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## The Effect of Circumferential Pitch of Steam Turbine Blades on Torque as Compared with "Biplane Effect" on the "Lift" of Aerofoils


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# THE EFFECT OF CIRCUMFERENTIAL PITCH OF STEAM TURBINE BLADES ON TORQUE AS COMPARED WITH "BIPLANE EFFECT" ON THE "LIFT" OF AEROFOILS

By Robert Dowson, B.Sc., M.I.Mech.E.*

It is known that the "lift" of biplanes and triplanes per unit of wing surface is inferior to that of a monoplane, and that the degree of departure depends upon the gap-chord ratio. Published data indicate that the lift increases with increasing gap, apparently towards some limiting value. Regarding steam turbine wheels as rings of multiplanes, it may be asked, Is there any direct evidence that the torque on each blade is influenced by the presence of the other blades?

Many experiments have been made to determine the optimum circumferential pitch for a given blade shape, but most of these have aimed simply at arriving empirically at the best efficiency, without inquiry into the factors involved. The experiments described in the paper were carried out with low-velocity reaction blading, and were aimed at eliminating as many factors as possible, so as to make a clear issue of the effect of spacing the runner blades closer or wider apart. These experiments indicated that the effect of circumferential spacing on maximum torque was fairly critical, and that from the optimum point the torque declined less rapidly with increasing spacing than with decreasing spacing.

The decline in torque with increasing spacing is due probably to lack of proper guidance of the steam, only a portion of which gives up energy to the runner blades, the remainder doing little useful work. On the other hand, the curve of force *per blade* is found to be increasing with increased circumferential pitch, towards some optimum value (beyond the range of the experiments). This curve resembles closely that published for the increased "lift" on a biplane as the planes are spaced farther and farther apart.

The experiments are of interest on account of the above analysis and comparison, but no definite conclusion is possible without further investigation.

*Introduction.* Now that the science of practical hydrodynamics and aerodynamics is becoming more and more applied to turbines, pumps, fans, and compressors, it is of interest to inquire how far the knowledge of aerofoil mechanics is applicable to (for example) steam turbines. In practical steam turbines, it is not yet possible to use aerofoil blades, that is, streamline contours of small curvature. Almost all steam turbine blades have much greater curvature than aerofoils, on account of the necessity for utilizing the entire head of steam in a practicable number

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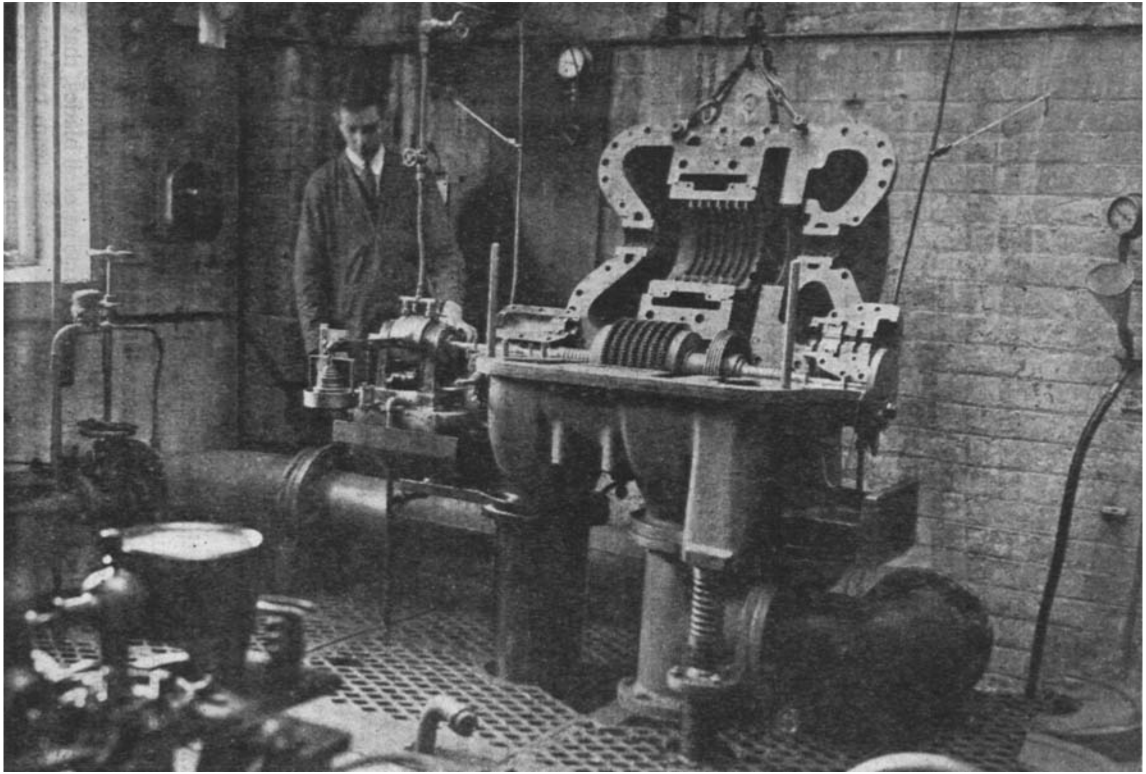

Fig. 19. Experimental Reaction Steam Turbine

Showing interchangeable cylinder liner with guide blades, and interchangeable rotor sleeve with runner rings.

of stages. Thus, for example, the effective head utilized by a modern steam turbine is about 300,000 feet, and not more than 80 stages can be adopted at most, on account of overall length.

In order to have a large enough rate of change of momentum of the steam jets of these stages, it is necessary to adopt blade discharge angles of not more than about 20 deg. From considerations of "flow-in" from the preceding nozzles, the inlet blade angles cannot be more than about 90 deg., and in impulse turbines the angle is normally much less. The nominal "outside angle" of normal turbine blades therefore, instead of being obtuse (as in aerofoils), is about 90 deg. or less.

The above limitations have already been discussed briefly by the author.* In the present paper, the point to be examined is whether the crowding together of steam turbine blades has effects similar to those noticed in aeroplane practice. In the latter, a biplane is stated to have less lifting effect than a monoplane of the same wing surface. The biplane arrangement is analogous to the circumferential pitching of steam turbine blades. Additional wing surfaces in series (one behind the other), analogous to pressure compounding in a turbine, are not adopted; and the reasons for this may have some bearing on turbine blading efficiency. Only the first of these problems is dealt with in the present paper.

The difficulties in the way of experimental investigation of such problems are considerable, being due mainly to the virtual impossibility of separating the variables. It is not easy to frame the experiments in such a way that alteration of one factor does not appreciably affect other factors, and so a "key" experiment is scarcely attainable.

*Circumferential Pitching of Steam Turbine Blades.* A common experiment in steam turbine engineering is to determine the best circumferential spacing of the blades by trial of various pitchings, until the optimum efficiency is obtained. Some years ago, the author was engaged in such experimental work with compound reaction blading of the Parsons type, and in the course of this work some careful tests were carried out with a single set of guide blades and four sets of runner blades, each having a different circumferential blade pitch.

*Experimental Turbine.* All the experiments were carried out in a small specially designed axial-flow steam turbine (Fig. 19) pressure-compounded with seven reaction pairs of rows of radial clearance blades, the same profile being used throughout. The mean diameter of the blade rings was $6\frac{7}{8}$ inches and the nominal blade length (assuming zero

* PROCEEDINGS, 1935, vol. 130, p. 45.

TABLE 9. EXPERIMENTAL SETS OF REACTION BLADING

| Set No. | Particulars of blading adopted |
|---|---|
| 17 | Comprising No. 17 cylinder liner and No. 17 runner sleeve (both standard) |
| 17B | ,, ,, ,, No. 17B runner sleeve (which had the same root spacing sections as the cylinder) |
| 17C | ,, ,, ,, a new standard (17C) runner sleeve bladed the same as No. 17 runner sleeve |
| 17C1 | ,, ,, ,, 17 C runner sleeve with every other blade removed |

TABLE 10. PARTICULARS OF EXPERIMENTAL PARSONS SETS

| Set No. | Blade No. | Sections | | No. of runner blades | Mean circumferential pitch, $p$ | | Mean opening, $o$ | | Mean ratio, $o/p$ | | Mean values of ratio, $o/p$ | Tip clearance | | Mean tip clearance, inches |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Cylinder | Runner | | Cylinder, inches | Runner, inches | Cylinder, inches | Runner, inches | Cylinder | Runner | | Cylinder, inches | Runner, inches | |
| 17 | 1A | 3B | 9B | 686 | 0·206 | 0·222 | 0·0787 | 0·0917 | 0·382 | 0·413 | 0·3975 | 0·0113 | 0·0207 | 0·016 |
| 17B | 1A | 3B | 3B | 567 | 0·206 | 0·267 | 0·0787 | 0·1357 | 0·382 | 0·5075 | 0·4448 | 0·0113 | 0·0123 | 0·0118 |
| 17C | 1A | 3B | 9B | 693 | 0·206 | 0·218 | 0·0787 | 0·092 | 0·382 | 0·422 | 0·402 | 0·0123 | 0·0125 | 0·0124 |
| 17C1 | 1A | 3B | — | 346 | 0·206 | 0·441 | 0·0787 | 0·246 | 0·382 | 0·558 | 0·475 | 0·0123 | 0·0125 | 0·0124 |

tip clearance) was $\frac{7}{8}$ inch. Four experimental sets of blading were used, as set out in Table 9. Figs. 20*a* and 20*b* illustrate these bladings.

The effect of blading the runner of set No. 17B with cylinder root spacing sections was to increase the circumferential pitch and reduce the number of runner blades. Since the runner blades of set No. 17 were deemed to have rather large tip clearance (0·0207 inch) a new runner, No. 17C, was made as a check. This had runner blades with 0·0125 inch

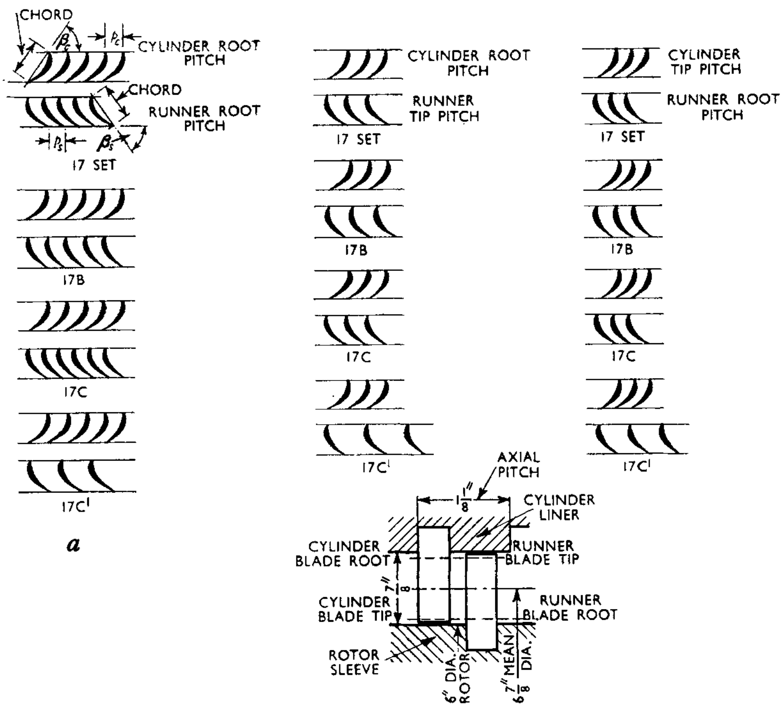


Fig. 20. Impressions of the Experimental Bladings, showing Circumferential Spacing at Root and Tip

tip clearance. The principal particulars of these four experimental sets of blading are summarized in Table 10.

*Experimental Data.* The primary experimental data required were as follows:—

(1) Initial steam pressure.
(2) Initial steam temperature
(3) Final steam pressure.

(4) Torque (or weight in scale pan of dynamometer).
(5) Revolutions per minute.
(6) Steam consumption.

To these were added the experimentally determined items:—

(7) Mechanical and frictional losses.
(8) Blade tip clearance leakage losses.

The nature of these experiments was such that the observations to be made were straightforward and involved no difficulty.

The effective steam pressure adopted throughout the experiments was 27·2 inches water gauge, with practically atmospheric exhaust (0·2 inch). The steam pressure was measured by a water column and the back pressure by a U-tube containing water. The barometric pressure was recorded by a Fortin barometer. The initial superheat was about 40 deg. F., the steam remaining superheated throughout the turbine. With this small range of expansion spread over fourteen rings of blades, the jet velocity (with equal degree of reaction) was of the order of 140 ft. per sec. or 96 m.p.h., being modified somewhat when an unequal degree of reaction existed—which was unavoidable.

Since all the experiments were made with a very small pressure drop, it was permissible to treat the working fluid as though it were water, i.e. non-expansive. The speed of the turbine was measured by a worm and wheel with 50/1 reduction ratio, so that at 3,000 r.p.m. the worm-wheel made 1 r.p.s. and was easily counted directly with a watch. A Frahm vibrating-reed tachometer was used as a check, but the observations were not used for the resulting calculations. The brake horse-power was measured by a 5 b.h.p. Froude water dynamometer, which could be accurately counterpoised by removing the gland packings so leaving the steelyard free on its ball bearing trunnions. The steam consumption was measured by exhausting the turbine into an atmospheric condenser with open outlet for the condensate. The measuring tank, when filled to the V-notch in the neck, held 104 lb. of water at the temperature of the condensate (70 deg. F.), and as the steam quantity was about 1,000 lb. per hour, the time required to fill the tank was about 6 minutes. The brake load on the turbine was varied simply by altering the weight in the scale pan, the turbine being given time, between the filling of the tanks, to change its speed and steam rate to fresh steady conditions.

