Paper 6280

The stability of tall buildings †

by

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Discussion

The Chairman, said that with this work, as with all research, what was important was the application of the results in everyday practice. If he had read the Paper aright, it seemed to him that the Author was really advocating that in design one should, calculate on a composite structure, so arranged as to eliminate sway, which meant taking into consideration the infilling or any other factor which contributed towards the stability and strength of a tall frame. In practice, of course, that was extremely difficult. Many frames were designed without any very clear idea of the details of the infilling. He did not know if that was the fault of architects or of engineers, but it certainly happened in practice, and he was very well aware of it, because as the Engineer to a local authority he was responsible for checking literally hundreds of structures of the nature in question which were submitted by structural engineers; at the time that they were submitted and had to be approved within a statutory period there were no details of infilling, or at least not sufficient details to make it possible to make proper calculations of the rigidity.

65. If the design were so accurate and economic as to have taken note of all the strengthening effects, which in most buildings would be due to infilling rather than bracing, what happened when, as was usual, somebody decided to cut openings in the infilling? Presumably the factor of safety would have to take care of it.

66. Crude though the observation might be, therefore, it was important to remember the practical conditions of application, while at the same time acknowledging and being grateful for the very meticulous and careful analysis of the factors themselves in formulating improved methods of design.

Dr M. R. Horne (Lecturer, Department of Engineering, University of Cambridge) said that there was one point of great practical importance on which he would place much greater emphasis than did the Author, namely, in relation to strain-hardening. In the Author's second example of frame instability, a four-storey frame bent " strong ways", the Author had stated in § 54: "it is unlikely that strain-hardening occurred except at the centre of the third-storey beam." It was important to remember that the conception of a plastic hinge was an idealization. Unless a hinge formed in a region of uniform bending moment, it never behaved as an ideal hinge and consequently its stiffness did not vanish. This was a direct result of strain-hardening and should be taken into account when dealing with the effect of plastic hinges on stability.

68. Fig. 24 showed experimental load/deflexion curves for 8-in. × 4-in. joists submitted to single- and two-point loads. For a two-point load (joist J.3), where between the loads the beam was under a uniform moment, the plastic hinge conception of zero rigidity at the plastic collapse load was almost attained, but under a single-point load (joist J.2) the beam retained a rigidity which, although much less than the elastic value, was far from negligible. The reason for this positive rigidity became evident when a

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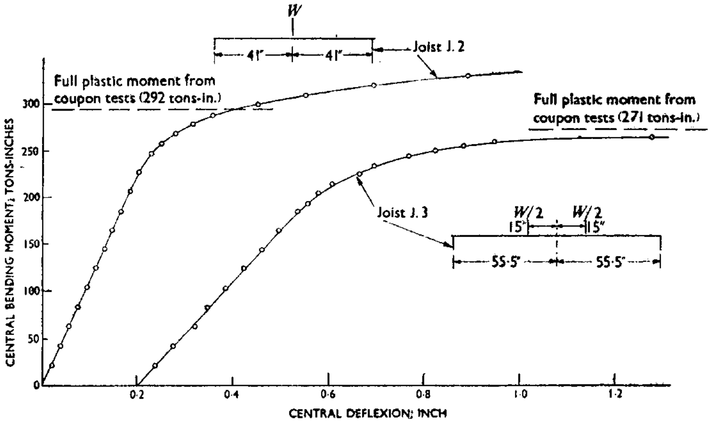


FIG. 24.—BENDING TESTS ON 8-IN. × 4-IN. ROLLED STEEL JOISTS

centrally loaded joist was coated with plumber's resin, thus revealing the yield lines during plastic deformation. The yield lines were observed to have a limited penetration towards the central axis, the curvature being limited by strain-hardening in the flanges. To increase the deflexion of the beam, it was necessary to increase the applied load and thereby induce plastic deformation to spread along the beam away from the section of maximum bending moment. The idealized condition of zero rigidity was in fact never realized.

69. It followed from these considerations that misleading results would be obtained if strain-hardening were ignored when dealing with the stability of partially plastic structures. The stress/strain curve for mild steel (Fig. 25a) was too complicated to

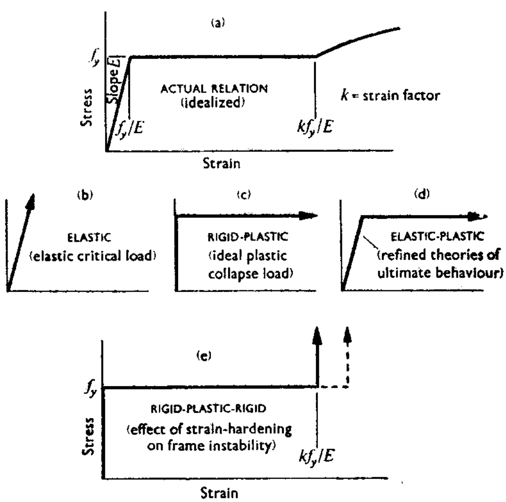


FIG. 25—POSSIBLE BASES OF THEORETICAL DESIGN

provide a manageable theory. The elastic theory of structures (Fig. 25b) considered only the elastic line. If this line was assumed to be infinitely long the collapse load was the elastic critical load. The simple plastic theory ignored elasticity and assumed an infinitely long plastic range (Fig. 25c). Refined theories of ultimate behaviour had assumed an elastic-pure-plastic relation (Fig. 25d), this being the basis of much work at Cambridge and of the present Paper.

70. Such theories were already highly involved, and, if strain-hardening was to be included, some drastic simplification became almost essential. Dr Horne had therefore studied what might be achieved by a rigid-plastic-rigid theory, as shown in Fig. 25e. A finite plastic range was assumed, after which the material was assumed to become rigid. This gross simplification had been suggested by careful observations on full-scale tests. Its validity depended on its success in interpreting the behaviour of such frames quantitatively. Small-scale tests using members of rectangular or tubular section could not here be expected to give reliable results, since their strain-hardening behaviour differed radically from that of rolled members.

71. Fig. 26 showed again the load/deflexion curve for an 8-in. × 4-in. joist under a single-point load. The dotted curve (Roderick[34]) had been obtained from a refined theory which took account of the true stress/strain curve as deduced from coupon

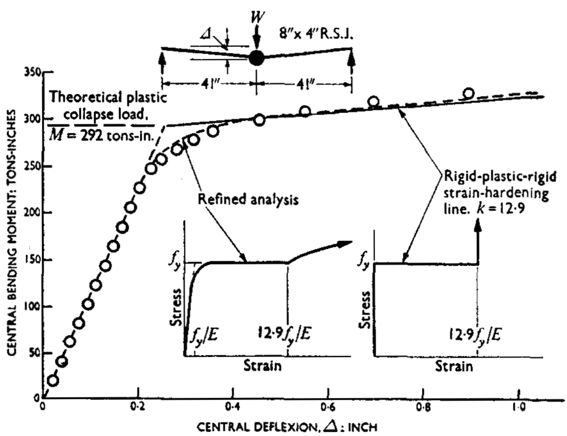


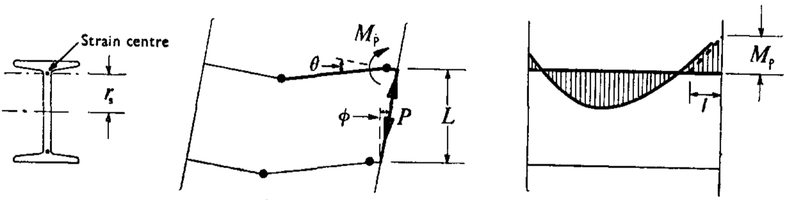
FIG. 26—BENDING TEST ON 8-IN. × 4-IN. ROLLED STEEL JOIST

tests. It agreed well with the experimental points. The rigid-plastic-rigid theory could be used to predict the *slope* of the curve after the simple plastic collapse load had theoretically been attained. There was thus obtained the continuous line in which the pure plastic deformation had been assumed to be equal to kf_Y/E, the strain at the beginning of strain-hardening as derived from coupon tests. The agreement between this apparently crude theory and the experimental points was remarkably good. It was thus reasonable to apply rigid-plastic-rigid theory as a test for the stability of frames at the plastic collapse load. It was found that a mechanism was stable provided that the inequality (1) in Fig. 27 was satisfied. The left-hand summation was made for all plastic hinges, where μ (called the strain-hardening factor) was the reciprocal of the strain at the beginning of strain-hardening. The plastic hinge

