Westminster Hall roof

J. HEYMAN

Mr R. J. Ashby, Department of Civil Engineering, Imperial College

I am astonished that Dr Heyman has based his conclusions regarding the structural behaviour of the Westminster Hall roof timbers on the loading system shown in Fig. 16.

71. The actual roof rafters are closely spaced and paired. The ridge member therefore carries no load and cannot transfer a point load of 12 tons to the apex of the 'truss'. Unless some account is taken of the effect of the upper purlin shown in Fig. 2, no loads are transferred to the timbers immediately over the collar beam. Nor is it established that the main purlin carries a vertical load of 12.5 tons. Those rafters which carry down to the wall are so solidly restrained at this point that the whole of the load component along their length must be borne by the wall. The purlins are only required to cope with the components normal to the roof.

72. The presence of the dormer windows is a complication. If the lower ends of the rafters over the windows butt against the lintel beam and lean against the main purlin, then this lintel will transfer all of the longitudinal component of the load to other rafters and thence to the wall. Much will depend on the nature of the connexion between the rafters and the main purlins.

73. The assumption that the hammerbeam timbers form a truss in the engineering sense is of doubtful validity. Johnson's dictionary in the revised second edition of 1827 gives structural meanings to beams, posts, purlins, columns and rafters but attributes no structural meaning to 'truss'. The notion that the collection of beams and posts could be treated as a unified whole starts in the mid-nineteenth century.

74. Since ropes and pulleys operated by men or draught horses were Herland's only means of lifting material it is possible to deduce the construction procedure.

75. The bracket formed of the hammerbeam, wall post and brace could be constructed at ground level. It would weigh about 3 tons and it would not be impossible to raise and cog it to the wall top as a single unit. It would carry a large load, provide a working platform, reduce the gap to less than the length of the longest beam, and leave the area below clear for manoeuvring the heavy timber.

76. The erection of the hammer posts to be capped by the main purlins and collar beams would then become a straightforward task. The style of the corner bracing to these members is in keeping with the idea that they formed a stable framework independent of the subsequent additions.

77. A ridge member is an essential aid to the erection of the rafters and a secondary structure over the collar beams is needed to support it.

78. The rafters stand on their lower ends and lean on the purlins. Some pegging would be dictated by prudence, but solid structural joints at the purlins and at the ridge would be extremely difficult to make.

79. This argument suggests that the main purlin only resists the components of the dead load perpendicular to the roof, and that the members designated the lower rafters in Fig. 4 and all the timbers above the collar beam have very minor roles as load carriers.

DISCUSSION

80. Needham Market roof emphasizes the importance of the hammerbeam bracket as an aid to construction. There are indications from the form of the jointing that the central double portal frames were put together before they were positioned on the hammerbeams. In contrast, Earl Stonham is a simple multi-raftered roof with no structural need for the additions. The few brackets needed for erection purposes would be difficult to remove and are made unobtrusive by decoration which is carved to look much more substantial than it really is. Just how thin much of it is can be seen in Fig. 15.

81. The angels at Needham Market are no less impressive than those at Westminster Hall but it would be difficult to imply any structural symbolism to the former. It is the location in space at the end of a bracket which greatly enhances the illusion of free flight and fosters the deceit that the roof holds them in rather than that it actually holds them up.

Mr A. M. Muir Wood, Partner, Sir William Halcrow & Partners

I hope this interesting Paper forms the first of a series of analyses of ancient rather than modern structures. I confine my particular remarks to four points.

83. Why, in § 13, does the Author refer to three modules of 23 ft? If the upper rafter was the same length as the lower rafter, surely we are concerned with four rather than three modules, unless the concept is applied a little tenuously to the overall dimensions of the hall.

84. I had always assumed that the major purpose of the hammerbeam construction was to aid in the erection of the roof truss. Once the wall post, hammerbeam and lower brace are in position, with the wall post maintained truly vertical, it is possible to complete the assembly of the truss without falsework. A detailed knowledge of the joints would allow the order of assembly to be confirmed.

