Discussion.

Professor Goodman.

Professor GOODMAN exhibited and explained a working model to demonstrate the changes of position of a journal rotating in a lubricated bearing which occurred under changes of load and speed.

Mr. Donkin.

Mr. S. B. DONKIN remarked that it was very satisfactory to him and, as he knew, to other members of The Institution to feel that the recommendations for future research which they had made in 1919, as Members of the Lubrication and Lubricants Advisory Committee, had produced such able Papers as those from the Authors, from the late Sir Thomas Stanton, and from others. He regretted very much the loss of Sir Thomas Stanton and his views on the two excellent Papers before the meeting. He could not help feeling that as the work progressed the prognostication made by that Committee in 1917 might come true. That prognostication, in commercial terms, had been that if the efficiency of lubrication and of bearings generally could be improved only to a reasonable extent —and evidence had been brought forward to show how that could be done—a saving might be effected of at least 25 per cent. of the £6,000,000 then spent annually on lubricants in this country. It was on that basis that the Department of Scientific and Industrial Research, which convened the Committee, had decided to continue the investigations, which had been carried on since then under Sir William Hardy. It was particularly interesting to read Professor Goodman's general summary (pp. 263 and 264) which showed how two general requirements—the prevention of seizing and the reduction of friction —required opposite qualities of the lubricant or features in the design of the bearing. Two striking things emerged from that summary, namely, the necessity for further research in order to reconcile those opposite characteristics, and the fact that Professor Goodman had carried the matter a great deal farther than he had done in his 1928 Paper. At that time Mr. A. E. L. Chorlton remarked in the discussion that he looked above all for facts on which to base design. Here there were certain quite definite facts on which to base design and the means of compromise.

Mr. Martin.

Mr. H. M. MARTIN observed that in articles[1] published in 1915 he had given a series of curves similar to *Fig. 4* in Dr. Swift's Paper.

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[1] *Engineering*, vol. c, p. 101 *et seq.*

Mr. Martin. On those he had based a table from which it was possible to determine the load that could be carried theoretically by any bearing. He had also drawn attention to the fact that there was a tendency for a vacuum to form near the trailing end of a bearing, and had pointed out the undesirability of such a condition. He thought, however, that Dr. Swift was going too far in asserting that negative pressures could not arise in practice. The point was no doubt one for experiment; but he had some recollection of reading several years ago of a case in which negative pressure was not merely found to exist but was also made use of. He could not now recall the reference, however, and his memory might be at fault. Areas of negative pressure tended to form near the trailing end of the bearing, and the heavier the load, the narrower the bearing must be if they were to be avoided. The fact that for very heavy loads narrow bearings were advisable had been discovered by the makers of calender rolls long before there was any mathematical theory of lubrication. He thought the experience of those makers rather conflicted with Professor Goodman's suggestion that a narrow bearing was less safe than one extending over a large angle. Dr. Swift supposed that the Reynolds equation applied without modification to all portions of a bearing in which the pressure was positive, and his theory reduced itself in short to the well-known principle that for stable equilibrium the potential energy of any mechanical system must be a minimum. He was not sure, however, that Dr. Swift's assumptions would be found to be adequate in all cases. There was evidence that the Reynolds equation broke down when a certain Reynolds number was reached. That had been beautifully shown by a model exhibited at the Institution of Mechanical Engineers some years ago by Professor B. P. Haigh, M. Inst. C.E., which represented a complete bush, the eccentricity of the journal in which could be varied. The lubricant used was glycerine, and the stream-lines produced could be readily followed by the eye. With small eccentricities the flow was what would be expected from the Reynolds equation, but with larger displacements of the journal a kind of vortex made its appearance, in which, however, the motion was still stream-line, not turbulent. Reynolds's suggestion that his equations held good till turbulence came in appeared therefore to be inaccurate. It seemed a fair inference that the same rule must hold good in the case of bearings, and it might be another cause of the failure of the Reynolds equation. He suggested that it was a matter for experiment to decide whether the effect in question did come in at a certain Reynolds number. There was no hint in the Reynolds equation of such a vortex. Reynolds's solution of his differential equation was based on the assumption that in its flow the fluid never acquired a sensible radial velocity. In Professor Haigh's experiment that no longer held when the eccentricity was large, and therefore it seemed quite possible that a similar condition might occur with a heavily-loaded bearing and a very small thickness of film, with a consequent somewhat rapid separation of the two surfaces afterwards. So long as the Reynolds equation held good, he thought Dr. Swift's criterion of the minimum potential was quite sound, but he suggested that the equation might break down under other conditions than when there was negative pressure, and also that it might hold in certain cases where there was negative pressure. Only those who were engaged in similar work could appreciate the immense labour that had been required to prepare the diagrams in Dr. Swift's Paper. Professor Goodman's careful and precise measurements of a bearing of a very practical type would be of interest in making some comparisons with theory. The question of the effects of end flow had been worked out with great perseverance by Mr. R. O. Boswall, who had obtained numerical solutions. The trouble was, however, that a bearing could not be relied on to "stay put"; it deformed under the load. Therefore, in comparing nominally similar bearings it was not certain that allowance would not have to be made for a certain factor of ignorance. It was, however, of interest to get some idea as to the order of the divergence to be expected between calculation and observation. For quantitative comparisons he thought the Michell pad was much better than a journal bearing; Dr. von Freudenreich had made an extensive research on it about 12 or 15 years ago, both theoretically and experimentally, and the results had been published.[1] The postulates of the theory of lubrication were in fact much better satisfied in the case of gear teeth than in the case of a journal bearing. It had been suggested that the theory did not apply to gear teeth, because there was no dragging in of the oil such as occurred with a journal bearing. But that assertion was based on a misapprehension. Gear teeth were covered with a film of oil, and as they approached, metallic contact could not occur until the oil had been squeezed out. That extrusion was resisted by the viscosity of the oil. He had given the mathematical theory of the subject in 1916, and had shown that there was good accord between theory and observation.[2] In a recent Paper[3] Professor Kingsbury had called attention to an error in the Reynolds formula for the friction, which was, he thought, the formula quoted in Professor Goodman's Paper on p. 245. Professor Kingsbury pointed out that it should contain a term dependent on the pressure-gradient. **Mr. Martin.**

