Our investigations have shown that these limitations greatly influence the fatigue life of rolling element bearings.

We congratulate the author for his valuable contribution to the prediction of rolling element bearing fatigue life. We wish him success in the further development of his model.

Additional References

Schlicht, H., 1968, "Strukturelle Änderungen in Wälzelementen," *Wear*, Vol. 12, 3, pp. 149–163.

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Zwirlein, O., and Schlicht, H., 1982, "Rolling Contact Fatigue Mechanism. Accelerated Testing versus Field Performance," Rolling Contact Fatigue Testing of Bearing Steels, ASTM STP 771, J. J. C. Hoo, Ed., American Society for Testing and Materials, Philadelphia, pp. 358–379.

Lorösch, H.-K., 1985, "Research on Longer Life for Rolling-Element Bearings," *Lubrication Engineering*, Vol. 41, 1, pp. 37–43.

Author's Closure

Drs. Noronha and Schlicht correctly point out that, in my attempt to be concise, I have failed to do justice to the consideration that the Schlicht-FAG model gives to *both* subsurface and surface defects as failure initiators. As I went to some pains to explain in Part II of my paper: the decision to confine the model to *surface* defects only, is based *not* on any physical claim that these are the only significant defects, but on the intent to show that *even with a stark simplification* such as the neglect of subsurface defects, a model well fitted to published results can be obtained.

It should be a warning to the builders of advanced models, that their labor may bring no more practical verisimilitude than does a suitable simplified approach. Needless to say, this kind of success is possible only within limited parameter ranges, whereas physically more sophisticated models may have a broader application field.

The discussors list three important generalizations desirable in my life model: three-dimensional contacts, non-constant traction coefficients across the contact area and inclusion of residual stress effects. Of these, the first two require extensive computational work without necessarily promising significant new general facts about model behavior. The question of residual stresses, on the other hand, is fundamental. To address it, the choice of a critical stress measure must be reexamined, as it makes a great difference to the results, whether the shear stress range, or the von Mises stress is taken as this variable.

Ioannides et al. explain in detail why they prefer in-depth physical models over simplified models. Given unlimited resources, time and skill, their approach is surely preferable. It does, however, tend to lead to a "large-laboratory monopoly" if not a manufacturers' monopoly in life modeling. The broader tribological community can be excused for wishing for an alternative, which I would call "textbook" modeling. That is what I have attempted.

Two of the specific comments in the discussion reflect a misreading of my papers.

1. I do not claim that life depends only on products of independent factors.

2. The variable δ in Fig. 4 of Part II is *not* a defect depth. It is defect depth *ratio*, applied to a constant contact. In the paper, defect depth is not treated in isolation.

The references cited in this discussion, of which only Ioannides and Harris (1985) is published in this country, are gratefully noted and will serve to broaden this author's knowledge of progress made by the Ioannides group.

Lastly, as a former long-time employee of a bearing manufacturer, this author is well aware of the constraints to open publication that the business interests of such an industry impose on its employees. The fact remains however, that this places those "inside" the manufacturer's operation at an advantage in knowledge over outside users, which a tribologist working independently may justifiably wish to mitigate.