85. If, as stated, the truss is expected to exert appreciable outward horizontal thrust on the wall plate, under all assumed conditions of loading, then this would with advantage be positioned near to the inside face of the wall. Presumably this may be one of the conjectural details of Fig. 3. The position of the windows appears to lead to inadequate weight of masonry at the trusses to avoid tension in the walls, except at the alternate trusses where flying buttresses are provided, unless the roof loading is applied near the inside face of the wall or unless, as suggested below, a certain proportion of the load is in fact transferred to a level below that of the hammerbeam.

86. Pippard's treatment of curved struts (§ 24) is elegant but appears to ignore certain points. The curved braces are proportioned in such a way that each will contain a straight chord between effective panel points; this is true also for the arch rib in view of the presence of the puncheon at the position of the lower purlin (Fig. 2 suggests that no such puncheon exists) and the continuation of the queen post below the collar beam. It is in consequence suggested that the flexibility of each curved member is nearer 10 than 100 times as great as those of the other members (§ 24). For the lower brace in particular, the flexibility appears to be much less than the value assumed, with the load from this member effectively applied to the wall post at a level considerably above the foot of the latter. This would have considerable bearing upon the load distribution of Table 1, although it is probably reasonable to consider the reduced values of effective flexibilities only applicable to the curved members in compression.

Professor A. J. S. Pippard, Past President

87. In this Paper, one of several contributions by Dr Heyman to the study of mediaeval structures, the Author refers to work by myself and Glanville (now Sir William) in 1926 and it is therefore pertinent to quote from the prefatory note to our original paper. The then Director of Building Research, Dr R. E. Stradling (afterwards Sir Reginald), wrote as follows:

'In the course of the investigation of building failures undertaken as a normal piece of work at the Building Research Station, certain cases of roof failure presented considerable difficulties. In particular when curved members are present the ordinary methods of stress analysis are not always applicable. Professor Pippard, a member of the Structures Committee of the Building Research Board, voluntarily undertook to look into this special problem and the present paper, written in co-operation with Mr Glanville of the Building Research Station, is the result of his work.'

88. It will be seen from this and from the title of the paper (Ref. 6) that the main problem was the treatment of curved members, especially in the analysis of redundant structures, and Westminster Hall roof, suitably idealized, was used primarily merely as an example to illustrate a new technique. It was obvious even then that there were many reasons why the actual stresses in it would almost certainly differ from those calculated by any method whatsoever, and I am in complete agreement with the present Author that for a structure of this sort there is only a state 'here and now', which changes with time. I suggest however that it is a little sweeping to extend this dictum to all structures: some, I think, would not be sufficiently modified by age or conditions to invalidate the results of classical stress analysis, and the evidence adduced by Dr Heyman in support of his generalization, derived from particular types of steel-framed buildings, does not appear sufficient to justify it.

89. It is very difficult, probably impossible, to re-capture one's mental attitude after the lapse of forty years but mine was certainly not dictated by a belief (as suggested in a technical journal of the time) that stress analysis was an end in itself. During the 1914-1918 war and for some years afterwards I had carried sufficiently heavy responsibility in safeguarding aeroplanes against structural failure to avoid treating such matters as mere academic exercises.

90. It is important, if the questions raised by the Paper are to be kept in perspective, to realize the changed attitude towards the technique of strength calculations during the half-century or so which has just elapsed. In England, before the first war, the behaviour of any but statically determinate frames was understood only by a very few (notably H. M. Martin of *Engineering*) who had been influenced by Continental and American thought, and the philosophy of strain-energy analysis, although it had been discovered and enunciated by Castigliano in his classic treatise in 1879, was unheard of by the vast majority of our engineers. Even several years after the end of the war it had still not penetrated most of our universities.

91. Redundant frames (and considerations of redundancy excluded the difficult problem of rigid joints) were treated either by Rankine's method of dividing them into a number of simply-stiff trusses assumed to be pin-jointed, splitting the loads between these trusses in an arbitrary way and compounding the stresses thus determined or by the even simpler device of ignoring inconvenient members altogether. The effects of elastic interactions were completely unappreciated.

