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DISCUSSION

A. Unsworth[5]

May I congratulate the authors on their treatment of this very difficult subject. So little is known about the basic mechanisms of joint lubrication that they could have been forgiven for lapsing into a less elegant solution. However I am pleased that they persisted and produced a most stimulating picture of human joint lubrication. However, I did feel that several points were open to some debate and clarification. For instance, having shown that neither "weeping" nor "boosted" lubrication mechanisms as envisaged originally by the respective workers, could have explained the total working of human joints, they use the work of Linn and Radin [42] to justify the consolidation theory. The authors claim that reference [42] shows that buffer solution lubricates cartilage almost as well as synovial fluid and therefore imply that the physical nature of the lubricant itself is relatively unimportant and hence classical hydrodynamic and boundary mechanisms cannot be responsible for Linn and Radin's results; but that consolidation can. However infact the results of Linn and Radin show that Buffer solution produces between twice and three times the frictional values of synovial fluid on cartilage. This suggests that there is a significant difference between the two fluids when used to lubricate joints and while this does not necessarily argue against the authors theory I feel that equally it cannot be used to support their views since clearly other factors as well as consolidation must be taking place in these experiments. Indeed Radin and his co-workers in several publications favour "weeping" and "boundary" lubrication both of which Drs. Mansour and Mow reject on the basis of their own work.

In contrast I think that the authors use the work of Unsworth, et al., well to support their analysis. The point regarding the more highly loaded system is I think well made. However if we look at Fig. 12, (taken from *Journal of Lubrication Technology*, Vol. 97, No. 3) we see that the friction increases with time under load and while the cartilage can provide fluid from its bulk to act as a lubricant for a time, eventually the cartilage becomes depleted and requires to imbibe some more fluid. This takes place with time but in the case of long walks or a hard game of football, how does the cartilage imbibe fluid while under load sufficient to keep the joint surfaces separate? From our work [4] at low loads fluid seemed to be drawn into the area of close approach by means of hydrodynamic action during the low load phases which occur in every activity cycle of the human joint.

I feel therefore that important as this work is, it does not describe

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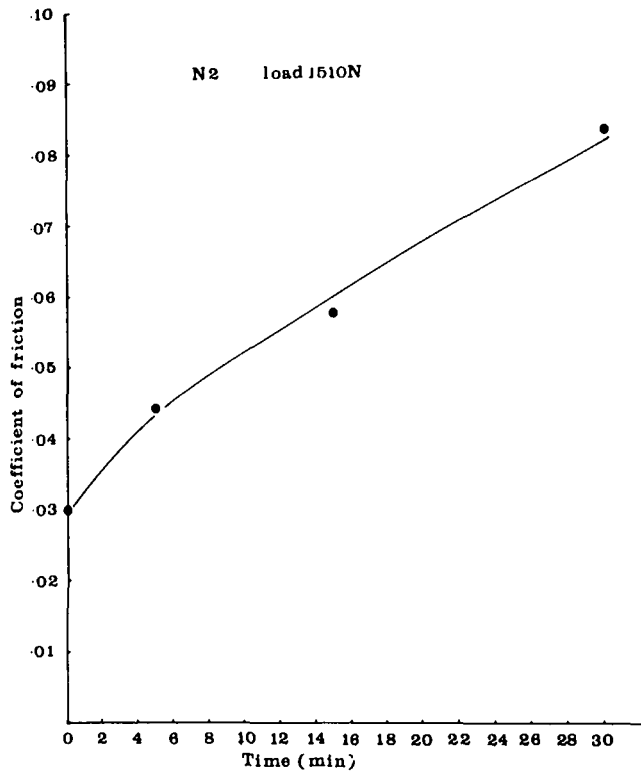**Fig. 12** **Increase in friction with time under load for a natural human hip joint**

all the mechanisms involved in the complex joint action as the authors suggest in their discussion.

On nutrition I feel that it is unfortunate that the authors use the experimental evidence of Maroudas, et al. (1968) in support of mechanical pumping of fluid in and out of cartilage since Maroudas, et al. specifically stated that the hydraulic pumping mechanism was negligible compared to the diffusion of nutrients into cartilage and maintained that joint movement only increased the transfer of nutrients flowing into cartilage because of stirring of the synovial fluid which thus prevented adverse concentration gradients being set up on the diffusion boundary. In fairness, Maroudas, et al. did not have the advantage of such a complete pattern of fluid flow available for their estimations of mechanical pumping of fluid.

I should also like to ask the authors what effect changing the value of Poisson's Ratio to the more recently discovered value, would have on the analysis?

Having made these comments I would like to thank the authors very much indeed for their most important contribution to the field.

J.F. Booker[6]

In several examples the authors have applied their very impressive analysis to the case of $\nu = .49999$ in order to approximate the (incompressible) case $\nu = .5$, for which their analysis has a singularity. It is understood that this numerical gambit can lead to considerable inaccuracy in more conventional elasticity problems. Have the authors made any tests, numerical or analytical, confirming the accuracy of their solution in this extreme application?

Authors' Closure

Professor Booker suggests that we provide a fuller discussion of the case where the Poisson's ratio $\nu = \frac{1}{2}$. We elected not to do so because of the following reasoning: In solving for the general case, the *analytical* solution is valid for $\nu = \frac{1}{2}$. However, in the *numerical* solution, the case $\nu = \frac{1}{2}$ gives rise to a zero in the denominator. The computer obviously cannot handle this situation. To do this numerically one can, however, use 0.4999 as a reasonable approximation of $\frac{1}{2}$. In this case, the computer does not see any singularities. In view of the fact that we do not have an explicit analytical solution, we cannot deduce analytically the behavior of the solution for $\nu \rightarrow \frac{1}{2}$. One can obviate the problem by assuming $\nu = \frac{1}{2}$ á priori. In this case, which incidentally is needlessly restrictive and degenerate, the displacement components simply satisfy the harmonic equation, and solutions are easily obtainable. Thus, we decided to solve for the general case even if we have to approximate $\frac{1}{2}$ by 0.4999. We do feel that this is a reasonable approximation. There is no evidence whatsoever that the numerical singularity to which Prof. Booker alluded actually does exist for these types of equations. Until the mechanically degenerate case of $\nu = \frac{1}{2}$ is actually solved, no precise answer to his speculation can be given.

We appreciate Prof. Unsworth's comments with regard to the difficulties of tackling this thorny problem of animal joint lubrication. Obviously, the theoretical model does not address itself to many of the points raised in his discussion. However, we do feel that much of the confusion concerning interstitial fluid flow and cartilage lubrication can be answered by this analysis. It took approximately forty years of research to reach this point. It is our hope that the theoretical models of joint lubrication can proceed in a more quantitative manner, and less of a conjectural and qualitative manner as evidenced by the research literature of the recent past.

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