[34] References 34 *et seq.*, are given on p. 521.

rotation was θ; M_P was the full plastic moment, l was the distance from the hinge to the point where the tangent to the bending-moment diagram intersected the zero-moment axis, and r_s was a dimensional property of the section as shown. The right-hand summation was made for all members which carried axial thrust, ϕ being the rotation of a member of length L and P the axial thrust in that member. If this inequality was satisfied, the frame might be expected to show no reduction of carrying capacity below the simple plastic collapse load.

72. This theory did not allow for the flexibility of the structure due to elasticity. An improved treatment made allowance for this and led to the inequality (2). Here λ_c was the elastic critical load factor and λ_p was the ideal plastic load factor.



$r_s = I/S$ = Strain centre radius
I = Second moment of area
S = Plastic modulus

By rigid plastic theory:

$$\mu \Sigma \theta^2 M_p r_s/l > \Sigma \phi^2 PL \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where $\mu = E/kf_Y$ = strain-hardening factor

Making allowance for elastic deformation:

$$\mu \Sigma \theta^2 M_p r_s/l > \frac{1}{1-\lambda_p/\lambda_c} \Sigma \phi^2 PL \quad . \quad . \quad . \quad . \quad . \quad (2)$$

FIG. 27.—STABILITY CONDITIONS

73. Fig. 28 showed the experimental load/deflexion points for a rectangular fixed-base frame.[35] This frame showed high strain-hardening and the simple plastic collapse load was easily exceeded the slope of the strain-hardening curve was predicted reasonably well by rigid-plastic-rigid theory.

74. In June 1958, a pair of slender pitched-roof portal frames had been tested to destruction at Abington, the frames being of 30 ft span and composed of 5-in. × 3-in. I-section, as shown in Fig. 29. Elastic-plastic analysis led one to expect an 11% reduction of capacity due to frame instability, but in fact, as Fig. 30 showed, the simple plastic collapse load of 4·88 tons had been comfortably exceeded. This result was in accordance with rigid-plastic-rigid theory, according to which the load/deflexion curve should have a positive slope given by the continuous line when collapse mechanism had formed.

75. Dr Horne had applied the theory to the two four-storey frames analysed in the Paper, the results being given in Table 4. The final column showed the maximum strain factor k which might be tolerated in the material if the structure was to be in neutral equilibrium when the plastic mechanism formed. For mild-steel sections the strain factor k (the ratio of the strain at the beginning of strain-hardening to the elastic strain at yield) had a value lying between about 6 and 15. The "weak-ways" frame could tolerate a strain factor of only 2·6 and would thus be unsuitable. The strong-ways frame, however, would tolerate a strain factor of 25 for concentrated loads and of 34 for distributed loads. On the basis of this theory, one would therefore judge that

TABLE 4

	Elastic critical load ÷ ideal plastic collapse load: $\frac{\lambda_C}{\lambda_P}$	Theoretical reduction of collapse load below ideal plastic load (*v.* wood) $\frac{\lambda_P-\lambda_0}{\lambda_P}\%$	Greatest strain factor *k* which will maintain stability at ideal plastic collapse load
Frame weak way FIG. 11	1·57	22	2·6
Frame strong way FIG. 18	6·0	12	25 (Point loads) 34 (Dist. loads)

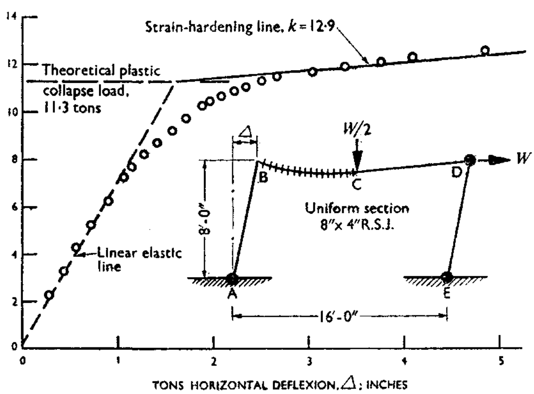


FIG. 28.—TEST TO DESTRUCTION OF FIXED-BASE RECTANGULAR PORTAL FRAME

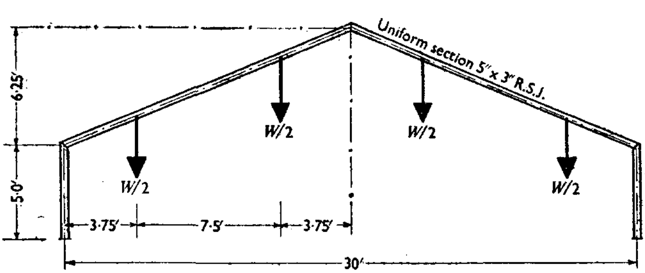


FIG. 29.—TEST TO DESTRUCTION OF 30-FT PITCHED-ROOF PORTAL FRAME ABINGTON 1958

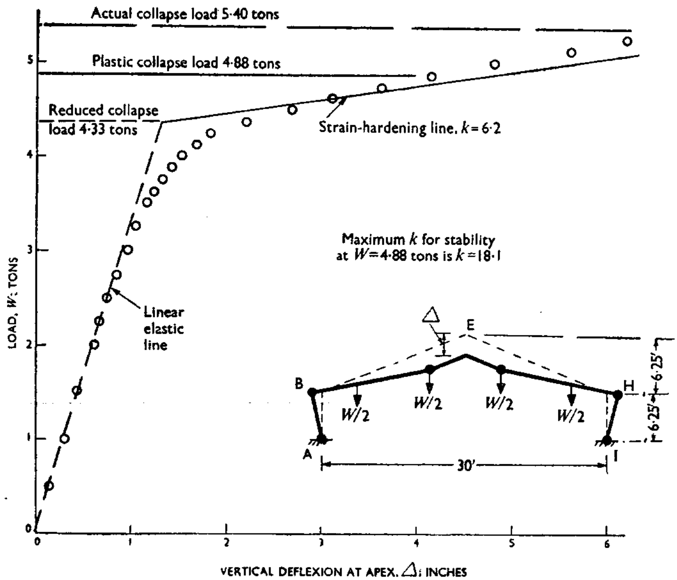


FIG. 30.—MODE OF COLLAPSE AND LOAD/DEFLEXION CURVE FOR FRAME IN FIG. 29

the Author's second frame would not actually show any reduction of carrying capacity due to frame instability.

76. Dr Horne emphasized that his calculations were not as yet based on sufficient experimental evidence to be accepted as final. Considerable data on the effect of strain-hardening were available, however, and he hoped he had shown how misleading it might be to ignore this very important factor when discussing the overall stability of partially plastic structures.

Professor Wilfred Merchant (Professor of Structural Engineering, Manchester College of Science and Technology) said that if the Paper was thoroughly understood by structural engineers, a revolution would have arrived.