92. Progress from this ingenuous outlook on structural theory to the more rational treatment used by the present Author only evolved slowly against considerable opposition from British engineers, and the process of development, although fascinating to us who have taken part in it, may be tedious to those who have simply inherited the results. But from this historical angle, apart from others, studies of Westminster Hall roof are of great interest.

93. Although in appearance it is much the same as when it was built, it has in fact, as the Author emphasizes, been changing so much through the years that at any specified time the 'real' stresses in its members could be only a matter of guesswork.

94. The original designer's approach to the problem of behaviour must remain a matter for speculation: the structural form was probably sheer inspiration. The results of analyses made since, on different assumptions and using contemporary techniques, all seem to lead to the conclusion that only if it had been possible to

DISCUSSION

measure the actual loads in the members at any particular point in time could a reliable estimate of their values have been obtained.

95. Fortunately, as the Author points out, the important issue is the overall behaviour and not the assessment of the numerical values of stresses, and to an understanding of this behaviour he has made an important contribution.

96. His realistic qualifications in regard to plastic design, especially the need to restrict it to structures in which deflexions are unimportant, should be noted: disregard of this particular proviso could result in a building which, otherwise safe, would be dangerously flexible under wind loads. This comment does not imply criticism of the method but is simply a reinforcement of the Author's caution that, like any other, it has certain limitations which can only be neglected by the user at some risk.

97. In connexion with the remark that plastic design technique has *recently* been applied to masonry structures it may not be out of place to remind the Author of studies made some thirty years ago on the behaviour of voussoir arches^{25, 26} when it was shown, both theoretically and experimentally, how 'degradation' from a three-times redundant arch rib to a collapse mechanism occurred by the successive formation of four inter-voussoir hinges and this prediction of collapse load seems to be the essential feature of plastic theory in the Author's definition.

98. I find it rather difficult to accept the suggestion that the angels 'supporting' the roof were a joke: I have always assumed that they were a symbolic expression of dedication *ad maiorem Dei gloriam*. Undoubtedly, mediaeval craftsmen, and much later ones working on buildings in the Gothic tradition (e.g. Bristol University), had a lot of fun in representing real people as gargoyles and in carving punning allusions to proper names. In an old church in Somerset for example, two long-gone Abbots, Beer and Lamb, are commemorated by stonemasons of their day by carvings of a foaming tankard and a sheep. I imagined however that angels, unreservedly accepted in the Middle Ages as supernatural entities, and as such fully capable of supporting a roof truss, as the Author recognizes, would have been exempt from such levity. This, however, is purely a guess on my part: perhaps the Author has evidence which will resolve the question.

99. In conclusion I offer my sincere thanks for a fascinating and scholarly study of a kind which rarely appears in the *Proceedings*.

Mr R. J. Mainstone, Principal Scientific Officer, Building Research Station

For more than 500 years the great hammerbeam trusses of Westminster Hall have been a *fait accompli*, demonstrating by their support of the roof (until gravely weakened by decay) the adequacy of the structural principle embodied. Dr Heyman has nevertheless responded, like Harvey and Pippard, to the challenge they present to our understanding and he has added to the interest of his Paper with some provocative general comments in the conclusions.

101. I hope that, in his reply, he will expand some of these comments and, in particular, both explain the distinction drawn in § 58 between 'elastic' solutions and those which he has presented and comment further on the relevance, in the present instance, of plastic theory. It is difficult to follow the argument here and to reconcile the confident attribution of functions to the members in §§ 61-67 with the earlier suggestion that the plausible solutions 'are merely possible equilibrium states for the truss'. And was the situation observed by the Steel Structures Research Committee (and later in my own fuller tests at Whitehall Gardens²⁷) indeed the same as that which, in the analyses reported, led to different loads in the members for different assumed boundary conditions or even as that which would, in practice, introduce a measure of indeterminacy into the behaviour of the roof? The differences between calculated and observed stresses in steel-framed buildings are either purely local or the result of self-straining or of interactions between the frame and its cladding. Both local

effects and self-straining have, however, been ignored in the analyses and are unlikely to be important in practice because of the type of connexions, and there is no cladding to interact with the truss.

