Some problems in the philosophy of structural design

by

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Mr H. J. Burgess (Forest Research Institute, Kepong, Selangor, Malaya), wrote that the timber engineer would applaud the Author's continued advocacy of the probabilistic approach to the study of structural safety, since timber design had to be founded on an elementary form of this approach. A more advanced form could presumably only be adopted when more was known about the statistical variation of the 'generalized entities associated with the responding and the disturbing system'. In a re-examination of design codes one found oneself pondering the likelihood that a piano and a wedding ceremony would be found at the same time in a low-income dwelling. Was not this the nearest kind of approach that could be made on the 'disturbing' side in the foreseeable future, and was it not the approach that lay behind the whole of the present system of ensuring the safety of structures?

73. The Author must also be supported when he deplored the present low status of stiffness criteria in design, a position that surely arose from the humanistic reason (neglected elsewhere, too, in the Paper) that deflexion calculations were lengthy, except for the simplest structures. The timber engineer was particularly concerned with this problem, especially in view of the importance of creep in timber, and it was hoped that a modest contribution towards its solution would appear shortly.^{31,32}

74. In the matter of load-factor design, however, it appeared even to the structural dilettante that the Author was barking up a number of wrong trees. Presumably he would not object to an elastic design of a two-hinged gable frame, say, in which a pre-loading cable at ground level applied a horizontal force H such that the moments at ridge and eaves were made equal. The essential point about the load-factor method was that certain structures were able to reach this same pre-loaded state without the complication of a preloading cable, and having reached it, return to elastic behaviour with an automatically-attained preload applied by the ground. It was the ability of steel to do this that was made use of in the plastic method, and in two separate parts of his Paper the Author had missed the whole point of the method when he said that this potentiality was not manifested until the structure was collapsing. Plastic yielding of a part of the structure was a safety valve automatically limiting the load applied by redundants. It provided a cut-off point, a truncation, for the tails of some of the Author's distribution functions.

75. In the example just given, either the preload (elastic) design or the plastic method would yield identical member sizes. In either case, deflexions under a given load would be greater than in the elastic design without preload, but deflexion criteria were not cast aside. They were merely a parallel subject to be treated separately, and the same applied to stability calculations.

76. The object of the load-factor method was to apply to various kinds of structures the same ratio of failing load to the working load in the design code, both these loads being derived from probabilistic considerations despite the fact that lack of data prevented their being undertaken in formal statistical terms. The Author begged the

* *Proc. Instn civ. Engrs*, vol. 22, August 1962, pp. 391-403.

question of what a 'true' load factor was when he made two unwarranted assumptions to provide an infinite tail to his distribution function: that there was no limit to the load that could be placed on a structure, and that structures are expected to last for ever. For example, regarding the first of these, every student knew that one could not pack an unlimited number of human bodies into a telephone box, no matter how many times one tried. Relevant to the second, a maximum wind speed could be written into a design code without saying anything about recurrence intervals. Incidentally, the number of buildings that had survived a long period when Engineers were happily unaware of recurrence phenomena seemed to indicate either that factors of safety (however defined) had been even more excessive than was generally admitted or that the assumptions behind plastic design were well justified. The Author would probably agree that every structure ought to have a chance of collapsing.

77. It was surely never denied that plastic design leads to 'a reduction in the real available safety factor'; the point was that the reduction brought this factor down to a level that had been proved satisfactory in other types of structure. The remark about a saving being achieved at the expense of structural safety was mere playing to the gallery, and the Author's 'proof' of a reduction in utility from his own premise of excessive deflexion really did not do him credit. This elastic treatment of logic certainly would not appeal to adherents of what the Author later called 'the collapse-happy philosophy'.

78. The generalities of the Author's conclusions again contained much that was not only challengeable but transparently false. For example, plastic design was not founded on St Venant's model, though this model had been used to simplify the subject for beginners, and the limit of normality was not necessarily either the limit of elasticity or the limit of utility. The remarks on 'healthy performance' of structures again begged the question; one could much more logically speak of the plastically-designed structure as flexing its muscles to resist the major assaults of life.