With these arrangements, the data recorded in Fig. 21 were obtained. Corrections were then applied as follows:—

(1) *Mechanical Losses.* For measuring such losses, a blank turbine runner sleeve of the same weight and dimensions was used. Two methods were adopted: (*a*) driving the turbine runner by an electric motor; and (*b*) making retardation tests, in which the turbine was

accelerated, uncoupled, and allowed to run down to rest, observations being made of the time interval and the number of revolutions per minute. An atmosphere of steam was maintained in the turbine during the tests. These two methods of testing agreed well.

(2) *Blade Tip Clearance Leakage.* By previous careful experiments,

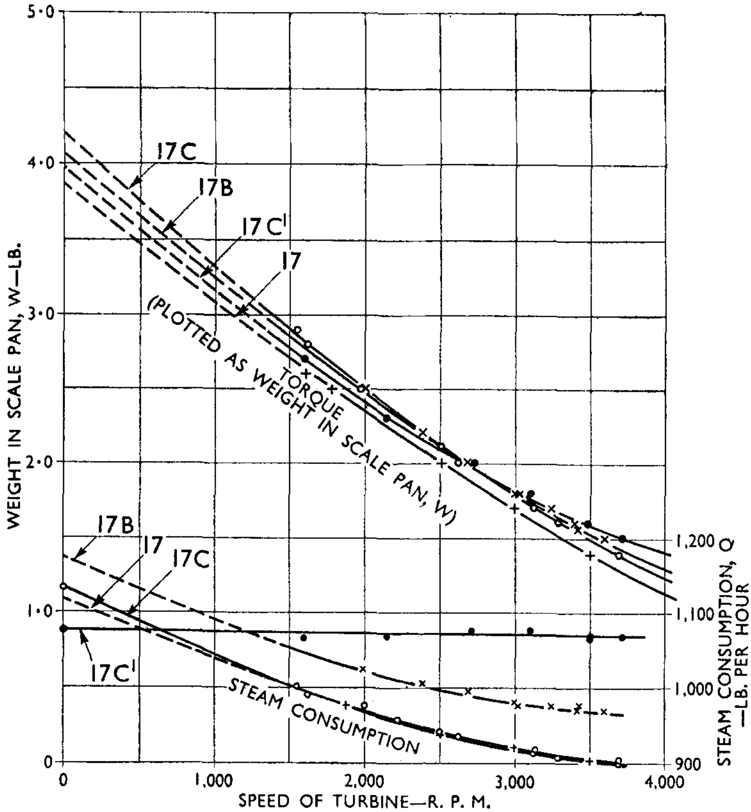


Fig. 21. Observed Data Uncorrected

the effect of tip-clearance leakage on efficiency ratio was determined for each portion representing 1 per cent of clearance area. The coefficient of discharge of steam over the blade tips (which were not knife-edged) was also determined. The latter figure enabled the steam consumption curves to be corrected to zero blade-tip clearance. To obtain the corresponding torque curves, correction was made for the reduction in steam consumption at zero tip leakage on the one hand, and the direct increase in efficiency ratio due to undisturbed flow on the other hand.

(3) *Observed Data Corrected for Mechanical Losses and Blade-Tip Clearance Leakage.* The observed data in Fig. 21 then became the

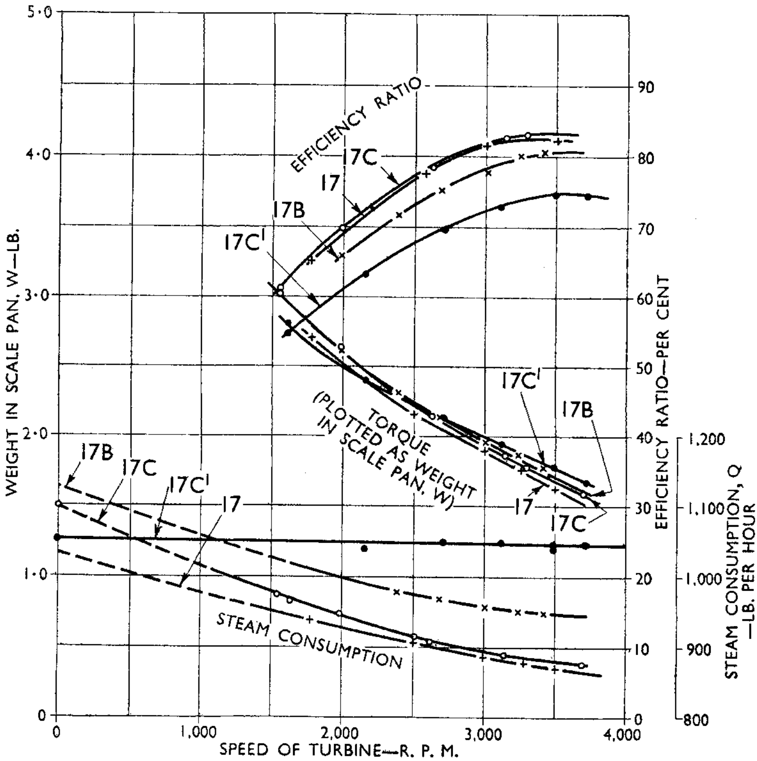


Fig. 22. Observed Data Corrected for Mechanical Losses and Blade-Tip Clearance Leakage Losses

adjusted data plotted in Fig. 22. Curves showing the efficiency ratio have been added.* These were obtained from the ratio

$$\eta = \text{Total horse-power} \times \frac{2{,}545}{Q \times h_a}$$

the total external output being the sum of the brake horse-power

* The comparatively low blading efficiency was due mainly to the fact that the blades were of an obsolete type (as used in the S.Y. *Turbinia* in 1897). The low Reynolds number at which the experiments were carried out in the small experimental turbine also appeared to reduce the efficiency.

measured by the dynamometer and the friction horse-power previously determined, while $h_a$ denotes the total isentropic heat drop in British Thermal Units per pound, and Q the steam consumption at zero tip clearance in pounds per hour. From these adjusted data the required conclusion—the effect of circumferential blade pitching on the torque—could be deduced. First, however, the following matters were taken into consideration:—

(1) The steam jet velocity from the guide blades.
(2) The angle of approach of the steam to the runner blades.

*Steam Jet Velocity from the Guide Blades.* Since the working medium might be treated as water (i.e. non-expansive) and since the discharge opening through the guides was the same in all tests (constant ratio $o/p$) * it might be expected that the steam jet velocity of discharge in any particular experimental blading set would be invariable whatever the speed; but the steam consumption curves showed that (with the exception of No. $17C^1$ set) such was not the case. The explanation was that at low velocity ratios the carry-over of kinetic energy from stage to stage was appreciable, being greatest—as would be expected—at standstill, where no external work was being done at all. Where the maximum efficiency occurred however—at high velocity ratio—the jet velocity was almost equal to that due to the isentropic heat drop $\Delta h_a$. In other words, the assistance to flow given by the carry-over was practically balanced by the losses in the blade passages.

Thus, taking set No. 17 (line 1, Table 11) the actual jet velocity from the guides when N=3,315 r.p.m. would have been (assuming no tip clearance disturbance)

$$V=\frac{Qv}{25\pi hkd}=\frac{874\times 28{\cdot}1}{25\pi\times 0{\cdot}382\times\frac{7}{8}\times 6\frac{7}{8}}=136 \text{ ft. per sec.}$$

where $d$ is the mean diameter of the blading, and $k=o/p$ for the guide blades; whereas the theoretical jet velocity $V_1=224\sqrt{\Delta h_a}=224\sqrt{0{\cdot}361}$ $=134{\cdot}5$ ft. per sec., $\Delta h_a$ being $h_a/14$ B.Th.U. per lb.

The steam jet velocity from each row of guides was evidently inversely proportional to the discharge area of the guide and runner blades respectively, and was obtained by multiplying the nominal heat drop per row of guide blades by the ratio

$$\frac{o/p \text{ for runner blades}}{o/p \text{ for cylinder blades}}$$

* See Third Report of the Steam Nozzles Research Committee, PROCEEDINGS, 1924, p. 469.

TABLE 11. JET VELOCITY FROM GUIDE BLADES AT A GIVEN (HIGH) VELOCITY RATIO

Total number of rows of blades, 14.
$h_a$=isentropic heat drop over turbine=5·05 B.Th.U. per lb.

| Set No. | Ratio $o/p=k$ | | $\Delta h_a$, B.Th.U. per lb. | $V_1$, ft. per sec. | N, r.p.m. | Flow Q, lb. per hour | V, ft. per sec. | $V_m=(V+V_1)/2$ ft. per sec. | U, ft. per sec. | Ratio, $U/V_m$ |
|---|---|---|---|---|---|---|---|---|---|---|
| | Cylinder blading | Runner blading | | | | | | | | |
| 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 |
| 17 | 0·382 | 0·413 | 0·390 | 139·6 | 3,315 | 874 | 136 | 137·8 | 99·5 | 0·722 |
| 17B | 0·382 | 0·5075 | 0·478 | 155 | 3,620 | 942 | 146·5 | 150·75 | 109 | 0·723 |
| 17C | 0·382 | 0·422 | 0·398 | 141 | 3,360 | 881 | 137 | 139 | 101 | 0·728 |
| 17C[1] | 0·382 | 0·558 | 0·527 | 162·5 | 3,920 | 1,042 | 162 | 162·25 | 117·5 | 0·728 |

Thus for set No. 17,

$$\Delta h_a = \frac{5{\cdot}05}{14} \times \frac{0{\cdot}413}{0{\cdot}382} = 0{\cdot}390 \text{ B.Th.U. per lb.}$$

TABLE 12. TORQUE LOAD PER BLADE

| Set No. | N, r.p.m. | Weight in scale pan W, lb. | Weight at mean diameter W′, lb. | Flow Q, lb. per hr. | No. of runner blades, $n$ | Ratio $W'/n$ | Rotor root pitch, inches |
|---|---|---|---|---|---|---|---|
| 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| 17 | 3,315 | 1·72 | 7·90 | 874 | 686 | 0·0115 | 0·1923 |
| 17B | 3,620 | 1·65 | 7·58 | 942 | 567 | 0·01338 | 0·233 |
| 17C | 3,360 | 1·75 | 8·04 | 881 | 693 | 0·0116 | 0·191 |
| 17C1 | 3,920 | 1·58 | 7·25 | 1,042 | 346 | 0·0209 | 0·382 |

TABLE 13. BIPLANE EFFECT ON LIFT OF AEROFOILS

| $\frac{\text{Gap}}{\text{Chord}} = \frac{g}{c}$ | Factor $\lambda$ |
|---|---|
| 0·40 | 0·61 |
| 0·80 | 0·76 |
| 1·00 | 0·81 |
| 1·20 | 0·86 |
| 1·60 | 0·89 |

*Angle of Approach of Steam to Runner Blades.* It is known that with aerofoils the relative angle of approach of the medium is important, for it influences the lift; and in comparing aerofoils it is necessary to take this important variable into account.

In calculating the results, therefore, it was advisable to preserve the same precaution with the turbine runner blades, and to make all comparisons at equal angles of approach. Thus in Fig. 23, by choosing the same velocity ratio U/V from all four sets of experimental data, and having also the same steam discharge angle $\alpha$ from the guide blades, it followed that the angle $\theta$ at which the steam approached the runner blades was also constant. This, however, involved different values of the velocity of approach $V_R$ because of variation in the degree of reaction.

*Final Comparison of Performance of Experimental Bladings.* The final data could now be drawn up as in Table 12. Col. 3 gave the total (corrected) scale pan load W on the brake at the speed specified, but it was of interest to alter this to load W′ at the mean diameter of the

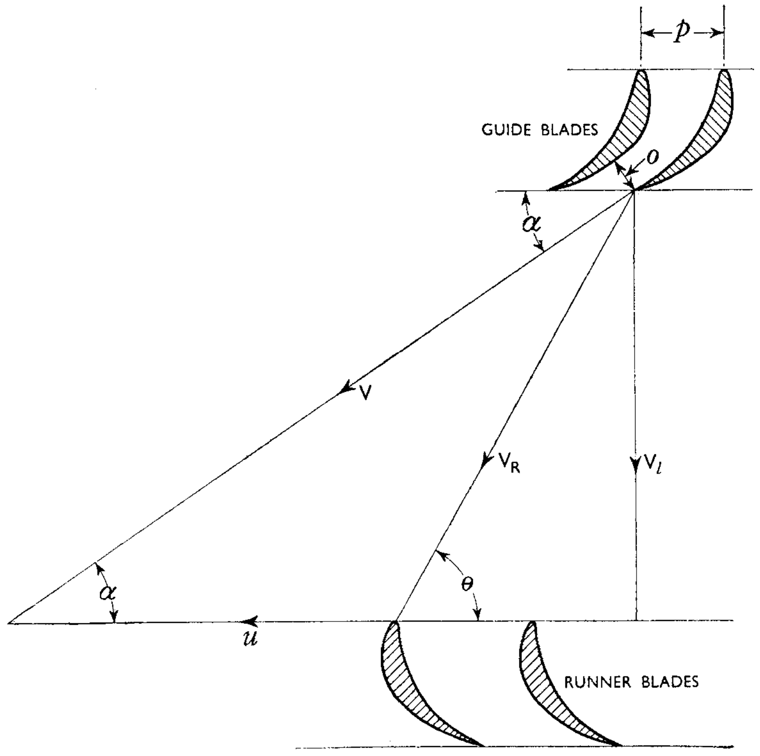


Fig. 23. Diagram of Velocities ($\alpha = \sin^{-1} o/p$)

blades. The quantity W′ in Table 12 was obtained from W by multiplying by 15·787 inches $\div 3\frac{7}{16}$ inches, or 4·59. A curve AB of torque or, more correctly, the force W′ per runner blade, was then constructed (Fig. 24).

