contact phenomena has been taken into account it will not lead to any substantial changes in the conclusions of the paper regarding the geometric analysis of the ball motion or the friction torque, computed on the basis of slip motion. Referring to Figs. 1 to 3 of the paper, it is a geometric fact that no matter how the ball pivots, the four circles on the ball through A, A_1, C, C_1 on the ball at the edge of the contact bands and the corresponding circles over the pivot through A, A_1 and over the race through C, C_1 cannot all have the same length ratios. Since at these extreme points of the contact bands (through C, C_1) slipping must take place, the main conclusions obtained must be valid.

The predicted ball motion has been verified experimentally, as stated in the paper, by rotating the outer race in the direction opposite to the pivot and at such a speed that the ball center was maintained at rest, and observing the motion of the ball with a low-power microscope. The rotation diameter of the ball was found by attaching small magnets to the ball surface.

Again the torque was calculated by multiplying the local Hertz pressure by μ and by the lever arm about the pivot axis. This moment for the ball-pivot contact is larger than for ball-race contact; since the actual friction force may be less than the maximum value used, the conclusion was drawn that the actual friction moment is represented by the smaller of two moments, and that slipping will occur between the ball and the race. A more complicated motion at ball-race contact may occur—partly slipping, part twisting and untwisting—but this does not change the magnitude of the computed maximum friction torque. The authors doubt whether the friction torque which would be computed from the complete theory when the latter becomes available would be appreciably less than the torque computed in the paper. They cite the observed agreement with the *minimum* measured friction torque as a partial proof of this belief.

It is pointed out in the Barish discussion, for the case of radial bearings one must add the friction between the ball and inner race as well as between the ball and the outer race. Contrariwise, in the paper such an addition was not carried out. The reason is very clear. If no slipping takes place between the ball and the pivot no energy can be lost in friction at that contact. Moreover, the friction torque over the contact between the ball and the race must be transmitted in turn to the contact between the ball and the pivot, by Newton's third law of equal action and reaction (neglecting gyroscopic effects for the balls for rapid rotation.) In the radial bearing case the slipping is of an entirely different type and rotation takes place about the points A, A of Fig. 1 of the Barish discussion, and about two similar points over the contact with the outer race.

In conclusion the authors thank Mr. Horak for the much shorter derivation of the final equation for the friction torque.

On the Use of Power Laws in Stress Analysis Beyond the Elastic Range[1]

S. B. Batdorf[2] and E. Z. Stowell.[2] This paper shows that although desirable from the point of view of simplicity, a power law for the stress-strain relation in the theory of plastic deformation must be used with caution. The authors also show that the simplifications occur only if a single power law is applied over the whole range. However, the particular example chosen to illustrate these points may result in an unduly pessimistic impression as to the possibility of handling problems with satisfactory approximation by use of a power law. The purposes of this discussion are to show that much better results can be achieved using a different law, and to call attention to certain considerations which may serve as a guide in the proper selection of the power law to be used.

One should have a rough idea of the range of the stress-strain relation to be approximated. In the example of the paper, the power law used is shown as a dotted line, and the more realistic stress-strain curve as the solid line in Fig. 1 (the dashed curve will be discussed later). We have added, as points C and D (Fig. 1 of this discussion) the highest stresses occurring when the plastic domain extends to $10\,r_0/9$ and $4\,r_0/3$, respectively. It is clear that the dotted curve is a very bad approximation to the applicable portion of the solid curve in the first case and not too good in the second.

