Zeitschrift: Ingénieurs et architectes suisses

Band: 111 (1985)

Heft: 1-2

Artikel: The development of constructional steelwork: suggestions for further research in structural mechanics

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DOI: https://doi.org/10.5169/seals-75600

inférieures à celle correspondant à un assemblage boulonné.

5. Conclusion

Cet article veut montrer quel est le degré de connaissance dans le domaine du comportement à la fatigue des constructions rivetées. Les essais effectués jusqu'à présent, tant à l'ICOM que dans d'autres institutions, et les résultats que nous avons donnés ci-dessus permettent de se faire une idée sur la question. Nous comptons compléter ces essais par l'étude à la fatigue d'autres poutres prélevées sur des anciens ponts encore en service.

Remerciements
Les auteurs de cet article tiennent à remercier MM. Saluz, Kummer et Rabemanantsoa, qui ont permis de réaliser une partie des essais effectués à l'ICOM, ainsi que les personnes qui ont aidé à une bonne mise en forme de cet article, en particulier M. Steinhauer du département des matériaux de l'EPFL.

Cela nous permettra de disposer de résultats expérimentaux supplémentaires pour effectuer une approche cohérente, si possible probabiliste, de l'évaluation de la durée de vie de constructions métalliques, but général de notre recherche, dont cet article ne présente qu'un aspect. Il sera ainsi plus facile de déterminer la durée de vie restante d'une structure métallique en service depuis un certain temps. Cette question devient chaque année plus actuelle à cause du nombre toujours croissant d'ouvrages au sujet desquels il s'agit de décider si on peut les maintenir en service, ou s'il faut soit les renforcer soit les remplacer.

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The Development of Constructional Steelwork: Suggestions for Further Research in Structural Mechanics

by Leo Finzi, Milan

Summary
This paper focuses attention on the present and near future of constructional steelwork, so far as can be foreseen today. Its object is to identify those areas that have attracted less research, are less well known than others, and so the fields where further developments in Structural Mechanics would be more profitable for constructional steelwork in general.

1. Introduction

In the mid 18th century, when Leonhard Euler began to tackle the problems of instability in compressed struts, he certainly did not realize that he was opening the way to the construction of steel skeletons for buildings of one hundred floors and more, or for trusses sunk deep below the sea to support gigantic offshore drilling platforms, or for arched road and rail bridges with spans covering hundreds of metres.

The same sort of thing could be said of many other scientific pathfinders in the later 18th and early 19th centuries, men like Cauchy, Coulomb, De Saint Venant and Navier, when they layed the foundations for the theory of elasticity. Their seminal studies were quickly fostered and exploited by the Industrial Revolution, so that the second half of the 19th century saw the construction of such daring and illustrious works as great suspension and arched bridges, or the boldness and lightness of the London Crystal Palace. To a large extent this rapid progress was due exactly to the fact that the engineers of the day could draw on the results of research into the Mechanics of Solids and the Theory of Structures.

In the course of 1984 we celebrated the centenary of the death of Alberto Castigliano who, with other contemporary scientists, layed down the rules for identifying the state of stress and strain in hyperstatic structures. Once again, mastery of this kind of problem was quickly translated into actual structures — perhaps one of the most beautiful being the Ponte di Paderno, a bridge over the river Adda built in 1889 by the Swiss engineer Giulio Röthlisberger, who studied under Culmann and Ritter at ETH in Zurich. However, this is by no means an exclusively one way process. It is certainly true that scientific progress has often furnished the essential premises behind the design work for new and original buildings with their relative technologies and contruction methods. But the reverse is also true.

Think for a moment of prestressed concrete technology. At the beginning of this century it raised problems for the scientists concerning rheological behaviour and the cracking mechanisms in concrete. Or again, the behaviour of metal struts in the range of mean slendernesses, which opened the way thirty years ago for so many studies, both theoretical and experimental, on the effects of residual stresses in relation to the manufacturing process. Then, just as a final example, consider the instability of shells. Technological reality showed their actual behaviour to be so different from what the classical theorems in this field suggested, that the whole question had to be opened up again, stressing the effects of the initial geometrical and mechanical imperfections. This paper takes the standpoint of the second of these two possible approaches. Or, to put it another way, it is written from the point of view of a structural engineer, whether he is designer, technologist or constructor, and is directed towards scientists engaged in research into the Mechanics of Solids and the Theory of structures. Its purpose is to draw attention to certain problems that have still not been solved, or that have been given solutions of limited practical value, and to certain methods or algorithms which are highly esteemed by research scientists, but that in practice are not so effective and perhaps deserve less commendation.

