vice reservoirs in the midst of the low lying country which is to be irrigated and thinks that these ponds would mean a considerable loss of head, or else spreading out the water to dry. Where these ponds are used in America the country is fairly undulating and that will also be found in some places here. But in such places, as in the lagoons of the River Murray and its Riverina tributaries, where the author has suggested, the country is so flat that these could only be of use for irrigation by pumping. Contrary to the decision of the courts of an adjoining colony, which held that the water should not be held in any lagoon or water course by a dam of more than seven feet above the average bed level; such ponds to be really useful should be held by dams of at least fourteen feet, and that only when by dredging or natural depressed formation the bulk of the water in the lagoon was of greater depth than the height of the dam. At first blush it will seem that the pumping of water from these ponds would be a serious drawback. But the cost of such pumping should never exceed 3/6 an acre foot, and could be done either by itinerant pumping engines, similar to the traction engines which at present serve such districts, or else in the case of the larger lagoons, or locked water channels, the plant could be placed on a punt and moved from place to place as required.

Since the paper was written it has come to the author’s knowledge that the Victorian Water Supply Department has adopted the units “cusec” and “acre foot.”

Discussion closed.

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**PAPER.**

**THE QUEBEC BRIDGE DISASTER.**

Read by Professor W. C. Kernot
(Past President).

In continuation of my paper of November last, p. 108 of vol. viii., and my remarks thereon of December last, p. 131, I desire to submit the following statement:—

Since the latter date the various engineering journals have published the very full and complete report of Messrs. H. Holgate, J. G. G. Kerry and John Galbraith, who were appointed as a Royal Commission to investigate the causes of the accident. This report shows that the view generally taken at the time, namely, that the collapse started by the lateral buckling of a part of the lower or compression chord of the southern anchor arm was quite correct, the section marked A9L and its fellow on the other side of the bridge marked A9R, being completely doubled up, and all their transverse latticing destroyed. It further shows that signs of
distress in these parts were visible and were a subject of discussion for more than a fortnight before the accident. Also, similar signs of distress at one point of the cantilever arm became apparent more than a week before the collapse. Two days before the end careful measurements were made, and showed A9L to be bent into a single curve with a versine of 2 inches in a length of 40 feet. Now a horizontal deflection of one inch in 100 feet in the handrail of a bridge is plainly visible, so that 2 inches in 40 feet, or five times as much, must have had a most alarming appearance—and yet the addition of weight was allowed to proceed. It is not my business to pronounce judgment on those responsible for this awful catastrophe. Had these men ever seen a column broken in a testing machine? Had they ever even tested a cardboard model? If they had, surely they must have recognised the terrible danger, the emphatic warning.

After the horse was stolen they proceeded to lock the stable door. In other words, they brought their magnificent testing machine of 1000 tons power, of which they sent photographs to me years ago, into action. A model of A9L of one-third full size, and consequently of one-ninth the strength and one-twenty-seventh the weight of the original, was prepared and broken. It gave way with 22,150 lbs. per square inch on the net section, whereas it was calculated that the compression in the bridge might, under extreme conditions of load and wind, reach 29,500. The actual compression at the time of the accident was computed by the Commission as 17,910 lbs. per square inch. The difference between this and 22,150 may be accounted for, in my opinion, in three ways.

1. Small sections of metal usually are slightly stronger per square inch than large ones.

2. It is probable that the workmanship of the model was better than that of the original, which is admitted to have been slightly defective from the first and to have been repaired.

3. The model was compressed between pins one foot in diameter fixed in direction, and that direction the one in which the model failed. In the bridge itself such assistance was by no means certain.

These three considerations abundantly account for the difference of stress at time of failure. The mode of failure was as expected. The excessively slight latticing referred to by me on p. 113 of vol. viii., and which was made worse by being insufficiently riveted at the ends, gave way at many points. At most it failed by shearing the rivets, which had an effective area of barely half that of the bar they fixed. At one the fracture was complex, the bar being badly buckled and the rivets strained. The longitudinal parts were bent in an S curve at the centre panel of the bracing, but elsewhere remained nearly straight. Had this experiment been made before the bridge was constructed it would have revealed the appalling fact that instead of the safety factor being at least three, as it ought to have been, it was considerably less than unity.
Shortly after the reading of my last remarks the idea suggested itself of trying a small experiment of my own. Visits to an adjoining State delayed my making the cardboard models I contemplated, and when at last they were completed, a long and severe illness prevented my testing them. At last, however, this was accomplished, with the following results:—