102. I have wondered whether sufficient distinction has been drawn, both here and elsewhere in the Paper, between the theoretical model of the truss which was analysed and the real truss, between the real truss as built and the much weakened and patched one which can now be seen, and between this Westminster truss and others which bear some resemblance to it. To 'construct reasonable equilibrium distributions' the engineer has to simplify and idealize but, if it is desired to understand how a structure carries its loads and not merely to demonstrate that it can do so somehow, it is essential to ensure that the theoretical model is sufficiently representative of the particular structure in the state considered. It is unfortunate, therefore, that the details of the Westminster truss were illustrated only in Viollet-le-Duc's partly conjectural drawings and that considerable weight has been placed both on details of other roofs which differ in important respects (as correctly noted by Baines in the case of the Eltham Palace roof) and on the present decay at the foot of the wall posts which is no more relevant than the more extensive decay in the neighbourhood of the wall plates to the behaviour of the truss as originally built.

Fig. 18. Westminster Hall Roof: details of construction based on Baines's survey. Members shown in broken lines on right are obviously of secondary importance and have been ignored in the discussion

DISCUSSION

Fig. 19 (left). Statically determinate analogue of truss assumed in deriving solution (m) for vertical load and, on right, estimated axial forces

Fig. 20 (right). Alternative analogue, with same geometry as Pippard's, assumed in deriving solution (n) for vertical load and, on right, estimated axial forces

103. Very complete data on the construction are available in Baines's report and an attempt has been made to illustrate it more adequately in Fig. 18. The following features are notable in relation to the pin-jointed truss analogue used by Pippard and Heyman:

- (a) the hammerbeam, collar beam, hammerpost and wall post are all continuous members and are stiff enough to develop considerable moments if subject to flexure at intermediate nodes;
- (b) the lower arch rib and curved brace terminate at about mid height of the wall post; they can carry load only if the wall post provides a vertical reaction (by compression at the foot) and a horizontal one (by flexure associated with tension in the hammerbeam), and they are both stiffer, by an order of magnitude, than the notional equivalents of Pippard's analogue;
- (c) only between E and H' and in the collar can appreciable tension be developed and there can be little horizontal reaction between the hammerposts and the collar where the section of the post is greatly reduced and that of the tenon is only 2% of that of the post; since, also, the upper and lower rafters are connected only through the post there can be little transmission of thrust from one to the other.

104. Clearly the pin-jointed truss analogue misrepresents the range of behaviour of which the real truss was capable in several important respects and it is difficult to regard any of the solutions presented as a 'possible equilibrium state'. Consideration of the deflexions of the linear analogue shown on the right of Fig. 18 suggests, however, that it can be considered as two largely independent parts—an upper triangle including the collar, and a lower part comprising chiefly the arch ribs, hammerbeams and hammerposts, the lower braces and the wall posts—and that the behaviour of the lower part will approximate closely to that of the statically determinate system shown in Fig. 19 under vertical load. The lower rafters can be ignored in relation to the primary action of the truss because the relative stiffnesses are such that they will go