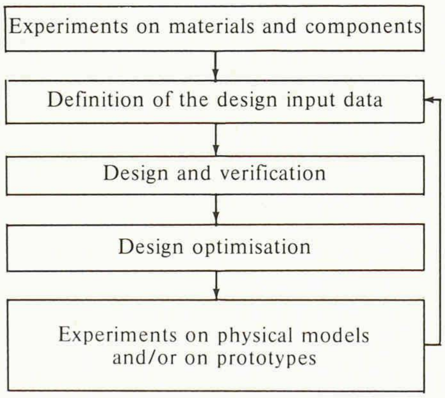
2. The mathematical Tools

C. Trusdell, who is well known for his penchant towards the paradox, recently held a conference in Milan with the title, «The Computer, the Ruin of Science». He pointed out that such a very powerful and precious tool might in the end dull or deaden the critical faculties of the scientist. However, if we look at the results that have been obtained in the field of structural engineering since the advent of the computer, it must be admitted that the analysis of the static and dynamic behaviour of even such highly complex structures as shells, space trusses, hanging roofs and so on has taken really great steps forward. An undoubted contribution here is the possibility to consider and store a vast range of possible load conditions, temperature effects and the interaction between the structure itself and its foundation soil. Methods such as those employing finite elements and boundary elements are today highly efficient tools for the structural engineer. More work, however, is still needed, and a number of targets have yet to be reached. On that is worth mentioning here is the possibility of using constitutive laws that are less elementary than the by now classical elasto-viscoplastic laws met with in geotechnical problems, or in ultimate limit state structural analyses, often associated as well with second order geometrical effects that cannot be neglected.

Beyond this immense amount of work, achieved with the help of the computer, a further question arises. Would it not be possible to revise and reform the traditional analogical approach, which was so fruitful up to the middle of the 20th century, but which now seems to be nearing exhaustion? It seems to me that any serious attempts to move in this direction would require the combined efforts of mathematicians and mechanical engineers for a re-interpretation of the theorical mechanical behaviour of structures.

But whatever happened to the experimental method, traditionally one of the two branches of research? Here we must unfortunately recognize that there has been a regressive tendency. Although in fact experimental work has continued on structural models, very little is being done on the materials themselves or the elementary components. Experimentations is needed here, too, in order to arrive at more appropriate laws and improved hypotheses for use in numerical models and design in general.

The study of any structural typology should include the following sequence:



Although there may be some praiseworthy exceptions, such as in the case of compressed steel struts mentioned earlier, the general trend today is to leave out the first step and refer to experimental results of the part, even the distant past, applied to materials and technologies that may well have changed quite considerably. One of the most seductive approaches at first sight might seem to be through a physical model of the structure as a whole. This should solve the entire problem, or at least supply suitable and sufficient input data. There are, however, two serious drawbacks. It is extremely difficult to account for the real imperfections, mechanical, geometrical and end restraints, in the model, and although this approach might solve the design problem of an individual structure, it is of little use for a structural typology.

The time would seem to be ripe for scientists engaged in research into the Mechanics of Solids or the Theory of Structures to turn once again, and more insistently, to fundamental experimental work. But at the same time it would also be extremely valuable if, by sifting through the mass numerical results now available, they could create new analogical methods for dealing with structural problems.

Among the various mathematical tools now available we should not forget the methods based on probability theory for defining the loads, strength and, in the end, the safety of building.

This is an approach of considerable interest, and its validity is by now universally recognised. But it seems to me that it certainly needs much further development and perhaps even re-formulation before it can be satisfactorily used for really solving structural problems.

First of all, a probability approach presupposes a degree of information on the extent of the parameters involved that in fact is almost never available in full. This is especially true for the actions that a building has to resist.

Above all, however, these estimates concern expected events (static collapse, out-of-service conditions) that by their very nature have very low probability of occurence. The results obtained when working close to the outer limits of the field of validity of a theory are necessarily much less reliable than would otherwise be the case. Probability theory has certainly enabled us to take a new, more judicious and realistic approach to the problem of safety and to define also qualitatively the significance of the simultaneous occurence of several unfavourable events. But equally certainly it does require much more information for the correct quantification of the parameters involved. Even some re-thinking on the theories to be adopted might well be advisable.