Assistance in locating this curve was provided by the supposition that if the runner blades had been pitched so closely that they all touched, the torque would have been practically nil.

This enabled the zero point A to be found, the root pitch with blades touching (Fig. 25) being 0·079 inch.

Curve CD represents the relation $n \times p = \text{const.} = 132$, and shows the total number of runner blades $n$ for any given root pitch $p$. Combining curves AB and CD, the curve EF of total force W′ was established. The reason for not plotting W′ directly was that while the curve EF had a

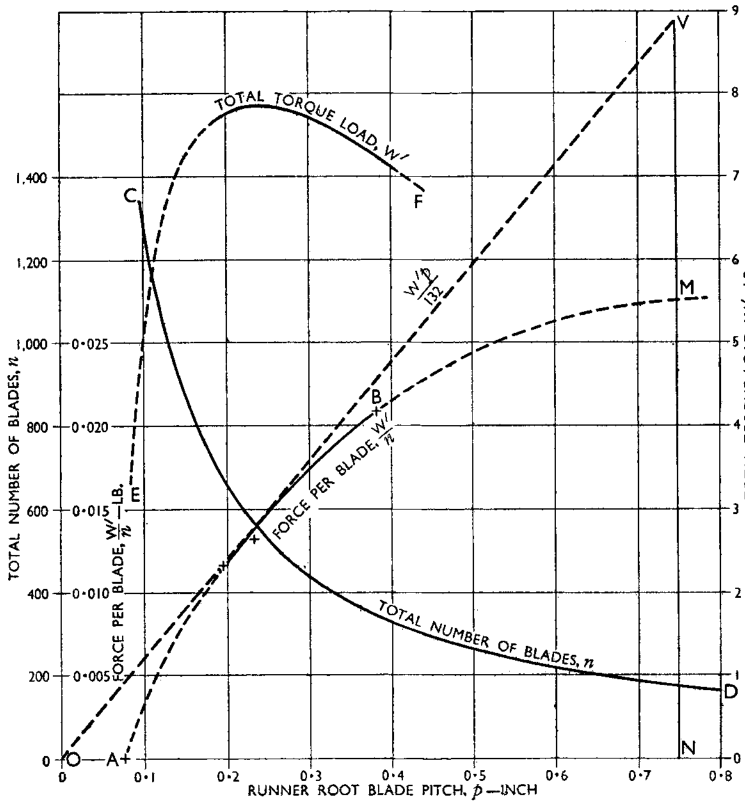


Fig. 24. Curves showing the Effect of Root Pitch of Runner Blades on Torque Load per Blade and Total Torque Load

pronounced peak and would be difficult to locate with the data available curve AB was more readily established.

*Comparison of Results with Aerofoil Performance.* It was then possible to compare W′/$n$, the force per blade in the experimental reaction blading, with "biplane" effect in aerofoils. Referring to Fig. 26, the data given in Table 13 were taken from Lionel Marks's "Mechanical Engineer's Handbook," 1916 ed., p. 1253. The factor $\lambda$ is a multiplier to be applied to the lift coefficient $C_L$ of a monoplane wing. It shows how the lift coefficient of a biplane increases as the gap ratio increases. This is equivalent to saying that when two planes are superposed, one above the other, the lift per plane increases as the gap increases, towards a limiting value which approaches that of a single plane by itself. The curve *ab* in Fig. 28 shows the effect graphically. It is seen that even with

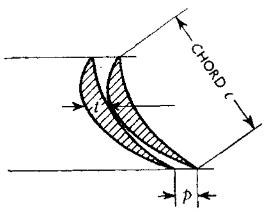


Fig. 25. Impressions of Blades, showing Root Pitch when Runner Blades are Touching Gap $g$ = zero and pitch $p = t$

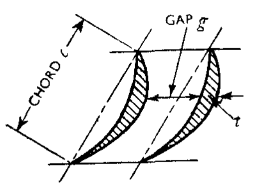


Fig. 27. Diagram Defining Gap-Chord Ratio for Steam Turbine Blading

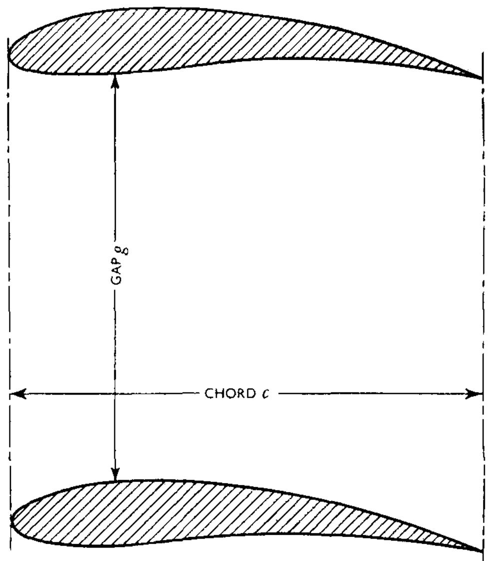


Fig. 26. Diagram Defining Gap-Chord Ratio for Aerofoils

large spacing in a biplane, the lift per plane is not equal to that of a monoplane of equal surface.

The corresponding curve AB (Fig. 28) for the turbine was obtained from data collected in Table 14, in which the gap-chord ratio $g/c$ (Fig. 27) was added, as well as the pitch-chord ratio $p/c$. This enabled the curve AB in Fig. 24 to be replotted as shown in Fig. 28.

*Conclusions.* If the total driving force on the runner blades were unaffected by the circumferential pitching $p$, so that W′ remained constant when the runner blades were spaced wider apart, then $W'/n$, the

TABLE 14. TORQUE ON STEAM TURBINE BLADES AND GAP-CHORD RATIO

The data for W′/*n* are plotted on a base of *g*/*c* in Fig. 28.

| Runner root pitch, inches, | Pitch-chord ratio, *p*/*c* | Gap-chord ratio, *g*/*c* | Force per blade W′/*n*, lb. |
|---|---|---|---|
| 0·079 | 0·183 | 0 | 0 |
| 0·10 | 0·231 | 0·0485 | 0·0035 |
| 0·15 | 0·346 | 0·164 | 0·00825 |
| 0·20 | 0·462 | 0·28 | 0·0117 |
| 0·30 | 0·693 | 0·511 | 0·0175 |
| 0·40 | 0·924 | 0·744 | 0·0215 |
| | | | Conjectural |
| 0·5115 | 1·18 | 1·00 | 0·0246 |
| 0·75 | 1·73 | 1·55 | 0·0275 |

force per blade, would vary directly as *p*. This is indicated by the straight line OV (Fig. 24). Actually it is found that W′/*n* is given by the curve AB. This curve indicates that W′/*n* is increasing towards some limit, and is similar in shape to that of the lift on a biplane when the two planes are spaced farther and farther apart.

The fact that the rate of increase W′/*n* is falling off, in spite of increasing mass-flow of steam, indicates that the guidance of the jets is becoming less and less complete. At the conjectural peak of the curve AB (beyond the range of the experiments) it could be argued that the lift per blade was a maximum, but that much of the steam was being wasted. In the biplane, the same effect is indicated. When the planes are spaced so far apart that the lift is a maximum, the spacing is so wide that presumably some of the air passes between the planes without much downward momentum being imparted to it. In an aeroplane this does not matter, but in a steam turbine it is of vital importance to make *all* the steam give up its energy.

Now the curve EF of W′ or total lift (Fig. 24) indicates that the optimum effect, from the point of view of power development, is reached when *p* is about 0·23, i.e. at a value far below that necessary for maximum lift per blade. This indicates that when *all* the steam is being properly guided, there is some form of interference which makes the lift per blade much less than the maximum attainable.

If the number of runner blades were to be made so small that the circumferential pitch was say 1 inch, it would be evident that a good deal of the steam was passing without undergoing maximum deviation, and the question arises, at the point of optimum lift per blade, what is the actual amount of steam that is doing work? The same question arises in the case of an aeroplane. For an aerofoil of "pterygoid" form Lanchester * has shown that the ordinary Newtonian principles of momentum will apply if the mass of air acting on the plane is taken as equiva-

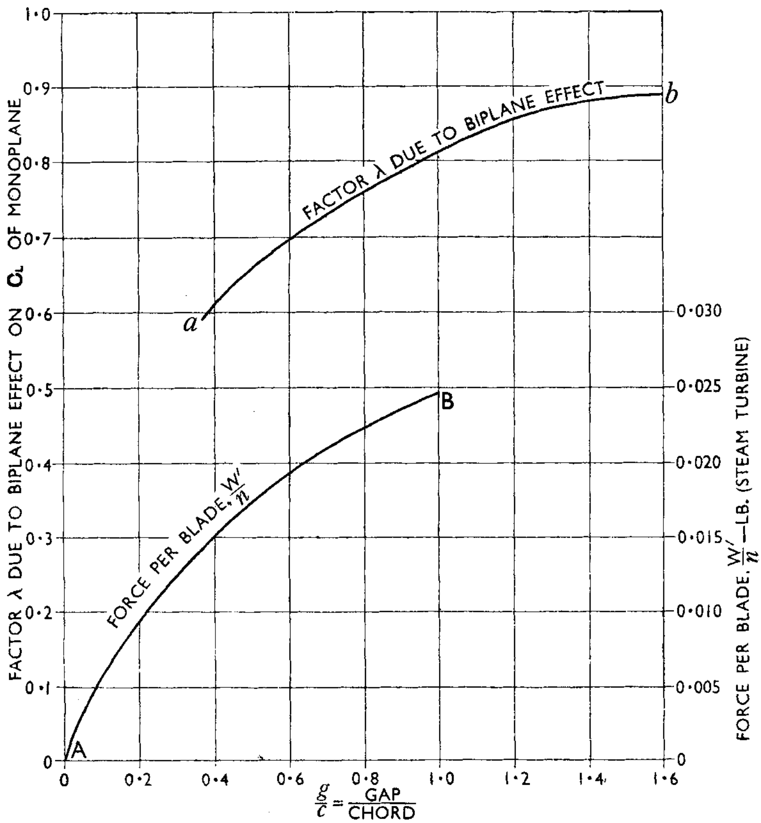


Fig. 28. "Biplane Effect" on Lift of Aerofoils Plotted Graphically Together with the Effect of Chord-Gap Ratio λ on Torque Load per Blade $W'/n$ in a Steam Turbine

lent to that which passes through an area which he has called the "peripteral" area, and which is equal to $\frac{\pi}{4}L^2$, where L is the span of the plane.

* "The Aerofoil in the Light of Theory and Experiment", Proc. Inst. Automobile Eng., 1915, vol. 9, p. 171.

For the turbine, it might be argued that the proportion of steam being usefully employed at the point M (the conjectured peak of curve AB in Fig. 24) is MN/VN=0·62. The root pitch of the runner blades would then be 0·75 inch (0·86 inch at mean diameter) giving 25 blades per row. Since the nominal length of each blade was $\frac{7}{8}$ inch the peripteral area was 0·60 sq. in. or 0·00416 sq. ft., i.e. 0·104 sq. ft. total. The complete annular area was $\frac{\pi}{4}(D^2-d^2)=0\cdot132$ sq. ft., so the ratio

$$\frac{\text{Total peripteral area}}{\text{Total annular area}}=\frac{0\cdot104}{0\cdot132}=0\cdot79$$

as compared with 0·62 evaluated above. The agreement, although not good, seems to be of interest, for it indicates that wide separation of the blades is necessary to secure maximum force per blade.

In these very approximate calculations several matters introducing errors have purposely been omitted. For example, owing to the unequal degree of reaction existing when the pitch of the runner blades is increased, the velocity of approach to the runner blades is greater than it is with equal degree of reaction. On the other hand, the relative acceleration of the steam in the runner blades is diminished. The general conclusion, however, seems to be that the lift per blade increases towards a limiting value in the same manner as the lift of widely spaced aerofoils.

In the Kaplan water turbine, it is evident that much greater spacing apart of the blades is adopted than is common in a steam turbine, and the known high efficiency seems in a measure to be due to it. Dr. A. Stodola, Hon.M.I.Mech.E., writes * of the Kaplan turbine: ". . . the developed section shows the well-known unusually high ratio $p/w$ (pitch/width), so that the guiding of the water filaments obviously cannot be perfect, and the particles outflowing at the middle of the stream have smaller peripheral components than those flowing along the bucket. The mean outflow angle is certainly larger than the bucket angle. . . . An increase of the pitch is generally advisable, with the deliberate increase of the bucket angle so as to attain, with the reduction of the friction loss, a desired mean turning. The permissible amount, of course, can be determined only by experiment."