Table 2

Member	Node	Flexibility $in/ton \times 10^3$	Heyman basic (a)	Heyman full (b)	Pippard (g)	Flexibility $in/ton \times 10^3$ or EI* $ton in.^2 \times 10^6$	Mainstone Fig. 19 (m)	Mainstone Fig. 20 (n)
Crown post	DG	3.12	0	-4.6	-14.0		-8.6†	-11.7†
Upper rafter	FG	3.08	-7.29	-4.5	1.0		-2.1†	-0.15†
Collar	{ FD	1.26	-8.56	-6.7	4.0		1.25†	0.08†
	{ D						0†	0†
Lower rafter	EF	2.59	-22.46	-16.4	8.5		0	0
	{ FC	0.31	0	-2.7	-18.5	0.31	-14.2	-12.6
Hammerpost	{ CH	0.43	0	-0.1	-7.1	0.41	-8.25	-6.0
	{ EB	0.19	0	-2.7	-4.8	0.17	5.7	0
Hammerbeam	{ BH†	0.42	0	0.1	6.4	5.44	429	330
	{ H'					5.44	297	2.9
	{ H/H						0	
Arch rib	{ AB	7.10	0	-5.0	-20.3	0.69	-18.5	-21.05
	{ BC	2.68	0	-6.3	-25.4	2.74	-12.9	-16.1
	{ CD	13.2	0	-4.6	-18.6	11.3	-9.0	-11.75
Lower brace	AH†	696	0	-0.13	-7.5	58.0	-9.5	-4.65
Upper brace	HD†	960	0	0.00	2.8		0	0
Wall post	{ EA	3.20	(-24.00)	-19.0	1.5	5.11	0	0
	{ AA'						786	
							-24.0	
Thrust at wall plate			12.7	12.0	0		0	0
Thrust at wall post corbel			0	1.3	9.7		7.7	10.15
Deflexion of hammerbeam, in			0.08	0.11	6.7		0.28+0.09x§	

Loads in tons (tension positive); moments in tons inches in italics; * values of EI in italics; † for no deflexion of collar beam; § x = spread of walls at corbel level, in ; ‡ BH, AH, HD respectively for solutions (a), (b), (g) and (n).

DISCUSSION

slack and, though it is less certain that there will be no external reactions at E or E' the relative stiffnesses again ensure that these reactions will be small and hardly significant. At F, F and D the ridge load will be distributed by the collar in whatever ratio is required for equilibrium of the lower part, there being little associated deflexion of the collar unless there is considerable lack of fit between the principal members of the upper triangle.

105. The axial forces in the members calculated for the system of Fig. 19 are represented on the right half of the figure in the same manner as in Fig. 17 and both they and the moments at the intermediate nodes of the hammerbeam and wall post are given in Table 2 as solution (*m*). The calculations were made with lengths and sections of members scaled *ab initio* from Baines's drawings and the full validity of the solution is sensitive to some of these. Accepting them as correct and ignoring initial lack of fit and relative shrinkage, the hammerbeam would be just clear of the wall post at E and just in contact with the wall plate at E', but the reaction at E' would be negligible and the effect on other loads correspondingly small. The horizontal movement of F relative to D is sensitive to vertical deflexion of the collar but, for the near-zero deflexion to be expected, this movement is zero for a spread at the wall corbels of about 1 in. E and F separate by about 0.4 in.

106. The solution is compared in Table 2 both with Heyman's basic and full solutions (*a* and *b*), with Pippard's No. 1 (*g*) and with a further solution (*n*), illustrated in Fig. 20, in which the geometry of Pippard's frame is retained but a statically determinate mode of behaviour similar to that of solution (*m*) is assumed. Solution (*n*) is, of course, open to many of the objections made to the original solutions but, being intermediate between these and solution (*m*), it helps to clarify one of the most important features of the truss—the manner in which the horizontal thrust is both carried well below the top of the wall and much reduced in magnitude by the combined adoption of the hammerbeam principle and of an arch rib and brace meeting the wall post at about mid height.

107. I agree that the 'actual' state of the truss at any time is essentially indeterminate. Baines's survey showed, moreover, that there were differences between the trusses. Solution (*m*) is not, therefore, an 'absolute' one. I consider, nevertheless, that it represents better the functions of the principal members than any of the solutions presented in the Paper and, if I am correct in this, it is worth noting that it shows these functions to be much closer to those previously ascribed by men like Viollet-le-Duc and Baines. They are also closer to what could be inferred about Herland's intentions from the relative proportions of the members and details of the connexions. In defence of Viollet-le-Duc, it should also be stated that there is no evidence whatever that he was misled by the common mediaeval (and earlier) conceit of the angels.