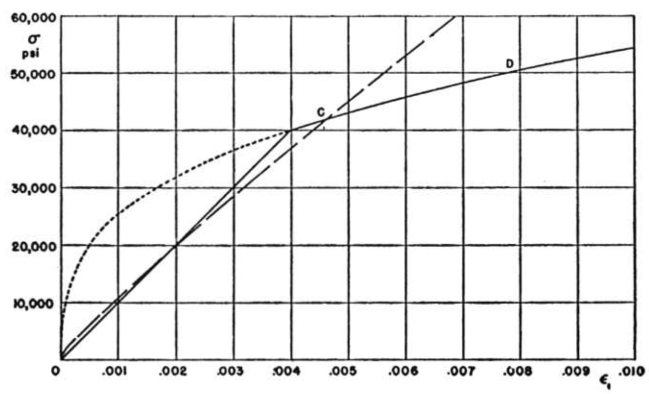

Fig. 1

In the second place it is desirable to assess the relative importance of good approximation in the elastic and plastic portions of the stress-strain curve. This can be done very roughly beforehand, at least in such cases as the example of the paper. To do this, let us assume initially that the problem is a purely elastic one. One may now ask, what fraction of the applied load is absorbed in the immediate neighborhood of the applied force, i.e., $r_0 < r < 10/9\,r_0$ or $r_0 < r < 4/3\,r_0$?

To answer this question we take $m = 0$ in Equations [20] and [25] of the paper and obtain $\alpha = 2$, so that from Equation [21]

$$S_1 = \frac{\sigma_1}{p} = -(r_0/r)^2$$

or

$$\sigma_1 = -p\left(\frac{r_0}{r}\right)^2$$

The radial load at the distance r is $2\pi r\,\sigma_1$; it follows that the initial load has dropped only 10 per cent at $r = 10\,r_0/9$ and 25 per cent at $4\,r_0/3$. In the actual case, the regions under consideration are in a plastic state and therefore support less than the 10 per cent and 25 per cent of the load just computed elastically. Since most of the load is absorbed elastically, it would seem more important to obtain a fair fit to the elastic than to the plastic part of the stress-strain curve.

Applying these thoughts, a calculation was made on the assumption that

$$\sigma_i = 45{,}000\left(\frac{e_i}{0.005}\right)^{0.9}$$

which is given as the dashed curve in Fig. 1 of the present discussion, and is a fair fit to the solid curve up to point C. The results of the application of this law are shown in Figs. 2 and 3 of this discussion, which correspond to Figs. 2 and 3 of the paper. The

[1] By Alice Winzer and W. Prager, published in the December, 1947, issue of the Journal of Applied Mechanics, Trans. ASME, vol. 69, p. A-281.

[2] Langley Memorial Aeronautical Laboratory, Langley Field, Va.

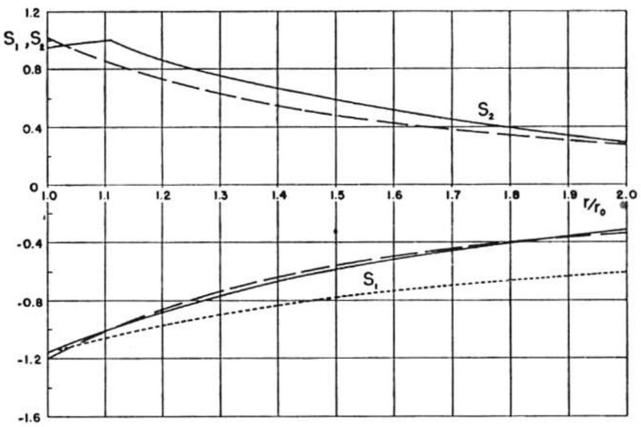

Fig. 2

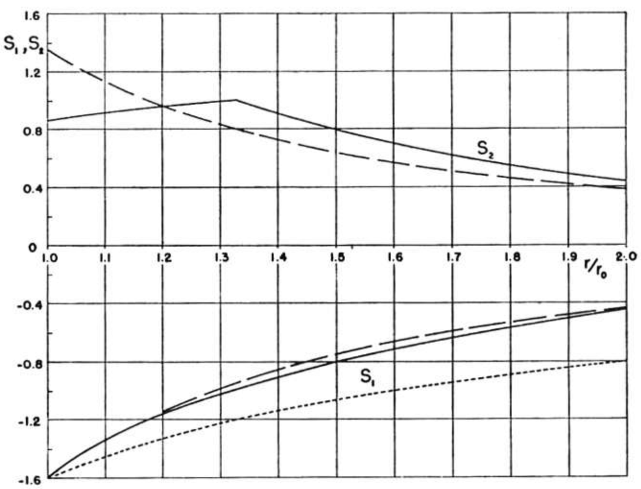

Fig. 3

solid, dotted, and dashed lines correspond to the similarly designated stress-strain curves of Fig. 1. Whether the accuracy is satisfactory will depend upon the purposes of the calculation; in any event, the dashed curves are far better than the dotted ones (which give zero for the circumferential stress, S_2), and could no doubt be improved in the case of Fig. 3 by a better choice of power law.

