radius. Therefore, due to elastic deformation of the contacting surfaces, it is reasonable to assume a relatively flat contact ellipse which will not "wrap around the ball over an arc" to the extent proposed by Mr. Batra. Further, it cannot be assumed that the Poritsky assumptions [4] are valid at $C \rightarrow \infty$ and not valid at other values of C. Also, the coefficient of spinning friction varies with both stress and geometry under lubricated conditions. Hence, Mr. Batra's suggestion "to first derive a value of f_s from $C \rightarrow \infty$ and then use it to predict M for other values of C'' is not realistic "to provide an estimate of the influence of geometry."

For practical bearing applications, bearing race conformity usually ranges from 51 to 55 percent. As a result, Mr. Batra may be putting too much significance on the slopes of Fig. 7. The authors, however, agree with Mr. Batra that "a comprehensive elastohydrodynamic analysis may explain the experimental data more adequately."

Mr. Leveille's first question has been answered in reply to Mr. Harris' comment. With regard to the second question posed by him, preliminary NASA experiments on spinning torque as a function of spinning speed indicates an extremely small increase in torque with speed. The speed effect on torque is not only a function of viscous drag outside the contact but also of pressure-viscosity effects, heat generation within the contact, and lubricant shear rate. As previously discussed, an elastohydrodynamic model for spinning torque can best describe the interrelation of these variables.

In the determination of the effect of viscous drag, the value of h_0 was arbitrarily chosen as a representative film thickness in the contact zone. The range of h_0 which could have been selected was very small and, therefore, would not affect the values of viscous torque as calculated from equations (6) and (10).

Mr. Leveille's fourth question can be answered by referring to Fig. 7 which contains the torque data for the lubricated case. In this figure, as pointed out by Mr. Leveille, the torque values do not show a zero intercept for zero load. If the values of torque due to viscous effects in Table 2 were subtracted from the torque values in Fig. 7, the torque-load curves would be shifted downward and would then be more nearly coincident with the zero load-zero torque intercept on the curve.

The third question posed can be answered by drawing an analogy to a friction welding process. The test specimen surfaces exhibited both metal transfer and adhesive wear. As stated by Dr. Flom, et al., "the confined contact area in spin-

[8] Jones, A. B., "Ball Motion and Sliding in Ball Bearings." *Journal of Basic Engineering*, Trans. ASME, Series D, Vol. 81, No. 1, Mar. 1959, pp. 1–12.

ning actually prevents immediate formation of (and lubrication by) surface films which can be formed more readily in linear sliding."

Regarding the fifth question, the data of Fig. 7 are for lubricated conditions. As previously stated herein, the mathematical model [4] is derived for dry contact. However, for Fig. 3 for unlubricated contact, the data does fit the mathematical model.

In answer to the discussor's fifth question, the effects of radial runout on the spinning torque were considered before the experimental work was started. In the design of the test apparatus, a major consideration was the minimization or elimination of radial runout within the contact zone. The radial runout was measured under test conditions with capacitance probes. The runout was from zero to a maximum of $\pm.00015$-inch. Under identical test conditions there was an insignificant difference in torque between tests having the maximum runout and those having no runout.