78. The Author had described the method leading up to the combined elastic and plastic design, and Professor Merchant was particularly interested in the final section of the Paper, for at Manchester they had been working in the same field. He felt that he should say something about the background. They had been asked to produce simple diagrams by which structural engineers could check their calculations and which could be used by engineers who had the responsibility for approving designs. To the best of his knowledge, there had not previously been anything available in the country for checking such tall narrow buildings. The Code of Practice specifically excluded the problem. Before the research work which the Author described there had not been a logical method of dealing with such problems and no simple diagrams applied to them. But, of course, buildings had been erected and had remained standing.

79. The Author had made a very valuable remark in § 62, that "it is dangerous to attach too much importance to the apparent reliability" of present methods. The buildings which had been standing up must have been helped considerably by their cladding and so on. Engineers were beginning to understand what happened when the cladding was removed.

80. In the Paper reference was made to only three cases of sway. Professor Merchant referred to the results of some work done at Manchester. The calculations were very complicated, and he hoped that before long, and perhaps in the immediate future, digital-computer programmes would be devised, and then there would be a large number of examples available; but in the mean time, and even with present methods, Dr Salem had calculated more than 300 cases.[36, 37]

81. In Fig. 31 the ratio of failure load to rigid-plastic load was plotted against the ratio of the rigid-plastic load to the critical load, and some of the 300 results were shown. In the Paper the Author had stated that the plastic load was more important and should be given more prominence. What the Manchester team had found (which was amazing

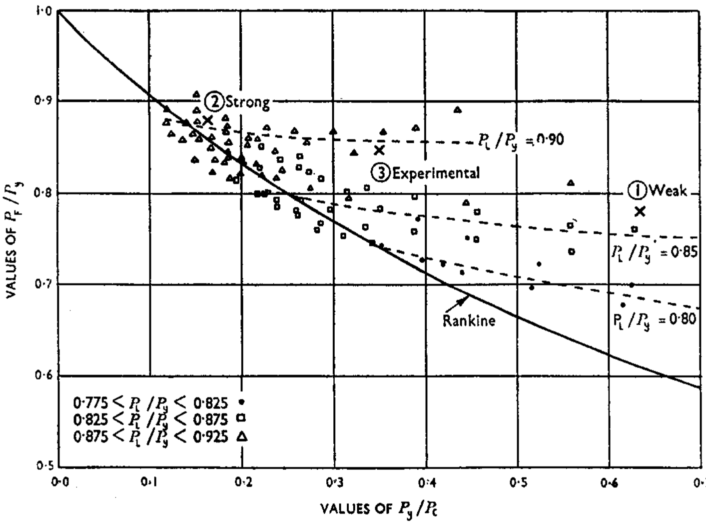


FIG. 31

and for which they did not offer a theoretical justification) was that the Rankine load served as such a good lower bound. The Rankine load was very simply obtained. The Author's three examples plotted quite nicely on Fig. 31. These and the results worked out at Manchester told much the same story, and from the engineering point of view it was now a very simple story. The Rankine curve was almost an acceptable lower bound.

82. In order to take advantage of the loads above the Rankine curve one had to do more refined calculation; but engineers would be taking their profession a long way from superstition if they started to use some of the elementary lower bounds instead of continuing in the rather awkward position that, as engineers, they had always to put up structures whether they understood how those structures acted or not, that the only way to do it was to have rules and, if the structures did not fall down, they could continue to use the rules. Safe structures had been put up in that way, but he would submit, so far as the particular problem under consideration was concerned, that it was not true that the research teams were trying to complicate something for which there was already a simple solution; in fact they were trying to introduce simple principles where no principles at all existed at the moment.

Dr Edgar Lightfoot (Reader in Civil Engineering, University of Leeds) said that with the post-war developments in structural theory, and particularly those due to Professors J. F. Baker and W. Merchant and to the Author, it was now possible to survey the general problem of the strength design of structures with far greater understanding. Such a perspective of the structural design field was really a logical requirement in an introduction to the subject of the theory of structures at the undergraduate level in a university, and the Author would probably be pleased to know that his Paper had already been used in this way in early lectures at Leeds this year. However, the general philosophy of structural design still needed further consideration, and it was necessary to consider other relevant matters, such as the effects of non-ductile materials, fatigue, and the like, in structures. Had the Author looked into the general non-linear theory for structural behaviour, which appeared to be the next analytical development? A linear elastic theory depended on the assumption of an indefinite linear stress/strain relation and neglected instability effects; it also neglected non-linear elastic restraints and changes in the geometry of the structure, so it was hardly satisfactory for present purposes here.

84. Was it possible to formulate a general non-linear elastic analysis, which would take into account the degenerating stiffnesses in structural members as the yield stress was exceeded? The differential analysing machine used at the Building Research Station seemed to have produced remarkable curves in Fig. 4. These would probably mean more to the structural analyst, however, if they were expressed in terms of stiffnesses, particularly since a basic theory in terms of such deteriorating stiffness during non-linear structural behaviour would surely be established soon.

85. The calculation of the collapse load by the Merchant-Rankine theory involved two separate major calculations, one for the load factor against plastic collapse and another for the load factor against elastic instability. The actual load factor for collapse was likely to be between 1·5 and 2. It was possible to get to 1·5 by straightforward linear elastic analysis, but was it possible to progress in the range from 1·5 to 2 by means of similar methods which could be developed in terms of non-linear elasticity? It did not seem far to go.

86. Collapse could be defined by several different (but related) criteria. It was generally desirable to avoid load/displacement graphs in rapid calculations, so the idea of zero deteriorating stiffness seemed a promising alternative. The Author had mentioned something on these lines. This seemed to be a further reason for giving the curves in Fig. 4 in terms of deteriorating stiffnesses. Incidentally, the simultaneous occurrence of zero stiffness at each and every joint and storey did not seem to be a necessary requirement. That it should be assumed so had been taken over from elastic instability theory, but ideas should not be transferred in that way without question. The collapse of frame No. 3 illustrated this point. The Author called it an unexpected collapse, but it should not be unexpected if non-linearity was properly taken into account.

87. In Fig. 19, where the Author had plotted the deteriorating load factors, it would have been to put them in order of development as the plastic hinges formed, instead of giving a random collection. If, in addition, the load factors against elasto-plastic collapse with instability ignored had been given in these respective cases, it would then be possible to plot the deteriorating elastic instability and the increasing elasto-plastic load factors as the hinges were developing towards collapse. The intersection of these two curves would give a good indication of the actual load factor for collapse. This method of presentation had already been utilized by Professor Merchant in some of his early papers. Instead of deflexion, either equal horizontal intervals could be used for the different stages, or "percentage plasticity" could be used as a variable.

88. There was naturally great interest in how the collapse load factor varied with the method of design employed and the quantity of steel required. It should be possible to show that the actual load factor against collapse was being maintained with these refined, or even improved slightly, while the weight of steelwork was being reduced. Had the Author any information on this?

89. What was, ultimately, going to be the best design method for steel building frames? Several had been postulated. The very rapid moment-distribution methods,

which had been evolved recently, were now being successfully employed on skyscraper designs, using the conventional permissible stress methods. There was the very attractive method of plastic-collapse design which Dr Horne had evolved at Cambridge. Could that be developed to allow, fairly easily, for instability effects as well? Lastly, there were the rapid design methods using nomograms. If these were the final answer, engineers had better come to accept them. (At present they did not like them and would certainly prefer an overall method which they could more easily understand.) It seemed very pertinent now to ask: "Where do we go from here?"

