Discussion.

The PRESIDENT said that when the Council of The Institution had considered the allocation of the present evening to the discussion of the Paper on "The Mechanics of the Voussoir Arch," describing a piece of general research work, they had thought that the Members might also like to see some films of practical tests on road bridges carried out for the Ministry of Transport. They had therefore asked the Building Research Station to show those films. He desired to make it quite clear that at present the work of Professor Pippard and that of the Ministry of Transport were quite independent. Neither section had gone far enough for inferences to be drawn from one to the other. The President.

Professor PIPPARD, in introducing the Paper, said that during the past 3 or 4 weeks the Authors had found that the opening paragraph of the Paper was somewhat unfair to certain early writers. At the end of the 18th century Boistard, a French engineer, had made experiments and had observed that arches failed by hinging about a number of sections. Those observations had enabled Lamé and Clapeyron to propound a theory of instability and to give a trial-and-error method of determining whether a given arch was suitable under given load-systems. Those investigators had only considered the case of symmetrical load-systems. Investigations had been made rather later in Great Britain by, amongst others, Messrs. Moseley, Barlow, and Snell. Snell in 1846 had read a Paper [1] to The Institution in which he had given examples of Lamé's and Clapeyron's method, and had attempted to extend it to the case of unsymmetrical loading. The result had not been too successful, for several reasons which Professor Pippard had not time to discuss that night, but from that time that aspect of the problem appeared to have been completely lost sight of in a mass of literature dealing with design methods based on the hypothesis that no tension might develop at the joints between the voussoirs. The Authors of the Paper under discussion proposed to outline the development of the theory of the voussoir arch in a later Paper. Professor Pippard.

Professor Pippard then showed a cinematograph film and a series of lantern-slides illustrating the work described in the Paper. The theoretical influence line of the horizontal thrust for an encastré

[1] George Snell, "On the Stability of Arches, with practical methods for determining, according to the pressures to which they will be subjected, the best form of section, or variable depth of voussoir, for any given intrados or extrados." Minutes of Proceedings Inst. C.E., vol. v (1846), p. 439.

Professor Pippard.

arch (*Figs. 11*, p. 298§) had been found to be incorrect for the lowest values of H/W, and considerably better agreement between theory and experiment was actually the case, showing without doubt that when the abutments were rigid and the loads were not too large, elastic theory was applicable to the problems.

A model was also exhibited by the Authors which showed the formation and transition of the "pins" in an arch mounted on skew-backs.

Further work was in progress with the object of extending the results to the stage of practical application and design, and the Authors hoped to report on that work to The Institution at a later date. He would conclude by expressing, on behalf of his co-Authors, their great obligation to the Department of Scientific and Industrial Research and to Dr. R. E. Stradling, M. Inst. C.E., for having the film made showing the experimental work.

The President.

The PRESIDENT then called upon Dr. Norman Davey, of the Building Research Station, to show films of tests on road bridges.

Dr. Davey.

Dr. NORMAN DAVEY said that no doubt many had seen references to a series of tests which the Building Research Station were carrying out for the Ministry of Transport. He had been invited to show that evening films of some of the tests, which had been made under his supervision. The films illustrated tests on two humped-back canal bridges built in 1793. One bridge was a stone voussoir arch, elliptical in form, carrying the road over a disused canal near Derby, and the other was a brick masonry arch which carried the road over the Stratford-on-Avon canal at Yardley Wood, about 5 miles south of Birmingham.

In order to apply the load, steel beams were mounted on timber cribs erected on the road surface. Upon those steel beams a timber deck was erected, and upon that deck dead load was stacked. The load was applied to the bridge by hydraulic jacks. To measure the load there was a steel block upon which was mounted a strain-gauge. Knowing the calibration of the block, it was possible to obtain a measure of the load transmitted to the arch. The deformation of the structure under various conditions of loading was observed by measuring the deflexion of the arch-ring at various points along its span, by measuring the spread of the arch and its rotation at springing, and by measuring strains in the masonry of the arch by means of gauges mounted on its intrados. Deflectometers were generally placed at the springing point and at the eighth-span points.

Dr. Davey then showed the films.

§ Page numbers so marked refer to Journal Inst. C.E., vol. 4 (1936-37). (December, 1936.)—SEC. INST. C.E.

The PRESIDENT, in moving a vote of thanks to the Authors and to the exhibitor of the films, observed that bridges which were supposed to be dangerous appeared, from the films shown, to be a good deal stronger than was generally thought to be the case. **The President.**

Professor C. E. INGLIS was particularly interested in Table I (p. 286§) in which the experimental and theoretical values of H/W for a solid two-pin arch-rib were compared. The agreement achieved had been astonishingly good, the discrepancy nowhere exceeding 1 per cent.; that was all the more remarkable since, from the theory given in the Appendix (pp. 304 *et seq.*§), it would appear that in calculating H (the horizontal component of the abutment-thrust), the contribution due to shear-deformation had not been taken into account. Admittedly, shear-deformation would only affect H to a small extent; it was impossible to calculate to what extent, since the dimensions of the rib were not given, but he would have thought that, having regard to the high degree of accuracy with which the values of H/W were tabulated, the contribution made by shear-deformation might possibly have been taken into account. **Professor Inglis.**

The behaviour of a two-pinned voussoir arch with the span slightly increased or diminished as shown in *Figs. 5* (p. 290§) and *Figs. 8* (p. 292§) was particularly interesting. The meandering of the resulting central hinge as a non-central load was steadily increased was a most illuminating conception, and when once it had been visualized the explanation was almost obvious, but in that explanation he would differ entirely from the explanation given in the Paper. The explanation set forth on p. 293§ was based upon the conception of least strain-energy, and the Authors stated that it was clear that the arch would assume the form which, for any given load, caused it to store the least amount of strain-energy. That might be clear to the Authors, but it was not at all clear to him, and their use of the principle of least strain-energy in that instance seemed to him not merely unnecessary but also quite unconvincing. The variation of strain-energy in one particular body was not being dealt with; comparisons were being made between the strain-energies in different bodies. It was true that they were built up of the same bricks, but since the central hinge was in different positions they were really different mechanisms, and although it might be capable of proof, it certainly was not obvious that the principle of least strain-energy had the extended application claimed for it in the Paper. The phenomena under consideration had no connexion whatsoever with strain or strain-energy; that was evident from the fact that the meandering of the central hinge would take place in precisely the same way if the voussoirs were made of absolutely rigid material,