79. There were other points on which one could enter the lists against the latter part of Mr Su's Paper, but this might be a little unprofitable since the overall impression given by this part was of 風聲鶴唳草木皆兵.*

Dr D. V. Reddy (Assistant Professor of Applied Mechanics, Indian Institute of Technology, Madras), wrote that the Author needed to be commended for the boldness of his approach in the 'verdict' on plastic design. While some comments, which seem almost intentionally provocative, certainly offered food for thought, it was unfortunate that the Author referred to collapse load analysis as 'collapse-happy philosophy'. Thanks to extensive analytical and experimental investigations by many research workers, plastic design was now placed on sound footings and at least three countries included it in their codes of practice. Much of the criticism of plastic methods of analysis was due to psychological differences between applied mathematicians and engineers, which could be traced to the age-old controversy between phenomenological and microscopic approaches to analysis.

81. The Author's statement that plastic design did not take into account the ductility of steel seemed to imply that very little difference was made between cast-iron and steel. It was a well known fact that the load factor was based on ductility while the factor of safety in elastic design neglected this important potentiality.

82. If the Author's argument that the economy resulting from plastic design was due to a 'reduction in the real available safety factor' were extended, it followed that the solution of redundant structures as statically determinate ones would also improve the 'safety factor'! The load factor was a more exact assessment of the possible collapse load of the structures and there was no reason why steel sections should not be reduced in size if experience and research warranted it.

83. The problems raised by the difference between test frames and actual structural

* Translation. 'One scared by the sound of the wind and the cry of the cranes, fearing ambush in every tree and tuft of grass'.

frameworks were common to both plastic designers and elastic designers. The study of the plastic behaviour of composite structures was a logical extension of the work on frameworks and considerable interest was now being shown in it.

84. The work done by the Steel Structures Research Committee, consisting of many eminent men, indicated conclusively considerable discrepancies between measured and calculated strains and it was indeed surprising that the findings had recently been contradicted! Many of the readers who had done some strain-gauge work would agree that strain gauges were not all as reliable as the Author would have one believe. The 'strain-happy' philosophy would not lead one nearer the truth than 'collapse-happy' philosophy.

85. The 'structural disease' of the limiting mechanistic state would continue to be endured until the Author or any one else could offer a more positive approach to the determination of the 'healthy performance' of the structure. However, as a macroscopic approach towards the determination of structural behaviour, which was always a function of many inexact parameters, plastic design was likely to play an increasing role in the design of more logical structures for a long time. The structural engineer could not afford to disregard the wisdom of Aristotle's remark that 'it is the mark of an instructed mind to rest content with that degree of precision which the nature of the subject permits and not to seek exactness where only an approximation of the truth is possible'.

The Author (now Senior Research Engineer, The Aeronautical Research Institute, Bromma, Stockholm, Sweden), wrote that Mr Burgess and Dr Reddy had raised several interesting questions. The following discussion would be focused mainly on the anatomy of plastic design because this was the issue on which both Mr Burgess and Dr Reddy found it difficult to agree. Before entering on the principal subject, the Author thought it desirable to say a few words about the probabilistic approach to structural design.

87. The load factor, according to the convention in plastic design or its associates, would be associated with a zero load rather than an infinitely large load if it were the 'true' one. In Mr Burgess' telephone box, the infinite load factor occurred when no one was inside. Could an unlimited number of human bodies have been packed into the box, the load factor would be zero.

88. It was a negative and apologetic attitude to propound that every structure ought to have a chance of collapse, even if the term chance was interpreted in the probabilistic sense. This seemed to be a misconception resulting from some abuse of the probability theory. Some students even invented such terms as 'probability of survival', 'survivalship', etc., which only exhibited their ignorance of the fact that probability was merely an abstract measure of confidence in deduction. Uncertainty in deduction cannot always be equated with the chance of collapse. The probabilistic theory of structural life was in the process of rapid development, and would shortly be introduced to the civil engineer.³³

89. As to the fallacies of plastic design, it would be useful to consult a few previous publications which had covered a number of issues.³⁴⁻³⁹ An article dealing with mathematical fallacies in plastic theorems was forthcoming,⁴⁰ and only philosophical problems would be taken up in the following.

90. According to a founder of plastic deformation theory, which is the plastic theory used for structures, 'the mathematical theory of plasticity owes its development to the demand for more realistic methods of determining safety factors of structural members and machine parts'.⁴¹ However, plastic theory was a sheer mechanistic theory while the determination of safety factors was essentially a probabilistic problem. Would a fisherman climb up a tree for a catch?