Professor B. G. Neal (Professor of Civil Engineering, University College of Swansea) referred to the Author's emphasis on the importance of preventing side-sway in tall buildings and his suggestion that this could be done in one of three ways: first, by constructing a stiff spine or end-frame; secondly, by bracing, or thirdly, by making use of composite action. The Author had pointed out that in many buildings composite action must have been used, though perhaps unconsciously, by the designer.

91. The first two methods would perhaps be cumbersome from the design point of view, and Professor Neal had the impression that the Author would favour the use of composite action. Did the Author consider that by making use of stanchion casings and the walls themselves the designer should exercise his own discretion on the question of side-sway, or should this be a matter for a Code of Practice? To press the point further, it would be interesting to know just how far the Author would go in taking account of composite action. Would he be prepared to use the stanchion casing as an integral part of the stanchion? That idea had been put forward by the late Dr Oscar Faber 3 or 4 years ago; since the casing had usually to be provided, why should the stanchion not be designed as a composite member? Secondly, would the Author consider that wall panels could not only be called upon to prevent side-sway, but also to assist in preventing the buckling of individual stanchion lengths? That would be expecting the wall panels to do a great deal more than merely prevent side-sway, but would it be a practical possibility?

92. Reverting to the question of stanchion design, it had always seemed to Professor Neal that the designers of steel frames were handicapped by having to use I-sections for the columns as well as the beams. Whereas the I-section is practically perfect for members carrying transverse loads, it is almost the worst section possible for carrying axial loads. A far better section for columns would be a box section. Probably that had never been used extensively because it was usually too costly to fabricate welded box sections. However, he had heard that rolled box sections up to 16 in. sq. were now commercially available in use in Italy. Small box sections were also available in the United Kingdom, and it was hoped that larger ones would be available soon.

93. Perhaps the Author would comment on the impact of this on the problem of stanchion design, because it would appear that the consequent reduction in slenderness ratio and the elimination of torsional instabilitity would ease the position for the plastic designer.

Mr H. S. L. Harris (Lecturer in Engineering, University of Cambridge) referred to the comparison between the encased frame and the frame with a 4½-in. brick panel with a door opening (Table 3). The addition of the brick panel appeared to have had no effect on the load at which the first crack appeared, but had almost doubled the ultimate load. If the door were of standard size there would have been very little brickwork left above it. Where was the door and how big was it?

95. The Author had shown a diagram of failure loads plotted against elastic critical loads (both expressed as multiples of the rigid-plastic loads) for a large number of structures. Would the Author include that diagram in his reply, for it contained much information which was not in the Paper?

96. Mr Harris regretted that the Author's third example of frame instability, § 55, gave values of λ_p, λ_c, and an experimental value for λ, but no value of λ obtained by the

type of analysis used for the other two examples. Would the Author comment on the agreement between analysis and experimental collapse loads?

Professor A. J. S. Pippard (Professor Emeritus of Civil Engineering, Imperial College of Science and Technology) added a small historical note to the Paper. It dealt, he said, with a side issue, but he felt it was proper to refer to it to do justice to the work of two people which was apt to be forgotten. Professor Pippard had had the pleasure of working with Arthur Berry during the 1914–18 war and had seen something of the development of continuous-beam theory with which his name was associated.

98. At the outbreak of that war the problem of the stresses in biplane spars was of vital importance. These members were laterally loaded struts with a number of supports and presented the same fundamental problem as building-frame stanchions. Quite early in the war the essential analysis had been made by two members of the Air Department of the Admiralty, Harris Booth and Harold Bolas. (In that Department, the popular name for the new method had been HB^2!).

99. That work had been published as a Confidential Information Memorandum by the Department and it happened that a copy had fallen into the hands of Arthur Berry, who was, Professor Pippard believed, at that time the Vice-Provost of King's College, Cambridge. He, a Senior Wrangler, had recast it and produced the functions referred to in the present Paper, to ease its practical application. In that way Berry made the original "HB^2 method" a considerably easier tool for the designer.

100. Professor Pippard had no desire to detract in any way from Berry's very valuable work, but he felt that some credit should be given to the two engineers whom he had mentioned.

Mr G. P. Manning (Consulting Engineer in private practice) said that the collapse of a structure was very seldom due to a wrong assessment of *known* data or to mistakes in calculation; it was almost invariably due to the fact that the designer had completely overlooked some method of failure or some direction of failure. The Paper made no mention of three possible factors which might assist in the failure of a structure.

102. The Author had mentioned the torsional failure of single columns and the torsional failure of a frame, but it was possible for some structures to suffer from general torsional failure without any side-sway at all. The problem had arisen, for example. in the case of the aluminium dome at the 1950 Toronto Fair supported by triangular stanchions round its circumference. It had been argued that, from whatever direction the wind blew, there would always be two or three stanchions on each side to resist it.

103. There was, however, another mode of failure which had been completely overlooked. A high wind never blew quite constantly in either strength or direction, and if the structure were struck by a turbulent wind, it would be possible for it to fail by rotating about its vertical centre-line, without any side-sway.

104. A similar collapse could occur in a reinforced concrete building where the columns were all very thin in elevation and the lowest (garage) storey devoid of cladding.

105. Mr Manning then pointed out that the contents of the Paper were completely applicable only to structures which were founded on solid rock. As he had often said, the effective structure did not finish at the underside of the base-plates, but extended for a considerable distance below the underside of the foundations. He would not agree that any structure had been completely designed unless the design included a consideration of relative settlement of the foundations.

106. The third method of failure was one on which neither the Author nor Mr Manning could give much useful guidance. Before beginning to design a steel-framed structure it was necessary for the engineer to make some kind of guess about how many members of that structure might be torn out, mutilated, or overloaded, subsequent to its completion. Any designer who overlooked this possibility might find himself in serious difficulties.

Mr N. J. B. Alexander (Graduate civil engineer, British Railways Eastern Region) referred to a small investigation which he had carried out on some results quoted by the Author. Table 2 showed the collapse load factor and other load factors for his three frames. Mr Alexander had compared these graphically with some load factors which he had available for the collapse of single-storey portal frames and found consistently less deterioration of strength with more storeys. Was that fortuitous? If it were actually the case, it seemed to suggest that there was a most economic number of storeys for any building. Could that be founded on fact?

Dr J. C. Chapman (Department of Civil Engineering, Imperial College of Science and Technology) added a footnote to Professor Neal's remarks on the sections of stanchions by mentioning that at Dusseldorf, at the present time, a building of 25 or 26 storeys was being erected with box-section internal stanchions and tube-section external stanchions. The floors were about 8 cm thick.

Mr M. W. Low (Engineer, Ove Arup & Partners), referring to the first two of the three examples worked by the Author, said that he found himself in rather the opposite position to the Author and Professor Merchant, in that he had a number of experimental results but hardly any verifying calculations. This, however, was being remedied at the moment on the electronic computer at Cambridge. He had submitted a Paper [38] to the Institution which dealt with the results of these tests, so that he did not propose to go into them in detail on the present occasion, but had taken the Author's two examples and tried to predict for the particular frames in question what the effects of frame instability would be, using the results of his own model tests. These tests had been made on three-, five-, and seven-storey models of rectangular mild-steel section and, as had been pointed out by Dr Horne, they did not exhibit the strain-hardening effect to any great extent.

