Further discussion on an article published in the

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The effect of varying the pre-tension and area of reinforcement on the ultimate bending strength of concrete beams*

by J. M. Prentis, M.Sc.(Eng.), Ph.D.

Further contribution by K. Hajnal-Kónyi, Dr.Eng.,
M.I.C.E., M.I.Struct.E., M.Am.Soc.C.E., M.Cons.E.

Dr Prentis now admits that beams A1 and NA1 failed in bond, but still maintains that all his other beams failed in compression. All his evidence is based on a single test by Hognestad and Janney which "makes nonsense" of my calculations. He accuses me of misapplying a quotation from Janney's paper because "all Janney's bond failures occurred at loads only slightly in excess of the cracking load", whilst all except two of his beams, which are the subject of the present discussion, "failed at loads considerably in excess of the cracking load".

Dr Prentis seems to have ignored the footnote to my contribution which, although very short, should have explained to him the reason for the big difference between cracking moments and ultimate moments in his beams. In Janney's beams, the destruction of the bond between the loading points and the supports was the end of the test because the wires had no end anchorages, whilst in the beams tested by Dr Prentis the end anchorages allowed a further increase of the load until the beams failed "in compression" as a result of the extensive elongation of the no longer bonded but still end-anchored wires.

In an end-anchored beam which is not over-prestressed, there is generally a considerable difference between cracking moment and ultimate moment, and since Dr Prentis's beams were end-anchored, the ultimate moment was bound to be much bigger than the cracking moment. Thus, Dr Prentis's main argument against my comparison of his tests with Janney's cannot be maintained.

Dr Prentis's further remarks on bond failure are based on a mistake in logic. The single test by Hognestad and Janney has proved that bond failure can occur by flexural bond stresses even if the average bond stress between loading point and support is very low. From this Hognestad and Janney have rightly concluded that "flex-

ural bond stresses at the crack are the best clue . . ." as quoted by Dr Prentis. It does not follow from this conclusion that bond failure cannot occur if the flexural bond stress is low. Indeed, high local bond stresses do cause bond failure, whether they occur in the region of zero shear or in the region of maximum shear. Dr Prentis relies entirely on flexural bond stresses as the only criterion of bond failure and does not seem to realize that there is a difference between a "sufficient" and a "necessary" condition.

I pointed out in my first contribution that the bond stresses in my Tables are average values and that, in fact, much higher peak values occur. The lowest average in my Table I is 159 lb/in².

This is related to the whole distance between loading points and supports. The value of 190 lb/in² in the beams of Hognestad and Janney which Dr Prentis accepts as a limiting value is related to one-third of this distance. It is obvious from Figure B that the average of 190 lb/in² related to one-third of the distance corresponds to an average very much smaller than 159 lb/in² if it is related to the whole distance.

If the surface conditions of the wires in Janney's and Dr Prentis's tests were similar, bond failures in Dr Prentis's beams must have occurred already at a smaller load than the maximum (see footnote to my first contribution). For the purpose of assessing the ultimate moment, the beams should therefore be considered as non-bonded, end-anchored beams. I have always admitted the possibility of applying Professor Baker's theory to such beams.

I am in full agreement with the last paragraph of Dr. Prentis's reply, which is a quotation from Hognestad and Janney. I therefore admire his courage in presenting a diagram purporting to determine the limit between bond failure and compression failure. I have never seen a diagram based on more misunderstood experimental evidence than his Figure J.

This brings me back to Figure 4 of Dr Prentis's original article. It would not have been objectionable had he stated in his caption what he really did plot but, with the caption as published, his diagram is definitely misleading.

*Pages 143-150. Discussed in *Magazine of Concrete Research*, Vol. 8, No. 23, pp. 112-123.

Further discussion

It is obvious from his reply that he does not believe in plotting experimental results in the usual manner and considers an "experimental verification" of a formula of "little purpose". He is, of course, fully entitled to this view on which I do not wish to make further comments. His Figure J seems to show that even beams which, in his own interpretation, failed in bond can be made to suit Professor Baker's theory, although the theory itself is based on the assumption that such failure does not occur.

The reader has to realize, however, that the abscissae in the lower part of Figure J representing the neutral axis n bear no relation whatsoever to experimental results.

I am at a loss to understand the meaning of "qualitative" strain readings. In his reply Dr Prentis says that "the experimental values of n_1 are inaccurate". If so, it is unfortunate that values of n and n_1 are included in Table 1 with an apparent accuracy of 0.01 in. and 0.01 respectively.

The discussion between Professor Baker and Dr Prentis and Figures M and L in Dr Prentis's reply are of particular interest because they reveal the arbitrariness in the assumption of the basic coefficients that are necessary for the application of the theory. This has been and still remains my main objection to Professor Baker's theory, particularly if it is applied to beams with bonded wires (i.e. beams which *truly* fail in tension or compression).

In this connexion I have to mention that before I succeeded in producing tension failure in the two beams with untensioned 12 gauge wires*, I obtained bond failure in several similar beams. There was no ambiguity regarding the mode of failure since the wires, which originally protruded from the ends of the beams, disappeared in the concrete like worms in the ground, and a wide crack opened at which the concrete was crushed. This happened at loads far in excess of the load at first cracking, but well below the load corresponding to the ultimate tensile stress of the wires.

I did not publish the results of these tests because they have proved absolutely nothing else but the fact that failure is caused by sudden slipping of the wires if the bond is inadequate and that the load at which this happens is unpredictable. However, I have no doubt that with a suitable choice of $F'e'e_c$ all these beams could have been made to fit Dr Prentis's Figure J.