91. From such a wrong start, the plastic theory soon landed itself into a to-be-or-not-to-be dilemma. To collapse meant that the plastic-designed structure was not a structure after all, or at most a badly-designed structure. Not to collapse

meant that the plastic-designed structure was an elastic structure, or at least not a plastic nor collapsible structure. Both prospects were equally dispiriting to the theory.

92. There existed a methodological counterpart to the above philosophical dilemma, namely the magnitude of plastic deformation. Should it be large or not? In some cases, it could be infinitely large,⁴²⁻⁴⁷ whereas in others, it should be infinitely small.⁴⁷⁻⁵¹

93. The plastic deformation should be infinitely large so that plastic hinges, moment redistribution, collapse mechanism, etc. could appear. On the other hand, it should be infinitely small so that no appreciable geometrical change in the structural configuration could occur. Was it possible for any plastic surgery to reconcile these two diagonally-opposite conditions?

94. Two operations had so far been performed with a view to covering up the 'large-or-not-large' scar. The first was far from convincing: only special cases where the geometrical change altered only the scale and not the shape of the material body were employed to exemplify, or rather to enliven the plastic theory. The favourite examples were spheres, long tubes, circular holes, and other symmetrical configurations under internal pressure, which was again symmetrical. Conclusions drawn from such special cases were then used to pepper the otherwise tasteless theory.

95. The second type of operation was rather romantic: the plastic theory was alleged to be valid only before the deformation 'ran away', and a 'point of collapse' was consequently invented. It was interesting that the definition intended to define 'collapse' only produced a 'point of collapse'.²¹ If 'collapse' remained undefined, how could it be used to define the 'point of collapse'? Moreover, a structure could either collapse or not collapse, but could not be at the 'point of collapse' whether implying both cases or neither. This slip in definition could hardly be regarded as accidental because alternatives were so painfully truth-telling.

96. It was worth noting that the so-called rigid-plastic material was invented after the plastic deformation theory had been cornered, and this material was thought to be able to rescue the theory which was found inapplicable to the more realistic elastic-plastic material.^{48,52} However, although the rigid part dissolved some theoretical difficulties, the plastic part precipitated them by introducing an appreciable geometrical change. In order to brace the prefabricated theory, no plastic behaviour was tolerable. In other words, only a rigid-rupture or rigid-fracture material could appease the mathematical theory of plastic deformation, which could therefore neither solve any 'deformation' problem nor be applied to any 'plastic' material. This was probably the first theory, whose title had so little bearing upon its content.

97. When the plastic theory was first applied to structural concrete, it was criticized by a 'steel-plastician' on the grounds that only the ductile steel could enjoy a collapse treatment, and the brittle concrete could only be an outcast in any plastic theory.⁵³ Nevertheless, since the theory, if applicable to anything at all, was only applicable to a rigid-rupture material, this criticism should have been reversed. As a matter of fact, a more authoritative plastician did advocate the application of plastic theory to concrete.²⁶

98. However, the concrete plastician, indiscriminately following the too-brittle-to-be-plastic advice, hastened to alter the title 'plastic method' to ultimate load method, load factor method, etc. without any change in its plastic content. Such titular changes were significant because they revealed a desperate but unnecessary hunt for a refuge, which should have been sought by the critic.

99. Returning to the detailed points, it should be mentioned that the analytical and experimental investigations in connexion with plastic theory, no matter how extensive, had so far failed to resolve the basic to-be-or-not-to-be contradictions, which were a matter of life and death for plastic theory.

100. The gable frame mentioned by Mr Burgess was one of the few favourites in plastic analysis. Since the so-called potentiality could not be materialized without

plastic hinges and their accompanying fantasies, it was perfectly legitimate to regard the point of collapse to have been reached and the structure to be collapsing at the advent of the potentiality. On the other hand, after being relieved by a load factor of say two, the actual load could not or should not reach the intensity which would induce any plastic hinges as conjured in the design stage. Obviously, the safety valve automatically limiting the load was not any plastic yielding which was to push the structure to the brink of collapse, but the artificial load factor which was to bring the structure from the brink of collapse back to safety.