3. The Materials

The use o perfectly elastic-plastic instead of a perfectly elastic constitutive law opened the way to a great deal of research work that has been particularly fruitful, with results that are of practical utility and relatively easy to apply. In other words, this is a field of studies that is well organized and solidly based. However, the impact on engineering practice has been surprisingly limited. The fact of the matter is that the ultimate limit state, which corresponds to transforming the structure into a kinematism, is only one of the limits that must not be exceeded. Others, which put the structure out of service through excessive compliance or mechanical degradation (cracks, local fractures, local buckling etc...) are often even more important. The interpretation of such conditions places more emphasis on the size and nature of the strains rather than on the state of stress, and this il less satisfactorily dealt with by the theory of perfectly elastic-plastic bodies. Here, too, a great deal of research effort is still required, but courage is needed to face the problem with genuine originality. From this point of view it might be interesting to classify the various possible types of constitutive equations in order to identify those that might best satisfy the requirements of a particular material assigned to a certain structural type and conditions of use.

There is a particular class of materials that still requires a great deal of research-soil and rocks. The great variety of possible situations calls for a parallel diversification of rheological models to represent them. Here, too, there is still much to be done, much experimental and theoretical work to prepare new models that can satisfactorily interpret actual behaviour and facilitate the analysis of the stress-strain state. And it is worth underlining that here too the strains predominate.

It is also worth pointing out that, at least for some classes of materials, and here I am talking above all about metals, new horizons are being opened up thanks to modern «Fracture Mechanics» which is managing to combine the structure of the atomic lattice with the overall behaviour of the material. This will facilitate a more rational evaluation of constructional defects when faced with high or low cycle fatigue.

As to the polyester resins, which would like to become competitive with metals, there is still much to be learned — too much, as yet, both in terms of their characteristics as materials and, even more, as structural elements. There are certainly ample possibilities for experimental and theoretical investigation in this field, which so far has not attracted the amount of attention that it deserves.

4. Structural Shapes

In our field, the computer has perhaps been most helpful for structural shapes. By now, in fact, there are no great problems to be faced in the elastic analysis of structures made up of beams, membranes or plates, forming there dimensional systems of even considerable complexity. The static and dynamic stress analysis of residential and industrial buildings with orthogonal frames has become a mere matter of routine. But even much more complex structural systems can be dealt with by the computer. Today we can follow numerically, step by step, the mode of oscillation of a large span suspension bridge as one or more trains cross it, or analyse the aeroelastic stability of its deck with variations in wind spectra, or study its response to an earthquake.

The same sort of thing could be said of a large hanging roof. Although its shape and boundary conditions may be highly complex, and it may be subject to a considerable degree of geometrical non linearity, it can nevertheless be followed at all stages, from initial erection to its response to various weather conditions. However, in order to identify the problems that are still open, and so the fields of enquiry that may be most attractive for research, there is a particular point to be borne in mind — the problems that predominate today are involved with large dimensions and repeatability.

Bridge spans may be as much as a mile in length, or even more. Vast roofs can cover playing fields and space for as many as 100,000 spectators. Offshore drilling platforms can deal with depths of even several hundred metres. Great power stations are immense reinforced concrete fortresses, with walls several metres thick incorporating enormous quantities of steel. Even the orbiting space stations which are being planned for the future will have extraordinary dimensions, in terms of kilometres, as well as being subject to great thermal gradients, though not, of course, to problems of dead weight. But the theory of models has taught us, and it is an everyday experience, that it is an unacceptable over-simplification to apply the results obtained from the analysis of one building to another which is similar but of much greater size. So it might be said that these great new dimensions call for great new ideas. Are they in sight?