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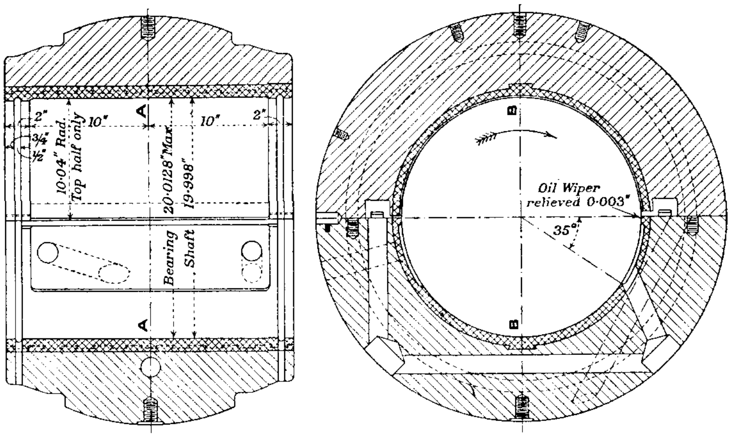
[1] "The Brown Boveri Revue," Jan.–April, 1917.

[2] *Engineering*, vol. cii, p. 119.

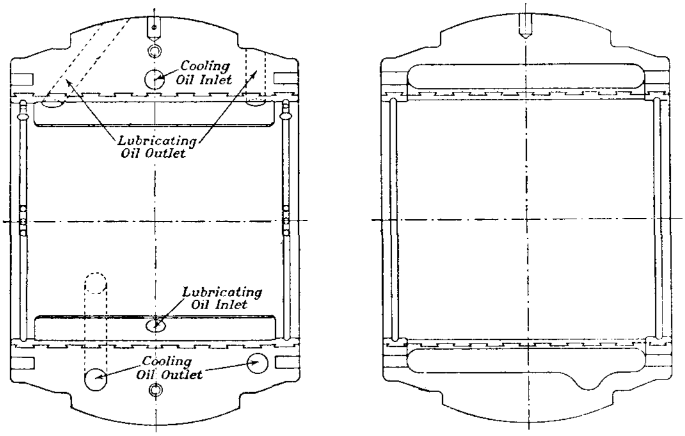
[3] Trans. Am. Soc. Mech. Eng., vol. 53, A.P.M., p. 59.

Mr. Martin. It was that term which was responsible for the rise of pressure observed by Professor Goodman, and it was the fundamental term in the case of gear-teeth friction.

Figs. 1.



SECTION BB. SECTION AA.



BOTTOM HALF. TOP HALF.

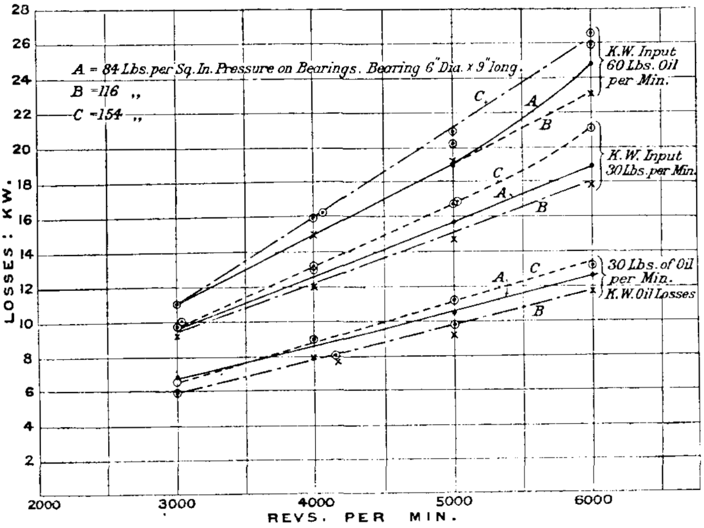
PLANS OF JOINT-FACES.

Mr. Couling. Mr. S. A. Couling showed some diagrams representing experimental results and practice of the British Thomson-Houston Company. In the standard bearing shown in *Figs. 1*, which carried about 22 tons at a journal speed of 130 feet per second, the radial clearance was only 0·0075 inch, or one-fifth of the amount suggested by Dr. Swift. In *Fig. 2*, which showed the friction losses of a bearing

Mr. Couling.