There were two models, equal in size and weight, 19 inches long, and 6 1/2 ounces of material. The material was ordinary cardboard stuck together with strong gum. One model was made to resemble A9L and the other compression parts of the Quebec Bridge. The other had much stronger latticing, containing about 6 times as much material, while the longitudinal parts were made lighter in order that the total weight might be unchanged. Each model was in external dimensions one-thirtieth the size of A9L. In order that the conditions of the test might be as nearly ideal as possible, and the models free from all lateral constraint, they were compressed together between two plates of iron, each resting against a bicycle ball of .35 inch diameter, these balls being carefully adjusted in the exact axis of the model. The testing was conducted in the University testing machine, which is capable of being read to a single pound of pressure. The result was that the Quebec model failed by the yielding of its weak diagonal bracing with 306 lbs.; while the other, with lighter longitudinals but heavier bracing, carried 508 lbs., or nearly double as much.

Now I do not desire to build too much on little card models. I know full well that the structure of cardboard is very different to that of steel, and gum is by no means an exact equivalent of a group of rivets. But this I think I may fairly say, that had a few such models as mine been tested by the designers of the Quebec Bridge, they would have given them such a warning that they would certainly never have used the form that was so unsatisfactory in cardboard without further tests in metal—and had they done so the greatest structural disaster in engineering history would have been averted.

The question of what ought to have been done had those in authority realised the urgency of the case, say, a week before the catastrophe, is perhaps worth discussion. Well, first, I would say all work should have been stopped, and all persons ordered off the bridge. Thus loss of life would have been obviated. Secondly, in order to save the structure, or as much of it as possible, the following course might, I think, have been reasonably followed. A small party of picked men, availing themselves of a time when there was no wind, might have fitted timber struts, as suggested by me on p. 111 of vol. viii., to assist any member showing signs of buckling. This would be, confessedly, a dangerous task; but not more so than, for example, the demolitions after the great Flinders-street fire of about 12 years ago, or the more recent one at Wallach's in Elizabeth-street. This being done, further proceedings might be characterised with more confidence. Weight might have been removed by throwing heavy
articles into the river, or by dragging them off the bridge by a long wire rope leading to an engine on shore; no person being allowed on the bridge while any operation causing the slightest jar was going on. Meanwhile, the experiment with the model of A9L would have been made, and would have demonstrated the fatal weakness of the compression elements of the bridge.

Possibly small charges of dynamite fired from a distance by electricity might have been used to detach successive portions at the outer end. Experience and observation of bridge wrecks in South Africa during the war induces me to think this course not impracticable. By the time the overhanging part of the bridge had been shortened by, say, 200 feet, the stress on A9L would have been reduced by about 30 per cent., and then operations of demolition might have proceeded more actively, and the number of men allowed on the bridge increased to, say, a dozen. When 300 feet of length had been disposed of, the stresses on the weak parts would have been reduced fully 50 per cent., and the structure have become fairly safe. Then dynamiting and other extreme measures could have been discontinued, and the rest of the structure cut into lengths, taken down and stacked, to be re-erected with improved compression details. In this way, I believe, fully, three-fourths of the bridge might have been saved for future reerection; while in the event of a general collapse, the loss of life would probably have been nil, and certainly very small.

That the building of a bridge at Quebec should be finally and absolutely abandoned would be a disgrace to the engineering skill and enterprise of the 20th century. But when the new bridge comes to be designed I would put in a plea for the suspension system, with cables of straight parallel wires as at Brooklyn and Williamsburg, New York. For spans of over 1000 feet this has great advantages over the cantilever. First, every ton of material used will carry more than double the weight in the form of wire cables than in the form of eye bars and struts as used at Quebec. Second, the erection is simple and easy, the wires being put up one at a time and then finally bound together, or served, as the sailors say, into one rope. There is no risk or difficulty, and the separate weights to be handled are small; whereas with the cantilever system the weights to be lifted are large and unwieldy, and the operation of making the final junction in the centre, and simultaneously detaching the girder portion from the cantilever, difficult and dangerous, as the experience at the Forth Bridge showed.