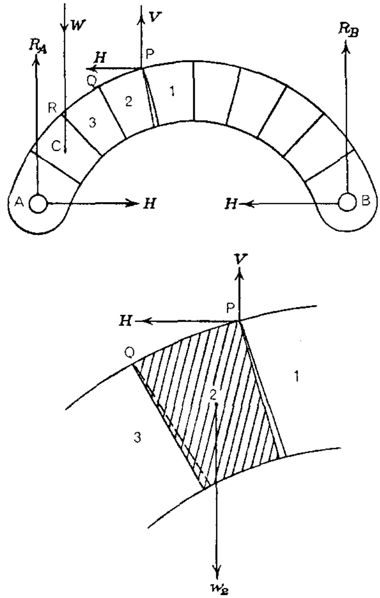
§ *Ibid.*

Professor Inglis.

in which case strain-energy could not be stored. In point of fact the problem was a simple example of rigid-body statics, and its explanation called for nothing deeper than a knowledge of how to resolve forces and to take moments.

With reference to *Figs. 15*, he would assume that when a load W acted through C, the central hinge had moved to P. By considering the equilibrium of the portion of the arch which lay to the left of P, the values of H and V, the horizontal and vertical components of the reaction at P, could at once be calculated, and, as W increased, the values of H and V would also increase. The equilibrium of

Figs. 15.



voussoir No. 2 had then to be considered: the joint between voussoirs Nos. 2 and 3 would remain closed until the sum of the moments of H and V about Q overbalanced the moment of W_2 about Q. When the overbalance occurred the joint between voussoirs Nos. 2 and 3 would open and that between Nos. 1 and 2 would close. For some greater value of W the hinge would make another move, changing its position from Q to R.

The load which caused the change in the pin-positions set forth in Table IV (p. 294§) could thus be calculated with simplicity and perfect precision. He would like to have done that, but information con-

§ *Ibid.*

cerning the weights and sizes of the voussoirs had not been available. In view of the extreme simplicity of the conditions involved he would expect the calculations thus performed to show a much closer agreement with experiment than those recorded in Table IV. Discrepancies of 10 per cent. or more were not altogether satisfactory, and strengthened the belief that the application of the principle of minimum strain-energy to those calculations, if not actually illegitimate, was certainly inappropriate. Professor Inglis.

Why had the Authors based their explanations on the principle of least strain-energy ? Even if it had been applicable it was like using a steam hammer to crack a nut. The principle of least strain-energy had its uses in leading to solutions, but it gave no information which could not be deduced by ordinary statics combined with geometry, and all too often it completely obscured the true inwardness of a problem in mechanics. Such criticisms as he had made were, however, small in comparison with his admiration for the Paper.

Miss LETITIA CHITTY referred briefly to the following contribution by Professor Southwell, entitled "A Statical Theory of the Voussoir Arch": Miss Letitia Chitty.

*** Professor R. V. SOUTHWELL observed that, in place of inexact and unsatisfactory rules, based on formulas which were in fact restricted to continuous ribs of steel or cast iron, the Paper showed that an exact and reliable theory could be based on precisely that feature which at first sight would seem to complicate the problem; namely, that the mortar between the individual voussoirs could not safely be assumed to offer resistance to tension. Professor Southwell.

The one possible criticism of the Paper would be that those not previously acquainted with the problem might not be led to realize how great was the change in outlook which its results suggested. For that reason an alternative presentation of the theory might perhaps be useful, although in the nature of the case it would lead to exactly the same conclusions; that was bound to be the case, since it was concerned with the same phenomena, and merely considered them from a different angle. What followed was an attempt to emphasize that the problem was really statical, by showing that no essential feature would be lost if the voussoirs were completely rigid.

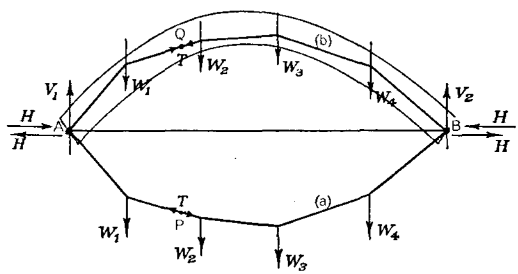
When an arch having frictionless pins at the abutments was in equilibrium, the vertical loads which it carried (including its own weight) had to be balanced by thrusts acting through the centres of the pins. Assuming that equilibrium was possible, a fundamental

*** This communication on the Paper was received prior to the Council's decision that the Paper should be read and discussed at an Ordinary Meeting. —SEC. INST. C.E.

Professor Southwell.

theorem in statics asserted that the "line of thrust" (giving the directions of the reactions between contiguous voussoirs) could be determined provided that the horizontal component (H) of those reactions were known. It was an "inverted funicular"—that was to say, it could be found by "reflecting" with respect to a horizontal plane the curve which a weightless string would assume under the same system of vertical loads, if its ends (at the same level) were held apart by horizontal forces H. *Fig. 16* illustrated that statement. Curve (a) was the funicular for the system of vertical loads W_1, W_2, W_3, . . . etc., to which the arch was subjected; the ends of the string were held fixed at A and B by vertical forces V_1, V_2 and by horizontal forces H. Curve (b) was the image of (a) with respect to a horizontal plane through A and B. Clearly, if the string (a) rested in equilibrium with a tension given by T at the point P, then a rod bent into the form of (b), and loaded by the same vertical forces and by

Fig. 16.