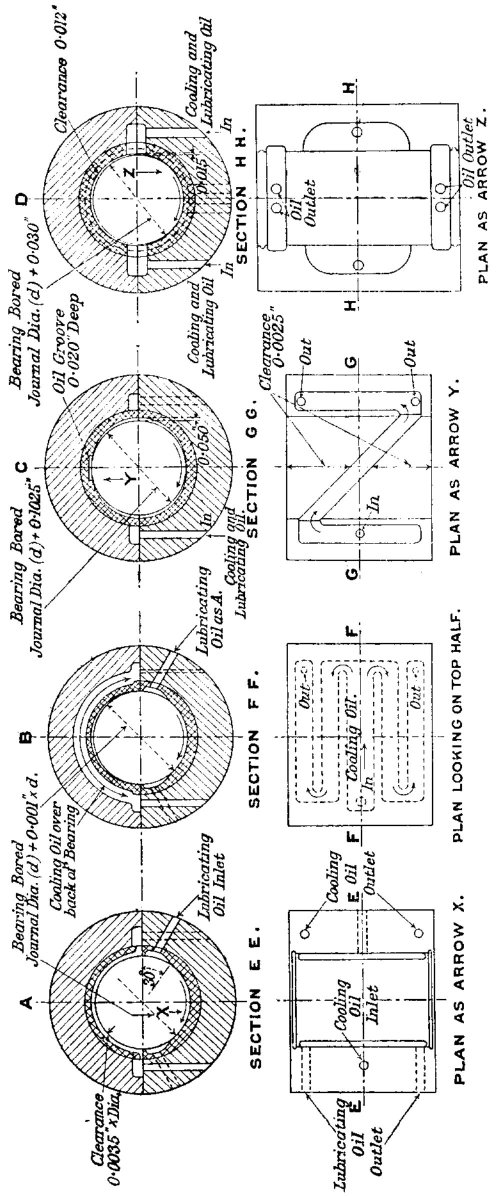
6 inches by 9 inches, with a radial clearance of 0·004 inch for the bottom half and in addition 0·02 inch total clearance on the top, a curious thing was that A, the curve for the lightest load (84 lbs. per square inch), came midway between curves B and C (116 lbs. and 154 lbs. per square inch), showing that the bearing was not economical for that load, owing probably to excessive thickness of the oil film, but was better suited for a heavier load. *Figs. 3* represented different types of experimental bearing oil-ways, and really confirmed Professor Goodman's summary on p. 262. Each type had different characteristics as regarded temperature-rise and losses. The first type of bearing was something like *Figs. 1*. The radial clearance (0·004 inch) for that test bearing gave a $\Delta\left(\frac{p}{\lambda U}\cdot\frac{r^2}{R^2}\right)$ of about 1, which corresponded to *Fig. 7* of Dr. Swift's Paper, and an eccentricity of about 0·3. Type C was a bearing having a very restricted running or vertical clearance, but the arc of contact was small and the clearance was 0·05 inch on each side. *Fig. 4* showed the temperature effects corresponding to the forms of oil-ways A, B, C, D, in *Figs. 3*. In form B, although there was the same load and the same inlet temperature of oil, and other conditions were exactly similar, yet the bearing temperature-rise at the bottom was 50 per cent. more than for type A. In *Fig. 5* the curves for C and B had changed

Fig. 2.



Mr. Couling.

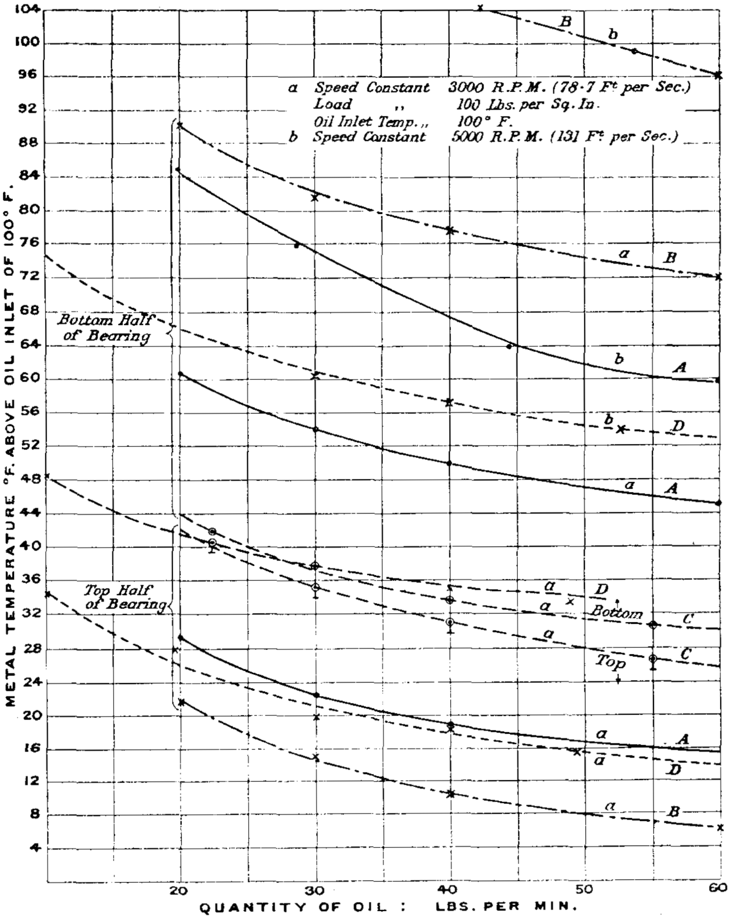
Figs. 3.



Mr. Couling.

places. This figure represented the losses. C, which had the lowest bottom bearing temperatures, had the largest losses. He also showed a lantern-slide which illustrated the danger of too large a clearance for high-speed bearings of lengths 1·2 to 1·5 times the

Fig. 4.

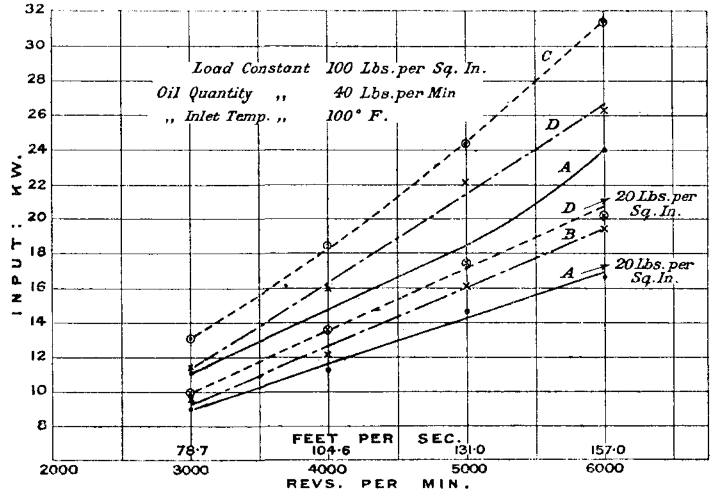


bearing diameter. The bearing, 8 inches by 12 inches, had 0·045 inch radial clearance on the top half and 0·005 inch on the bottom half, and instead of the oil spreading over the length of the journal, it concentrated so that the oil took the shortest path from the inlet side of the bearing to the outlet side.

Mr. Cornock.