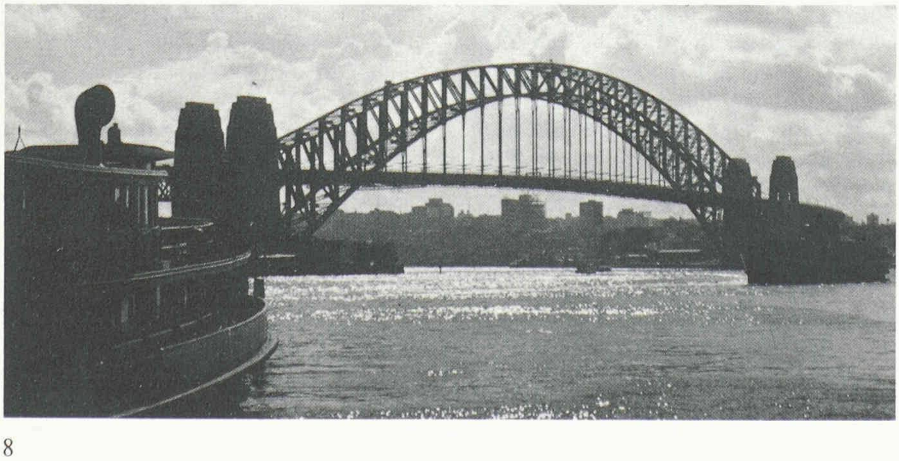
It seems probable that two dimensional structures may offer some of the more promising fields for possible innovations, whether they will be shells or sets of shells, or pretensioned textiles or cable systems. It seems to me that great size requires intelligent structural geometry. And the same sort of point could be made for structures involving a high degree of repetition. Since this is a matter of mass production, it generally implies processes of pressing, bending or extrusion which permit shapes that are freed from the constraints of traditional elements.

5. Structural Details

Structural details such as joints, connections, anchorages, bearings and restraints in general play a part of growing importance in the building industry. This is partly a matter of cost, especially when common, standard constructions are involved, but partly also because of their complexity — and this is certainly the case for very large structures. A thorough study of the structural detail almost always implies a two if not a three dimensional analysis. Besides a non linear type of analysis, it often even has to take into account thermal transients, as for example when weld shrinkage may be important for very thick elements.

This particular field is an exception to the general tendency. Here the experimental method has been preferred to the theoretical approach, whether numerical or not. So it seems probable that Structural Mechanics may have much to offer, for example concerning:

- the effects of imperfections depending on the technology of the manufacturing process of the material or element (tearing, welding defects, indentation effects etc.);
- the residual stresses due to the technological processes employed (punching, flame cutting, welding etc.);
- the study of joints as «equivalent springs» for the purposes of dynamic analysis. In fact there are no such things as perfectly rigid joints or perfect hinges, and the correct evaluation of their dynamic characteristics in overall terms is of primary importance. From this point of view the concept of the “minimum restraint” necessary to prevent certain instability phenomena is still rather vague. In fact there is a whole range of richly varied problems regarding one and two dimensional instability.



6. Concluding Remarks

As a structural engineer who has lived through the developments in steel construction of the past forty years, and taken part in the work of various committees engaged in the definition of design, application and erection specifications, what can I suggest to a young research scientist? How can he use his solid basis in physics and mathematics, and his expertise in numerical processing for problems in mechanics, to contribute to the progress of constructional steelwork? The following guidelines would seem promising.

a) On a sound basis of experimental work, the establishment of constitutive laws for both traditional and new materials, such as polyester resins, glassfibre — reinforced resins, textiles, rocks and soils. Many of these cases are highly anisotropic and show considerable dependence on time and load history.
b) The analysis of structural details (joints, connections, foundation restraints) through the technique of subdivision into discrete elements, in order to evaluate their overall behaviour and flexibility. The identification of optimization processes in terms of weight X unit cost.
c) The refinement of techniques for the non-linear dynamic analysis of structures through a more realistic and solidly based evaluation of the energy dissipation characteristics of the system, and of its resources in terms of ductility.
d) The setting up of structural optimization criteria correlated with the choice of the characteristics adapted for the boundary conditions, both external (restraints) and internal (joints and connections).
e) The re-formulation of theories on the stability of shells, starting from the numerical results that take into account geometrical and mechanical imperfections, to arrive at solutions that would be more general and less empirical than those available today.
f) A re-definition of the actions that a construction has to resist, whether they be normal, or exceptional, and of their various possible combinations, from the standpoint of probability theory, but giving more importance to a balanced distribution of risk rather than real probabilities.
g) A more systematic estimate of what is required by those parts of a construction (cladding, partition walls, ceilings, fixed and movable equipment) that have to cohabit with the structure in order that a better combination may lead to a more lasting union.
h) The acquisition of improved knowledge on the behaviour of mixed systems (steel — concrete, steel — rock or soil, steel — resins etc.).

The topics contained in this list differ considerably in methodology and complexity. But most if not all for them are examples of those relatively unexplored areas where considerable doubts still remain, and that deserve to attract the attention of the new generation of research scientists. Let us hope that they will not choose to follow, as some of their predecessors sometimes did, the wider and easier paths, but that they will feel the need to face the really important problems with a correct sense of priority.

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