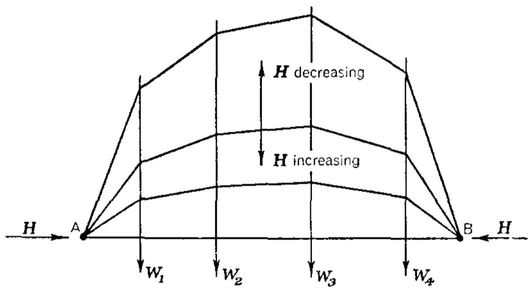
horizontal thrusts of magnitude H, could sustain those forces by thrusts alone, the thrust in the rod being T at a point Q which was the image of P. The only difference was that, whilst the form of (a) was stable for small disturbances, the form of (b) was unstable. Thinking of (b) in that way, it was not difficult to see that its form was unique for a given value of H: there could not be two configurations of equilibrium.

The theorem also permitted the effect of an alteration in the value of H to be stated: for any thrust λH the line of thrust could be obtained from (b) by a simple change of ordinates in the ratio $1 : \lambda$. Thus, if $\lambda > 1$ (H increased) the line would become flatter, whilst if $\lambda < 1$ (H decreased) it would become more sharply curved; in either event its shape would exhibit the same features (*Fig. 17*). A family of "inverted funicular" curves thus corresponded with any given system of vertical loads. Each was a possible form for the line of thrust, provided that H had the appropriate value. The flatter the curve, the larger was the associated value of H.

Professor Southwell.

It was next necessary to examine whether equilibrium was in fact possible. He would show that the criterion, in respect of an arch which could not sustain tension, was whether or not a line of thrust could be drawn for the given load-system, passing through the centres of the abutment pins, and lying entirely within the arch. The meaning of the last requirement would be clear from what followed.

Fig. 17.



He would suppose first that the line of thrust went above the arch (*Fig. 18*), so as to cut CD, the plane of contact of two voussoirs A and B at a point E outside the extrados, and he would denote the thrust at E by T. That force had to be balanced by the action of B on A: if S and N were its components along and perpendicular to the face CD, then S had to be balanced by a tangential, and N by a normal, action. The tangential action, which had to be

Fig. 18.

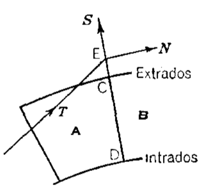
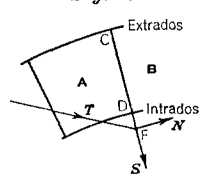


Fig. 19.



supplied by friction between the voussoirs, did not call for further consideration at the moment. If, however (as was required by the hypothesis regarding the mortar), the normal actions of B on A were wholly compressive over the face CD, then it was clear that their resultant would be a force cutting the face (at right angles) somewhere between C and D, and that force could not be in equilibrium with N. A similar argument showed that the conditions indicated in *Fig. 19*, where the thrust-line cut CD at a point F inside

Professor Southwell.

the intrados, were also inconsistent with equilibrium. Thus the following theorem had been proved: tensile strength was demanded of the mortar, unless the line of thrust lay between C and D at every face CD.

It followed from the theorem that H, when the mortar was incapable of resisting tension, was bound to have a value lying between somewhat narrow limits. Some thrust had to be applied to the arch in order to maintain it in equilibrium. If that thrust were small, the line of thrust would cross some face CD at a point outside the extrados; the adjacent voussoirs would tend in consequence to separate by rotation about a line of contact, or " hinge," at C (on the extrados); the ends would tend to spread and, unless that spread were resisted, the arch would collapse flat. If the thrust were great, the line of thrust would cross CD at a point inside the intrados, " hinging " would occur about a line of contact at D (on the intrados), and the ends in consequence would tend to approach: in so doing (if the abutments were fixed) they would cause a reduction in the value of H. It was not essential to equilibrium that the abutment hinges should be exactly at their designed positions, provided that in their actual positions they were capable of sustaining whatever thrust the arch might impose: equilibrium would result when H had attained a value such that every face CD was cut between C and D by the line of thrust, and in those circumstances it was fairly clear that friction would be sufficient to provide the necessary tangential resistance (that was, to the component denoted by S in the last paragraph).

Usually, when the loads were such that equilibrium was possible, more than one funicular (of the family mentioned earlier) could be drawn to lie wholly inside the arch, and consequently there was a range of possible values for H, bounded at the one extreme by that thrust which would make the ends approach, and at the other by that thrust which was just insufficient to prevent the ends from separating. Seen from that standpoint, no new feature was presented by an arch which was not hinged to the abutments; the only difference was that (since the line of thrust no longer had to go through definite hinge-centres, but had merely to cut lines of finite length at the abutments) the range of H, for a given load-system, was somewhat wider.

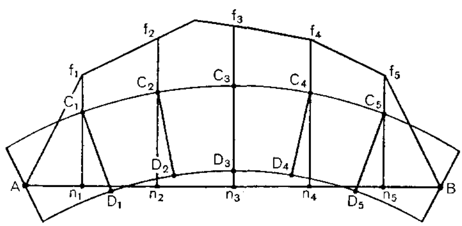
Given the vertical loads, it was easy to predict the point at which hinging would occur, and the range of possible thrusts. Taking first the case of an arch (*Fig. 20*) with pins at the abutments A and B, he would let C_1D_1, C_2D_2, . . . etc., represent the surfaces of contact of adjacent voussoirs, C_1, C_2, . . . etc., being on the extrados, and D_1, D_2, . . . etc., on the intrados of the arch. For any value H_0

Professor Southwell.

the inverted funicular corresponding with the specified loads could be drawn, and then through C_1, C_2, . . . etc., ordinates $f_1C_1n_1$, $f_2C_2n_2$, . . . etc., could be drawn as shown. He would let r_1, r_2, . . . etc., denote the ratios f_1n_1/C_1n_1, f_2n_2/C_2n_2, . . . etc. Then as H was steadily decreased from a high initial value, and in consequence (p. 10) the relevant funicular became steeper, it was clear that the extrados would first be crossed, and hinging in consequence would first occur, at that section for which r had its highest value.