110. Taking the first example in the Paper, and assuming that the effective uniform beam-stanchion stiffness ratio was $\frac{3}{4}$, he would estimate a collapse load factor of 1·66 on the basis of the tests. The Author's figure was 1·73, so that there was a fair measure of agreement. In the second example, where there was approximately equal stiffness between beams and stanchions, the estimate on the basis of the tests was 1·87, and the Author's figure was 1·9. While, therefore, designers might be put off by the extensive mathematics involved in doing the excellent analyses quoted by the Author, the future seemed more hopeful if sufficient calculated and experimental results could be plotted on a diagram such as had been suggested by Professor Merchant. Two examples had been shown already of such a diagram from which the designer could very quickly estimate the effect of instability. The two cases above showed that it could be done.

Dr N. W. Murray (Senior Research Assistant, Department of Mechanical Engineering, University of Sheffield) emphasised that a designer could determine critical loads by use of the models. As pointed out in § 11, the P/P_E-values applied independently of the L/r-ratios, so that the designer could use high L/r-ratios and obtain the critical load. He believed that that was what Dr Chandler had done at Manchester. Having obtained the critical load of the model, it was then possible to scale up the axial loads by applying the scale constant used earlier. This applied directly when the failure took place by buckling within the plane of the structure, but if buckling took place by buckling out of the plane, where there was torsion of the members, then the torsion properties would have to be scaled as well.

112. It was thus possible to eliminate a great deal of the calculation necessary in determining critical loads in the examples quoted in the Paper. In the first example the Author had calculated the critical load nine times, and in the third example ten times; Dr Murray knew from experience that there was a great deal of work involved.

113. The method of model analysis was one which was as near to a direct design as possible. By using the method it was possible to build a model and determine its critical load, and, if it was too small, take out a member and put in a heavier one. Had the Author any particular views on the use of models?

Mr Hsuan-Loh Su (Research Assistant, Imperial College of Science and Technology) observed that as far back as 1908, when discussing the buckling of latticed struts in connexion with the collapse of the Quebec bridge, Timoshenko had stressed the important influence of the shear force upon the stability of such struts. His analysis had been reproduced in his later works.[39, 40] This pioneering work seemed to have been forgotten by the research workers in the field of framework stability.

115. Comparison of Merchant's method with Timoshenko's revealed that both methods were intended for solving the same problem under different titles, i.e. the tall framework and the battened strut. It could easily be verified that results obtained from Timoshenko's formula, even under some simplified assumptions in order to simulate a tall framework to a strut, was comparable with the results worked out elaborately by Merchant's approach which could provide only an extrapolated approximation after all.

116. It was surprising that the shear force, which played the most important role in the stability of framework, had hardly been mentioned explicitly by any authors recently engaged in this field. The reason might be that the "no shear" pattern used by Professor Merchant in developing his theory and *m, n, o* functions tended to cover the real action of the shear. In fact, this "no shear" pattern would induce the maximum shear likely to appear.

117. The Author's investigation furnished interesting evidence of the important shear effects on instability. First, the forming of the plastic or some other hinges changed the shear pattern and diminished the framework resistance to shear force simultaneously. Both of these effects tended to magnify the shear influence. Secondly, the prevention of sway reduced the shear and consequently reduced the instability. Thirdly, the cladding and the infilling materials, being a factor of reduction of shear in the framework, also increased the stability.

118. Mr Su agreed with the Author that too much mathematics was unwarranted in the stability analysis. In an engineer's or structural designer's view, Timoshenko's simple formula was more than adequate for any practical case. There, in Mr Su's opinion, future research on the static framework stability along Timoshenko's line would prove practical, more promising, and more rewarding than the current approach

Mr R. W. Pearson (Senior Structural Engineer, Chief Structural Engineers' Branch, Ministry of Works) remarked that little had been said in the discussion from the viewpoint of the practising engineer who built buildings, and it might be thought that such engineers did not know much about the most economic way to build them. It was, however, elementary that the no-sway design was the most effective method and that it was much more economic to design for lateral forces on a few very rigid members than to split them up over a lot of much less stiff ones. From that point of view his interest lay mainly in Table 3 and he would ask some questions which related more to further tests rather than to elucidation.

120. What designers wanted was more data on the effects of infilling panels in producing the required stiffness for eliminating side-sway. The 4½-in. brick panel was a case in point. They wanted to know how big a panel was satisfactorily stiffened by a 4½-in. brick wall, and at what size the thickness should be increased to 9-in. What could they allow on these factors for concrete?

121. The Paper, perhaps naturally, dealt with steel frames, but today concrete frames were also of general concern. Taking a panel inside a frame, how should the enclosing members be designed to resist the effects of the infilling panel? In Fig. 23b, showing the breaking of the beam above the panel, there was obviously an appreciable moment put on the beam. What were the forces at the corners, and what anchorage should be given between the members at the corners? What effect was there in the case of multiple panels, one above another, as in a multi-storey building?

122. Finally, he would ask that engineers and architects in general should become more conscious of the fact that panels should be provided at certain points in a building

and should be sacrosanct and not have doorways or other openings cut in them, at least with reference to someone who knew the functions which they might be performing.

Dr R. K. Livesley (Lecturer, Cambridge University Engineering Department), recalled that some years ago a programme[41] for the elastic analysis of rigid-jointed plane frameworks and trusses had been developed on the Ferranti Mark I computer at Manchester University. At Dr Wood's suggestion, this programme had later been extended to cover the instability effects of axial forces,[42] and had been used for the rapid determination of critical loads. Similar programmes had recently been developed for other computers such as the "Pegasus" and "Mercury", and a programme for the elasto-plastic stability analysis of plane structures was being tested on the new Cambridge computer EDSAC II.

124. This new programme was based on the same matrix iterative scheme as had been used in the original Manchester version. Forces and moments calculated from an initial linear analysis were used to modify the stiffness matrices of the members, and these matrices were then combined to give the modified stiffness matrix of the structure. Solution of the modified load/displacement equations gave a second set of internal forces and moments, and the cycle was repeated if necessary. The theory given in the original papers[42] could very easily be extended to include the development of plastic hinges, provided that the shape-factor effect was neglected and the hinges occurred at the ends of the members (the latter was not a serious restriction, since any beam which had a concentrated load acting at a point within its length could always be treated as two beams with a "joint" at the loading point). A simple parabolic law was also assumed for the variation of fully plastic moment with axial force. These approximations could lead to errors in certain problems, but should not be serious when the collapse was a side-sway one of the type considered by Dr. Wood. A more exact approach would either make the programming extremely complicated and increase the running time considerably, or reduce the range of structures to which the programme could be applied.

125. The advantage of the matrix method used was its complete generality. The programme would deal with any plane frame or rigid-jointed truss of any degree of redundancy, and up to a limit of 99 degrees of freedom (i.e. 33 joints, since each joint has 3 degrees of freedom). To prepare a frame for machine solution it was merely necessary to number the joints and punch a data tape giving, for each member:—

(a) The joint numbers at the two ends.
(b) The member constants E, A, I, L, and α (the angle of the member to the horizontal).
(c) The fully plastic moment under zero axial load M_p, and the axial force P_Y which would produce uniform yielding over the section.
(d) An estimate of the axial force P, if this was available (this helped to reduce the number of iterations required).

126. This information was followed by the values of the joint loads. The printed output consisted of the deflexions and rotations of the joints, together with the internal forces and moments. The latter were checked against the external loads for equillibrium, and any discrepancies printed. When the iterations for one analysis were complete, the structure could be analysed for a new load factor by setting appropriate switches on the machine's console, without any other data input being required.