101. Any reduction in the real available safety factor had to be justified. Experience was not a sufficient answer in the scientific age. Excessive deflexions of plastic-designed structures were widely known, and had recently caused an accident in which a heavy overhead moving-crane fell off the rails because the supporting plastic-designed framework flexed its muscles to persist in its major assault on lives and property.

102. Plastic design was founded on the collapse philosophy and the so-called mathematical theory of plastic deformation, but the latter was founded on the St Venant's model. Whether this model should be regarded as an aid in simplifying the subject for beginners or as a noun in the valuable rheological language was not of immediate concern.

103. The limit of utility and that of normality had been discussed in the Paper. To equate such limits to the limit of elasticity was a typical imposition inseparable from the collapse-happy philosophy, which simplified or rather falsified the reality to two and only two extremes: either elastic or plastic. It did not seem to have been realized that elastic design was only applicable to elastic structures. Similarly, an evaporation method would be applicable to evaporating structures. If collapsing structures existed, why not collapse-happy design! It was a strange logic to suggest that critics of plastic theory were necessarily those who used a pin-jointed design for a rigid-jointed structure.⁵⁴ It was even stranger to argue that since there were some defects in elastic theory, the validity of plastic theory would follow.

104. The differences between applied mathematicians and engineers did not seem to be purely psychological, although the psychological differences had been fully exploited to publicize plastic theory. In the age of experts, few knew much about what others were doing. Both mathematicians and engineers, while suspecting the half of the theory they understood, simply believed that the other half must have been all right. When the theory was introduced to mathematicians, emphasis was invariably laid on its engineering aspects. When the theory was presented to engineers, its mathematical coating dominated the entire picture.

105. Such psychological subtlety was utilized not only in the case of mathematicians and engineers, but also for engineers working in different fields. To structural engineers, more examples were driven on machine parts than on structures. To mechanical engineers, more were produced on structures than on machine parts. However, in the case of an aeronautical audience, which was familiar with structures, machine parts, and mathematics, such plastic manoeuvre could not but admit a fiasco.

106. The difference between cast iron and steel was well known. The question was whether this ductile potentiality could ever be utilized in any sound design. Such potentiality could not be utilized until the structure became ductile and started perform some 'plastobatics', which could only be treasured by the collapse-happy designer.

107. Dr Reddy's comparison between isostatic and hyperstatic structures required some clarification. In some cases, an 'improvement' might be achieved by chance. The crucial point was whether the structure itself was isostatic or hyperstatic. Using a right method to analyse a wrong structure or vice versa could only lead to a wrong answer.

108. The discrepancies claimed by the Steel Structures Research Committee

were not only a result of unsatisfactory methods of measurement but also a consequence of inadequate methods of calculation. It was worth mentioning that in a recent paper supposed to discuss an application of plastic theory, much more elastic than plastic theory was used.⁵⁵ The most amazing feature of the paper was that an enormous amount of space was devoted to presenting an elastic analysis, which could be found in many textbooks.

109. Attention should be paid to the fact that several major criticisms had been raised in discussing plastic publications during the past few years, but none of them had been squarely answered. The only exception (about the acceptability of non-rigid joints in plastic theory⁵⁴) proved erroneous. On the other hand, after such unanswerable criticisms, leading plasticians had begun a silent retreat. Not only was elastic theory to be used in 'plastic' design, but also no more plastic research was to be carried out in future. The stage was at present overwhelmed by some minor characters who were still unaware of appearing in the finale of the collapse drama.

110. A positive approach to determining the structural health was already in the process of evolution. It was well known that the ancient elastic design was insufficient for supersonic aircraft, missiles, and space vehicles. Yet, no plastic salesmanship has succeeded in penetrating into these fields.

111. Any physical science was indeed an approximation to the truth. The more a science advanced, the better the truth could be approximated. Nevertheless, the plastic theory, founded upon fallacious mathematical analyses and erroneous diagnoses of physical phenomena, was more a fiction than a science. In the history of science, it was not unusual to find that a minor inconsistency was often enough to spark off an explosion sufficient to destroy an unsatisfactory theory. Many major contradictions were apparent in plastic theory, and none of them could be camouflaged in any diplomatic vocabulary. This could not but lead to the belief that plastic theory and collapse-happy philosophy would probably remain as a casualty in the genetics of mechanics and structural engineering.

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