In exactly the same way, drawing ordinates $F_1D_1N_1$, $F_2D_2N_2$, . . . etc. (not shown in *Fig. 20*), through D_1, D_2, . . . etc., and denoting by R_1, R_2, . . . etc., the ratios F_1N_1/D_1N_1, F_2N_2/D_2N_2, . . . etc., it could be shown that hinging would first occur, as H was steadily increased, at the intrados of that section for which R had its lowest value.

Fig. 20.



Moreover, if $r_{max.}$ denoted the highest value of r and if $R_{min.}$ denoted the lowest value of R, it was easy to show that the range of possible thrusts would be given by

$$H_0 \cdot r_{max.} < H < H_0 \cdot R_{min.} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

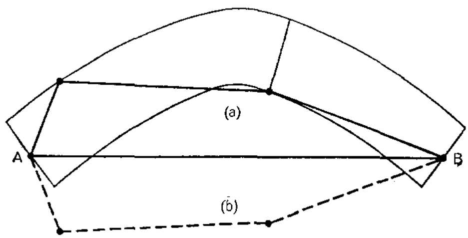
Hitherto he had dealt with cases in which equilibrium was possible, but clearly, if $R_{min.} = r_{max.}$, only one value of H was permitted by the condition (1), and no value if $R_{min.} < r_{max.}$ It was therefore necessary to consider why in those circumstances the arch was bound to collapse, even when its abutments were fixed.

From what had been shown above, when $R_{min.} = r_{max.}$ hinging could occur simultaneously at one point on the intrados and at one point on the extrados, as well as at the abutment pins; the arch would therefore behave like a linkage of three rigid members, hinged to one another and to the abutments. The statics of the problem would not be altered if those curved members were replaced by three straight rods, and the abutments (rigidly connected by the earth) by a fourth rod AB shown as (*a*) in *Fig. 21* (p. 14). The result (in effect) was the mechanism known as a four-bar chain.

Professor Southwell.

Considering with that mechanism its image with respect to *AB* (shown as (*b*) in *Fig. 21*), it was easy to see that if either one were in equilibrium, the other would also be in equilibrium under the same vertical loads. If however (as was evidently true) the equilibrium of the lower (image) mechanism were stable, so that the potential energy of the loads would be increased as a consequence of any small displacement, then the potential energy of the upper system was

Fig. 21.



bound to be decreased as a consequence of the corresponding (image) displacement, and it followed that the equilibrium of the upper system was unstable. Therefore, if hinging could occur at the intrados and extrados simultaneously, the arch would be on the point of collapse.[1]

When the arch was not pinned to the abutments, but bore on them over finite surfaces $C_A D_A$, $C_B D_B$, as indicated in *Fig. 22*, the line of thrust was no longer required to go through fixed points (A and B,

Fig. 22.

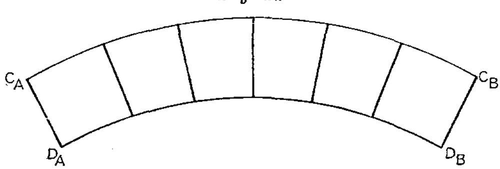


Fig. 20), but had merely to pass between C_A and D_A and between C_B and D_B. Therefore, in place of a single family of funiculars, a wider range of funiculars was now possible, limited by the occurrence of hinging at either abutment or at both. It would still be possible to deduce an inequality of the form (1), defining the range of possible thrusts, but evidently hinging at the extrados of the arch was bound

[1] It could sometimes appear that hinging became possible, simultaneously, at two points on the intrados or at two points on the extrados. Such circumstances would not, however, result in collapse, because it was kinematically impossible (when the abutment pins were fixed) that hinging should in fact occur at both points simultaneously.

Professor Southwell.

to be accompanied by hinging at the intrados of one abutment or of both, and vice versa, and so four families of "limiting funiculars" called for consideration :

(i) a family passing through C_A, C_B (*Fig. 22*),
(ii) ,, ,, D_A, D_B,
(iii) ,, ,, C_A, D_B,
(iv) ,, ,, D_A, C_B.

Usually $r_{max.}$ would be obtained from a funicular of class (ii), and $R_{min.}$ from a funicular of class (i); $r_{max.}$ would be lowered and $R_{min.}$ raised as a consequence of the wider freedom of choice, so that the range of H would be wider when pins were not fitted at the abutments.

Reverting to pp. 12 and 13, a slight modification of the argument there given would be convenient when it was desired to investigate the behaviour of the arch under a concentrated load W which was steadily increased, as described on pp. 293–5§.

For a horizontal thrust H_0, he would denote by D_d the full-scale depth of the funicular drawn for the constant or "dead" loading, and by D_w the full-scale depth of the funicular drawn for a unit concentrated load replacing W. Then the depth for a concentrated load W would be $W.D_w$, and (since loads could be superposed as regards their effects on the bending moment and therefore on the funicular depth) the depth D_0 of the funicular drawn for W acting in combination with the dead loading would be given by

$$D_0 = D_d + W.D_w \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

That was for a thrust H_0: according to the argument previously set down, the depth of the funicular corresponding with any other value H would be given by

$$D = D_0 H_0 / H \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Further, hinging would occur at a point on the extrados if $D > D_h$, where D_h denoted the full-scale height of the potential hinge above AB, the line through the abutment-pins. Similarly, hinging would occur about a point on the intrados if $D < D_h$.