If this is so, it indicates that the Newtonian principles of momentum, although applicable, cannot necessarily be applied directly to a turbine (such as the Kaplan, which admittedly gives high efficiency and is well within the field of practical engineering) any more than it can be applied

* "Steam and Gas Turbines", A. Stodola, translated from 6th German ed. by L. C. Loewenstein, 1927 (McGraw-Hill, New York and London), vol. 2, p. 1006.

to aerofoils. The only way of explaining the lifting action of aerofoils is by the vortex theory and it is to be inferred that the same is true of turbine blades. The application of the vortex theory is therefore necessary in order to obtain a true understanding of the action of fluid flow in turbines and similar machines.

## *Discussion*

Dr. G. GERALD STONEY, in the following communication, which was read at the commencement of the discussion, wrote that supersaturation, although of great interest from a scientific point of view, had little bearing on either the design or the efficiency of a steam turbine. If supersaturation existed to any extent in a turbine, which was rather doubtful, it reduced the available heat drop; but on the other hand it also reduced the wetness and the terminal loss as the exhaust volume was diminished. There was also more reheat. Thus on the balance the effect of supersaturation was negligible. The change in the design due to assuming supersaturation was also small and might be neglected.

The Wilson line had always seemed to him to be indeterminate, depending on how the steam was expanded, and the amount of turbulence in that expansion. It was remarkable how all the determinations of the Wilson line in a nozzle confirmed the late H. M. Martin's forecast in 1918 when he deduced its position from C. T. R. Wilson's value for the size of the condensation nuclei, with no other experimental basis to go on.

Supersaturation, however, did not end the turbine designer's troubles. What was the state of the steam at the exhaust end? For example with a vacuum of 29 inches of mercury and 15 per cent wetness, part of the water adhered to the inside of the casing and the steam was much wetter at the tips of the blades than at the roots, as shown by the erosion of the tips. Also in modern turbines the velocity of the steam might be above the critical velocity, which was the velocity of sound in the medium. What was the velocity of sound in a thick fog? He had been unable to find any record of it. For a vacuum of 29 inches and with saturated steam the homogeneous head, given by $H=144pV$, was 46,000 feet. With 15 per cent wetness, $H=39{,}300$ feet.

Again, should $\gamma$ be taken in wet steam as 1·3 or 1·1? Thus the critical velocity, $c_t=\sqrt{\gamma g H}$, might vary from 1,390 to 1,180 ft. per sec.; and the discharge might vary accordingly. Fortunately practical considerations generally required the exhaust end to be more cramped than was necessitated by theoretical considerations and thus the whole of the above problems did not matter in most cases.

Mr. C. D. Gibb (*Member of Council*) said that Mr. Dowson's paper might lead the uninitiated to believe that big improvements in turbine design were yet to come, but since turbine efficiencies were already in the region of 90 per cent, a further advance in efficiency of 1 per cent would mean that the losses would have to be reduced by 10 per cent. The rate of improvement had of necessity slowed up very much for several years past, and it would inevitably slow up still further in the future.

He agreed entirely with Mr. Dowson that a new outlook was probably necessary to obtain the extra 1 or 2 per cent efficiency, which was perhaps the maximum which could be hoped for. But the thought which crossed his mind in considering the paper was this: did aerodynamic experts know what was the efficiency of their aerofoils, and was it very much superior, if at all, to that of present-day turbine blading? If his memory and knowledge were correct, the aerofoil designer deliberately tried to deflect the air flow from the upper surface of the wing, thereby increasing the lift on the wing. That seemed to be a condition which could not be overlooked in turbine blading, and was probably one of the major factors determining the effect of pitch on torque. He could, however, fully understand the difficulties which the problem presented, because it seemed impossible to make one change without altering at least two and possibly three other variables affecting the performance of a turbine blade. In these circumstances empirical methods, although unavoidable, were of use only when they rested on an adequate theoretical basis (such as the modified Newtonian mechanics mentioned by the author). Wind tunnel researches combined with modified Newtonian mechanics had undoubtedly placed the aeroplane where it was to-day. The function of the blades of a turbine was aerodynamic, and he agreed with the author that modern turbine blading ought to be reviewed from that point of view.

Mr. Benjamin Hodkinson said that he spoke in the first place on behalf of Mr. H. L. Guy, who was unable to attend the meeting. Mr. Guy remarked that Mr. Dowson's paper focused attention on both the similarities and the differences of flow through blades, and between and around aerofoils. The progress resulting from a greater understanding of the flow around an aerofoil had been so marked that it was natural to examine whether similar methods could be applied to the flow between turbine blades. While many had been investigating the question, Mr. Guy believed that it was correct to state that up to the present very little assistance had yet been obtained in this way in the design of turbine blades.

The results in Mr. Dowson's paper brought to light one of the characteristic differences between the flow through turbine blades and between aerofoils. The comparison was summarized in Fig. 29, in which the results were expressed in terms of the optimum thrust for each type of blade or aerofoil. On the right of the diagram was shown the variation in lift for two aerofoils in biplane formation plotted

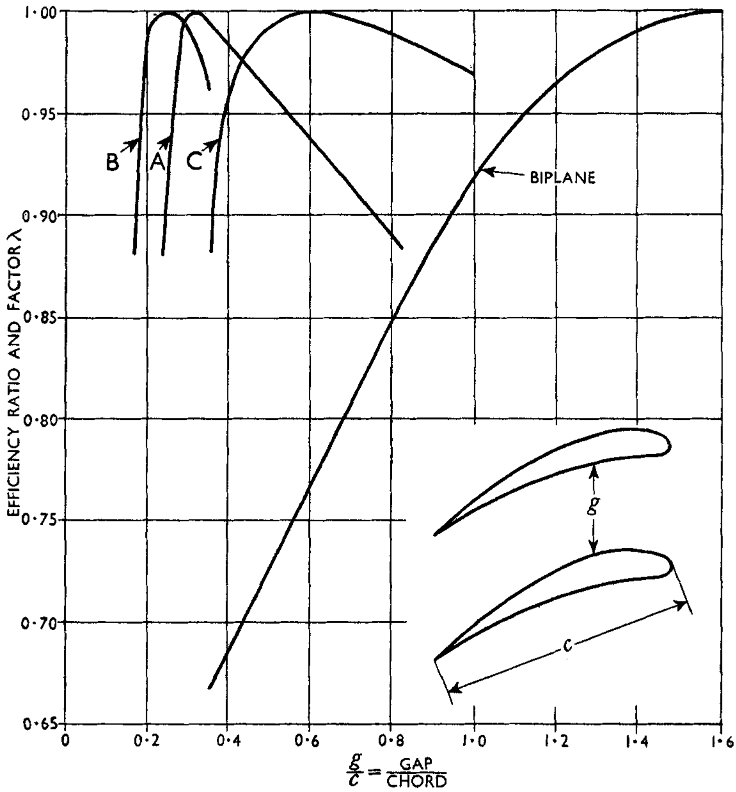


Fig. 29. Comparison of Blading Efficiency and "Biplane Effect" Coefficient λ

against gap/chord. Curve A represented Mr. Dowson's results, and curves B and C had been added by Mr. Guy from the experimental results he had obtained to show how the position of the curve could be varied by the character of the passages themselves.

It needed emphasizing that in the limit there was bound to be a fundamental difference between the flow between turbine blade passages, and between aerofoils or around a single aerofoil. Turbine

## Table 15. Efficiency and Gap-Chord Ratio

Seven stages; mean diameter $d$, $6\frac{7}{8}$ inches; heights $h$ of both moving and guide blades, $\frac{7}{8}$ inch; pitch of guide blades, 0·206 inch at mean diameter; opening of guide blades, 0·0787 inch at mean diameter; ratio $o/p=0{\cdot}382$; tip clearance of guide blades, 0·012 inch; overall adiabatic heat drop, 5·05 B.Th.U.

| | | | | |
|---|---|---|---|---|
| 1. Runner blading reference | 17 | 17B | 17C | 17C[1] |
| 2. Runner blading pitch $p$ at mean diameter, inches | 0·222 | 0·267 | 0·218 | 0·441 |
| 3. Runner blading outlet opening $o$, inches | 0·0917 | 0·1357 | 0·092 | 0·246 |
| 4. Runner blading ratio $o/p=k$ | 0·398 | 0·508 | 0·422 | 0·558 |
| 5. Runner blading tip clearance, inches | 0·0207 | 0·0123 | 0·0125 | 0·0125 |
| 6. Guide heat drop $=\frac{5{\cdot}05}{14}\times\frac{o/p \text{ for runners}}{o/p \text{ for guides}}$, B.Th.U. per lb. | 0·376 | 0·478 | 0·398 | 0·527 |
| 7. Hence guide outlet velocity $=224\sqrt{\Delta h_a}=V_1$, ft. per sec. | 137 | 155 | 141 | 162·5 |
| 8. Speed, r.p.m. | 3,315 | 3,620 | 3,360 | 3,920 |
| 9. Quantity corrected to zero tip clearance, lb. per hour | 874 | 942 | 881 | 1,042 |
| 10. Guide outlet velocity $=\frac{Qu\times 144}{3{,}600\pi hkd}=V$, ft. per sec. | 136 | 146·5 | 137 | 162 |
| 11. Blade speed $=U=\frac{\pi\times\text{r.p.m.}\times 6\frac{7}{8}}{60\times 12}$, ft. per. sec. | 99·5 | 109 | 101 | 117·5 |
| 12. Velocity ratio $=2U/(V+V_1)$ | 0·722 | 0·723 | 0·728 | 0·728 |
| 13. Torque expressed as weight at $3\frac{7}{16}$ inches radius (corrected for mechanical loss), lb. | 7·90 | 7·58 | 8·04 | 7·25 |
| 14. Efficiency $=\frac{W'U}{550}\times\frac{2{,}545}{Q\times 5{\cdot}05}$, per cent | 82·5 | 80·6 | 84·6 | 75·0 |
| 15. Force per blade $=(W'\times p)/(\pi\times 6\frac{7}{8}\times 7)$, lb. | 0·01162 | 0·01338 | 0·0116 | 0·0209 |
| 16. Gap=G, at mean diameter, inches | 0·145 | 0·188 | 0·140 | 0·362 |
| 17. Gap/chord=G/C | 0·334 | 0·433 | 0·323 | 0·835 |

blade passages usually involved curvatures which in a single aerofoil would give rise to stalling. The pitch of the turbine blades or nozzles was, however, such that the stream was circumscribed by the neighbouring blade boundary, and the breakaway associated with stalling was thus avoided. In the case of biplane aerofoils, if the gap was progressively increased until it became extremely large, each aerofoil was bound to act as a single aerofoil, and the flow would be unaffected by the presence of the second aerofoil at a considerable distance.

Mr. Hodkinson, continuing on his own behalf, said that curves such as B and C could be added to cover quite a large range. Those particular curves had been obtained by means of a blade and nozzle tester which took static observations, something like an aerodynamic balance, and would correspond most naturally to the biplane curve in the paper. It would have been easy to select curves of efficiencies direct from turbine tests. He had calculated a few such curves, which fell rather near to curve A, but it would be possible to produce others to cover a considerable range. Suffice it to say that Mr. Dowson's curve A fell amongst a large assortment of results obtained from different designs of blades by at least three different methods, which afforded a degree of corroboration.

Difficulties must be expected chiefly because, as had been observed, turbine blades were usually thought of as co-operating instead of interfering with their neighbours. Mr. Dowson was aware of this, but implied in his paper that turbine blades might interfere with each other. He mentioned that turbine blades had a big camber and a high Reynolds number. There were also effects due to compressibility, though the merit of Mr. Dowson's tests was that these were to some extent avoided by the use of a small heat drop. There was also the factor of aspect ratio or span, which covered a wide range in turbines but not in aeroplanes. Finally, there was the consideration that turbine blades might be regarded as part of an infinite cascade.

Mr. Dowson's curves gave the maximum efficiency at a pitch of 0·2 or less, and a gap-chord ratio of about 0·25, whereas Fig. 29 showed it at a gap-chord ratio of about 0·33. This was because Mr. Dowson's figures gave the pitch and gap at the rotor diameter, whereas the mean diameter of the turbine was the basis in Mr. Guy's Table 15. Curve A was obtained from line 14 of Table 15 by making the maximum unity instead of about 84 per cent. He asked how Mr. Dowson calculated the mechanical losses, and how the clearance corrections were established.

The work of Harris and Fairthorne* might be mentioned. They had experimented with cascades of aerofoils, and plotted the loss per degree

* Aeronautical Research Committee, Reports and Memoranda, No. 1206, 1928–9, vol. 33, p. 286.

of deflexion of the stream against the gap-chord ratio. This quantity was a criterion of efficiency. The best value, that is the least loss for a given deflexion, occurred at a gap-chord ratio of 1·5, which corresponded very closely with Mr. Dowson's Fig. 28.

With regard to the paper by Mr. Binnie and Dr. Woods, Mr. Guy expressed his pleasure that there were independent workers in this country interested in pursuing this type of investigation, which had unfortunately been very much neglected by such workers since the time of the Steam Nozzles Research Committee of the Institution. Mr. Guy hoped that a general discussion on the significance of supersaturation in turbine design and performance might ensue. He was bound to state that though they had been vigilant in the matter since its significance was first stated by Mr. Martin, they had found little evidence that the presence of undercooling in a multistage turbine had any material influence on its design or performance. It was evident that in divergent nozzles undercooling of the character reported by the authors occurred. It was, however, questionable whether the influence of supersaturation was not limited, because stabilization took place very rapidly in a multistage turbine, in which the individual pressure drops in each stage were either less than the critical or only a little above. There were many evidences which suggested that stabilization took place. It was, of course, probable that such undercooling affected efficiency, but in normal design its influence must be covered by the correction factors which were introduced, and which had themselves been deduced from tests.