127. The programme made extensive use of the magnetic-tape auxiliary store attached to EDSAC II. Unfortunately, while the basic computing speed of EDSAC II was approximately 50 times that of the Ferranti Mark I, the rate at which information could be transferred to and from the auxiliary store was about the same for the two machines. Considerable programming effort had therefore been put into reducing the number of magnetic transfers in the new programming to the minimum. Only the upper triangle of the stiffness matrix of the structure was stored (since it was always symmetrical), and only the non-zero elements were retained. It was estimated that a single linear

analysis of a structure with 33 joints (say an 11-storey, 2-bay rectangular frame, or a 15-panel bridge truss) would take about 10 min of machine time.

128. While the programme was mainly designed for investigations into elasto-plastic instability, it was hoped that it would also prove useful for routine jobs of linear analysis.

The Author, in reply, said he was conscious of the fact that that part of the Paper dealing with composite action had been concentrated in a small section at the end and so he thought it was advisable to point out that the title of the Paper was "The stability of tall buildings", not the "instability of tall frameworks". He felt that the Chairman's assumption that he was advocating composite design on an extensive scale, was based more on his (the Chairman's) knowledge of the work of the Building Research Station on composite action rather than on the limited uses that the Author had managed to incorporate in the Paper. But it was noticeable that other members had pointed, at the other end of the scale, to the elaborate nature of the bare-frame analyses. Since the Author has also been engaged in producing rapid design methods for bare frameworks which did not dodge on the one hand the issue of elasto-plastic frame instability, nor on the other hand the implications of the theory of composite action even in its present elementary state, the aims of the present Paper were:

(i) to show by elaborate analysis just what was at stake when speaking of "frame instability";
(ii) to demonstrate the importance in tall frames, with "comprehensive" mechanisms of collapse (e.g. Fig. 11c), of the deterioration of collective stiffness because of plasticity;
(iii) to outline existing rapid design methods, and indicate problems facing future methods; and
(iv) to show how the stiffnesses of frames, and the very loads on frames, were affected by composite action.

130. Mr Harris regretted that no elaborate analysis accompanied the test of the model frame of Fig. 22. A cinematograph film had been taken of the collapse, and only a few frames had depicted the collapse between Figs 22a and 22b. It had occurred so suddenly that practical observations of the extent of plastic zones at collapse were difficult to detect. So in spite of Dr Murray's comments in § 113 the Author believed that the use of models was of limited value. He also considered that the use of the concept of subsitute frames[15] in determining critical loads was quicker than building models.

131. Dr Lightfoot had asked how knowledge of non-linear elasto-plastic properties of members might be extended to direct, if simplified, design of structures. In this respect, the general problem divided into two distinctly different issues. When the ratio of P/P_E was low it was feasible to interpret beams and stanchions as being comprised of discrete hinges with completely elastic portions in between (except for single-curvature in stanchions). That was what lay behind charts like Fig. 15, which showed that extensive yielding before the gradient (i.e. stiffness) of the moment-rotation curves was impaired could be permitted. Moreover similar charts for stanchions had justified the general programming of the *sway-collapse* of frames for computers, e.g. Mr Livesley's E.D.S.A.C. and a similar Building Research Station programme, and also Merchant and Salem's calculations for strong-axis bending of frames. It would seem therefore that there was good prospect of developing Dr. Lightfoot's "principle of multiples" in frameworks to include discrete plastic hinges in multi-storey frames failing by sidesway, provided the Author's "stiffness distribution" technique [6, 15] was used for speedy convergence.

132. However, as Mr Pearson had observed, it was an elementary point that sidesway should be eliminated (but more difficult than he might think to persuade architects and engineers to follow this rule), with the result that much more slender stanchions could be tolerated. This localized frame instability, but the problems were none the

less difficult, since the direct load could now exceed the Euler load for elastically restrained stanchions. It was important that Fig. 4 be understood. In Merchant's terminology for an *elastic* strut:

$$\frac{M_A}{EK} = s\theta_A + sc\theta_B \text{ for no-sway}$$

$$\frac{M_B}{EK} = sc\theta_A + s\theta_B$$

$$\therefore \frac{M_A}{EK} - \frac{cM_B}{EK} = s(1-c^2)\theta_A$$

$$\text{or } \frac{M_A}{My'} = \frac{cM_B}{My'} + \frac{EK\theta_A}{My'} . s(1-c^2) \quad . \quad . \quad . \quad . \quad . \quad (7)$$

being the moment-rotation characteristics at end A, for any given moment at the other end B, in non-dimensional units. At the Euler load (a particular case—Fig. 4) the stiffness, or gradient, $s(1-c^2)$ became zero, the carry-over c being unity, while the intercept was the value of cM_B/My'. More important still was the rapid negative stiffnesses (gradients) which set in under plastic conditions; e.g. with zero moment at the far end increasing rotations required rapidly increasing restraining (negative) moments to preserve equilibrium. Fitting such curves, and similar ones for beams (like Fig. 15) together, so as to make zero collective stiffness, had produced the set of results which Mr Harris had inquired about, and which was now presented as Fig. 32. This gave about 180 results for no-sway cases to add to Merchant's 300 results for sway cases. There were both strong stanchions (I-section) and weak stanchions (rectangular), with many different beams. The scatter was extraordinarily like Merchant's results. Fig. 32 was for s/c loading pattern only. The Author's three multi-storey examples, however, showed up in their proper places on this plot. The reasons for the scatter were under investigation.

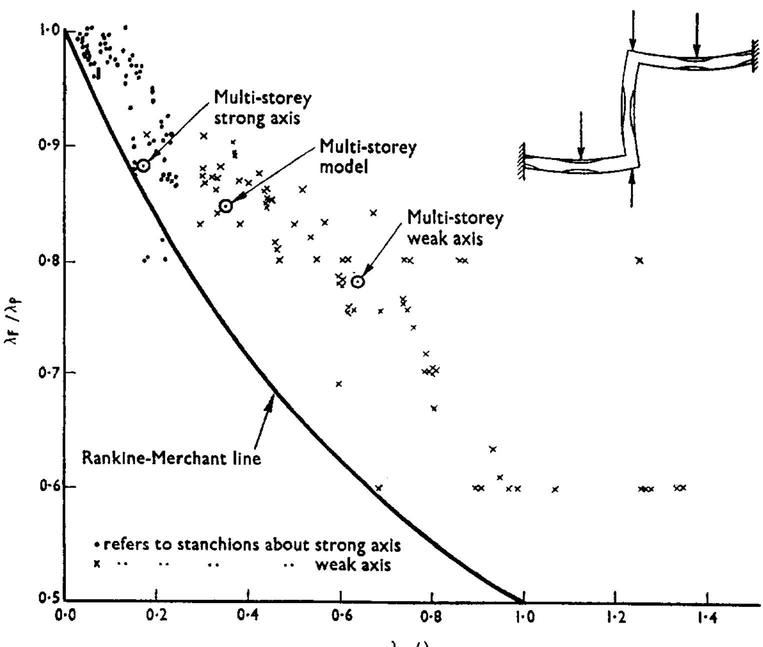


FIG. 32.—COLLAPSE OF BEAM-AND-COLUMN FRAMES

133. Obviously the design procedure for slender stanchions was going to be difficult, and it was here, the Author thought, that both Dr Lightfoot and Professor Merchant underestimated the power of nomograms (Fig. 8), when supported by additional clauses, as an *aid* (no more) to rapid design. As yet, Fig. 32 applied to bending about one axis only and *s/c* conditions, and was not yet a full design procedure like the "near-to-collapse" Building Research Station design (Note A.58). Professor Merchant had emphasized (§ 82) that research teams "were trying to introduce simple principles where no principles at all existed at the moment". Thus Fig. 7 illustrated what the designer all too often forgot—the importance of the work of Berry (as distinct from Perry). Taking the immediate surrounding frame as a suitable basic substitute frame for stanchion design, then the nomograms predicted instantly the maximum elastic stanchion moment. Engineers who had used Claxton Fidler's "characteristic points" graphical method for bending moments in continuous beams would have observed that two points, located one at each end of the nomogram, once again fixed the line representing the end bending moments—this time without any trial and error, for a continuous stanchion, instability functions, etc.[4] If this device—faster than an electronic computer—was rejected, what was the rational *simple* alternative? There were other more difficult possibilities but in the meantime the Author would like to see this method given a trial, if only to improve the appreciation of fundamental principles, but particularly because it could allow various degrees of composite action to be included.