Fixing attention on any one of the potential hinges, and making use of (2) and (3), it could be seen that hinging would first occur at that point when $D_h = D$, so that

$$HD_h = HD = H_0(D_d + W.D_w) \quad . \quad . \quad . \quad (4)$$

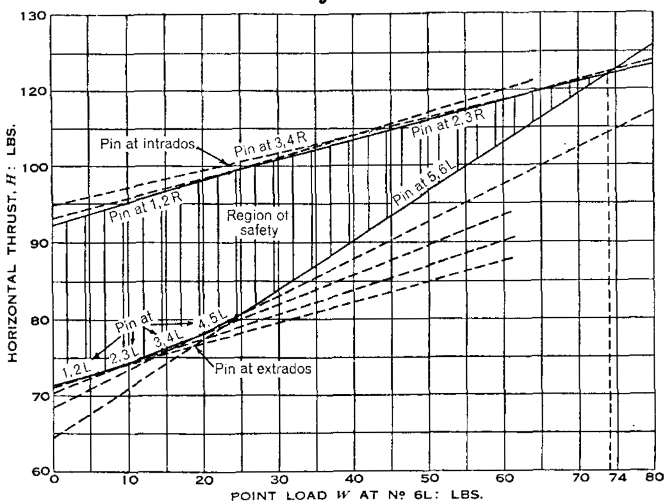
That was a linear relation between H and W, in which the coefficients H_0, D_h, D_d, D_w could be determined by standard methods. It gave a "line of safety" for the hinge-point considered, and safe

§ *Ibid.*

Professor Southwell.

values of the thrust would lie above the line if the hinge were on the extrados, and below the line if the hinge were on the intrados. By considering all possible hinge-positions, a number of lines could be determined, bounding a "region of safety" as shown in *Fig. 23.* That diagram was fundamentally the same as *Fig. 10* (p. 296§), but it had been obtained by a different method: it had been drawn for Professor Southwell by Mr. K. N. E. Bradfield, using equation (4)

Fig. 23.



to determine the positions of the different lines. Mr. Bradfield's calculations had given the following figures for a point load at 6 L, and with $H_0 = 1$:

Pin at Extrados.

Hinge-position:	1, 2 L.	2, 3 L.	3, 4 L.	4, 5 L.	5, 6 L.
D_h	1·120	1·080	1·000	0·883	0·728
D_d	80·034	76·855	70·325	60·353	46·992
D_w	0·298	0·342	0·385	0·426	0·466

Pin at Intrados.

Hinge-position:	1, 2 R.	2, 3 R.	3, 4 R.
D_h	0·869	0·833	0·762
D_d	80·104	77·499	72·227
D_w	0·256	0·215	0·176

§ *Ibid.*

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As W increased, the boundary of the " region of safety " (*Fig. 23*) changed from one line to another. The different lines cut obliquely, and hence the limiting values of W could not be deduced with great precision. That geometrical difficulty was reflected in the analytical process, whereby a critical value of W was determined by eliminating H from two equations of type (4): W appeared as a quotient, and both the numerator and the denominator were small differences of relatively large quantities. However, the analytical method ought to be preferable on the score of accuracy; it led to the following results, which were directly comparable with Table IV (p. 294§):

TABLE VII.—LOADS CAUSING CHANGE OF PIN-POSITION.

Condition of span.	Position of pin changing from	Load at 6 L.	
		Experiment.	Statics.
	Extrados		
Spread	1, 2 L–2, 3 L	5·0	5·9
	2, 3 L–3, 4 L	11·5	12·3
	3, 4 L–4, 5 L	18·5	20·3
	4, 5 L–5, 6 L	24·5	24·1
	Intrados		
Contracted	1, 2 R–2, 3 R	25·0	23·5
	2, 3 R–3, 4 R	63·0	64·6

In *Fig. 23* the region of safety disappeared (that was to say, its depth became zero) when $W = 74$. That was in exact agreement with the Authors' results (p. 297§).

Throughout the preceding remarks the voussoirs had been treated as rigid, and the fact that that assumption had been found adequate to explain the experimental observations was a proof that the problem was really one of statics. Since that conclusion appeared to contradict the more usual theory (based on the notion of an elastic arch), it might be useful to consider how far the two theories were really in conflict.

The new theory suggested that the " rule of the middle third " had no relevance to the problem, in that the voussoirs would remain *in contact until the thrust-line passed outside any possible hinge*; that was to say, outside the arch itself. In case that conclusion should be thought to clash with the accepted engineers' theory of flexure, it was perhaps worth while to remark that what that theory really asserted was that, given continuity of the material, a tensile stress would be created if the line of thrust passed outside the middle third. Let it be supposed, however, that that tension could not be sustained, so that a gap resulted; there was no reason why the stress should

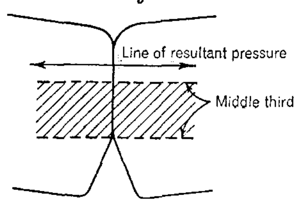
§ *Ibid.*

Professor Southwell.

not be wholly compressive over the bearing area which was left, and if two blocks were imagined pressed together in the manner of *Fig. 24*, it was clear that that was in fact what would happen. If the resultant pressure acted outside the middle third, tension would be required to close the gap, but in the absence of such tension there was no failure of equilibrium, and the gap would be inversely proportional to the rigidity of the block; that was, it would be zero when the block was completely rigid, as assumed in the previous consideration. What *Fig. 24* really demonstrated was the difficulty of making accurate allowance for elasticity in the voussoirs: it was clearly desirable that that should prove to be unnecessary, as it did according to the presentation given above.

As the resultant pressure moved nearer to one edge of the area of contact, the local intensity of pressure would increase and might ultimately attain a value in excess of the crushing strength of the masonry. That, however, was a question not for elastic theory but for experimental study, and the results of such study could be easily incorporated in rules for design.

Fig. 24.



Like the Authors, Professor Southwell had made no attempt to suggest the bearing of their work on the practical aspects of design. It was clear that its influence was bound to be far-reaching, and his own conjecture was that it would be in the direction of simplifying rather than of complicating existing practice. In another field, it might be a first step towards a theory of the mechanical principles of Gothic architecture.