The authors desuperheated their steam by spraying. In view of the statement by Professor Callendar that moisture globules could exist in superheated steam, it seemed likely that they would occur in this case. A suitable method of avoiding the presence of water would be to use steam, not a fraction of one second "old", but which had been cooled through a long pipe line and was several seconds old. Fortunately, other workers had found that phenomena of the kind under investigation were unaffected by the presence of moisture globules.

Taylor's theory referred particularly to laminar type of flow, or at least to a state of flow in which the velocity at any point was constant with time; if a varying pattern of flow were taken into account, the equation of continuity (p. 263) must include an extra term, a time-derivative of the density. This might not matter seriously, however, because the paths of individual particles in a turbulent stream were not so much curved as might be thought, considering how violent turbulent motion was. The microscope showed the individual paths to be seldom inclined at more than half a dozen degrees from the mean line.*

* Proc. Roy. Soc., series A, 1932, vol. 135, p. 656.

One point which struck him was that the ratio of pressure at which the condensation began, to the pressure at which the fluid crossed the saturation line, was of the order of 0·5 in the present experiments. Previous workers, including Yellott and Holland, had found a mean value of about 0·45, but ranging from 0·40 to 0·47 perhaps, and increasing with increase of pressure. Such a method of presenting the results, instead of using an entropy chart, might be easier to remember and more rapidly applied. Professor Callendar had stated that the Wilson line coincided with the 3 per cent wetness line. That was convenient to remember, though only a rough approximation, of course. Yellott and Holland indicated that time was a deciding factor, because they could obtain earlier condensation by using a divergence of 2 deg. instead of the 4 deg. used by the present authors.

The last part of the paper raised the important question of maximum discharge in a divergent nozzle with small pressure drop. He himself had made such a nozzle, intended to give a constant discharge coefficient over a wide range of pressures. It was well known that it could be done, but he had not gone far with the tests. As early as 1914, however, the phenomenon was referred to by Fisher in the Proceedings of the Institution.* A slight change could be made in a turbine nozzle without affecting the efficiency very much but at the same time giving quite a considerable change of discharge coefficient. It was difficult to predict discharge coefficients. Moreover, the cross-sectional area was sometimes difficult to measure, and even then it was not always a certainty that this was just at the neck or the narrowest part of the flow. It might be a good thing if the authors could give more information relating to the discharge coefficients. They had given everything else, so that a reader could form his own conclusions from the experiments; that was a great merit of the paper. He had found the paper very easy to follow because of the completeness of the presentation.

Measurements of velocity would also be useful, but were very difficult. A hot-wire anemometer had been suggested, and one naturally thought of impact tubes, but they would be affected by the pressure not being constant. An impact tube might give a reading in a region of high pressure as if it were a region of high velocity. One naturally wondered whether it was not possible to devise some mechanical means of measuring velocity, such as a spring which was distorted by the stream. He had had some little success in that direction. Measurements of temperature in the authors' work would also be very desirable, but any thermometer put in a supersaturated stream was affected by condensation upon it. Yet it was said that turbine exhausts sometimes

* PROCEEDINGS, 1914, p. 927.

showed temperatures below saturation. That raised the question as to whether steam was transparent to radiation. Opacity seemed desirable here. He had not been able to find data on that point. Simple gases were fairly transparent to radiation, and complicated gases were not; steam might behave as a complicated gas, though of simple chemical composition, because water vapour was said to be opaque to radiation. It was perhaps opaque only to low-temperature radiation.

Mr. B. POCHOBRADSKY remarked that it was quite true that some of Mr. Binnie's and Dr. Woods's tests merely confirmed results obtained by previous investigators, such as Professor Stodola (whose tests had been published in the German edition of his textbook as far back as 1922), or Dr. Rettaliata and others, but the confirmation had particular value because of the greater precision with which the results were obtained.

The second group of tests referred to undercooling and subsequent sudden condensation. Here the tests gave more precise measurements both of the pressure rise and the total heat than any he had so far seen, and the wonderful test on distribution of pressure and velocity in the throat of the nozzle was of great importance, and threw light on the discharge coefficient, to which reference had already been made. If the velocity was distributed in the way that the investigators had found, it was easy to imagine that the discharge coefficient must be affected.

He would like to know whether the steam used for the tests was quite clean—i.e. derived from distilled water—and, if so, whether condensation would start earlier if the steam contained some impurities, say in the form of fine dust or even water droplets. He thought that that would clear up some doubts which had been felt as to the point where condensation started.

The second paper, by Mr. Dowson, was of more direct interest to the turbine designer in its application to the pitch of turbine blades, rather than in its indirect proof of "biplane effect" on the lift of aerofoils. By that he did not mean to minimize the chief object of Mr. Dowson's investigation; on the contrary, Mr. Dowson showed broader vision than a mere turbine designer. Mr. Dowson had used four different rotors, and apparently the number of blades was different in each. According to Table 10, p. 270, two rotors had the same blade section, another had a different section, and for the fourth the blade section was not indicated. He was interested in rotor 17C, which had the maximum number of blades, despite which the mean opening of the blades was, according to the table, larger than the mean opening when a smaller number of blades was used as in rotor No. 17. It might be that the efflux angle of the blade was altered at the same time.

Fig. 22, p. 274, was of great interest to the turbine designer, and indicated that the highest blading efficiency had been obtained with the largest number of runner blades. Whether that would continue to be the case was very doubtful; indeed, Fig. 24, p. 279, suggested that Mr. Dowson would anticipate a gradual reduction in efficiency when the number of blades was further increased.

Mr. Dowson's conclusion that the driving force on the runner blades was affected by circumferential pitch, in the sense in which the term was used in the paper, was confirmed by tests which had been carried out at the works with which he was associated, though those tests differed in layout rather widely from the author's. A wind tunnel was used, in which air was forced through the turbine blades by means of a fan, the blade shapes being varied within very wide limits. Some blades were what most engineers would call the impulse type, whilst others they themselves called "aerofoil" blades, though perhaps they did not quite constitute an aerofoil. The blades were, of course, stationary and the air flow was investigated.

Two results were found which he would like to mention in connexion with the paper under discussion. One was the loss of energy in the blades, which was always a minimum at a certain pitch. The loss, however, varied according to the shape of the blade, and in the case of the two extreme shapes he had mentioned, differed by almost 1 per cent of the total energy. Moreover the "aerofoil" blade, which gave the smaller loss, also showed its minimum loss at a smaller pitch. It was of great interest, however, that the loss per square inch of blade surface was actually the same.

The second factor which they had established was the efflux angle, which also had a great influence on the efficiency of the turbine. The inlet angle was in every case the same, but with the "impulse" blade the efflux angle gradually increased, the increase being somewhat more pronounced the greater the pitch. The tests were not carried to zero pitch; they took rather long, but it appeared that the efflux gradually approached a minimum with reduced pitch. The "aerofoil" blade had a much larger efflux angle in any case, and its increase with increased pitch was much more pronounced. Even if the pitch was fixed at the minimum energy loss, the efflux angle was slightly larger, and it was not possible to get the highest driving force on the blade with that pitch; the effect of the efflux angle was in fact so pronounced in the case of the aerofoil blade that the pitch would have to be substantially reduced in order to obtain the highest thrust on the blade.

He thought those results very largely confirmed Mr. Dowson's tests, and he might add that they had been carried out at velocities which very nearly corresponded with those of Mr. Dowson's tests.

Mr. R. LIVINGSTONE said that there was some little difficulty in interpreting the results of the tests which Mr. Dowson had carried out because of their small scale. In actual turbines the ratio of the blade length to diameter was very much greater than Mr. Dowson had chosen for his small-scale experiments. One should therefore be cautious in applying the results to full-scale machines. At one time he used to consider the ratio of the pitch to the width of blade as one of the most important criteria in turbine design from the point of view of efficiency. It was difficult, however, in the later expansion stages of a turbine to adopt the pitch desired. With the increasing outputs demanded to-day, the shape of the blade was determined as much on mechanical grounds as from considerations of the flow of steam, and it was necessary to employ very thick sections at the root of the blade. The passage for the steam was therefore very different at the root of the blade and at the top. Under such conditions, the refinements of aerofoil design were hardly practicable. The difficulty did not apply to the same extent in the middle portion of the turbine, where the pitch–blade width ratio was not so great, and it was certainly possible that in the case of those blades, aerofoil design might give a small improvement.

In the case of the long blades, assuming 60 inches mean diameter and blades about 15 inches long with 1 inch mean pitch, where the pitch was only $\frac{3}{4}$ inch at the root and $1\frac{1}{4}$ inches at the top, it could be seen that quite apart from the necessity of having very much more metal at the root than at the tip, there was inherently a very poor shape of passage for the steam. Nevertheless, Mr. Dowson's research was useful in isolating certain factors which varied the blade efficiency, and probably fuller knowledge of the effects of thrust and hydrodynamic design would lead to improvements.

The paper by Mr. Binnie and Dr. Woods was of great interest. He had been struck by the coefficients taken for the increase of friction as the steam expanded through the nozzle. From points 7 to 17 a friction coefficient of 2 per cent was sufficient, but from point 17 onwards a coefficient as high as 25 per cent was necessary. He asked whether the authors could give some idea of the total friction over the whole range of heat drop. Fig. 5, p. 239, showed a standing wave said to be due to the absence of a transition curve between the throat of the nozzle and its expanding portion. He wondered whether other tests had confirmed that the absence of the transition curve did form the standing wave, or whether the authors could give some explanation of why its absence should give rise to a standing wave.

The explanation of compression after supersaturated expansion to the Wilson line was almost a revelation to him; it was the first time he had seen so clear an exposition of what actually happened when steam was expanded beyond a certain point. He would like to know what were the corresponding conditions for expansion from a point which was itself below the Wilson line. He was not quite sure of his authority, but he believed that it had been stated that in expansion in a nozzle even under the Wilson line, with a certain percentage of moisture in the steam, the moisture content was not changed during the course of the expansion. The steam was presumably restored to a condition of thermal equilibrium after passing through the following blade in a multistage turbine, but what would be the effect on the subsequent stages if equilibrium was not quite restored ? Did its state descend again to the Wilson line, and than perhaps to the wet line, or even beyond that ?

He noticed that the authors, in their estimate based on the conservation of energy showed that if compression occurred from A to B, condensation of the gases to moisture did not affect the relative velocity of the moisture particles. He wondered if that could be true. It would seem that the formation of moisture would itself cause a reduction of volume and a consequent reduction of velocity. At any rate in actual turbine practice the water particles appeared to move at a lower velocity than the remainder of the stream, as evidenced by the cutting of the blades on the back instead of on the inlet edge.

Mr. R. H. Parsons said that the analogy which Mr. Dowson drew between turbine blades and the wings of an aeroplane reminded him that very many years ago an inventor named Mr. Phillips had constructed a flying machine with a lifting surface made up very much like a venetian blind, in which all the slats were more or less of aerofoil design—in fact, almost exactly like turbine blades. The practical issue raised by Mr. Dowson's paper seemed to be whether the efficiency of a reaction turbine could be improved by increasing the pitch of the blades. He thought that the answer was given by Mr. Dowson's own experiments; Fig. 22, p. 274, showed that the efficiency ratio of his experimental turbine fell off with every increase of blade pitch beyond the standard adopted by his firm. Mr. Dowson kept the pitch of the guide blades constant and only varied that of the running blades. Since the action of the fixed and running blades in a reaction turbine was identical, there would seem to be no logical reason for increasing the pitch of one without the other. In fact, in the case of a Ljungström turbine, it would not be possible to know which set of blades to deal with, if the pitch of only one were to be increased. Presumably, however, if Mr. Dowson had increased the pitch of both, he would have obtained a still more accentuated falling off of efficiency. He remembered quite well the late Mr. H. M. Martin, whose opinion on turbine design was respected by everyone, telling him that he considered that an increase of pitch of turbine blades would be an improvement. That view was probably based on the aerofoil theory, but the curves in Mr. Dowson's paper showed that Mr. Martin was probably wrong.

The lower curves in Fig. 22 giving the total quantity of steam passed showed that for the most widely spaced blades the quantity was constant for all turbine speeds from zero to nearly 4,000 r.p.m., which seemed to imply that in this case the angle at which the steam entered the blading was immaterial, and in fact that the blading might not have been there at all so far as any interference with the steam flow was concerned. The other blades were both shown to pass more steam at zero speed than 17C1, with the widest pitch. That was an unexpected result, and he asked if the author could give a reason for it. The curves appeared to have been extrapolated back to the zero point, which might have something to do with it.

Referring to the paper by Mr. Binnie and Dr. Woods, he remembered that Mr. Martin, at the time of his death, was making experiments on the point of condensation of expanding steam. He was working, not with a nozzle, but with a cylinder whose piston could be pulled out at a given moment with very great rapidity. Connected to the cylinder at the inner or closed end was an optical pressure indicator. Mr. Martin's idea was that a falling weight should pull out the piston with a known velocity, and an indicator diagram would be obtained showing a descending pressure line up to the point at which condensation took place, when it would jump up again and then continue to descend. That method of approach to the problem of finding exactly where the point of condensation occurred differed from any other which he had seen suggested.