W. P. Roop.[3] This paper relates to the question whether the stress-strain curve of a material strained into the plastic range up to, say, 3 or 4 times the elastic limit, can be represented adequately by putting the stress equal to the strain, raised to a constant fractional power. The advantage of such an assumption is demonstrated, and its accuracy is tested in the case of a specific structure made of a specific material.

It is shown how, if the power law is adequate, the law of superposition, which so greatly facilitates solution of elastic problems is, in some degree and in a limited sense, still available.

In general, if the forces in a system by which a given structure is loaded are all increased in proportion with each other in a given progressive loading, the loading is said to follow a fixed path, as in a diagram in which the co-ordinates are components of load. This does not in itself assure that the stresses in the different parts of the metal of the structure will all follow the same path, since, at points of high stress intensity, plastic flow will enter into the action to a different degree from that at points of low stress intensity.

But if the stress-strain curve is a "power" curve, the stresses, at least in the type of structure investigated, will increase at all points throughout the structure in the same progressive pattern as the loads. This simplifies calculation of distributions, and it is taken as an assumption in obtaining the radial distribution of radial and hoop stresses in an infinite plate loaded by radial pressure in a circular hole.

The real question at issue now appears to be this: If the stress-strain curve cannot be represented by a power curve, how much error is incurred in solutions for distribution when the power law is assumed as an approximation? Naturally, this will depend upon the divergence of the assumed from the actual stress-strain curve.

For example, a more realistic assumption is now made for the form of the stress-strain curve. This still does not correspond exactly with the observed curve, but it does eliminate a large discrepancy between the assumption and the fact in substituting a straight line for the lower part of the curve in place of the continuous curve of the power law. The process of inferring the distribution under these circumstances requires a numerical integration. No observations on the distribution in the actual material are reported.

The distribution calculated from the more realistic assumption about the stress-strain curve differs strongly from that calculated by the simpler method. It is inferred that the power law must be used with caution.

J. Twyman.[4] Ilyushin's assumption that the secant shear modulus is proportional to a power of the octahedral shearing stress has no physical basis and cannot be justified unless it both (*a*) leads to a simple anaylsis and (*b*) yields reasonable results when this analysis is applied to specific problems.

The assumption certainly fulfills condition (*a*) but when applied to the problem of pressure in a circular hole in a plate, it produces the surprising result that the circumferential stress is everywhere zero. This is known to be incorrect for the elastic zone,[5] and, since no sharp discontinuity of stress can exist at the boundary with the inelastic zone, it must be incorrect for some, if not all, of the inelastic zone also.

The authors' modification, using the more realistic stress-strain curve, yields a much more reasonable result. In the elastic zone, the numerical equality of radial and circumferential stresses is in agreement with the classical theory of elasticity, and there is no discontinuity of stresses at the yield boundary. However, the method still suffers from the following disadvantages:

1 It is limited to the solution of problems where strains are so small that their products may be neglected.

2 It is limited to small displacements.

3 It is still based upon an approximation to the simple tensile stress-strain curve.

4 The assumption that the transverse contraction ratio is 0.5 in both elastic and inelastic zones is not supported by experimental evidence.[6]

5 It involves the solution of a very complex differential equation by a tedious method.

The problem of pressure in a circular hole in a thin plate has already been solved by Swainger,[7] using a step-by-step method

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[4] Civil & Mechanical Engineering Department, Northampton Polytechnic, London, England.

[5] "Theory of Elasticity," by S. Timoshenko, McGraw-Hill Book Company, Inc., New York, N.Y., 1934, p. 57.