Mr. Chettoe.

Mr. C. S. Chettoe said that the Paper showed how a voussoir arch loaded with dead load in conjunction with a single point-load, or a suitable group of point-loads, behaved initially as an elastic arch and afterwards, as the point-loads were increased, changed gradually to a three-pin condition as the line of thrust passed outside the arch-ring. Voussoir arches in practice were usually fixed and not two-pinned, but it was evident that the theory could be extended to cover the present case, and in fact the Authors had touched upon that extension.

Mr. Chettoe.

The usual method which had been adopted in the past for the design of voussoir arches was that of the line of pressure, which could be rapidly obtained. That method was still used to a large extent for checking the design of those arches. The line of pressure was supposed to pass through the middle third and, if so, the arch was considered to be satisfactory; since, however, in practice it was very difficult to be sure that the line of pressure was accurate, the method was not altogether satisfactory. The Authors had shown, however, that the elastic theory was applicable until the joints opened in the case of an unrestrained rib or vault. The main difficulty in calculation was in obtaining the value of the modulus of elasticity. In a brick arch the width of mortar-joints was often quite considerable, and the width and the value of the modulus of the joints had to be considered as well as those of the bricks. Experiments had shown in many cases that the value of the modulus of elasticity might be quite low. One test carried out for the Ministry of Transport at a bridge at Chester had given a value for the modulus of elasticity of about $0{\cdot}3 \times 10^6$ lbs. per square inch.

A considerable section of the Paper was devoted to the effect on the arch of spread of abutments. He felt far from certain that the amount of abutment-spread which would be met with in practice in the case of a reasonably designed arch with moderate loading on good foundations would be enough to affect the stresses on the arch very much, or would require to be taken into account in design. He had worked out a case of a five-ring brick arch having a span of 24 feet with a rise of 7 feet 6 inches. Assuming the value of the modulus of elasticity to be $0{\cdot}3 \times 10^6$ lbs. per square inch, and the total spread to be 0·01 inch (a value actually obtained in a recent test), the spread of the arch was equivalent to a fall in temperature of $8\frac{1}{2}°$ F. Assuming the arch to be loaded with the Ministry of Transport's equivalent-loading curve, the stress at the crown was increased only by about 5 lbs. per square inch, whilst that at the springing was increased by from 8 to 9 lbs. per square inch. The thrust-line in those circumstances was little affected by the spread which occurred. Spread was not normally considered in the design of reinforced-concrete arches, and it seemed to him probable that in many cases the amount of spread actually obtained in voussoir arches was not enough to have any very great effect on the stresses or in the way in which the arch behaved. The proportions of the arch in the Authors' experiments were not very dissimilar to those of a brick arch, but they were actually of steel, which was relatively a very rigid material, and probably the amount of spread which was given to the arch was quite appreciable. Nevertheless, the question of spread was one of very great importance because, although there might not be very

Mr. Chettoe. much spread in normal cases, yet in some instances considerable spread had occurred, due, for instance, to bad foundations, mining subsidence, etc. He had come across several cases; one was in a mining valley in South Wales, where both sides of the valley had moved in towards each other and the abutments of the bridges in that valley had moved in appreciably. Another interesting case was that of a bridge in Lanark, where for some geological reason the two sides of the valley had moved in, and the arch, which appeared to be perfectly satisfactory, had collapsed. In those cases some rotation of the abutments might also have occurred.

He did not feel that it was yet clear that voussoir arches in practice, whether of brick or of masonry, under reasonable loads necessarily ceased to behave as monolithic vaults capable of solution by the elastic theory, assuming that the value of the modulus of elasticity could be calculated. The Authors' experiments had been carried out on an ideal rib. Even if under working live loads an arch calculated on the elastic theory showed a tendency to open up at the joints, in practice that calculated effect might not occur, as the joint might in fact be capable of taking some tension.

The Authors' results had been obtained on an unrestrained rib, but in practice voussoir arches were frequently constructed with concrete or brick backing or haunching which extended part of the way up the vault. They were also restrained by spandrel-walls, on which parapets were usually constructed, whilst the voussoirs were covered by filling which had become consolidated by the passage of road or rail traffic. In the case of road bridges the strength of the road metalling and surfacing might have some effect on the arch. If the arches were narrow the effect of the spandrel-walls might be quite considerable in restraining them. Temperature-fall, rib-shortening, and spread of abutments would all tend to drop the arch, and the third virtual pin would rise with a reduced load at the crown whilst the application of partial live loading would tend to cause the unloaded portions of the arch to rise. If the net result of all those effects caused any particular point in the arch to rise, such movement would be resisted by the spandrel-walls and filling. If, on the other hand, the net effect were to cause a drop at a particular point, there might be some arch-action in the fill and spandrels. It seemed possible, therefore, that that restraint might change the point at which the arch, or some part of it, departed from behaviour as an elastic vault and commenced to become a three-pinned statically-determinate structure.

Mr. Wilson. Mr. J. S. Wilson remarked that the early arches had clearly been designed by those who had actually constructed them, or at any rate by those who had been very closely associated with their construction, but trouble had arisen when philosophers and mathematicians had got to work. They had had no experience of constructing arches, and the extraordinary conditions of about 150 years ago had developed. He would mention a bridge which Sir John Soane had had to construct. Sir John had had no confidence in the arch, and in that little bridge every voussoir had been clamped to the adjoining voussoir by iron cramps, whilst in order to keep one voussoir from slipping on the other, he had put two cast-iron blocks, half embedded into each voussoir, in every joint; in addition to that, he had bound the whole arch transversely with what he had called "cart-wheel" iron! Those who ought to have known the theory of the arch had been arguing at great length as to whether the depth of the arch ought to be proportioned according to the span or according to the radius of curvature at the crown; they had not been able to get any further, so Sir John Soane, who had had to design the arch and to be responsible for it, had not taken any risks. Mr. Wilson.