Mr. I. V. Robinson said that Mr. Dowson had provided a striking instance of the correlation of two separate and distinct branches of engineering, aerodynamics and steam turbine engineering. It was said of the late Lord Kelvin many years ago that it was his great desire to make such researches and discoveries as to correlate all the natural sciences. Lord Kelvin did not succeed in his lifetime, but physicists had since then gone a long way towards that ideal. Engineers could hardly follow physicists so far, but he believed that they also had progressed towards Lord Kelvin's objective, and Mr. Dowson's paper was a good example of such progress.

Mr. Gibb had pointed out that turbine blading efficiency had now attained over 90 per cent, and hence to effect a further improvement of even 1 per cent would mean a reduction of one-tenth of the losses. He would like to emphasize that the end which was being striven for was not to obtain higher efficiency, but a cheaper unit of electricity. It was a good thing to know how to improve the blading efficiency from 90 to 91 per cent, but it was not necessarily worth while to make use of the knowledge on every occasion.

He had been very much struck by Mr. Hodkinson's remark about "old steam". He had defined it as steam more than one second old. It was a brief life and a giddy life which steam had in a turbine. The importance of the point was apparent in connexion with the work of the International Steam Tables Conference. Professor Callendar had been the first to use a flow method of investigation of the properties of steam, with a small apparatus of his own design, and initially of his own construction, through which the steam was passed at a constant rate. The other investigators used a static process, which might be described roughly as a bomb full of steam to which they added or from which they subtracted heat; from the ensuing variations in the pressure and temperature they were able to deduce their results. Professor Callendar's results differed appreciably from those obtained in America, Germany, and Czechoslovakia, and he recalled a discussion at one of the conferences as to whether there was any difference between the properties of static steam and flowing steam. The other three investigators had all subsequently tried a flowing calorimeter, and there was now every indication that the results of the conference—whose work was not finished yet, though it had started in 1922—would constitute a table of the properties of steam correct to within less than one part in 5,000, with the various investigators in mutual agreement.

Mr. L. J. Cheshire expressed particular interest in Fig. 18, p. 260, of the paper by Mr. Binnie and Dr. Woods, which gave the rise in pressure in the divergent nozzle when pressures above the critical were obtained at the outlet of the nozzle. He had thought it worthy of interest to calculate the theoretical conditions which could occur in a nozzle of the precise dimensions which the authors had used. The full lines in Fig. 30 showed the two alternative conditions after the throat which would satisfy the requirements of continuity, i.e. which would give steady flow and would fill the nozzle. There were two lines and two only which would satisfy them, and the other lines obtained by the authors would not fit in for the whole length of the nozzle. Stodola's work had shown how lines of this kind could be calculated on such a diagram, but the point he wished to bring out was the relation between the actual results which the authors obtained and those calculated from the conventional theory. The dotted lines represented the author's results. Taking the case of the recompression line which started from approximately critical conditions, it would be found that their curve reached a value of about 0·82, whereas the theoretical curve reached a value of about 0·9. That indicated a compression efficiency of between 75 and 80 per cent, which in itself was of interest as showing that tolerably high efficiencies of compression could be obtained in a diffuser of that type.

The upper curve showed that to obtain streamline flow there must

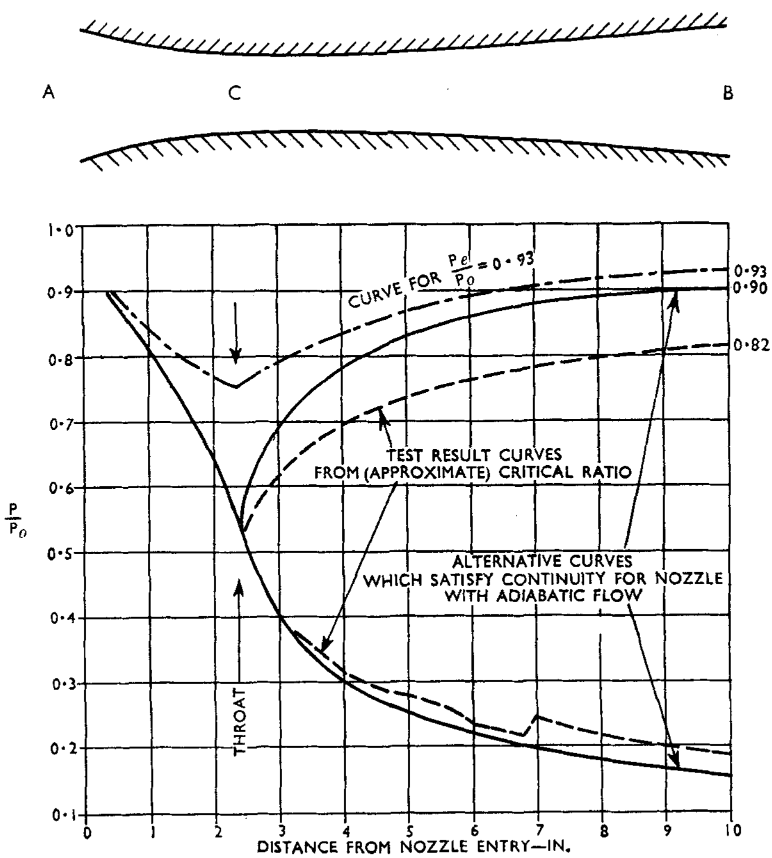


Fig. 30. Calculated Pressure Distribution for Nozzle as in Fig. 1, p. 230

inevitably be a lower pressure at the throat than at the outlet, and that it was necessary to pass a greater quantity of steam than would be given by calculating from the throat area and final condition only. Such conditions did occur in steam turbines and constituted a link between the two papers which had been presented that evening. A nozzle as sketched at the top of Fig. 30 could be regarded as the top and bottom respectively of an aerofoil, and the streamlines would follow approximately the same shape as the nozzle. If an aeronautical engineer were asked what the flow through such a nozzle ought to be, with an expansion ratio of 0·93, he would not dream of considering the throat area at all. There, he thought, lay one of the important differences between the aeronautical and the thermodynamic approach to nozzle flow, and turbine design in general. Illustrating the point further by means of Fig. 30, at A there was a pressure of 20 units, and at B a pressure of 18 units. The turbine designer would probably consider that the area at C controlled the flow through the nozzle, whereas the aeronautical enginer would say that the conditions at B with streamline flow would be uniform across the section, and hence these would determine the mass flow, provided that at C the area was not so small as to cause the velocity of sound to be reached.

He was sure that phenomena of this type occurred in steam turbines, and frequently what was considered to be a converging nozzle was really, if the outlet area was regarded correctly in relation to the point of minimum cross-section, a divergent nozzle. Moreover, he sometimes found in experimental work that the flow through a turbine with nominally converging nozzles was greater than would be calculated by conventional thermodynamic theory, even with highly superheated steam.

It was perfectly true, as Mr. Gibb had remarked, that it was possible to obtain a blading efficiency of more than 90 per cent. Assuming an efficiency of 92 per cent, what accounted for the 8 per cent loss ? He thought the skin friction could never approach a value of 8 per cent, and personally he did not see why it was necessary to adopt a pessimistic attitude and say that there was little hope of doing any better. The aeronautical approach could be of help in calculating the skin friction in the blades of a turbine; it would certainly be found difficult to account for anything approaching 8 per cent, when the other losses—windage, leakage, and so on—had been accounted for.

Mr. A. M. BINNIE, in reply, dealt first with the measurement of velocity in a nozzle. He remarked that it was undesirable to insert a Pitot tube into so small a nozzle as the one which they had used, since the inevitable disturbance to the flow might have been serious. It was interesting to recall that an independent method existed of determining the velocity in the divergent portion of the nozzle. If a small irregularity or nick was made in the divergent wall, a shock wave would be formed. Using a nozzle with glass sides Prandtl * had photographed

* See DURAND, "Aerodynamic Theory", 1935 (Springer, Berlin), vol. 3, Fig. 7, Plate 6.

waves of this type. The measurement of the angle which they made with the axis of the nozzle enabled the velocity of the stream to be easily calculated in terms of the local velocity of sound.

Mr. Pochobradsky asked for information about the boiler water. In their own apparatus they used raw water straight from the city main. An attempt to perform experiments of the kind in question with absolutely clean steam, free from solid impurities, would indeed be difficult. A boiler and piping of steel would be out of the question, owing to the danger of minute particles of rust being carried along with the steam; the whole apparatus would have to be constructed of glass.

Another point which had been raised concerned the friction loss. Perhaps they ought to have made it clearer that what they called the friction loss was not entirely due to friction. They were faced with the problem of correlating their observations with the predictions of a defective theory. The simple theory, as they had shown, was slightly in error because it assumed uniformity of conditions over a cross-section. Hence the differences between the simple theory and their observations were due partly to friction and partly to the defects of the theory. As Mr. Hodkinson remarked, Taylor's theory was applicable only to streamline flow. No way was possible of analysing turbulent flow by the velocity-potential method.

The absence of a transition curve was suggested as the cause of the undulation in the pressure curve, and if they had been starting afresh they would have inserted one. There was no abrupt change in the direction of the walls at the points where the circular arcs ran into the straights, but there was a discontinuity in the radius of curvature. This radius increased suddenly from a constant value to infinity, and presumably that might well have some effect on the streamlines.

With regard to desuperheating and the age of the steam, they had little space at their disposal and so made no attempt to use "old" steam for their experiments. The desuperheater consisted of a $\frac{1}{4}$-inch pipe inserted into a tee in the $1\frac{1}{2}$-inch steam main. The $\frac{1}{4}$-inch pipe projected into the centre of the stream and terminated in an elbow, blocked by a plug. In the plug, which faced upstream, an $\frac{1}{8}$-inch hole was drilled. The steam in this region was at a pressure of about 40 lb. per sq. in. gauge. At frequent intervals it was found necessary to clean out the $\frac{1}{4}$-inch pipe, and it therefore appeared that the water was mostly evaporated before it mixed with the main steam supply. The word "spray" used in the paper was perhaps somewhat misleading. After passing the desuperheater the steam, before reaching the 4-inch tees from which the nozzle drew its supply, had to negotiate a sharp elbow, a valve (where its pressure was halved), and a bend. Thus a proper mixture was ensured, but to work with steam which had had a life of 3 seconds was not possible.

Mr. Livingstone had raised some questions to which unfortunately their experiments did not give a reply. In their tests they observed only one discontinuity in the pressure distribution, but they were using steam which was initially superheated and the expansion was not carried very far. The back pressure was comparatively high and the nozzle somewhat short. Had they used steam initially wet and expanded it to a lower pressure, the results might have been different, but he could not express an opinion on that point. With regard to the velocity of the water drops at the peak B (p. 253), they were compelled to assume in their calculations that this velocity was the same as that of the remainder of the fluid.

He would reply in writing to other points raised in the discussion, and his reply will be found on p. 305.

Mr. ROBERT DOWSON, in reply, referred to Mr. Gibb's mention of an efficiency of 90 or 92 per cent. He himself was concerned not so much with answering the question whether the efficiency could be improved in practice, as with explaining why it was comparatively so low. He agreed with Mr. Cheshire that 92 per cent was so far short of 100 per cent that it required some explanation. If the methods of the aerodynamic engineer were used to work out the surface drag on turbine blades, it was not possible to assess the loss at more than about 2 per cent, so that there remained at least 6 per cent to account for; and it seemed to him that a good deal of that amount must be due to laminar flow breaking up into turbulent flow, probably at the blade tails. In other words, he thought that in many turbine blades the curvature was so great that separation occurred at the blade tails, no matter how hard one tried to avoid it. He put that forward as an opinion, without having conclusive experimental evidence to support it.

Mr. Hodkinson asked about the method of determining the losses in the small turbine. As stated in the paper, a blank rotor was used which was revolved in steam at atmospheric pressure. It was driven by an electric motor and the losses were determined in the usual way. In the retardation test the usual procedure was followed. The rotor had no blades; its moment of inertia was determined by calculation and by experimental methods. It was run up to speed by a slip coupling and then uncoupled and allowed to run down in the usual way, first by itself and then coupled to a dynamometer of known moment of inertia and with a small weight in the scale pan. Thus retardation curves were obtained and the losses were deduced from them. The two methods which were used agreed fairly well.

In reply to Mr. Pochobradsky, exactly the same blade profile was used throughout, and no intentional change was made to the chord angle at all. He thought he was right in saying that the ratio of opening to pitch was greater in $17C^1$, which was the 17C set with every other blade cut out. They were simply chipped out and the rough edges filed smooth, and it was found by measurement that the ratio of opening to pitch was increased, the alteration doubling the pitch and more than doubling the opening. He agreed with Mr. Pochobradsky that on the whole, from the tests referred to and from other tests, an increased pitch in turbine blades did not appear to offer any gain, and indeed had rather the opposite effect. It was still necessary to explain, however, how Kaplan was able to use a deliberately increased pitch with markedly good effect.

In reply to Mr. Livingstone, he would like to say that it was not necessarily his intention to substitute aerofoil blades for ordinary blades. What he advocated was substituting the aerofoil theory of Newtonian mechanics in place of the conventional mechanics, and applying it to turbine blading, because it was possible to explain matters by means of the aerofoil theory which could not be explained by any other theory. For instance, if all the blades except two were removed from a turbine wheel, leaving one at each end of a diameter, that turbine would rotate and would do work, although obviously most of the steam would leak through without effect. If it were asked how much steam was acting on those two blades, the old Newtonian theory of "discrete particles" could not give a reply, but the aerofoil theory would do so.

