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"On the Problem of Stiffened Suspension-Bridges, and its Treatment by Relaxation Methods." †

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Mr. R. F. C. Booth thought it too early to predict the possibility of sensible economies due to a consideration of the "u-effect". Even if, however, such economies were to prove negligible, the investigation would still be one of those essential contributions of mathematics which strengthened the foundations upon which the designer's art was based. That point was important because there was a noticeable tendency amongst practising engineers to decry occasionally a refinement of theory, on the ground that its effect might be buried beneath the mass of mechanical uncertainties which existed in any large structure.

In spite, however, of the development of more accurate methods of analysis, it was important to keep in mind the very real virtues of the elastic theory, most elegantly applied to the problem of the stiffened suspension-bridge by Max am Ende and Dr. D. B. Steinman. That theory enabled the structure to be regarded, not as a wanton breaker of Hooke's law, but as an entity whose elements all strictly obeyed that law. The divergence of the structure, as a whole, from the principle of superposition was to be regarded as a measure of the errors introduced by the use of that theory; those errors were then to be corrected by the application of an appropriate factor to the results. In the light of elastic theory, a stiffened suspension-bridge of given proportions and properties lay at a definite point somewhere between a totally stiffened bridge, for which that theory gave a correct result, and a totally unstiffened bridge, where the maximum error occurred. That point might be defined as the "degree of flexibility" of the bridge, to which a numerical value was easily assigned. That was am Ende's symbol "D", or Steinman's "N." If proper curves or Tables were constructed for permanent use, then values for H, bending moments, shears, and deflexions were immediately forthcoming for various loadings in terms of "D" or "N." Those results might then be corrected by the factors mentioned, which had been obtained by Steinman from a wide experience of the design of large bridges.

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Results having a surprising degree of accuracy would be obtained. In that way a large bridge might be brought to an advanced stage of design within a few hours, with the knowledge that a check by the deflexion theory would lead to minor changes only.

If it were shown that the Authors' refinement led to practical economies, it was to be hoped that it would prove possible to link their theory empirically to the elastic theory, just as Dr. Steinman had linked thereto the currently accepted form of the deflexion theory.

Professor C. E. Inglis, Vice-President, observed that the Authors had indicated how a displacement which hitherto had not been included in suspension-bridge calculations could be taken into account, although the cost of doing so was admittedly very great—so great, indeed, that very few would have the ability or the perseverance to adopt the Authors' method.

Since so much of their mathematics had to be taken on trust, he wished that the Authors had seen fit to illustrate the findings of their intricate analysis by numerical examples worked out for particular cases, so that it would have been possible to appreciate at a glance how far results deduced by the more exact theory differed from those obtained by the simpler methods generally employed. Without such evidence there was a feeling that the discrepancy for a normal form of catenary was not likely to be very noticeable, and possibly would be almost entirely masked by uncertainties in the flexibility of the cable-towers and the rigidity of their foundations.

The Authors asked engineers to take their analysis on trust, and from an inside knowledge of their reliability he was quite prepared to do so, but others might not be so trustful, and the average engineer would certainly hesitate to take advantage of the findings of mathematical processes which he could not even faintly comprehend. The practical engineer liked to keep his feet on the ground, and comparatively crude methods of analysis, as long as they did not take him out of his depth, were preferred to mathematical refinements which were beyond ordinary human reasoning.

For the determination of the primary stresses in frameworks, whether they were statically determinate or not, he rather resented the intrusion of differential equations, and for determining stresses and deformations in a suspension-bridge with a stiffened roadway he believed that a perfectly straightforward method of attack could be adopted. As the Authors pointed out, the difficulty of the problem lay in the fact that Hooke's law and the principle of superposition did not apply, but that difficulty could always be overcome by employing a step-by-step process, the load whose effect was considered being applied in successive increments. For each increment (and probably not more than three would be necessary), the added stress could be computed on the assumption that Hooke's law applied, and from the added stresses thus determined, the consequential change in the form of the structure could be deduced. The technique required involved nothing more than the ordinary standard analytical or graphical methods for determining total stresses and displacements, whilst tower-movements as well as elongations due to stress and temperature could all be taken into account. There was no need to consider which displacements should be included and which should be left out, for all could be included with equal facility, and, although the actual labour involved would certainly be considerable, it was felt that that would be more than offset by the directness and honesty of that perfectly straightforward method of attack.