Mr. Wilson had examined arches of all types from time to time—brick arches, masonry arches, and Telford's type of cast-iron arches. The more arch-constructions were examined the more confidence was had in them. The odd thing about an arch was its extraordinary power to suffer deformation without loss of strength. He would mention a few examples. There was Perronet's epoch-making flat arch at Neuilly, near Paris, which had a span of about 130 feet. When the centering had been removed the crown of the arch had fallen 8 inches. That bridge had stood since 1774, and it had carried all the traffic to which it had been subjected. As soon as the Pont d'Alma across the Seine had been built, one of the abutments had gone down over 1 foot, but the engineers had simply re-arranged the road-level and had rebuilt some of the spandrel-walls, and the bridge had remained until a short time ago. Then there was Telford's bridge at Gloucester. That had gone down so much at the crown that there was practically no curvature at the centre, but it still carried its load. He had seen beautiful old bridges with trees 4 or 5 inches in diameter growing out between the joints; such bridges were still in existence, and they were carrying quite successfully all the traffic to which they were subjected.

By making experiments with the Authors' models, students would gain a confidence in the arch form of construction which few students of the last generation ever got, and from that point of view Mr. Wilson thought that the work described in the Paper was of great value.

The Authors' arch was of steel with a very high modulus of elasticity; Mr. Chettoe had just pointed out that a brick arch probably had a modulus of elasticity of $0{\cdot}3 \times 10^6$ lbs. per square

Mr. Wilson. inch, or about 150 tons per square inch, as compared with about 13,000 tons per square inch for steel. Some experiments should be made with material more like that which was used in practice, and allowance should be made for the deformation of arches. In practice there was a centering which had to support the arch while it was being built, and as soon as building was commenced the centering deflected or compressed, so that the joints between the voussoirs were not perfect. Further, when the centering was taken out the arch was bound to deflect still further. That initial deformation upset the theoretical position of the line of thrust, which had such a great bearing on the stability of the arch. Some years ago he had designed some centering for a brick arch where there had been hardly any room underneath. He had designed the centering as very light steel ribs, and by temporarily loading and unloading parts of the centering, it had been kept in the right shape. By leaving gaps in the brickwork which were filled in just before removing the centering, the preliminary deformation of the centering was allowed for.

He desired to congratulate those who had carried out the experiments on actual bridges, but he thought it was hardly a fair test on the bridge to put the load all at the centre. He suggested that the Ministry of Transport should make an attempt to destroy an arch-bridge under actual working conditions; for instance, a 10-ton lorry should be driven backwards and forwards over the bridge and a record kept of how long it took to damage or destroy the bridge. He believed that, with the bridges tested, if a 30-ton or 40-ton lorry were used the test would go on for almost an indefinite period.

Mr. Adams. *** Mr. Haddon C. Adams observed that the objects of the experiments described in the Paper, and of others to follow as stated on pp. 281§ and 303§, were commendable if somewhat ambitious, in that they attempted to find a more rational basis of design than that founded on the elastic theory. There were bound to be many stages in the search, and there would also be a constant temptation to press the conclusions too far; it appeared in fact as if the temptation was occasionally a little too great for the Authors' restraint. The condition in the arch when two adjacent voussoirs were in contact only at their upper or lower edges was one which the designer could not contemplate with equanimity: the experiments were carried out mainly in a range of conditions which lay completely outside the designer's scope. Those conditions were interesting, but as far as the designer was concerned the arch as a monolithic structure had already failed; the designer's main interest was bound to centre in conditions at a much earlier stage.

*** This contribution was submitted in writing.—Sec. Inst. C.E.
§ *Ibid.*

Mr. Adams.

In the section of the Paper headed "Review of Accepted Basis of Design" (p. 282§), the requirements as there set out would probably be fulfilled by very few voussoir arches which carried road traffic if present-day loading were considered, and if allowance were made also for temperature-variations. Fortunately, other factors played an important part in sustaining the structure, even if they were not recognized as valid in design. In the design of arches the old method of balancing the thrust-line inside the "middle third" was largely abandoned, and indeed very few real voussoir arches were at the present time being constructed for road bridges.

On p. 283§ it was stated that "Not only will the thrust of the arch cause slight spreading of the supports, but settlements of the foundations, or even earth-movements under the foundations, must be treated as serious possibilities which may cause alterations in the span after the arch is erected." The statement that ". . . . earth movements under the foundations must be treated as serious possibilities . . ." in arch-design was rather alarming. Most designers would decide at once that such a site was completely unsuited to the construction of a fixed-arch structure unless piles could be used to prevent the anticipated movement. The effects due to temperature-variations and rib-shortening (and, in concrete arches, of shrinkage and plastic yield) were similar to a slight movement of the abutments, but those movements were of a specific nature, confined to one direction, and moreover, they could be fairly accurately estimated and their effect allowed for. Movement of the foundation-bed (to any material extent) was another question; how was it to be estimated and how was provision to be made for it? The designer would say that it had to be prevented, or else that that type of design had to be abandoned.

The method of applying the load was rather open to criticism, and it would be very much better, if design were the object of attack, to load the arch in some way with filling placed above the voussoirs and distributed over the whole of the extrados.

The statement on pp. 285 and 286§ that "The voussoirs were provided with slots and pins to prevent relative rotation, but no other restraint was given," appeared to be misleading. Relative rotation between voussoirs would ordinarily be taken to mean rotation in the plane of the arch; in fact, however, such movement was not restrained, and did take place—it was really the main feature of the whole experiment. Presumably the Authors intended to convey that restraint against buckling laterally was provided by the slots

§ *Ibid.*

Mr. Adams. and pins. Such a provision was reasonable, as in an actual vault its breadth would give the same effect.