It seemed to him that the aeroplane itself was as much a momentum machine as a turbine. The efficiency of a turbine was defined as the work done by the continuous change of momentum of the steam divided by the theoretical power required to drive the turbine or applied to the turbine. In an aeroplane the sustentation was due to continuous change of momentum, and one could define the efficiency of an aeroplane as the ratio of the momentum actually produced by the curvature of the aerofoils divided by the engine power required.

He thought that the remarks of Mr. Parsons were covered by the replies he had made to Mr. Pochobradsky; he did not see how one could generalize and say that increasing the pitch in turbine blades must necessarily be bad, in the face of what had been done by Kaplan.

He would reply to the other points in writing, and his reply will be found on p. 308.

*Communications*

Dr. F. Engel wrote that he was particularly interested in the remarks regarding the influence of increasing back pressure, in the paper by Mr. Binnie and Dr. Woods. It was certainly surprising that this subject had not yet been dealt with adequately in textbooks, especially in view of the fact that as early as 1839 St. Venant and Wantzel* published a comprehensive paper on the flow of a compressible medium through orifices and nozzles, extending their investigations over a wide range of back pressures.

On the other hand, various workers, apparently without knowledge of the paper by St. Venant and Wantzel, came to contradictory results. G. A. Hirn,† for instance, obtained a constant flow through an orifice for a back-pressure ratio as low as 0·2. T. E. Stanton ‡ found that the back-pressure ratio for which, with further increase in its value, the condition on the upstream side was influenced, depended on the geometrical form of the discharging device. For a thin-plate orifice he established a value similar to that found by Hirn. This led to the conclusion that the limiting value of the back-pressure ratio, for which the condition on the high-pressure side was just not influenced, depended on the geometrical form of the orifice and might have a value between, say, 0·2 and 0·9. The low value of 0·2 was closely related to the contraction of the jet flowing from a thin orifice. Unfortunately the contraction could not yet be determined on mere mathematical lines, as all attempts to relate the expression $\pi/(\pi+2)$, due to von Helmholtz, to a circular opening seemed to have failed. Reynolds gave only very general outlines without presenting a final solution.

Reynolds § himself, in a paper to which the authors presumably referred, somewhat confused the issue by comparing turbulent flow of water through a convergent nozzle with supersonic flow in a convergent-divergent nozzle. Whilst his final result was correct, it was rather doubtful whether this comparison was permissible, as in both cases neither the geometrical form nor the state of flow were similar. From a physical point of view, rapid flow in the diverging outlet of a side-contracted open channel might be compared with the flow, at the velocity of sound, of a compressible medium through the outlet of a convergent-divergent nozzle, a fact which some years ago was brought to his attention by Professor D. P. Riabouchinsky.

* *Journal de l'École Royale Polytechnique*, 1839, vol. 26, p. 85.
† *Annales de Chimie et de Physique*, 6th series, 1886, vol. 7, p. 289.
‡ Proc. Roy. Soc., series A, 1926, vol. 111, p. 306.
§ Reynolds, Osborne. Papers on Mechanical and Physical Subjects, 1901 (Cambridge), vol. 2, p. 311.

As the authors themselves had conducted flow tests through nozzles and constricted open channels, he would like to have their opinion on the limitations and possibilities of applying the results of water tests on models to prototypes which had to work in air, or vice versa. These problems might be thought rather academical, but there appeared to be a certain interest in this question, as indicated in a paper recently presented to the Institution by Mr. A. Fage.* In conclusion, he asked the authors if the approach length before the nozzle should be longer and also of the same section as the nozzle entrance, to ensure a normal velocity profile.

Dr. H. J. Gough, M.B.E. (*Member of Council*) wrote, in connexion with the paper by Mr. Binnie and Dr. Woods, that the use of a row of holes in the nozzle wall for the measurement of static pressures, as mentioned on p. 232, had the disadvantage that the presence of the upstream holes might produce the effect of a rough wall and so affect the readings at the holes further downstream. At the National Physical Laboratory, suitably designed search tubes, inserted from the upstream end of the nozzle, had been found satisfactory, and had the advantage that the whole cross-section could be explored; this would have been useful in checking Taylor's theory for the flow through the throat (referred to on p. 241).

Turning to the reference, on p. 240, to the loss due to friction in the nozzle, National Physical Laboratory experiments with air suggested that the figure of 20 per cent taken for the loss of kinetic energy due to friction was rather too high. An example given by Stanton † showed that at a pressure ratio of about 0·09 (greater than that in the present tests) the loss of kinetic energy was roughly 10 per cent. This suggested that the discrepancy between Reynolds's theory and the experimental results obtained by the authors could not be completely explained by the effect of friction.

On the very interesting subject of standing waves, it might be useful to mention that National Physical Laboratory experience went to show that small standing waves like those in Figs. 5, 11, and 12 might be caused by almost microscopic irregularities in the nozzle walls, and were not necessarily due to a fault in the general profile.

With regard to the variation of discharge with back pressure (pp. 260 and 261), was it not quite well known, and contrary to the author's statement, that the pressure in part of the divergent portion of a nozzle might fall well below the back pressure ? The principle was used in

* Proceedings, 1935, vol. 130, p. 3.
† Proc. Roy. Soc., series A, 1931, vol. 131, p. 125.

wind tunnels giving speeds greater than that of sound, as, for example, in the National Physical Laboratory high-speed wind tunnels.

Using air, Stanton * showed that the maximum back pressure attainable before the discharge began to fall off, was dependent on both the length and divergence angle of the nozzle. He also quoted similar results obtained by other experimenters. This explained the wide difference between the limits of back pressure given by Stodola. Similar measurements had been made by Mellanby, using steam.

In connexion with Mr. Dowson's paper, it might be useful to recall that experiments with air at the Royal Aircraft Establishment † had indicated that for small spacings the interference effect was more serious than was shown by the curve for $\lambda$ given in Fig. 28, p. 282.

Mr. G. G. McDONALD wrote that Fig. 8 in the paper by Mr. Binnie and Dr. Woods, showing measured and calculated distributions of pressure ratio across the nozzle throat, was of the greatest interest and value. It was a pity that the authors had not measured the pressure at points nearer the boundary of the nozzle in order to complete the experimental curve. It was interesting to note that the mean height of the Taylor curve practically coincided with the dotted line showing Reynolds's assumption, while the mean height of the experimental curve was about 0·547, if its ends were extrapolated to meet the ends of the Taylor curve.

He had recently, while analysing some of the authors' figures, devised the following simple empirical and reasonably accurate expressions ‡ to replace those given in equations (1) and (5):—

$$\mathrm{R}_c = \mathrm{P}_t/\mathrm{P}_o = 1{\cdot}111 - n/(n+1) \quad . \quad . \quad . \quad . \quad (29)$$

and

$$dR_c/dn = -1/(n+1)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

respectively. These might be of interest to the authors and to others making nozzle calculations.

The entrance efficiency $\eta_t$ of the authors' nozzle might be taken as 0·98, allowing for a loss of heat drop of 2 per cent between inlet and throat. The mean index $n$ of the polytropic expansion § up to the throat might be expressed as

$$n = \{\gamma+1+\eta_t(\gamma-1)\}/\{\gamma+1-\eta_t(\gamma-1)\} \quad . \quad . \quad . \quad (31)$$

where $\gamma = 1{\cdot}3$. Equation (31) gave a value of 1·293 for $n$ for the authors'

* Proc. Roy. Soc., series A, 1926, vol. 111, p. 322.

† Aeronautical Research Committee, Reports and Memoranda No. 1206, 1928–9, vol. 33, p. 286.

‡ *The Engineer*, 1938, vol. 165, p. 389.

§ *The Engineer*, 1932, vol. 153, p. 685.

nozzle. If the value 1·293 were substituted for $n$ in equation (29) $R_c$ was found to be 0·547, which was the same as the mean height of the extrapolated experimental pressure curve.

It would seem from the above calculations that although the actual pressure might vary considerably across the nozzle throat, yet the mean pressure at the throat was substantially that obtained from the ordinary formulæ which neglected curvature effects.

Professor G. I. TAYLOR (Cambridge) wrote that he was very pleased to see that Mr. Binnie and Dr. Woods had used his calculations to explain the distribution of velocity in the throat of a steam nozzle. He asked the authors whether they had been able to make any deductions about the way in which condensation on drops took place, using the measurement which they had made of the time taken for the condensation ($2 \times 10^{-5}$ second) and the sizes of the drops when condensation started (Wilson's result).

Mr. A. M. BINNIE wrote that he had endeavoured to find a reply to Dr. G. G. Stoney's question concerning the velocity of sound in a fog. Wood * stated that the velocity of sound in air was only slightly affected by humidity. The presence of water vapour produced a slight lowering of mean density $\rho$, the value of the ratio $\gamma$ of specific heats being practically the same for dry air or air saturated with water vapour. The calculated velocity in air saturated with moisture at 10 deg. C. was from 2 to 3 ft. per sec. greater than in dry air. The same author remarked † that measurements of the velocity in water vapour by various observers gave values increasing from 401 metres per sec. at 0 deg. C. to 424 metres per sec. at 130 deg. C.

He thanked Mr. Hodkinson for drawing his attention to the fact that the ratio of the pressure $P_A$, at which condensation began, to the pressure $P_S$ at which the fluid crossed the saturation line, was about 0·5 in these experiments. To elaborate this point, he had examined five of the tests mentioned in Table 6. The results obtained were given below in Table 16.

In reply to Mr. Livingstone, he could not state with any exactness the total friction loss over the whole expansion because at the end of expansion the state of the steam was not known. It was only up to the foot A of the pressure rise that the difference between the simple theory and the observations could be estimated with any precision. In test 74, selected as an example because the initial superheat was high

* "Textbook of Sound", 1930 (Bell, London), p. 231.
† Ibid., p. 244.

TABLE 16. VALUES OF PRESSURE RATIO $P_A/P_S$

| Test No. | 50 | 46 | 61 | 52 | 58 |
|---|---|---|---|---|---|
| $P_A$, lb. per sq. in. | 10·10 | 8·39 | 6·94 | 5·93 | 4·87 |
| $P_S$, lb. per sq. in. | 18·5 | 16·2 | 13·4 | 11·1 | 9·3 |
| $P_A/P_S$ | 0·55 | 0·52 | 0·52 | 0·53 | 0·52 |

and the pressure rise far down the nozzle, the isentropic heat drop to point 25 (immediately before the pressure rise) was 97·1 B.Th.U. per lb., while the actual drop was calculated to be 91·7 B.Th.U. per lb., showing a discrepancy of about 6 per cent.

To Dr. Engel's first question he must reply that they had had no opportunity of testing Venturi flumes on a large scale. Perhaps the manufacturers of these devices could supply the information desired. In reply to his concluding question, he thought it important that the reservoir at the inlet end should be of sufficient cross-section to keep down the velocity of approach. Had they employed a reservoir of the same cross-section as the nozzle entrance cut in the flange, namely, 3½ inches by 0·38 inch, the velocity in it would have been of the order of 400 ft. per sec. Moreover, a reservoir of this shape would have been awkward to construct.

He did not think that Dr. Gough had made an altogether satisfactory comparison between the friction losses in Stanton's and their own experiments. He had examined Stanton's Fig. 4, which presumably was the one referred to, and his calculations led him to suppose that the *overall* loss of heat drop (down to the beginning of the parallel part) was somewhat greater than 10 per cent. He must make it clear that their own figure of 20 per cent applied only to a certain portion of the divergent part of the nozzle. He thanked Dr. Gough for his suggestion that the slight undulations in the pressure curves were due to small irregularities in the nozzle walls. The shock waves which resulted had been mentioned in his verbal reply (p. 298). His experience with Venturi flumes had shown him that a small nick in the diverging wall—even a grease spot on it—at the level of the water surface would cause a small standing wave analogous to a shock wave. It must, however, be remembered that flow at supersonic velocities was susceptible to disturbances of another kind, and Hooker * had shown that an undulating pressure variation (similar to that which they themselves had observed) was possible without the formation of shock waves when the diverging boundary walls were straight. Other

* Proc. Roy. Soc., series A, 1934, vol. 145, p. 52.

papers on this subject were those by Rayleigh*, Prandtl†, Taylor‡, and Hooker§.

Mr. McDonald had remarked on the absence of pressure tappings close to the curved walls. There were two reasons for this, lack of space and the fact that errors of workmanship (if present at all) would be more likely to occur at the curved walls than at any other part of the nozzle. Mr. McDonald had drawn attention to Fig. 8, in which the mean heights of the Taylor and Reynolds curves were almost the same. This coincidence extended also to the discharges calculated by the two methods. As was well known, the simpler theory predicted || that the mass discharge $Q_R$ through a throat of width $2h$ and height unity was

$$2h\left(\frac{2}{n+1}\right)^{\frac{1}{n-1}}\sqrt{\frac{2gnP_o\rho_o}{n+1}}$$

which, when $n=1{\cdot}30$, reduced to $0{\cdot}6673(2h)\sqrt{gP_o\rho_o}$. To afford a comparison with this, Taylor's method could be extended to obtain a value for Q. From a consideration of the conditions existing at the throat, this mass discharge $Q_T$ could be equated as follows :—

$$Q_T=2\int_o^h-\left(\frac{\partial\phi}{\partial x}\right)_{x=0}V^*\rho dy \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

where $V^*=\sqrt{\dfrac{2gnP_o}{(n+1)\rho_o}}$ was the critical velocity.

