

hance the possibility of the bearing to squeal or chatter. Converging surfaces, of course, encourage the development of at least a thin hydrodynamic-type water film.

The results of this program allow certain recommendations to be made that will be helpful in establishing the design approaches and designs for quiet bearings. The first recommendation is that the bearing be designed to produce converging surfaces for forward rotations. This can be done in an initial installation by tilting each stave slightly. The disadvantage of this approach, however, is that wear of the rubber surface in dirty waters will eventually result in a condition which will generate diverging surfaces. A second recommendation is that elastomeric compounds which will retain the low wear characteristics of the current material while carrying low friction materials should be formulated which will generate low friction surfaces. A number of similar formulations of low friction material with plastics have been made and are being offered commercially.

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Appendix

Effect of Sliding Speed on the Coefficient of Friction

Some tests on hard and soft rubbers were conducted to measure both static and kinetic friction coefficients as a function of load and speed. Typical friction-time curves are shown in Fig. 18. Since kinetic friction changed continuously during sliding, it was difficult to compare data taken at different loads and speeds. Plots of static friction, as a function of load and speed, are shown in Fig. 19. It was found that the static friction coefficient decreases with increasing speed and decreasing load.

The strain-rate and the interface temperatures would affect the shear strength of the junctions (see references [16] and [17]) as much as 50 percent. The increase in shear rate, or decrease in interface temperature, would increase the shear strength of the junctions. Speed increases both the strain rate and temperature. The shear strength could decrease if the effect of temperature were more significant than that of strain rate and vice versa. Change in shear strength as a function of sliding speed accounts for change, to some extent, in kinetic friction and entirely in static friction.

DISCUSSION

R. L. Orndorff, Jr.¹ and J. H. Kramer²

The test samples used in this study were molded by the BFGoodrich Company. The designs and the metal backings were furnished by Dr. Bhushan. BFGoodrich is the originator of the modern rubber bearing and the largest manufacturer.

The author in the introduction lists a number of variables that determine the journal speed at which the journal becomes completely hydrodynamically supported. He should have included rubber hardness and thickness. The combination of these two factors, as determined by the bearing designer, probably has as much influence as any of the other factors on the threshold hydrodynamic velocity.

In his discussion of the test apparatus, the author states that the use of a flat glass plate sliding on a curved rubber element was the geometrical and mechanical equivalent of a curved shaft sliding on a flat rubber element. This may be so, but the way the rubber deflects

is entirely different. In the first case (flat plate on a round rubber element) the rubber tends to deform and bulge at the sample edges. In the second case (curved shaft sliding over a flat rubber surface) the rubber surface bulges upward and forms rounded, low approach angle ridges that do not scrape off the lubricant. This action is important because in actual operation the rubber initially conforms to the journal due to the elastic nature of the rubber. The conformation or shaping of the rubber eventually becomes permanent due to the compression set characteristic present in some degree with every rubber compound. (Wear, as stated by the author, does have some long-term effect, especially in dirty water, but it is not the main factor in relatively clean water operation, and on a short-time laboratory test.) We have observed in our test samples the formation of a pocket in the rubber, directly under the journal, due to the elastic deformation-compression set action without any measurable rubber weight loss. The pocket actually traps high pressure fluid hydrodynamically pumped by the rotating journal. We find that we can determine the optimum formation of the pocket by specifying the circumferential width of the bearing land, the rubber thickness, and the rubber hardness. No other bearing material offers this pocket-forming capability.

The author raises some interesting and significant points in his

¹The BF Goodrich Company, Engineering Products Group, Akron Ohio. Mem. ASME.

²Section Manager, Molded Goods Engineering, The BF Goodrich Co., Akron, Ohio 44318. Mem ASME.

discussion of the factors affecting noise and how they are reflected in the testing of his samples. His conclusion about softer rubber being more likely to squeal is historically correct. However, we have found that if we reduce the rubber thickness a softer rubber can be used which results in much lower coefficient of friction values at low journal velocities. (We attribute this to the fact that the softer rubber surface asperities are more likely to bend rather than penetrate the thin water film present at low journal velocities.) We believe that the author would have found significant performance differences on his apparatus if he had reduced the rubber thickness to 0.125 in. (3.2 mm).