He strongly criticized conclusion (*g*) on p. 303§, in which the Authors stated that in most actual arches there would probably be a spread of abutments, that the problem of the arch would then be statically determinate, and that any design method should therefore take into account the probability of the structure assuming the three-pinned form. Such a conclusion, jumping from an ideally-constructed laboratory model in steel (and that only a narrow strip), to an arch built as a broad vault of masonry or brick, with mortar joints and solid filling over the vault, was completely unjustified. It was a big step from one to the other and many careful experiments would have to be performed, sound deductions drawn, and criticisms answered before the designer of an arch could accept the suggestions advanced by the Authors. Was the present Paper intended to be the foundation of a new design method ? The designer, of arches at least, would say that those foundations were suspect, and that it would be necessary to prove them or else to change the design.

The Authors. The AUTHORS, in reply, wished to express their thanks to Professor Southwell for the interest he had shown in the work and for his presentation of an alternative treatment for calculating the "pin" positions. They were in agreement with him and with Professor Inglis that when the structure had assumed a three-pinned form the phenomena observed could be explained without appeal to considerations of strain-energy. They would emphasize, however, that the actual construction used and illustrated in *Fig. 10* (p. 296§) was obtained solely from a consideration of the static equilibrium of the possible three-pinned arches.

No calculations involving strain-energy were required in the construction of that diagram except for the $H-W$ line for the two-pinned arch, which was only shown for purposes of argument and which would not be needed in an actual investigation of stability. In one sense *Fig. 9* (p. 294§) was unnecessary, but when the Authors first observed the sequence of pin-formation the energy-explanation had at once occurred to them, and they felt that it was not without interest and value. That particular diagram was not used for purposes of calculation, although it gave the same results as that shown in *Fig. 10,* which had been reproduced by a different method in *Fig. 23* (p. 16) by Professor Southwell. Professor Southwell drew attention to the small errors which might be introduced due to the obliquity of the intersecting lines, and that was a partial answer to the point which Professor Inglis raised in reference to Table

§ *Ibid.*

IV (p. 294§). The differences between the calculated and theoretical results shown there were partly due to that cause. It was, however, not always possible to state exactly the load that caused a transition of the pin, and some of the discrepancies were, therefore, due to experimental uncertainty; the Authors disagreed with Professor Inglis's suggestion that they might be due to an inappropriate treatment except to the extent that the points of intersection in *Fig. 10* might have been calculated instead of having been taken from the graph. In later work that had been done. **The Authors.**

The method of calculation suggested by Professor Inglis and illustrated in *Fig. 15* (p. 8) might prove somewhat lengthy in application, and the Authors believed that their construction was quicker than either that method or the alternative given by Professor Southwell. Experience alone, however, could show whether or not that was the case, and it was probable that the choice of any particular method would depend largely upon the personal predilection of the user.

Professor Inglis appeared to have misunderstood the Authors' arguments on p. 293§, and they regretted that their statement was perhaps not as precise as it might have been. Professor Inglis appeared to think that the Authors had invoked Castigliano's theorem, but that was not so; the argument had been based on a general theorem in mechanics, namely, that of minimum potential energy,* and there was, they believed, no question as to the validity of that appeal.

The attention of both Professor Inglis and Professor Southwell had been directed to that part of the Paper which dealt with the arch when it had assumed the three-pinned form, and that was, in the opinion of the Authors, an aspect of primary importance and interest. It ought not to be forgotten, however, as other contributors had pointed out, that in many cases the structure might never assume the three-pinned form, and design-methods ought to take that into consideration. With certain limits of load, if the abutments were rigid, the problem was essentially an elastic one and the assumption of rigid voussoirs could not then lead to a solution of the problem, so that strain-energy analysis or an equivalent method became essential. The Authors had not arbitrarily neglected the shear-deformation which was referred to by Professor Inglis. The effect had been calculated but it had been found to be quite negligible in the particular case considered.

The other contributors to the discussion had tended to overlook the object of the Paper and had criticized it from the point of view

§ *Ibid.*

* H. Lamb, "Statics," pp. 112 *et seq.* Cambridge, 1928.

The Authors. of application to design. The temptation to do so was natural, but it had been a deliberate decision on the part of the Authors to exclude that aspect at the present stage, as they wished to concentrate on the fundamental mechanics of the problem. Further work was in progress, and an arch of 10 feet span was ready for test in the laboratory. That was built of concrete voussoirs, with mortar joints and would, it was hoped, meet one of Mr. Wilson's points and show how the properties of the materials used modified the results which had so far been obtained on the steel model.

There appeared to be some misconception on the part of Mr. Chettoe and also of Mr. Adams. Whilst it was convenient for demonstration purposes to assume that the arch span was contracted or spread, reference to p. 297§ would show that the same critical load had been obtained when no movement of the abutments had taken place. Incidentally, those two contributors did not seem to be in agreement as to the possibility of abutment-movements, but there was sufficient evidence that such movements might occur and might be sufficient to destroy the monolithic nature of the structure. The arch might, however, still have a considerable reserve of strength, and it was surely of importance to form an estimate of that, especially in the case of existing bridges. In that connexion the remark of the President on p. 7 was significant. Whether or not that contingency ought to be considered in any particular arch was the responsibility of the engineer concerned, but since there was at least a possibility of its occurrence it could not be ignored without substantial reason. Mr. Adams had placed the correct interpretation on the statement on pp. 285§ and 286§. The pins and slots were used to prevent rotation out of the plane of the arch.

In a later Paper which was in process of preparation the Authors hoped to discuss the implications of the present work in relation to design, to explain in more detail the behaviour of the fixed-end arch, and to report on experiments in which the material used was a more normal one for that type of structure. A number of the points raised by Messrs. Wilson, Chettoe and Adams had important bearings upon that next stage. The Authors thanked them for their suggestions and hoped to be able to answer some, at least, of their questions when the experiments in hand were complete.

§ *Ibid.*