134. This led to Professor Neal's important questions concerning the extent of permissible composite action, which the Author did not wish to evade. He would surely expect any appeal to composite action at present (and perhaps always) to be both conservative and approximate. Moreover, to be fair, one must realize that a weakness of plastic theories of (bare) frame design was the assumption that the working frame loads were known, and that factoring the *building* loads was the same thing as factoring the frame loads. For that to be true everything would have to be elastic at plastic collapse! So it should be accepted that the answer was safe and approximate.

135. At the moment the best tried-out applications were allowances for tee-beam action (e.g. the new Imperial College Buildings), beams supporting brick walls (very extensive applications[33, 43]), and encasement of stanchions (B.S.449), while the use of end walls was permitted by B.S.449 and S.S.R.C. "Draft Rules", in some cases avoiding wind calculations altogether. The forthcoming revision to B.S.449 regarding the effects of encasement would make it imperative to examine what could be done for rigid frames—else there would be an anomaly.

136. With regard to the stability of tall buildings there were four separate degrees to which composite action could be applied:—

(*a*) A very low order indeed should suffice to overcome plastic frame instability provided that comprehensive mechanisms were not allowed to extend through more than two storeys.

(*b*) A higher order panel action to counteract sidesway. This was already in use with load-bearing wall construction without frames. Tests on composite wall-frame panels in racking shear had so far shown that the composite strength C exceeded the sum of the separate wall strength W and the plastic frame strength F, preserving ductility at collapse and a greatly enhanced stiffness controlled by the wall until failure. It was necessary to impose an increased load factor as the contribution of the wall increased. The Author suggested that $\lambda = 8$ could be used with an ultimate wall strength W given by 80 lb/sq. in. for brickwork in shear (i.e. 10 lb/sq. in. working stress). For other ratios of W/F the following (an exponential formula) might conservatively be adopted:

W/F:	0	$\frac{1}{2}$	1	2	3	4	8
Composite λ:	2	3·8	5	6·5	7·1	7·6	8
	↑ All frame						↑ See wall

Coupling this with $C=\left\{1+\frac{2}{W/F+F/W}\right\}\{W+F\}$ giving $C=F$ when $W=0$, $C=W$ when $F=0$, and $C=2(F+W)$ when $F=W$ (a reasonable interpretation of test results), gave a saving of steel and a rising load factor all in one. For eliminating sidesway not less than a 9-in.-thick wall should be used and also W should not be less than F. The frame should always be designed to take the full vertical load, giving automatic practical limits.

(*c*) It was safe to adopt composite action for substantial end walls (which should never be knocked down), special spine walls, or lift shafts. Mr Pearson had rightly observed that there was a proper function for the maintenance engineer to see that structural diaphragms were not tampered with (as so often happened with load bearing walls and tie-members of perfect trusses!) In reply to the Chairman, the Author pointed out that if major, and even minor, infillings were not known beforehand it was doubtful what meaning could be attached to frame calculations. But with regard to lesser infillings it was a question of the probability of more than a certain percentage being knocked down in any one storey when there was a factored vertical load and a gale blowing simultaneously. It must be remembered that the design of bare frames was, in the end, entirely dependent on the (probable) load factor.

(*d*) The Author would not recommend Professor Neal to stop any one single stanchion buckling by using internal partitions. But substantial reductions of beam sizes might be effected where the major load happened to be the wall-panel load, and in such cases elastic composite beam design was more logical and more economical; moreover practically no bending moment would be transmitted to the stanchion from a tremendously stiff composite wall beam, allowing consideration of floor loads only and, for safety, acting on the bare beam. The particular frame design methods being put out by the Building Research Station allowed that kind of treatment if desired.

137. Mr Low's experiments confirmed the Author's own conclusions and he looked forward to the proposed design rules. Both Mr Harris and Mr Low would be interested in an approximate analysis of the experimental test frame (Fig. 33). In this case mode (*a*) had been expected to occur before independent beam failure (*b*); but in fact mode (*c*) had taken over, partly because of $P.\Delta$ effects, but more because of the critical buckling associated with that story.

138. The Author agreed with some of Dr Horne's observations. It was true there must be some benefit from strain-hardening. Moreover it was the Author's original intention to include in the Paper a section dealing with the desirability of steels for tall buildings with no well-defined yield point—but that was a new subject. He supported any research devoted to finding properties of practical hinges; it was strange that even now there was no adequate theory—e.g. how long was a "hinge"?

139. However, plastic theories had gone ahead assuming idealized hinges—upper bound theorems (Neal and Symonds); lower bounds (Partridge, Horne); locked-up stresses vanishing at collapse; shake-down theorems; idealized plastic stanchions; combination of partial mechanisms to give a comprehensive (minimum weight) collapse, settlements being of no consequence (likewise slip at joints, relative beam spans, and stiffnesses irrelevant)—all these depended on idealized plasticity and not all were conservative. Only recently Mr J. S. Ellis of the Cambridge team, discussing a collapse mechanism with nineteen hinges, had written[44]: "Of course the use of the collapse mechanism to analyse a framework is based on the assumption that the terminal moment-end-rotation curves of the columns have a long flat peak. . . . (Horne 1957)". It seemed that Dr Horne was now changing the terms of reference somewhat. Strain-hardening was going to be variable, the most ductile steels being the worst off. Not all full-sized portal frames, even with excessively high critical loads, had shown such a favourable curve as Fig. 30.

140. The Author did not object to approximations for "hinges" and he appreciated

that this would raise λ_{det}, but rigid-plastic-rigid theory, a delayed re-introduction of infinite stiffness, was going to need expert handling. The deflexions of Fig. 21 (strong-axis frame) had to be divided by 50. The first four hinges only required 0·40, 0·22, 0·08, 0·05 degrees of rotation at constant moment for collapse—the rest was elastic breakdown. This seemed to make but little recognition of strain-hardening. In the other weak-axis frame no rotation, even of ideal hinges, was required at all. Now it was misleading to look at a hinge characteristic like Fig. 34 and observe that there was always positive stiffness left. The discrete-hinge theory (e.g. the programmes of Livesley and the Author's colleague Vallance) assumed continued elastic stiffness right up to M_p. It was thus *unsafe* to begin with, and a considerable rotation at constant M_p could develop before that effect was balanced (Fig. 34). Moreover the shape factor M_p'/M_Y' increased rapidly with direct stress, and in addition *all* partial plastic zones were neglected in this treatment, increasing further the validity of a perfect hinge.