His discussion and conclusions about the use of friction-reducing additives in rubber bearings (similar to those used in self-lubricating metal and plastic bearings) are interesting and appear at first glance to offer potential for improvement. Over the past forty years many investigators have tried to do this. Most of these results have never been published because of their generally negative nature. However, new additives are constantly appearing in the market so eventually one may possibly work. Many additives will lower the coefficient of friction at low journal velocities. In some cases they also cause very rapid and unacceptable journal wear. By their very nature they are difficult to incorporate and adhere to the rubber. In a long-term application they usually rapidly wash or wear out and a much worse rubber situation is present as far as noise generation and wear rate are concerned. Some additives tend to form an emulsion which in turn acts to increase the mid-range coefficient of friction values. The same comments apply to surface coatings.

We believe that a molded surface, as smooth as possible, is the best rubber bearing surface. It should not be ground. The author is correct in stating that a smooth rubber surface will probably eventually become rough due to operation in dirty water during part of its life. We have found that replacing the metal backing with a plastic material gives a tighter fit in the housing grooves, resulting in less tendency to clatter. The plastic has inherently better dampening properties compared to brass. Molding and testing of rubber bearings in our laboratory with concave, flat, and convex lands have shown that the lowest friction and wear was always obtained with a flat land.

A. I. Krauter³

The author has presented a spectrum of interesting experimental results obtained from his laboratory apparatus. However, the paper does not contain an overall explanation or consistent physical model for the observed squeal/chatter phenomenon. This is a serious defect of the paper. Possibly associated with this is another defect—the interpretations of squeal and chatter given tend to obscure the actual nature of the phenomenon.

At Shaker Research, a program similar to the one discussed in this paper has been in progress. This program has resulted in a series of reports [18–21]; the last of this series gives a general elucidation of how the squeal and chatter originates. For that report both a laboratory apparatus and a mathematical model of its squeal/chatter behavior were used. Correlations were obtained by using the computer model to verify the predicted squeal tendency. The correlations indicated that squeal and chatter result from the growth of unstable vibration modes. Consequently, both squeal and chatter are manifestations of the same physical behavior—the limit cycle conclusion of the growth of the mode. If the unstable mode is associated primarily with stave support mass and stiffness, the frequency of the mode will be low (this vibration is typically termed chatter). If the unstable mode is associated primarily with rubber mass and (shear) stiffness, the frequency

of the mode will be high (this vibration is typically termed squeal). In either case, all parts of the system in general will be involved because each mode contains the motions of all parts of the system. In addition, there are potentially as many squeal/chatter frequencies as there are available vibration modes. The stability or instability of a particular mode is determined by how the inherent positive damping properties of the system (e.g., mechanical friction, hysteresis, internal damping) and the negative damping in the system (the negative slope of the friction-force-versus-speed curve) combine for that mode.

This explanation of squeal/chatter leads to specific illustrations in which the paper obscures the nature of the phenomenon. For example, in the abstract, the basic phenomenon is stated to be stick-slip. In actuality, the “stick-slip” is a *consequence* of one or more unstable modes. The basic phenomenon, if such is to be defined, is the manner in which the negative slope of the friction-speed curve arises. As another example, the introduction states that stick-slip may occur for breakdown of the lubricating film, but the issue of why the occurrence of stick-slip is unpredictable is not addressed. The occurrence is unpredictable because the slope of the friction-speed curve is not constant, but varies uncontrollably with contamination, surface finish, location of stave on glass surface, etc. A third example can be taken from the discussion of experimental results. In that discussion, it is stated that primary surface factors are surface roughness and relative moduli of the two surfaces. These, of course, enter the problem. But they may not be primary since many factors combine to produce the only destabilizing influence in the phenomenon—the friction-speed curve. Actually, lubrication of the stave/glass interface is more important than these “primary factors” because lubrication has a drastic effect on this curve. A fourth example also comes from the same section of the paper, in which it is stated that frequencies were not observed everywhere in the apparatus. This was true experimentally because the motions were probably too small to measure. The motions were there nonetheless as is evidenced by the detection of the frequencies by the load cells—load cells cannot indicate force without the simultaneous presence of motion at the same frequency. As a fifth example, the discussion indicates that the frequencies generated are a result of stick-slip at the interface. This implies that stick-slip is an input to available resonances. In actuality, the stick-slip is *part* of the unstable modal vibration and does not constitute a classical forced response situation. This leads to the final example—the misleading suggestion in the discussion of results that friction force level dictates the vibration frequency. The vibration frequency is in fact determined by the modal mass and modal stiffness. The level of friction force does not affect drastically these masses and stiffnesses; the primary effect of friction force and of its speed slope is to affect the damping in the system and, thereby, the modes which become unstable.