Mr. A. F. Cornock congratulated Dr. Swift on his Paper. Looking at the assumptions he had made in integrating his equations, the theory as it stood at present had the appearance of having reached finality. For that reason he suggested that it was desirable to investigate the equations of viscous motion under rather less restrictive assumptions. One assumption that Dr. Swift had made did not appear in his summary. That was, that those parts of the bearing beyond the points at which his integration gave zero pressures could be neglected altogether. If there really were high negative pressures, and as, in the case of a bearing running in an oil bath, there was a fairly copious supply of oil round the ends of the brasses, some kind of return flow of oil was to be expected; that was to say, instead of the oil leaking out of the bearing at the points where the negative pressures tended to occur, the oil might be expected to flow into the bearing from outside. That suggested that the endwise flow of oil in the bearing was as important a factor in a journal bearing as it was in a Michell block. With regard to the end flow, one point was noticeable at once on comparing some of the figures—for example, Fig. 53 (Plate 5), of Professor Goodman's Paper—with Dr. Swift's results. Dr. Swift found that in a large number of cases his curve of pressure became zero before the ends of the bearings were reached. Mr. Cornock selected Fig. 53 for that purpose because the 80° and 60° temperatures for 500 lbs.

Fig. 5.



Mr. Cornock.

showed acute humps and an acute drop. In all the experimental figures there was indication of a definite positive pressure the whole way round the bearing, and it never dropped to zero anywhere between the ends of the brass. It would perhaps be unfair to suggest that someone should attempt to integrate the equations of motion, taking into account all the other complications that arose, without suggesting a method by which it should be done. He thought there was a method by which the problem might be tackled, given sufficient time and patience. Picard had developed a method of dealing with very complicated differential equations. Even equations such as these, in which the eccentricity, the shape of the brass, and the shape of the shaft—supposing it to be not truly circular or not exactly straight—could be taken into account. In that method, the equation having somehow been reduced to the form of an ordinary (not partial) differential equation, the highest differential coefficient in it was taken and was equated to the remaining terms. If the remaining terms were not known they were assumed from practice, experiment, or " the light of pure reason," and the equation was solved accordingly. That gave better approximations to the other terms than were available before, and the process was then repeated. The method seemed to be complicated, but it could be carried out numerically with comparative ease: indeed, he thought it would probably involve less numerical work than Dr. Swift had already undertaken. He suggested that that method afforded a powerful means of tackling the problem; it was, of course, very general. He himself had taken it up for another problem almost as complicated, but having nothing to do with bearings; and it had occurred to him that here was a case where the method could be very profitably applied. There was one other theoretical point to which he wished to refer. Apart from the question of turbulence in the oil film, or circulatory flow, to which Mr. Martin had referred, the Reynolds theory as ordinarily expounded assumed that the second differential coefficient of the velocity with respect to the clearance was so large that differential coefficients in any other direction could be neglected. Although that might be true over a very large range of the bearing, he thought it probably broke down towards the ends, particularly having regard to the attitude of the bearing which Professor Goodman found. He thought it might reasonably be expected that at some point the actual velocity-gradient would become very nearly linear, and in that case, of course, the second differential coefficient vanished entirely. Therefore, to get a picture of what was happening in what he would call the critical regions of the bearing, it was essential to integrate the equations more rigorously to a second degree of approximation.

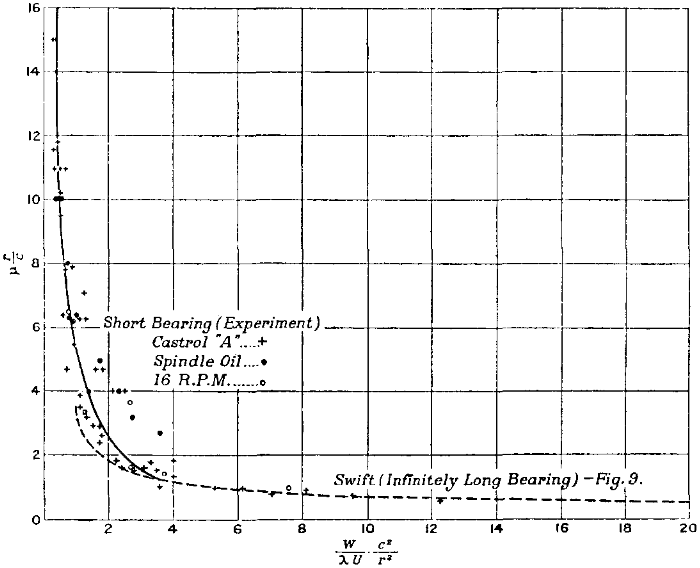
Dr. Rowell. Dr. H. S. ROWELL remarked that Professor Goodman's Paper appeared to him to be almost unassailable; the Author had contented himself with presenting a gold-mine of information on a very complicated subject. There were, however, two or three small points on which he desired to comment. On p. 245 the phrase "uniform thickness" occurred. That, of course, in a loaded bearing was hardly conceivable, and perhaps the Author would correct it. On p. 247 the diagram introduced by Dr. Sommerfeld was re-named. That was rather a pity. Dr. Sommerfeld had contributed one of the greatest advances in the theory of the subject, after 20 years of stagnation. Even though his diagram should eventually prove to be faulty, the time was hardly yet ripe for his name to be expunged. On p. 248 the use of pressure calculations in regard to end leakage was described. He was very pleased to note that, as some years ago he himself had advocated it in order to determine the end leakage. He found Dr. Swift's Paper more interesting and more vulnerable. On p. 267 the importance of end leakage was mentioned, but hardly adequately. It was often not sufficiently realized that without end leakage modern bearings would not function at all. That was one of the most stringent criticisms that could be raised against the conventional fluid theory. In modern bearings, and especially in high-speed automobile bearings, everything depended on end leakage. The more end leakage, the better the oil functioned. The main function of the oil in such bearings was cooling. Dr. Swift's *Fig. 2* was similar to the Sommerfeld diagram, but hardly in agreement with Dr. Sommerfeld's work. From *Fig. 5* the difference was seen in the much-criticized upward branch of the curve. In *Fig. 5* the curve was in close contact with the circle representing the locus of eccentricity, whereas in *Fig. 2* there was a considerable space between the Sommerfeld curve and the quadrant. That difference was very important. On p. 270 there was a rather sweeping attack on the work of Dr. Sommerfeld. In Mr. Rowell's opinion, the dismissal of that work was a little peremptory and summary. In a few lines of reasoning the conclusion was reached that: "The whole of the rising portion of the locus from Q to B must therefore be rejected as impossible." The facts were that Dr. Sommerfeld's theory assumed no traction, and after reiterated attack by mathematicians in every country of the world over 30 years, the mathematics used by Sommerfeld had been found to be flawless. Any defect there might be must therefore lie in the assumptions. Dr. Sommerfeld's main assumption was that there was no traction in the film. Taking the ordinary mathematical conditions of equilibrium, Sommerfeld deduced that the locus of the centre of the bearing was the curve here given; and, following it out to its conclusion, the