108. Finally, if it is desired to estimate the collapse load of a structure like the Westminster roof, is it necessary to invoke the safe theorem of plastic theory? Since appreciable self-straining is not possible it seems (intuitively) that the collapse load cannot be less than that estimated for any plausible statically determinate analogue of the structure arrived at by setting redundant loads at zero, and, if so, there is no need to assume ductility where none existed. Would Dr Heyman care to comment on this?

Dr J. Heyman

My references to plastic theory seem to have caused some confusion. It is not, of course, the collapse aspects of that theory which are in question here; whatever their differences in detailed interpretation, I think that I and all the contributors to the Discussion are agreed that the roof of Westminster Hall is, as originally designed, a long way from collapse. Rather it is the lower bound theorem of plastic theory that is involved. Perhaps in contexts like these the word 'plastic' should be dropped, and

the method referred to as one of lower bound design, safe design, equilibrium design, or some such description.

110. As was mentioned in the Paper (§ 56) *any* distribution of forces in equilibrium with the external loads can be used as a safe basis for analysis or design. In the present context the determination of a set of equilibrium forces involves two distinct steps, although there may be some interaction between these two steps. First, a conceptual model must be made of the roof, that is, it must be reduced to some skeletal form more or less amenable to structural analysis; second, a reasonable structural analysis of the model must actually be made.

111. This second task is not simple for any structure, and there are particular difficulties with the roof at Westminster Hall, whose joints might have considerable play, whose supports might spread and settle, and whose loading distribution is not known with any certainty.

112. My intention was, as Professor Pippard notes, to attempt an explanation of the overall mode of action of the roof, and not to make a detailed assessment of the numerical values of the stresses. The stress levels are in any case so low (§ 22) as to be of negligible importance to any account of overall structural action.

113. Thus the investigation must be directed towards the determination of a plausible set of forces for the roof, that is, a set of forces that can be imagined might actually occur, that does not contradict any of the known facts about the roof and its construction, and that does not require the roof to develop excessive deformation.

114. It was this last requirement, for example, that made me unable to accept any of Harvey's 'solutions', or Pippard's solution No. 1, whereas Pippard's solution No. 2 seemed plausible, and coincided in all essential features with my solutions (a)–(e) in Table 1. It is true that the large and unacceptable deflexions associated with Harvey's proposed modes of action depend directly on the values of stiffness assigned to the curved braces, and both Mr Muir Wood and Mr Mainstone think that these braces are much stiffer than I had assumed. Some numerical calculations on this point are made below.

115. However, the essential point is that my solutions (a)–(e) and other similar solutions (e.g. Pippard No. 2 referred to above) lead to very small deflexions of the roof, even with very weak braces, or indeed with the braces entirely removed. Harvey's solutions, on the other hand, or Mr Mainstone's proposed mode of action, discussed below, *demand* that the lower curved braces are both present and an order of magnitude stiffer than noted in Table 1.

116. This brings up in an acute form the question of the first task of the designer, that of making a suitable model for analysis. I agree with Mr Ashby that the idea of truss action is relatively modern (so, incidentally, are the angels at Needham Market, § 81); mediaeval architects would have had no concept of the structural analysis of a simple framework. Since the forces could not be 'seen', the corresponding mediaeval structure does not generally reflect these in the way that a modern building exhibits its mode of action. Very often a mediaeval roof seems to be a random collection of beams and rafters, and the engineer may well despair of undertaking any simple analysis.

117. This is not the case with Westminster Hall, however (nor indeed with the great Gothic cathedrals of a century or two earlier, whose rib vaults and flying buttresses express very clearly the way the structure supports itself). At Westminster there is a clearly marked truss every 20 ft of the Hall. These trusses carry purlins which in turn carry the common rafters. Hugh Herland may not have thought of them as trusses (although he obviously had clear notions of the strength inherent in triangulation), nor may he have been able to make a modern structural analysis, but his roof makes it clear to the eye that its strength lies precisely in those great trusses that were the subject of the Paper.