Now $$\frac{\rho^*}{\rho_o}=\left(\frac{2}{n+1}\right)^{\frac{1}{n-1}}$$

Hence, from equation (10), p. 263,

$$\frac{\rho}{\rho_o}=\left(\frac{2}{n+1}\right)^{\frac{1}{n-1}}\{1-\tfrac{1}{2}(n-1)(\theta^2-1)\}^{\frac{1}{n-1}}$$
$$=0{\cdot}62759(1{\cdot}15-0{\cdot}15\theta^2)^{\frac{10}{3}}$$

Thus equation (32) became

$$Q_T=0{\cdot}66726\ (2h)\ \sqrt{gP_o\rho_o}\int_o^1-\left(\frac{\partial\phi}{\partial x}\right)_{x=0}(1{\cdot}15-0{\cdot}15\theta^2)^{\frac{10}{3}}dy/h \quad . \quad (33)$$

* Proc. Lond. Mathematical Soc., 1880, vol. 11, p. 57, and *Scientific Papers*, 1899, vol. 1, p. 474.

† *Physikalische Zeitschrift*, 1904, vol. 5, p. 599; and 1907, vol. 8, p. 23.

‡ Aeronautical Research Committee, Reports and Memoranda No. 1382, 1930.

§ *Phil. Mag.*, 7th series, 1934, vol. 17, p. 651.

|| Ewing, "The Steam Engine", 1926 (Cambridge University Press), p. 225.

in which $\left(\frac{\partial\phi}{\partial x}\right)_{x=0}=a_1+c_3y^2$, and $\theta^2$ was given by equation (28). To evaluate this expression it was first necessary to expand the integrand by Maclaurin's theorem. The value of the integral was then found to be 0·99963,* and thus, from equation (33),

$$Q_T=0{\cdot}6670(2h)\sqrt{gP_o\rho_o} \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

It therefore appeared that, for the mass discharge, the two methods led to almost the same results, a conclusion identical with that reached by Binnie and Hooker (loc. cit.).

He was much obliged to Professor Taylor for his interesting suggestion, which he hoped to examine in detail at a later date.

In connexion with Mr. Dowson's paper, it might be mentioned that the Aeronautical Research Committee (in addition to Reports and Memoranda No. 1206, referred to by Mr. Hodkinson and Dr. Gough) had recently published a further paper † which bore on the same subject. It was concerned with the design of aerofoils suitable for use in cascades at right-angle bends in a wind tunnel.

Mr. R. Dowson wrote in further reply to the verbal discussion, that Mr. Hodkinson had asked how the clearance corrections were established. Experiments were carried out with a given cylinder and runner blading, starting with as small a tip clearance as possible (0·012 inch). The clearance of all the rows (both cylinder and rotor) was then increased in a series of steps, tests being made at each step, until the clearance was 0·10 inch. From these data, curves of efficiency ratio at a given velocity ratio were plotted on a base of percentage clearance area. No difficulty was found in extrapolating these curves back to zero clearance; and these, together with the curves of steam consumption, gave the required information. The experiments showed that with decreasing clearance the efficiency ratio improved at a rate greater than that at which the steam consumption declined, and the difference represented the effect of disturbance of the main flow by the leakage steam.

Mr. Pochobradsky referred to the omission (from Table 10, col. 4) of the designation of the runner packing section for set 17C. The runner blading was obtained from set 17C merely by cutting out every other blade, and so the packing sections were unaltered, but the pitch was doubled. The chord angle of the blades was not altered but the effect of removing alternate blades was to increase the opening *o* more than

* These calculations were performed by Mr. J. R. Green.

† Aeronautical Research Committee, Reports and Memoranda No. 1768, 1936.

the pitch $p$, and so the ratio $o/p$ was increased. As regards the comparison which Mr. Pochobradsky made between sets 17 and 17C, he himself had re-examined the data and found that there was an error in the stated number of blades in set 17. This was given as 679, but ought to have been 686; and this correction made the mean ratio $o/p$ for the runner blades 0·413 instead of 0·398. Notwithstanding this correction, the ratio $o/p$ still was less than 0·422 for set 17C with 693 blades, and the discrepancy was to be ascribed to the obsolete method of fixing of the blades by top-caulking the soft brass packing sections. With this method the number of blades per row and the gauging of the blade openings were subject to slight variations.

Mr. Pochobradsky's remarks on the effect of pitch on the efflux angle of impulse nozzles recalled the information given at the end of the First Report of the Steam Nozzles Research Committee of the Institution.* With a constant pitch the angle of efflux was shown to become progressively smaller than the geometrical angle as the pressure ratio was reduced, while the jets from Parsons nozzles showed an opposite tendency, i.e. they approached the geometrical angle at low pressure ratios. Thus without any change in pitch at all, considerable variations in efflux angle could take place.

Mr. Livingstone stated that in commercial turbines the ratio of blade length to rotor diameter was very much greater than that used in the small-scale experiments of the paper. The ratio actually used was $\frac{7}{8}$ inch/6 inches = about $\frac{1}{7}$, and this was above the lower limit commonly adopted in the high-pressure stages of a reaction turbine. When designing low-pressure blading, it was true that the mechanical considerations were often of more importance than the aerodynamic, but he himself had not exhaust blading particularly in mind. It was the general conclusion as applied to normal blading that was being considered.

Mr. Parsons pointed out the remarkably flat steam consumption curve shown by set 17C. This was noticed at the time of the experiments, and several check tests made, all with the same result. However, later experiments with reaction blading had shown the necessity for determining the part of the curve towards zero speed with a greater number of points, because the shape of the curve might depart considerably from its general shape at higher speeds. The actual " standstill " steam consumption was always troublesome to measure on account of the difficulty of applying a torque which would just prevent the rotor from turning. He was grateful to Mr. Parsons for pointing out an error in Fig. 21; this had, however, now been corrected.

* PROCEEDINGS, 1923, p. 1.

In his earlier remarks, Mr. Parsons stated that the practical issue raised by the paper seemed to be whether the efficiency of a reaction turbine could be improved by increasing the pitch of the blades. He himself had not had this intention; the paper came from a research department and did not pretend to deal with matters of purely practical or commercial interest. The object of the paper was perceived clearly by Mr. Robinson, whose remarks were most encouraging.

Mr. Guy had given to the paper the thoughtful study which he had expected, and his remarks, supplied by Mr. Hodkinson, were of great value.

He agreed with Mr. Hodkinson that turbine blades in a row should be considered as co-operating with one another, rather than as interfering with one another. In using the term "interference" he was looking at the matter from the viewpoint that the lift characteristic of a single aerofoil was very different from that obtained from the same aerofoil when it was situated in the midst of a cascade. This point was brought out well in the Aeronautical Research Committees' Reports and Memoranda No. 1206, which Mr. Hodkinson cited.

Mr. Gibb asked if aerodynamic experts knew what was the efficiency of their aerofoils. This question might be answered by citing the many published wind tunnel tests on aerofoils, showing the lift and drag coefficients. In this sense, the value of such aerofoils was fairly accurately known. When used in a turbine, however, they became members of a series of cascades; and the interference just discussed with reference to Mr. Hodkinson's remarks completely altered their aerodynamic characteristics, for they had to be crowded together sufficiently close to give the optimum total torque. In addition, it was not practicable to use aerofoils directly for this purpose; they were too flat and had to be adapted by being given greater curvature. Probably not much data existed to show the value of such adapted aerofoils when used as turbine blades. There seemed to be no reason, however, why equal or even higher efficiency could not be obtained from them as from orthodox turbine blades, which had been arrived at by turbine engineers by quite a different line of approach. As regards orthodox turbine blades, the disconcerting fact that stood out was that between the efficiency ratio actually obtained and that which could be expected after debiting all calculable losses, there was a discrepancy of 6 per cent. In view of this discrepancy it was essential to inquire what was the explanation, even if it were found that, in commercial practice, not much of the difference could be reclaimed.

His own view was that the obstacle in the way of realizing high efficiency was the necessity for using blading of relatively large curvature. If it were possible to use flatter blades in which the loss due to deviation was considerably reduced, then a substantially higher efficiency ratio might be realized. In addition, possibly it would be found that a larger pitch could be used than that deduced in the paper from blades of normal shape, and this would be of further value. Unfortunately it was not the efficiency ratio that mattered commercially so much as the heat consumption, and this necessitated as many stages in series as possible. With aerofoil blades, the number of stages would require to be still larger, and so it appeared to be impossible to expand high-pressure steam completely with such blading.

It might be, perhaps, that too much stress was placed to-day on heat consumption per kilowatt-hour, but so long as a power station sold electrical energy and nothing else, it seemed that this situation would remain. When industrial plants were considered, however, where heat was in demand as well as power, the matter was different. In a back-pressure turbine it was often important to get as much power out of the steam as possible before the latter was used for heating. This could be done in two ways, either by starting with the highest steam conditions possible, or by making the blading as efficient as possible. Was it better to install extra high-pressure and high-temperature boiler plant and then to use inferior blading in the turbine, or to adopt more moderate initial steam conditions and use blading of higher efficiency? Looked at from this point of view, there was every reason for further effort to improve blading efficiency, and if aerofoil blades were found to be the solution, there might be room in a back-pressure turbine to accommodate a sufficient number of rows of them.

Another object of the paper was to stimulate interest in aerodynamical methods of research. It was to be regretted that textbooks on aerodynamics and hydrodynamics gave unequal weight to theory and experiment, and were written in a manner which had small interest for the practical steam engineer. As a result he was unable to obtain a grasp of the science, and he would conclude that it was of little assistance to steam turbine design and of little practical value anyway. Nevertheless, as Mr. Gibb pointed out, wind tunnel researches combined with modified Newtonian mechanics had undoubtedly placed the aeroplane where it was to-day, and there were certain problems connected with steam turbines which came within the domain of fluid mechanics. For example, double-beat valves were found to be subject to forces much greater than those which could be accounted for by considerations of steam pressure alone. Again, blade breakages were not unknown and some of them could not be accounted for by any ordinary theory of either synchronous or forced vibration. If the blades in the final row of a turbine were placed too near an obstacle such as a rib in the cylinder exhaust, they were liable to break off. Were not these aerodynamic problems, and was it not possible that blade fractures were sometimes the result of that "flutter" which was the bane of the aeroplane designer? Supposing that "flutter" was able to cause fatigue fractures in turbine blades, who could yet predict whether it would be produced in a given group of blading by a given steam jet velocity?

Analytical methods of approach to these problems were not of much appeal to most practical engineers, who usually preferred "geometrical" concepts. They thought in mental pictures, which were often of much greater practical value than mathematical analysis. The late Lord Kelvin who was equally at home in either geometrical or analytical thought, was also very practical, and was stated to have said, speaking of problems in physics, "If I can imagine a model of anything I am thinking about, I understand it, but if I cannot imagine such a model in my mind, then I do not understand it." In the same way, when a practical engineer was approached about possible improvements in blading efficiency, he pictured in his mind not only the possible variations of existing blading that could be made but also the probable effects of making such changes. He would reflect that any improvements effected must be obtained by such concrete changes as alterations in blade profile, pitch, chord angle, etc., and would not unnaturally reply that the long experience of turbine builders probably had led them by now to the best combination of factors, or that these things could be settled only by experiment. This attitude, however, could be carried too far. It was equally true that some things could not be determined by experiment alone, however long the effort might be sustained. For example, experiments on pipe friction went on for about a century, and many practical data were established. Nothing very useful emerged until theory showed how to make all the data fall on to a single curve. Until theory supplied the key, experiment alone was unable to solve nature's puzzle.

It was often of value to look at a problem from an altered viewpoint, and Mr. Cheshire had related how differently an aeronautical engineer would determine a flow through a nozzle, in comparison with the orthodox treatment which a steam engineer would apply. Yet both no doubt would arrive at correct conclusions and probably each would learn something of value from the other. It was generally admitted, as Mr. Gibb had stated, that the function of turbine blades was aerodynamic, and therefore it seemed reasonable to examine them from the aerodynamic point of view. He himself had endeavoured to show that in one respect at least, steam turbine blading behaved demonstrably like aerofoils inasmuch as varying the pitch affected both in the same way. He had also mentioned that there was at least one highly efficient turbine—a water turbine—that utilized aerofoil blades and wide pitch. He maintained therefore that evidence was in existence of a vital connexion between the aeroplane and the turbine, and that it was contrary to experience to argue that the latter had nothing in common with the former. In his own view the same experimental methods, guided by aerodynamic theory, which had been and were being devoted to aerodynamic problems on behalf of the aeroplane, should be equally applicable to many turbine problems.

The science of hydrodynamics and aerodynamics indisputably was difficult, but it was one which the younger generation of steam engineers could not afford to neglect. Writing on "Problems of Modern Pump and Turbine Design", Professor W. Spannhake had well said*, "We cannot depend solely on the experience of yesterday. We must use all weapons of modern hydrodynamics in its experimental and mathematical branches, in order to solve these problems".

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* Trans. A.S.M.E., 1934, vol. 56, HYD-56-1, p. 225.