Mr. Rowell.

upward curve in *Fig. 5* with the bearing where the contact was so close at once disproved the validity of the theory in that region. He doubted very much if the negative-pressure contention had been properly established. Both Gümbel and Sir Thomas Stanton had certainly noticed negative pressures and suctions; but, as some of Professor Goodman's diagrams showed, the suction was not universal; and, as he would point out later, some of the factors in the subject were so conflicting and so complicated that it was extraordinarily difficult to come to any conclusions at all. He thought it was to be regretted that Dr. Swift did not make clearer where the work of Reynolds, Sommerfeld, and Gümbel began and ended. He had not had time to refer to the original Papers, but some of the expressions on p. 273 occurred to his memory as being very like Sommerfeld's. It would help the reader if the various foundation stones were pointed out in the Paper. It would also be interesting, he thought, if Dr. Swift could give a short sketch contrasting his own calculations with those of Gümbel. Gümbel had arrived at the elliptical curve in *Fig. 5*, and presumably by some parameter Dr. Swift had brought in the curves for the shorter bearings. It would throw the whole thing into far better light if Gümbel's conclusions and methods were put in contrast. One of the most extraordinary things in dealing with those curves was the convergence of the locus. On p. 279 Dr. Swift referred to " clear evidence that the locus ultimately converges on the point C." The locus must converge on the point C for static conditions; if the bearing came to rest, it fell to the bottom of the journal. He thought that was the explanation of a good many of the curves that started from the bottom of the quadrant. Gravity and lack of fluid must bring the bearing back to the lower apse of the journal. That did not mean that Sommerfeld was wrong or that Gümbel was wrong : possibly both could be right. At the foot of p. 279, in dealing with the application of the theory, there was a paragraph referring to the complications, in which somewhat contradictory statements occurred, on the unimportance of the centre locus, which, however, was described as the key to journal lubrication; and on p. 281 there was a paragraph on application to design in which the various factors were mentioned that made the conditions of the experiments extremely indeterminate; and yet those conditions were reproduced in all the experiments made in order to prove the theory put forward. Logically it did not seem right to ascribe disagreement between theory and experiment to the great complexity of conditions and in the same argument to adduce the experiments to support the theory. Every reason why the experiments might not support the theory was *a fortiori* a reason why the theory could not fit the facts. That appeared to him to

Mr. Rowell. require more justification. Without depreciating the magnificent work of the two Authors he thought attention should be drawn to the appalling rut into which the theory of bearings had fallen. Osborne Reynolds in 1884 and 1886 discovered the beautiful conception of the viscous fluid wedge, and his mathematics had been explained in 1904 by Sommerfeld and in 1914 by Gümbel, both in a very academic atmosphere. In 1920 Gümbel again attacked Sommerfeld and Sommerfeld replied. After all this, the Sommerfeld diagram and the Reynolds viscous wedge remained, but both seemed trivial in comparison with the effects of end leakage. He thought the experimental attack should now be reversed; instead of looking at the fluid film and the pressures at the centre of it, an endeavour should be made to investigate the flow from the ends of the bearing.

Fig. 6.



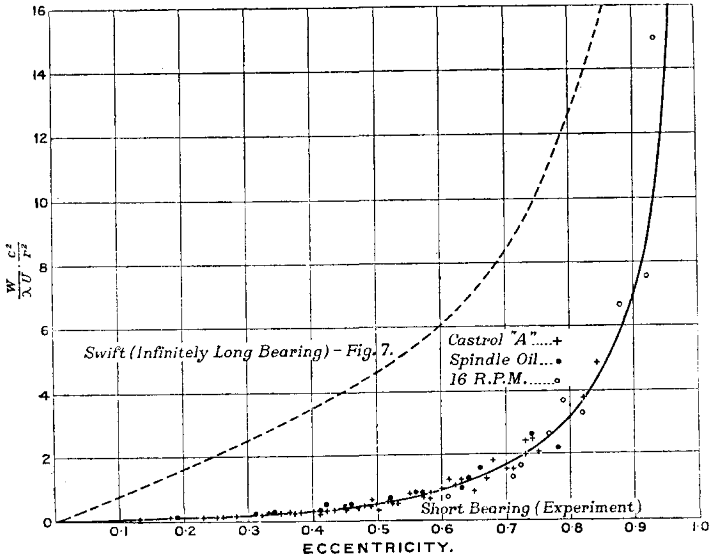
Mr. Bailey. Mr. T. H. Bailey inquired as to the nature of the bearing metal used in the experiments—whether brass, white metal, or other material.