118. It is entirely possible that some of the features of hammerbeam construction were devised, as Mr Ashby and Mr Muir Wood imply, as aids to erection, and cer-

DISCUSSION

tainly the roof could have been built with a minimum of falsework. This, however, was not the prime concern of the Paper, which was with the roof as built. Thus the ridge member may be, as Mr Ashby remarks, an essential aid to erection. I cannot, however, accept Mr Ashby's view that the ridge member does not transmit a load to the main truss.

119. I may well have made a poor estimate of the value of this load, but this is not relevant to the point of the Paper. The actual values of the forces in the members of the roof are of secondary interest, since they are so small; it is the way in which a load applied at the ridge is transmitted to the ground that is important.

120. For example, I referred (§ 59) to the small effects of assuming incorrect values for the flexibilities of various members. The truss of Westminster Hall is, according to my conceptual model, so 'nearly' statically determinate (in the sense that the redundant members are all very weak) that I believe the essential structure under vertical load to consist of nothing more than the rafters and the collar beam (Fig. 12(a)). In the original Paper it was shown that this basic pattern was modified only very slightly by the inclusion of the curved braces (ignoring for the moment the great arch) (Fig. 12(b)).

121. Since both Mr Muir Wood and Mr Mainstone suggest that these braces should be an order of magnitude stiffer, I have recalculated Fig. 12(b) for flexibilities of 69.6 and 96.0×10^{-3} in./ton for the two braces (cf. the values given in Table 1). The results are given in Fig. 21 and, comparing with Fig. 12(a), it will be seen that the forces in the main members still differ by less than 5% from their 'basic' values. The thrust at the wall plate is reduced by some 7%, from 13.5 to 12.5 ton.

122. There is nothing here to alter the original conclusion (§ 65) that the function of the curved braces is not to carry vertical load on the roof, but to resist movement of the roof under wind loading. At Westminster they do not have even this structural

Fig. 21. Forces in analogue of Westminster Hall, with stiff curved beams

function, which is taken over by the great arch rib. Thus my view remains that the lower curved braces at Westminster have no present structural function, although they would have been essential in the erection of the truss.

123. However, Mr Mainstone proposes a radically different model of the truss from the ones that, I believe, represent most closely the actual behaviour. Mr Mainstone's model is close to that used for Pippard's solution No. 1, with the difference that the upper curved braces and the lower rafters are assumed to go slack, and are hence removed from the analysis.

124. This removal of slack members is very reminiscent of Castigliano's treatment of masonry arches²⁸; it is also reminiscent of Professor Pippard's description (§ 91) of the early twentieth century engineer ignoring altogether inconvenient members in a redundant frame. (That this situation is now completely changed is largely

due to the influence that Professor Pippard has himself had on the teaching of structural engineering).

125. Let me say at once that I cannot accept that a roof truss of the type at Westminster Hall is in practice supported vertically by the corbels at the feet of the wall parts, in the way required by Mr Mainstone. The corbels may have been of use in erection; in the final state, however, the main function of the wall post is to prevent sliding on the top of the wall.

126. Brandon²³ and Cautley¹¹ are quite clear on this point, and Cautley quotes several instances of corbels which, while appearing solid, cannot in reality carry load, or which are not in contact with the feet of the wall posts; in many cases there are no corbels present at all. At Westminster, the fact remains that Baines found considerable decay at the feet of the wall posts, so that they were *not* transmitting load from the roof.

127. In addition to this fundamental objection, I have already noted that Mr Mainstone's solution demands that the lower curved brace be regarded as an essential part of the structure. There is a further anomaly in that the collar beam is required to be in tension. This member, and indeed all the members of the truss, are ill-adapted to tension; in particular, the collar beam is the main compression member resisting the inward bending of the two rafters.