The only objection to the suspension bridge is its flexibility under partial loading; but this, while very serious in small structures, diminishes as size increases, and the proportion of dead to live load becomes great. Then, again, the design of stiffening girders is much better understood than it used to be, while the discovery of nickel steels of great strength and small expansion under changes of temperature should aid greatly in reconciling the conflicting claims of change of dip of cables by temperature, and the making of rigid girders to carry irregularly distributed loads.
The old Brooklyn Bridge of 1595 feet span is at present carrying trains of 236 tons at considerable speed, though its stiffening girders are very imperfect in the light of present knowledge. Surely, then, a span of 1800 feet at Quebec should be easily rendered sufficiently rigid under trains of, say, 500 tons.

In conclusion, I would say that:

1. The Quebec Bridge was designed without proper experimental investigation of the strength of its compression parts, the internal bracing of which departed largely from previous approved practice.

2. For the sake of economy an unprecedentedly small factor of safety was adopted, such factor being considerably under 3 in large and vital parts, instead of not less than 4, as in British practice.

3. Owing to errors in estimating the weight of the structure, the actual stress became about 10 per cent. greater than was anticipated.

4. The structure gave way in the manner that might have been anticipated, and further, gave clear and unmistakable warning of approaching disaster for many days before the final catastrophe.

Finally, I would reiterate and emphasise my remarks of last year as to the importance of model experiments and systematic extensometry in connection with bridges of large size and novel design.

DISCUSSION:

The President said that Professor Kernot's views upon the cause of the accident had been submitted to the Committee of Enquiry constituted to determine the cause of the failure, and the Professor’s opinion had received due acknowledgment and consideration.

In relation to the question of tests, he could not find, in any of the published reports that had come under his notice, that it had been actually given in evidence that such tests had not been made. Was Professor Kernot acquainted with any positive statement?

Professor Kernot said he was not in a position to say that such a statement had been made. He had not seen it. But if they had made preliminary experiments they surely would have mentioned them. They had given full particulars of the experiments made afterwards. They had a great many experiments of the tension parts, but the compression parts had been left to take care of themselves. The engineering journals had not given the complete evidence, but had given large extracts. He had written asking for a complete report of the evidence, and when that came to hand he would perhaps have something further to say on the matter.

The President said that the inference certainly was that tests had not been made. Professor Kernot had suggested the testing
to destruction of certain members or portion of them. There were about 100 units of this particular type, and a model to one-third scale would only weigh one-twenty-seventh of the weight of the actual unit; hence a test of such a model would involve the destruction of not more than 1/2700 of the whole material used.

Was not the general lesson of the evidence, the criticism, and the Committee's finding, a corroboration of the feeling that had of recent years been growing that too much stress had been placed upon formulae, and a recognition that engineering was more than formulae? The necessity for close, personal acquaintance with the physics of the material, its weaknesses as well as its strength, to be acquired only by actual contact in the workshop or testing laboratory, had been emphasised in the evidence, and should be emphasised.

Mr. M. E. Kernot said the whole question brought out very strongly the desirability of conducting experiments in connection with all large works. As the result of his own experience he believed more and more fully in simplicity and actual check of work by independent methods. They should not check their work by rapid process, but should check it in different ways. They should always, as engineers, stand out for sufficient time to make their designs and work them out carefully. In checking their work they obtained a satisfaction they did not get in any other way. He would advocate relatively great expenditure in testing anything out of the ordinary groove. It would be very desirable to have a national testing laboratory, where complete appliances could be provided for making tests, and in that way they could be carried out at a minimum of cost. It would have a very wide scope. Such precautionary measures might have saved the Quebec Bridge.

Professor W. C. Kernot said he quite realised the inexpediency of rushing things through in a hurry. When they particularly desired to go into a matter very thoroughly, that particular thing would always be wanted at desperately short notice, and risks might be run and chances taken in the hope that it would be alright. He did not think the biggest span bridge in the world should be designed in a hurry. They should have had a great many models of that bridge, which should have been tested in a great many ways. Apart from the great loss of life and the deprivation of the public from the use of the bridge for many years, they would have prevented the loss of half a million sterling.