Only by careful surface treatment was it possible to change the mode of failure and to break the wires.

Later on I succeeded in reaching the ultimate tensile strength of the wires in a beam of the same size containing seventy 12 gauge wires in concrete of a cylinder strength of 4,300 lb/in². An analysis of this beam will be published in *Concrete and Constructional Engineering*.

With regard to over-reinforced beams, it is true that I did not originally believe in Professor Baker's prediction of an increased value of M/bd^2c_u with increasing prestress,

but I am always prepared to accept the result of tests as decisive and, indeed, I acknowledged this in the quotation criticized by Dr Prentis which was published more than three years ago.

In conclusion, I suggest that Dr Prentis should repeat his tests without leaving the end anchorages attached to the beams.

Further contribution by S. Revesz, M.Sc., D.I.C., A.M.I.C.E., A.M.I.Struct.E.

In his reply to the discussion in *Magazine of Concrete Research* No. 23 Dr Prentis says:

"neither of Mr Revesz's untensioned beams should have failed by steel fracture because this entails an increase in steel stress of about 300,000 lb/in². In one of these beams, however, the steel did fracture. It also fractured in beams G and H where the steel stress increased by about 200,000 lb/in². This means that Mr Revesz's wires must have had much better bond properties than Janney's and mine . . . According to Professor Baker's theory most of Mr Revesz's beams should have failed in compression."

From this it seems to me that Dr Prentis's only reason for describing the failures of beams (his and mine) as failures in compression is that the secondary effect of failure is the crushing of the concrete. Such a use of the term "compression failure" would be contrary to accepted usage, and would be misleading.

Surely Dr Prentis would not classify a tensile failure (by yielding of the steel and subsequent crushing of the concrete) as "compression failure"?

Dr Prentis set out to investigate pre-tensioned beams. Contrary to normal practice, he left the tensioning device in position to act as end-anchorage and to restrain the wires at the end of the beam during the test. I wonder if he realized that by so doing he prevented his observing any evidence of bond failure.

Reply by J. M. Prentis

I find in the second contributions of Dr Hajnal-Kónyi and Mr Revesz nothing which makes necessary a revision of the views I expressed in reply to their previous comments on my paper.

Dr Hajnal-Kónyi does not attack my analysis of the bond conditions based on the criterion of the flexural bond stresses but questions the criterion. I do not understand his views on this matter: Dr Hajnal-Kónyi introduced the work of Janney into the discussion and gave a clear description of the way in which the wave of flexural bond stresses is propagated along the reinforcing wires. And yet, when he comes to numerical analysis of my beams, he ignores all this and uses instead a method of calculation which is discredited by the test of Hognestad and Janney; a single test, to be sure, but a critical one.

If bond failure is a result of the flexural bond stresses, the conclusion that the failing load is independent of the

*HAJNAL-KÓNYI, K. Tests on concrete beams reinforced with 12 gauge wires of an ultimate strength of 120 tons/in². *Magazine of Concrete Research*. Vol. 3, No. 9, pp. 113-129.

shear (and hence the average bond stresses) cannot be avoided. The test referred to verified this conclusion.

Dr Hajnal-Kónyi now claims that the flexural bond stresses do not provide the only criterion. This view might be acceptable if it were supported by reasoned argument stating the conditions under which (a) the flexural bond stresses, (b) the average bond stress, (c) other factors, are critical. All we have so far is an assurance that the average bond stresses were the governing factor in my tests even though they were not in Janney's. Dr Hajnal-Kónyi's assurances, however often repeated, are not "sufficient".

In an effort to justify a misapplied quotation, Dr Hajnal-Kónyi draws attention to the footnote in his previous contribution. He considers that for the purpose of assessing the ultimate moment my beams should be considered as non-bonded end-anchored beams. Having come to this conclusion it would surely have been proper to delete a quotation referring to beams in which the ultimate strength depends upon the flexural bond stresses. Instead the behaviour of my beams is alleged to be very well summed up by the quotation. Quite apart from this, my previously stated objection stands.

Dr Hajnal-Kónyi has, apparently, unusual ideas regarding fair reporting: without any qualification whatsoever he takes four words from a forty-word sentence of mine to say "He (Dr Prentis) . . . considers an 'experimental verification' of a formula to be of 'little pur-

pose' ". The reader is invited to compare this with what I did say on page 120 of Vol. 8. The preceding paragraphs give my objections to what Dr Hajnal-Kónyi calls the "usual manner" of plotting experimental results. As I consider Dr Hajnal-Kónyi's method of plotting to be most unusual, I do not agree that the caption to Figure 4 is misleading. Apparently Dr Hajnal-Kónyi was not misled.

I have already dealt in detail with the problem of selecting suitable coefficients for Professor Baker's theory and have nothing further to add on this point. This theory is expressed in equations (1) to (4) on page 119. There is no implicit assumption that bond failure does not occur. Allowance for this contingency merely imposes an additional condition. There seems to be no difference in principle between saying that the steel stress must not increase by more than, say, 100,000 lb/in² if flexural bond failure is to be avoided, and saying that practical considerations demand that the prestress in the steel should be between say 100,000 and 160,000 lb/in².

Mr Revesz questions my use of the term "compression failure". I agree with the definition given in his first contribution that "compression failure is due to insufficient concrete strength in the compression zone". I hold the view that with the exception of beams A1 and NA1 all my beams come within this class as in each one an increase in cube strength would have resulted in an increased ultimate moment.