The paper also tends to be somewhat confusing in regard to specific technical points. For example, although in the test results section and in other places in the paper, the words “stick-slip” are used, these words are not defined. Also, it is not clear that stick (when defined as disappearance of relative motion between rubber and glass) actually occurred. In fact, results obtained at Shaker Research indicate that stick (as defined here) need not necessarily occur during squeal or chatter. Stick *can* occur if the amplitude and frequency of vibration are such that the velocity of the rubber momentarily during the oscillation attains that of the glass. As another example, it is not clear what the differences are between the terms *kinetic* and *static* friction—especially since static friction versus *speed* results are given in the paper.

The paper implies, in the introduction, a certain generality of results. In actuality, the experimental results obtained are specific to the rig used (this point applies to Shaker Research’s experimental results as well). Specifically, the modes found to be unstable depend on the specific conditions of the apparatus, including masses, stiffnesses, interface conditions (such as use of glass rather than Monel),

³ Shaker Research Corporation, Ballston Lake, N.Y. 12019

and geometry (such as use of a flat rather than curved slider). As a result, the objective of the work should have been to understand the squeal/chatter phenomenon for the apparatus used rather than to suggest generality of the experimental results obtained.

Regarding the experimental apparatus, numerous artifacts exist when compared to the shipboard bearing system. These include 1) planar motion that is kinematically different from the rotating shipboard motion, 2) intermittent motion that can prevent attainment of thermal equilibrium, 3) glass topography that differs from machined Monel, and 4) a flow environment (drops of water) that from a lubrication and heat transfer standpoint is dissimilar to submerged operation.

Regarding the experimental results themselves, the qualitative trends found agree with those obtained at Shaker Research. Specifically, the load and speed behavior reported agrees with those observed and predicted by Shaker's mathematical model. The influence of cleanliness similarly is in agreement with the experience of Shaker Research. This qualitative agreement indicates that the same basic phenomenon has been studied in both cases (apparatus-sensitive characteristics such as frequency are different) and that carefully constructed laboratory devices are useful in such studies.

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Author's Closure on the discussion by A. I. Krauter

As it is obvious from the discussion, MTI and Shaker were working on two parallel programs to understand squeal and chatter phenomena. There are fundamental differences in the approaches taken by MTI and Shaker. Shaker attempted to model the entire shipboard bearing system by introducing complex support structures which

became difficult to analyze. They varied damping and stiffness of the support system until they were able to obtain unstable modes. However, there was no correlation of the model parameters that produced instabilities with those that might occur aboard ship. They were also not able to examine the interface during sliding with their equipment. Our preliminary analysis had indicated that the noise generation is an interface phenomena between stave and shaft and, thus, received the focus of our attention. The most crucial result was that with an appropriate interface geometry neither squeal nor chatter would occur and would, thus, be independent of the support structure.

Now, the author would like to respond to some specific points raised by the discussor.

In the third paragraph, the discussor raises some questions then provides the interpretation which is generally consistent with our conclusions. Due to page limitation, the elementary details cannot be provided in the paper, and it is expected from the reader to fill in the details. In the same paragraph, he disagrees with our data that squeal frequency came from vibration of the rubber stave and not the pedestal. However, in the second paragraph, he stated that he found an unstable mode that was associated primarily with rubber mass and stiffness in the case of squeal. Therefore the author finds an inconsistency in his remarks and reaffirms own findings.

In the fourth paragraph, the discussor would like to know the definition of the words "stick-slip," and "kinetic and static frictions." When I wrote the paper, I assumed that all of the readers would be familiar with the basics. In case someone is not familiar, please refer to reference [10] in the text and [A.1] and [A.2] included in this reply. The discussor finds it surprising that the static friction changes with speed. This area is not well understood and well documented. Some reasons for this relationship are presented in the Appendix of the paper and Reference [14]. Further details can be found in a forthcoming paper by the author [A.3].

In the fifth paragraph, the discussor mentions that the results are valid for the test apparatus alone. We disagree with that conclusion since we feel that the noise generation is primarily a function of the interface conditions.

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