Professor Goodman. Professor Goodman, in replying, showed some diagrams (*Figs. 6–9*) illustrating comparisons between Dr. Swift's theory and his own experiments. There was a discrepancy. He did not say his experiments were right and Dr. Swift was wrong; but it must not be forgotten that Dr. Swift had assumed an infinitely long bearing, while he had been working on an unusually short bearing. In *Fig. 6*

Professor Goodman.

the results agreed extremely well. Dr. Swift's work was shown by the lower dotted line, while his own experiments were shown by the solid line. In that case the length of the bearing did not seriously affect the result. In *Figs. 7–9* there was a greater discrepancy, probably due to the fact that there was end leakage in the experiments which was neglected in the theory. It was just possible that there were other causes. If so, he was unable to give any explanation of them. The greater part of the discussion had related to the purely mathematical theory of the lubrication and friction of bearings.

Fig. 7.



One critic of his first Paper had regarded the results as useless and uninformative, and had suggested that it was a waste of time to carry on such experiments. Possibly the criticism was justified, but if he had done nothing else for his subject, he felt that he had done a great service in persuading Dr. Swift to look into the matter. He was convinced that Dr. Swift's work was second in importance only to that of Professor Osborne Reynolds. As to the Sommerfeld theory, the late Sir Thomas Stanton had made some experiments at the National Physical Laboratory on a bearing, measuring the pressures very accurately, and had compared Sommerfeld's theory with his own experiments. In an account of Sir Thomas Stanton's work[1]

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[1] *Engineering*, vol. cxxiv (1927), p. 312.

Professor Goodman.

it was stated that, according to Sommerfeld's theory, there was a vacuum of 2,700 lbs. per square inch. No one of course could accept a theory which led to such a result. His own experiments, described in Part I of the Paper, had been made largely with the object of showing where the error came in, and they did to a large extent reveal it. He had now, in Part II, gone much more fully into the matter, and he thought it must be admitted that his experimental results

Fig. 8.

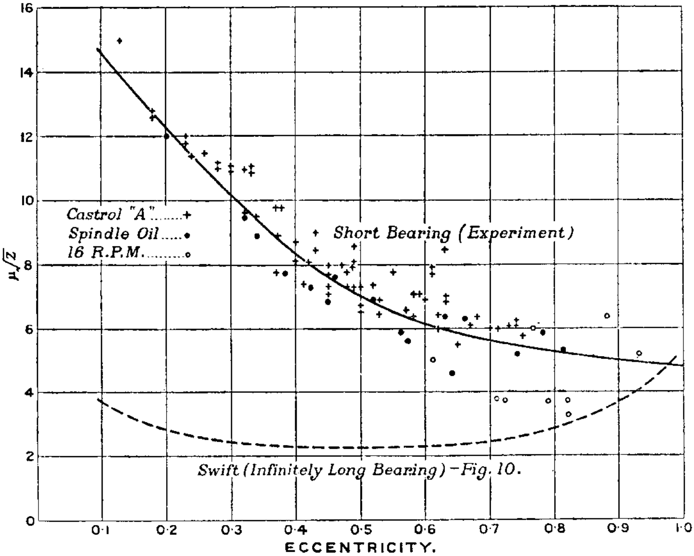
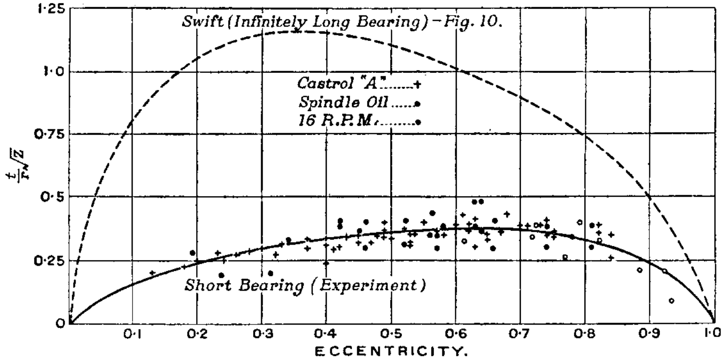


Fig. 9.



Professor Goodman.

amply proved the accuracy of Dr. Swift's theory, and showed that Dr. Sommerfeld's theory was quite wrong for at least one half of his curve. It could not be believed that there was anything in the nature of a high vacuum in the oil film. A slight negative pressure was sometimes found towards the " off " side of a bearing. He had investigated that point carefully, but he had never found a pressure of more than about 1½ lb. to 2 lbs. per square inch below atmosphere. He thought there was a great deal of truth in Mr. Cornock's remark that when the pressure became negative on the " off " side of the bearing oil leaked in rather than leaked out. If that were accepted, the results of the experiment agreed very closely with the theory Dr. Swift had brought forward. Some people spoke disparagingly of experimental results, and he had been told that he was wasting his time in doing such work. He would like to refer to one or two matters of history. Some 350 years ago there were many fantastic theories as to the movements of the planets. Tycho Brahe, a great Danish astronomer, putting theories aside, devoted his life to experimental work, accurately observing and faithfully recording. The Government made him a grant to carry on his work. Scientific men scoffed at him and even the Government at a later stage condemned his work; but shortly afterwards John Kepler, with infinite pains, by trial and error, evolved from Brahe's observations the world-renowned Kepler's laws, which, although empirical, fitted the facts with extraordinary accuracy. Then that giant of intellect, Sir Isaac Newton, followed with his marvellous theory of universal gravitation, but before publishing it to the world he deemed it wise to compare his theoretical results with Tycho Brahe's records. In 1931 the world was commemorating the great discoveries of Michael Faraday, the whole of whose work was of an experimental nature; the scientific men of his day sneered at it. Faraday, however, worked on steadily, faithfully recording the results of his experiments without ever making use of a mathematical symbol; but upon his records the great mathematical theory of electro-magnetism was founded by Clerk Maxwell. Once again despised experiments provided the foundation for a great advance in knowledge. Again, about 1885 the Institution of Mechanical Engineers asked Mr. Beauchamp Tower to carry out experiments on the lubrication of bearings, which he did with extraordinary care, and thereby discovered most unexpected and far-reaching results. Upon those experiments the hydrodynamic theory of lubrication was built by Professor Osborne Reynolds, which, with certain modifications, was the universally accepted theory to-day. Again experiments proved to be of value. Was it too much to hope that some day the experiments recorded in the Papers before the meeting would be of use to some such great men as those he had