141. Lawton and the Author had continued this process up to $\lambda = 1{\cdot}95$, not allowing any more hinges to develop ($\lambda_{det} = 2{\cdot}30$, Fig. 35). They had promptly discarded the result. The moments had overshot the full plastic moment at four new points as shown, including the foot of one column (not predicted by the mechanism). For those potential "hinges", in Horne's terminology, $k = 0$ and they strain-hardened at the original modulus E. Two additional partial plastic zones were clearly neglected. To sustain

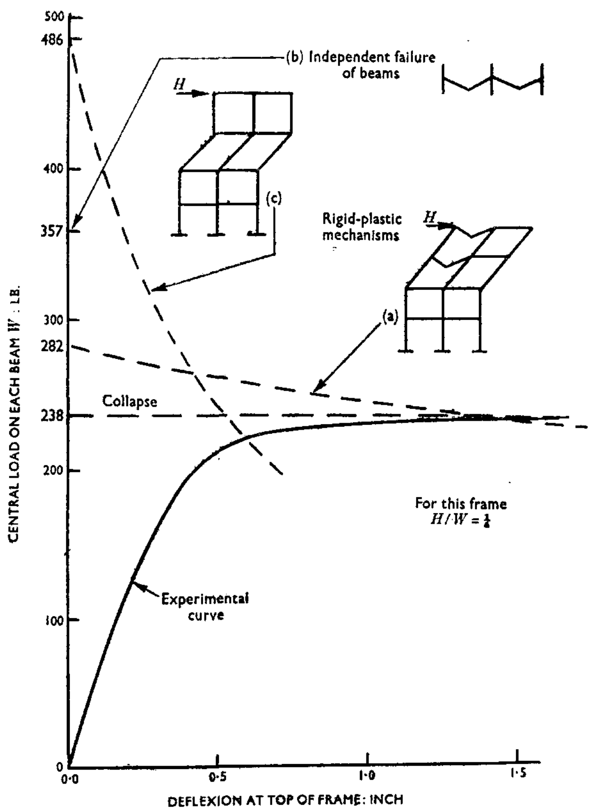


FIG. 33

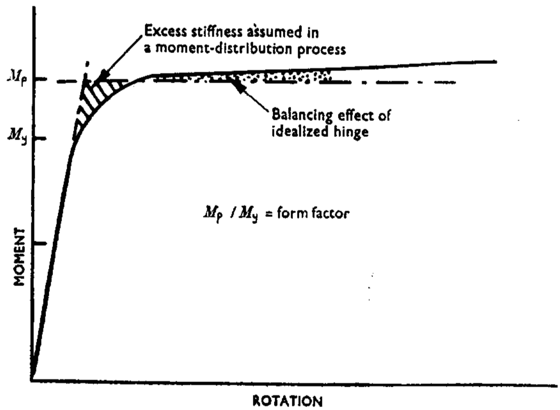


FIG. 34

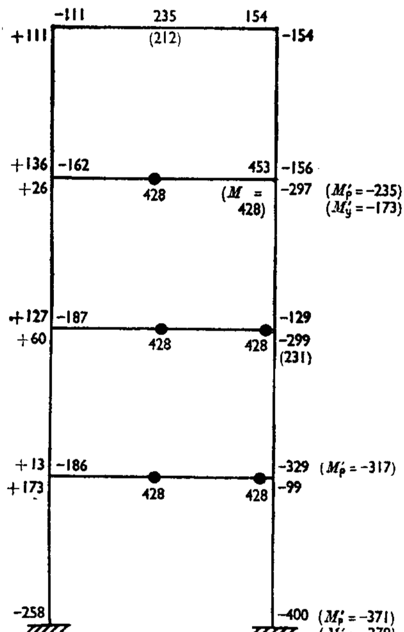


FIG. 35.—RESULT OF MOMENT-DISTRIBUTION ANALYSIS CARRIED OUT AT $\lambda = 1{\cdot}95$, IGNORING ALL FURTHER PLASTIC HINGES OTHER THAN THOSE AT $\lambda = 1{\cdot}90$

even that mixed collection of ideal and incredible hinges, the rate of distortion (sidesway) was now about 100 times the original elastic rate (Fig. 36) but with only 2·2° maximum hinge rotation (now No. 4). The left-hand stanchion was entirely elastic from ground to roof. That was a warning that elastic deflexions were important and that the zero collective stiffness concept λ_{det}, corrected for real hinges, would be too severe for practical use; before it was reached there was effective collapse and when it was reached, catastrophic collapse. The factor $1/(1-\lambda_p/\lambda_c)$ for magnification of arbitrary rigid link deflexions in Horne's equations (1) and (2) was quite inadequate to deal with elastic deflexions, but some idea might be gained of the real magnification if it was remembered that instead of λ_c for the original elastic frame, λ_{det} for a strain-hardening mechanism now controlled.

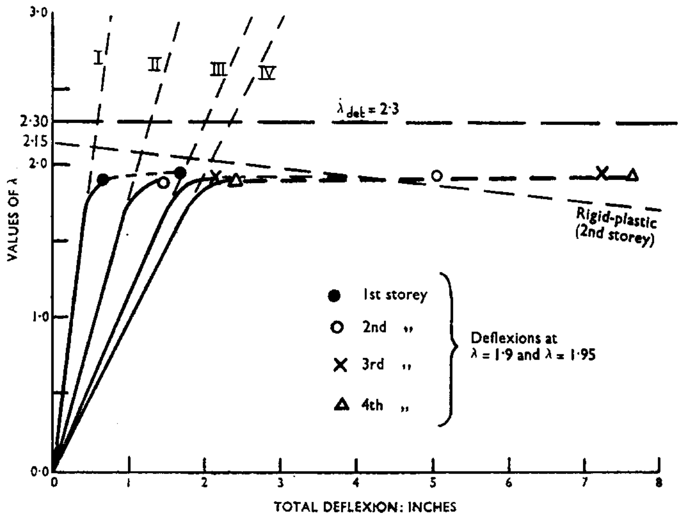


FIG. 36

142. But the principal feature still remained that all second-order effects, such as settlement (§ 57), now deserved to be put alongside strain-hardening. That was why the Author had paid so much attention to the concept of a general disturbance (§§ 16, 17). Mr Manning had mentioned many disturbances concerning which the Author had agreed; even rotational slip of footings could not be forgotten. Attention should not be drawn away from the importance of putting limits on the use of comprehensive mechanisms of collapse in tall buildings, nor of the suddenness of instability if it occurred in admittedly rare cases. (The load factor for brittle fracture could not be raised—for the same reason.) The presence or absence of warning of collapse must decide the load factor to some extent. Berry's work had rarely been properly appreciated, and that was why the Author was interested in Professor Pippard's historical record.

REFERENCES

34. J. W. Roderick, "The load-deflexion relationship for a partially plastic rolled-steel joist". Brit. Welding J., vol. 1 (1954), p. 78.
35. J. F. Baker and J. W. Roderick, "Tests on full-scale portal frames". Proc. Instn civ. Engrs, Pt I, vol. 1 (1952), p. 71.

36. W. Merchant, C. A. Rashid, A. Bolton, and A. Salem, "The behaviour of unclad frames". Instn Struct. Engrs, Jubilee Conference, Oct. 1958.
37. A. Salem, "Frame instability in the plastic range". Ph.D. Thesis, Manchester University, May 1958.
38. M. W. Low, "Some model tests on multi-storey rigid steel frames". To be published in 1959.
39. S. Timoshenko, "Theory of elastic stability". McGraw-Hill, N.Y., 1940.
40. S. Timoshenko, "Strength of Materials". McGraw-Hill, N.Y., 1941.
41. R. K. Livesley, "Analysis of rigid frames by an electronic digital computer". Engineering, vol 176 (July–Dec. 1953), p. 230.
42. R. K. Livesley, "The application of an electronic digital computer to some problems of structural analysis". Struct. Engr, vol 34 (1956), p. 1.
43. R. H. Wood, "Composite construction". Structural Engineer, Jubilee Issue, 1958, p. 135.
44. J. S. Ellis, "Plastic behaviour of compression members". J. Mech. Phys. Sol., vol. 6 (1958), p. 282.