[6] "Plastic Transverse Contraction of a Longitudinally Strained Metal," by K. H. Swainger, *Nature*, vol. 158, 1946, p. 165.

[7] "Compatibility of Stress and Strain in Yielded Metals," by K. H. Swainger, *Philosophical Magazine*, vol. 36, 1945, pp. 459–463.

based upon the actual simple tension stress-strain curve for the material. The method is simple, it allows for differing elastic and plastic transverse contraction ratios, and it gives no anomalous discontinuities in stresses, strains, or displacements. It is still limited to small strains and displacements, but a recent publication[8] by the same author foreshadows extension of his theory to cover any finite strains and displacements.

P. F. Chenea.[9] There appears to be a growing interest in stress-strain relations for the plastic range which suggest interesting generalizations although they do not apply even reasonably well to any material of engineering concern. Certainly simple theories of plasticity are desirable and generalizations are most welcome, but only when they lead to usable results. A. A. Ilyushin's procedure of drawing general conclusions from approximate properties of short segments of the stress-strain curve is most risky, and it is not surprising that the results are not in agreement with more realistic theories.

It might be thought that the authors' argument is limited by the assumption of Poisson's ratio (ν) equal to $^1/_2$ in the elastic as well as the plastic zones. In fact, this point was raised at the time the paper was presented. It is important to note that this is not so. For a general value of Poisson's ratio denoted by (ν) Equations [8] have the form

$$\sigma_1 = \frac{2G}{1-\nu}(\epsilon_1 + \nu\epsilon_2)$$

$$\sigma_2 = \frac{2G}{1-\nu}(\epsilon_2 + \nu\epsilon_1)$$

Equations [29] may be obtained directly in the usual manner and they are found to be true for all values of ν. The appropriate value of G, however, must be used in determining the displacement u. The effect of varying G is to alter slightly the radius of the boundary between the plastic and elastic zones, but the same general results will be obtained.

It is also important to note that the use of Poisson's ratio in the neighborhood of $^1/_2$ does not necessarily imply that the material is nearly incompressible. To see this it is expedient to write Poisson's ratio in terms of the bulk modulus K and the shear modulus G, as follows:

$$\nu = \frac{1}{2} - \frac{3G}{2(3K+G)}$$

Prof. P. W. Bridgman has shown that the bulk modulus K, is very nearly constant for most materials up to strains far beyond that which may be treated by the theory of infinitesimal strain. Therefore it is not the incompressibility of the material, but the rapid decline in the shear modulus G that accounts for the change in Poisson's ratio upon the entrance into the plastic range. In the case of metals, such as steel with a sharp knee in the stress-strain curve, the change in Poisson's ratio is very rapid, and the shear modulus G decreases to sufficiently low values to make Poisson's ratio approach $^1/_2$ almost as soon as the yield point is past. For other materials the change in Poisson's ratio is not so rapid, as the shear modulus does not vary as quickly. For these reasons the bulk modulus and the shear modulus are much more significant physical parameters for the treatment of problems involving plastic flow (as previously pointed out by the authors) than are Young's modulus and Poisson's ratio.

Authors' Closure

The authors wish to thank the discussers for their comments. They are particularly grateful to Captain W. P. Roop for his lucid formulation of the principal argument of their paper.

The authors welcome this opportunity of supplementing the reference given in footnote 10 of their paper. In his paper "On the Creep of Solids at Elevated Temperatures" (*Journal of Applied Physics*, vol. 8, 1937, pp. 418–432) Dr. A. Nadai has used a method of analyzing *creep* produced in a circular disk with a concentric circular hole by radial pressure which is uniformly distributed over the boundary of the hole. This method agrees essentially with the method used by the authors in the case where the analysis of the *plastic deformation* of such a disk is based on a simple power law. The authors are grateful to Dr. Nadai for drawing their attention to this fact.

[8] "Large Strains and Displacements in Stress-Strain Problems," by K. H. Swainger, *Nature*, vol. 160, 1947, p. 399.

[9] Instructor, Engineering Mechanics, University of Michigan, Ann Arbor, Mich.