128. A simple couple roof, over a small span, is shown in Fig. 22(a), and the expected deformation of the rafters is shown dotted. To prevent this bending, a collar beam can be inserted with advantage, Fig. 22(b). The rafters will, however, be weakened by this insertion, and deformation of the type shown dotted in Fig. 22(b) is still possible, to which reference was made in the Paper (§ 27). Further braces could be added as in Fig. 22(c) and these were sometimes extended as full scissor braces. Alternatively, the truss of Fig. 22(b) could be braced by the hammerbeam type of construction considered in the Paper.

129. I must emphasize my point (§ 60) about the 'skeletal' nature of my solutions to the problem. Thus, while the primary functions of the various members are as given in § 61 *et seq.*, it is certain that some members are fully capable, as noted by Mr Mainstone, of carrying some degree of bending, and indeed *will* carry some bending.

130. A major point of difference now becomes apparent between the approach of Mr Mainstone and that of myself. The plastic theorems give the assurance that an equilibrium solution is safe; for example, if a solution can be obtained (as in the Paper) in which bending is ignored, then any actual bending that may occur cannot weaken the truss. Thus, when a plausible and otherwise satisfactory account has been obtained of the primary mode of action of the truss, I can rest content, even though this primary force system must in practice be subject to the modifications imposed by inevitable imperfections of construction.

Fig. 22. Mediaeval roof types : (a) couple roof; (b) roof with collar; (c) braced roof (scissor-beam)

DISCUSSION

131. Standard elastic techniques were used to find the primary force system, but, as was pointed out in the Paper, the solution was not really an elastic one. Since the practical imperfections are both unforeseen and in any case incalculable, the solution can be regarded as no more than a plausible equilibrium state for the truss. However, all my solutions (a)–(e) are very close to each other, and, moreover, they are all insensitive to those practical imperfections that make an 'elastic' solution of a redundant structure so suspect.

132. Thus I believe my account of the action of the roof of Westminster Hall to be essentially correct. The primary force system does not seem to violate any known requirements dictated by the construction of the trusses, and it implies very small deflexions. As a 'safe equilibrium' designer, I wish to know no more.

133. Mr Mainstone, however, wishes 'to understand how a structure carries its loads'; if I follow him correctly, he would like details of those actions, such as bending imposed by continuity of the members, which I find to be of secondary importance. His solution is sensitive (§ 105) to the sections of some of the members, and ignores initial lack of fit and relative shrinkage. Should these occur, as they must, Mr Mainstone's solution, Fig. 19, would be considerably modified.

134. By contrast, my solutions of Fig. 17 (d) and (e) would be modified hardly at all by even quite gross practical imperfections, and they have already been shown to be insensitive to the precise stiffnesses assigned to the members.

135. I must confess here to some circularity in my train of thought. As a designer, I believe that any structure is a bad structure when it is sensitive to small changes in the numerical values of constants; and not only bad, perhaps dangerous. We have the evidence of 500 years that Herland's roof is a stable structure, and I would always seek, therefore, a solution which is also 'stable'. It is for this final reason that I find Mr Mainstone's account unsatisfactory.

136. Mr Muir Wood remarks on my reference to the span of the Hall being made up of three modules of 23 ft. The radius of the great arch rib is two-thirds of the span, so that the setting out points for the arch can be referred to a division of the span into three modules. As Mr Muir Wood implies, the hammerposts are placed at quarter-span.

137. The top of the masonry wall is about 43 ft from the floor of the Hall, and the wall is some 8 ft thick. Ignoring for the moment the cut-out of the window, a 20 ft bay of masonry would thus weigh more than 400 tons. Such a wall can resist at the top a horizontal thrust of 13 tons without the line of thrust falling outside the middle third, and it would need a thrust of about 40 tons to overturn the wall. The presence of the windows will reduce this margin of safety, but the flying buttresses appear in the light of a necessary precaution rather than as essential for stability. Certainly the mere violation of the middle third rule does not imply imminent collapse, as was indeed amply demonstrated by Professor Pippard in his investigations of the voussoir arch^{25, 26}.

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