Professor Goodman.

referred to? He did not wish it to be thought that he was foolish enough to look down upon mathematics. Not everyone could indulge in the mathematical flights of the Swifts and the Martins; some had to be content to fill a little place, spurred on by the hope that their efforts might be of use to better men. He was quite aware that most of the experiments he had made so far were of no use at present to the practical man; but he firmly believed that their value would some day be recognized, and that the time spent on them would not then be regarded as wasted. In reply to Mr. Bailey's question, he ought to have stated in this Paper, as he had done in Part I, that the bearing used was of Admiralty bronze. He was rather sorry he had used that material—not because it was not good, but because it was too soft for the purpose—and he had had a good deal of trouble on account of wear. He strongly advised the use of a harder material than Admiralty bronze for experimental bearings. The question of the advisability of using narrow bearings embracing a small arc, had been raised by Mr. Martin. From the earliest days of railways narrow bearings had been used on carriage- and wagon-axles simply because it was found that the friction was materially less than when a wider bearing was used. He investigated the matter in 1886, and the results were recorded in the Proceedings.[1] The formula given on p. 245 referred to a bedded bearing running with an extremely thin film of oil supplied by pad lubrication; for such conditions the expression was believed to be quite correct. The error in the Reynolds formula, to which Professor Kingsbury had called attention, related to a clearance bearing running with a tapered oil film, and not to such a bearing as the one referred to. The clearances mentioned by Mr. Couling were much smaller than was customary for large bearings. No bearing temperatures appeared to have been recorded, hence the discrepancies pointed out were not surprising. It must not be forgotten that the kilowatt losses included the losses in the motor, which were by no means negligible or even constant. The results shown in *Fig. 5* (p. 296) were most unusual. He had never come across another instance in which the curves were concave upwards, but there were many data showing curves concave downwards. There were so many pitfalls in friction testing that much caution was needed in accepting results obtained from machines not specially constructed for the purpose. Dr. Rowell's objection to the assumption of a film of uniform thickness had already been dealt with in the reply to Mr. Martin; the Author saw no reason to alter his description. All who were familiar with the theory of lubrication must admit the value of Dr. Sommerfeld's work, but when his theory

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[1] Minutes of Proceedings Inst. C.E., vol. lxxxix (1886), p. 421.

led so high an authority as the late Sir Thomas Stanton to indicate such an absurdity as a vacuum of 2,700 lbs. per square inch on the "off" side of a bearing, it was surely well to abandon a theory of that kind and courteous to the originator to cease coupling his name with it. He did not agree with Dr. Rowell's contention that "without end leakage modern bearings would not function at all. . . . The main function of the oil in such bearings was cooling." It was known that under severe modern conditions some cooling arrangements were essential, but the question might well be asked, Why should oil be used as the cooling medium ? Why not circulate water through a hollow shaft or through passages in the housing ? As a matter of fact a very large amount of the heat generated by friction was conducted away along the shaft and through the housings. In his own friction machine thermo-junctions were fitted to the inlet and outlet edges of the bearing, and no material rise of temperature could be detected, certainly not more than 0·5° F. The temperature of the oil in the clearance space was undoubtedly higher than in the bath under normal conditions (*cf.* Fig. 30, Plate 5). The mass of the oil film was several thousand times less than the mass of the shaft and housing; hence when the oil film came into contact with the shaft the temperatures of the two became identical. When the temperature of the oil in the bath was artificially raised above that of the shaft, the temperature of the oil in the clearance space was found to be less than that of the bath, thus clearly proving that the temperature of the shaft and bearing was the ruling factor. Professor Goodman.

Dr. Swift. Dr. SWIFT, in reply, cordially thanked the members for the keen and constructive criticism to which they had subjected his Paper. The controversial character of the discussion had no doubt been sharpened by the fact that the problem of journal-bearing lubrication had been approached by the two Authors from entirely different standpoints, which seemed all to the good in that it emphasized the width of the problem as well as the imperfect state of theoretical analysis. On his own side he was mainly concerned with criticisms of the theoretical treatment, and those had followed generally the lines that would be expected by any one familiar with Professor Osborne Reynolds's work and subsequent developments based on it. Indeed the careful reader would find in Reynolds's own Paper a most enlightening discussion of the various matters which had exercised the minds of speakers that evening, and an answer to most of their criticisms. It had been suggested that the fundamental Reynolds equation quoted at p. 267 was not exact, in that it neglected motion of the lubricant across the film and differentials of velocity-changes in other than the circumferential direction. It was obvious that any change from section to section in the form of the velocity-

Dr. Swift.

distribution across a film must necessarily involve some degree of circulation, due to the transfer of fluid from one stream-line to another, and in sections where the film was more than 3/2 the thickness at the section of linear distribution there must be an actual return of the fluid along certain stream-lines. That Reynolds realized this fact was clearly seen by reference to *Fig. 12* of his (Reynolds's) Paper, and that he realized its unimportance with surfaces which "are nearly parallel" by reference to p. 260 of his Collected Papers.[1] Analytical examination of the transverse flow showed that the ratio of transverse to surface velocity was of the same order as dh/dx, whose greatest value was er/R and therefore quite negligible in any practical form of bearing. That velocity-changes in the direction parallel to the axis of the journal might become important, as suggested by Mr. Cornock, was undeniable, and it was this very fact which, although it had been incorporated in Reynolds's original equations, made the problem of the short bearing with considerable end leakage so difficult to analyse. Mr. Martin had drawn attention to an error in the Reynolds friction formula, due to the omission of the pressure-gradient term. But so far as Dr. Swift was aware that error occurred only in his treatment of plane slide blocks, and since his fundamental general equations were correct, and since they were correctly applied in all other cases including the journal bearing, the criticism hardly appeared to be relevant. His observations on pp. 267 and 281 regarding the importance of end leakage, both in its bearing on theoretical treatment and in its practical function as a cooling agency, had been underlined by Mr. Cornock and Dr. Rowell, and lateral flow unquestionably had a great influence on the operation of short bearings. For that reason it would be futile to expect anything approaching a quantitative agreement between the results of the present theory and experiments on a short bearing, such as that used by Professor Goodman for example. But it gave some confidence in the validity of a theory to find that it agreed in a descriptive way with such experiments and was able to predict the effect of varying the conditions of operation. A comparison with experiment also gave some indication of the width of the gap to be bridged before a theory could be reckoned of practical value. He could not agree with Dr. Rowell, therefore, that it was unsound "to ascribe disagreement between theory and experiment to the great complexity of conditions and in the same argument to adduce the experiments to support the theory." Some descriptive idea of the influence of end leakage might be obtained by analogy from the Michell pad, for which the theory