Professor Southwell, in reply, expressed his regret that it had not been possible to consult with Mr. Atkinson, who had been mobilised with the R.A.F. That meant that he had to be held solely responsible for the remarks which followed.

He owed to Professor Inglis almost the whole of his early training in structural theory, and on that account it was with great regret that he found himself now in disagreement with almost all of what Professor Inglis had written. But it would serve no purpose to conceal the fact, and he could only state the position as he saw it, hoping that what was faulty in his statement would in time be proved erroneous, and that what (if anything) was of value would survive. The crux of the problem, as Professor Inglis had agreed, was the circumstance that Hooke's law and the principle of superposition did not apply. What was in question was how best to meet that difficulty.

The Authors' procedure had been (like that of Professor Timoshenko before them) to face the difficulty squarely and to show by a worked-out example that although the governing equation was not linear (so that solutions might not be superposed), it could yet be solved with all necessary accuracy in any case that might be presented. Their relaxation method was an alternative to Professor Timoshenko's method of computation, having, as they thought, some advantages; it could equally well have been applied to Professor Timoshenko's equation (3)—which, like their own equation, was non-linear—were it not for their belief that in it they had detected and corrected a flaw. That fact had necessitated two excursions into mathematics instead of one; for not only had the governing equation to be discussed from the standpoint of computation, but it had itself to be corrected by what seemed a more exact treatment of "u-effects". The cost of that might be great, but it had been defrayed entirely by the Authors, since the effect of their work for the computer was (*vide* section 17, p. 304 §) no more than a change of constants.

When Professor Inglis proposed a step-by-step method of solution, presumably he meant that as an alternative to the Authors' relaxation method of numerical computation. If so, however, it would be equally

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an alternative to Professor Timoshenko's method—and to any "methods generally employed"; since it started from equation (3) or from the Authors' amendment of that equation, it would be open to the objection (from Professor Inglis's standpoint) that a differential equation would have "intruded" into a statical problem; and why (it might be asked) should anyone look for yet another method, having already two that yielded results?

If, on the other hand, Professor Inglis's step-by-step process was meant to replace the Authors' discussion of "u-effects", Professor Southwell could only record his belief that the difficulties presented by the flexible catenary would not be overcome merely by taking them, as it were, in three gulps. (A cable and stiffening girder did not together constitute a framework, and in his memory of Professor Inglis's lectures those had recourse to differential equations when dealing with either singly: why then, when cable and girder were associated, should differential equations be resented?) However that might be, he had to contest an assertion made twice by Professor Inglis, that the Authors asked engineers to take their mathematics on trust: that he had found really incomprehensible. What, he would ask, could be done with mathematics except to present it in such detail that every step could be followed, and any error detected, by any reader having both the knowledge and the perseverance? That the Authors had done, and accordingly there was no question of their asking the "average engineer" to take results on trust: he might elect to do so (just as he might elect to take Euclid's theorems on trust), but that was his own affair.

Assuming that an engineer could be found with the patience to check their work, and assuming that he found it correct, the question, was all that elaborate analysis worth while, would of course still remain. The Authors believed that it was, on the ground that, although it was desirable to have simple methods, and still more desirable to know that they yielded trustworthy results, before that could be known they had to be tested, and test presumed a theory that was accurate, however complicated it might be. Professor Southwell was delighted to find that point of view expressed (and far better) in the opening remarks of Mr. Booth.

He entirely agreed with Mr. Booth that the need now was of comparisons between different methods; but that meant, of course, much work. It would not now, he feared, be done by Mr. Atkinson, and he could see at present no prospect of being able to do it himself; but he looked for such work to America, where interest in the problem was widespread. With Mr. Booth, he hoped that it would prove possible to link the new theory empirically with the elastic theory; and if that hope were also shared by American engineers, he had little doubt that the comparisons would be forthcoming.