Discussion postponed.
Mr. J. T. Noble Anderson has gone very fully into the status and emoluments of the Australian engineer as compared with others. With much that he says one must agree; it is satisfactory to feel that the reference to this important subject has lead to some consideration of the question by the members of the profession in Melbourne.

The most serious difficulty lies in the almost entire absence of private enterprises involving big engineering work in this country, together with the wholesale manufacture of engineers by the State Universities, for the majority of whom the best opening will be one offering but little more than a "living wage"; whilst, in the absence of any body, person, or department to determine the value of professional knowledge and training, it is only natural that the average layman should regard such as being about equal to that of clerk or "handy" man.

The insecurity of tenure applying to many appointments in America leads to a certain reckless disregard of certain professional etiquette, which is not desirable to introduce in Australia. It must be borne in mind that "spotless honour" is as much part of the engineer's qualification as is his skill and judgment.

Discussion closed.

THE QUEBEC BRIDGE DISASTER.

Professor W. C. Kernot read the following addendum:—

For the benefit of any one not possessing ready access to the journals containing details of the Quebec bridge I have prepared Figures 1, 2, and 3 from the "Engineering News" of April 10th and 23rd. Fig. 1 is a half elevation showing the position of A9L, whose failure caused the disaster. The full lines show the part erected, the dotted ones those not yet erected at the time of the accident. Of the other half of the structure nothing but foundations were in existence. The centre span is 1800 feet, rather larger than that of the Great Forth bridge. Fig. 2 is a cross section of A9L, its external dimensions being approximately 5 feet wide and 4 feet 6 inches deep, while its length was about 60
Fig. 3 is a plan showing the latticing, the weakness of which was the cause of the failure. The deck of the bridge is carried by cross girders attached to the main trusses, and is approximately at the level of the bottom of the central part of the bridge, or of the left hand end, where the short approach span is indicated by a single horizontal line.

[Professor W. C. Kernot exhibited cardboard models of the members—A9L and A9R—which were responsible for the disaster.] These parts of the bridge appeared to have been designed by guess work. The one model was made to represent A9L; in the other, the amount of material in the longitudinals was reduced, and with the material thus saved the lattice work was made several times stronger, the total amount of material in the two models being the same. In the testing machine the first model gave way at 306 lbs.; whilst the second, with the increased bracing but the same total weight of material, gave way at 598 lbs. Had the latter carried only 14 lb. additional it would have been exactly double the strength. His models were very unpretentious, and he would like to continue his experiments further, when he thought something better still might be obtained. From the way those models had behaved he would not despair of raising the breaking load to 700 lbs., or even more. But enough had been done to show that the form adopted at Quebec was certainly a very bad one. He might say in general terms that he was a great believer in experiments. He would like to see metal models at least quarter full size; but would rather have cardboard models than nothing. There were considerably over 100 of that particular form of member in the Quebec bridge, and if they had tested one model to destruction it would not have been unduly expensive.

In the Melton viaduct on the Ballarat railway there were just about 100 main girders. These were probably sufficiently strong and had proved satisfactory, but in a structure containing 100 main girders it surely was advisable to test one to destruction. Theory was all very well, but it should be checked by experiment in every possible way. The fact was that engineering problems were much more complicated than was commonly supposed. The mathematical treatment was simple and straightforward enough, but when they came to actual engineering all sorts of peculiar
things cropped up which they could not fully determine except by plenty of experiments.

Some three or four years ago he proposed to have standard designs for bridge girders. A competent board was to settle

**FIG. 4.**

![Cardboard model of Quebec system of construction](image1)

Fig. 4 represents the cardboard model of the Quebec system of construction. The upper end is bent to one side and the lattice bars badly strained and buckled.

**FIG. 5.**

**TESTED AS COLUMNS.**

![Improved construction model](image2)

Fig. 5 represents the improved construction, with less material in the longitudinal parts, but about six times as much in the latticing. The longitudinal parts are buckled at the upper end, while the latticing is almost uninjured. The peculiar arrangement of the latticing is due to the fact that after the main double system of triangulation was fixed it was discovered that the weight was somewhat less than that of Fig. 4, and the difference was made up by adding another single triangulation of lighter section. Whether this lighter triangulation really improved the result is not certain. In Fig. 4 the lattice bars are all flat, and therefore apparently not good for compression, but they are so thick and so well supported at short intervals by attachment to the longitudinals that it is doubtful if an angle section would have had any perceptible advantage.