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[1] "Papers on Mathematical and Physical Subjects," vol. ii. Cambridge, 1901.

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was more complete. In an article by Mr. Martin[1] valuable data had been collected showing the effect of the shape of the pad with a film thickness ratio of 2 : 1. For a pad in which $\frac{\text{transverse width}}{\text{length}} = \frac{1}{3}$, a ratio corresponding roughly with Professor Goodman's bearings, the unital load-capacity of the bearing was only about one-tenth of that for an infinitely wide pad, while the coefficient of friction was about ten times as great. No doubt end leakage from a journal bearing would have effects of a similar order, as was indicated by the comparative curves shown by Professor Goodman (*Figs. 6–9*, pp. 300–302), though it did not seem safe to adopt without careful examination a proposal made by Karelitz to provide for end leakage in journal-bearing theory by applying coefficients computed for plane slide blocks of similar proportions. The assumption that negative pressures could be ignored in the theoretical treatment had met with some criticism. In theory it was possible for pressures to persist as low as the vapour-pressure of the lubricant in use, but any pressure less than an absolute vacuum was a physical impossibility, and the theory developed by Sommerfeld set no limit on the pressure but demanded in many cases high negative pressures, as for instance under the conditions of the Stanton tests referred to by Professor Goodman. Slight depressions below atmospheric pressure had been detected on rare occasions by Professor Goodman and one or two others; but on the other hand there was abundant evidence from the work of Goodman, Stanton and others that the pressure could fall to atmospheric before the "off" point and remain so over the rest of the bearing arc, and Stanton had actually recorded as a result of systematic tests that there was a progressive withdrawal of the point of failing pressure from the "off" point as the load increased. In any case, whether or not negative pressures were rejected in the theory, it would be found, provided it were admitted as possible for the pressure curve to cease at atmospheric pressure in a divergent film, that the principle of stability, which had not been seriously assailed, would lead to the results recorded in the Paper without any modification. It was also interesting to note that the pressure-curves obtained in Stanton's large-clearance experiments agreed very closely with the boundary condition to which the stability theory led at higher eccentricities; namely $p = 0$ and $dp/d\theta = 0$ simultaneously. Dr. Rowell had complained that Sommerfeld's diagram was discounted in the Paper in a few lines on the ground of instability. The argument, however, was logical and appeared to afford a simple and direct demonstration of the fallacy of the theory, and Dr. Rowell

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[1] *Engineering*, vol. cix (1920), p. 233.

Dr. Swift. had been still more summary in his own exposure of the fallacy—"the upward curve in *Fig. 5* . . . at once disproved the validity of the theory in that region," and "the locus must converge on the point C." Dr. Rowell was in error in attributing the upward turn of Sommerfeld's curve to neglect of tractive forces in the film. Those had also been neglected in the present treatment and would be found to have no measurable bearing on the results, except at very small values of the load criterion; least of all in the region of big eccentricities over which the contention had arisen. In that connection, any difference which appeared between the Sommerfeld curves in *Figs. 2* and *5* was accidental and without significance, *Fig. 2* being merely diagrammatic while *Fig. 5* was plotted from computed figures. The uncertainty in Dr. Rowell's mind as to the nature and extent of earlier contributions to the theory of journal-bearing lubrication was not uncommon among engineers and was possibly due to the fact that a good deal of the literature of the subject was in the German language. Professor Osborne Reynolds had established the general hydrodynamic theory of lubrication and had applied it to various cases, in particular to the centrally loaded journal bearing. He had taken account of traction as well as pressure in his equations of equilibrium, and had given clear and adequate reasons for his neglect of unimportant factors. He had been careful to limit his examination to the range of what he called "complete lubrication," where the whole of the bearing arc was under pressure, realizing that above a certain load there would be rupture of the oil film, which would then only extend between the brass and journal over a portion of the whole arc, and a smaller portion as the load increased. Sommerfeld had devised a method of integrating Reynolds's differential equations, from which he had omitted the traction terms in his (Sommerfeld's) own development, though his integrating device would be equally applicable and convenient if those terms had to be included. In applying his method, however, Sommerfeld had not restricted himself to the Reynolds range, but had admitted the assumptions referred to on p. 269 over the whole range of eccentricities and had thereby countenanced negative pressures. His contribution, in short, was a valuable mathematical method applied without serious regard for physical conditions. Gümbel had realized the futility of any assumption which involved negative pressures and had been the first to propose that the pressure-curve should be regarded as terminating at the point where it fell to atmospheric pressure. But he had had no clear conception of any better assumption on which to base the boundary conditions, his purpose having been frankly to find some condition which would agree as far as possible with existing experimental data, obtained by Stribeck and Vieweg. In 1914 he assumed that the pressure was a maximum at the point of nearest approach; in 1917 that the pressure was zero at that point; in 1919 he treated the journal bearing as a plane Michell pad; in 1921 he assumed over the whole range that $p = 0$ and $dp/d\theta = 0$ simultaneously; and in his posthumously published book of 1925 he returned to the assumption for a half bearing that the pressure fell to zero at the point of nearest approach. Dr. Swift.