In the experiments the lever of the testing machine was allowed to fall only a quarter of an inch. Thus the primary fracture was obtained and the total destruction of the models prevented.

upon certain types, have them discussed from the mathematical point of view, from the point of view of manufacture, and from the point of view of erection and maintenance, and then, having arrived at what they considered the best types, make models of them and test them to destruction, and so verify or modify the results. These types could remain standard for, say, ten years,
when the board might be called upon to revise them and bring them up to date. Without going a mile from that place he could show them a girder made at great cost several times stronger than was necessary, and another in which the factor of safety was decidedly below what he regarded as judicious; and similar inconsistencies were continually to be seen in other cities.

With reference to the acknowledgment quoted by the President, in which the Government committee thanked him for assistance in connection with the Quebec inquiry, he might say he was immensely surprised when he saw it. He had written to them giving the results of some of his experiments and pointing out the extraordinarily small amount of metal in the lattice work compared with what was usually used in Victoria and South Australia, but that was all.

Discussion postponed.

PAPER.

THE WORKING OF SUBMARINE CABLES.

By Mr. J. C. Lockley.

When the second Atlantic cable was laid in 1865 rumour has it that it worked for only a short period and then ceased to speak. Its last word was the common commercial term "forward." I do not know if there is anything in omens, but the promoters accepted the dying word of this cable as their motto. About a year after a second cable was submerged and the first brought to life again. Since then cables have so multiplied that there is hardly a stretch of water a few miles wide on this planet whose bed is free from the nursing care of a telegraph cable. Those across the Atlantic are so numerous and bunched together that it must be a difficult problem for the cable engineer and navigator to feel assured he has got his own when it becomes necessary to lift one.

The first Atlantic cable was laid in 1858. The first engineer was under the impression that ordinary receiving instruments could be used for the reception of messages and used 500 Daniel cells for signalling. Having failed to get anything through by this method he then tried a powerful electro-magnetic machine, but with similar results. It was at this juncture that Sir William Thompson, who was acting as an assistant engineer at Valentia, invented the Mirror instrument and requested permission to use it on the cable with low power. This permission was granted, and all the signals received at Valentia were read from the mirror, but unfortunately the cable had been damaged to such an extent
He moved a hearty vote of thanks to Mr. Hall for his extremely lucid and interesting lecture.
The vote was accorded by acclamation.
Mr. T. S. Hall briefly returned thanks, and at 10.30 the meeting terminated.

DISCUSSIONS.

THE QUEBEC BRIDGE FAILURE.
The President called upon Professor W. C. Kernot to reply.
Professor W. C. Kernot said he had nothing to reply to. He thanked members for the attention they had given him on three separate occasions. Nothing had been controverted; and the matter having been submitted to a body of experts, he felt strengthened in his conclusions as to the bridge in question. He thought the accident was an entirely unnecessary one; and with proper arrangement such an accident ought not to happen. He would say once more that he believed in experiment. If they could get nothing better, a little cardboard model would tell them a great many things; but, of course, in the case of a bridge of the magnitude of that at Quebec he would have had experiments of a much more elaborate kind. If they had spent £5000 or £10,000 on experiments in extensometry, and experiments to destruction, the accident could not possibly have happened. They might have made a splendid bridge, saved the loss of half a million of money, and about 75 lives.
Discussion concluded.

THE WORKING OF SUBMARINE CABLES.
The President desired to know whether, in resuming the discussion, Mr. J. C. Lockley desired to add to his previous replies to questions.
Mr. J. C. Lockley said he had nothing to add in that respect. Since the paper had been delivered a great deal on the question of wireless telegraphy had appeared, and he thought it all tended to emphasise what he had said at last meeting. In dealing with wireless telegraphy they should understand that it was only in its infancy, although it had been greatly improved within the last year or so. But the trouble was, he thought, that those who were interested in it had been too eager to push it forward as a practical means of telegraphy. As a means of communication between lighthouse and ship, or ship and ship, or ship and shore it was really a most valuable adjunct to commerce and mankind generally; it undoubtedly had great and beneficial uses. But to
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