DISCUSSION • DISKUSSION • DISCUSSION

CHAIRMAN

I would like to thank all the authors of this morning, we have had a set of very informative papers and I hope we shall have a good discussion. We have 6 or 7 questions and two requests for further comments. I will try and fit it all in. The first is a question from Prof. Ross on Dr. Willam' s paper: Would Dr. Willam agree that the constitutive relationship for concrete derived from experiments is heavily dependent on the test equipment. For example, if the strain is controlled concrete can exhibit an extended strain softnening zone even in uniaxial tension.

Dr. K. WILLAM

I think Prof. Ross made a very good point in questioning the stability of the input parameters which we have to use in our analysis. Actually, I showed in my second last slide some experimental results on tensile test specimen by Hilsdorf et al., making the point, that the input parameters depend on the type of test, on the specimen geometry and on the loading rate; all that influences very heavily the values which we have to use as input data. Therefore, one can only say that the tests should simulate closely the actual conditions in the structure.

CHAIRMAN

The next question is from Mr. Cedolin: From the very interesting comparison you made in your paper on the analitycal treatment of biaxial stress states by means of variable bulk and shear moduli, tangent modulus and Von Mises yield criterion it would appear that in a finite element displacement formulation the use of bulk and shear moduli would be most efficient. I wanted to ask the reason why shear modulus is shown to be dependent on shear stress and not on shear strain, thus requiring you to solve a non linear equation when you want to pass from calculated strains to stresses. Secondly I wanted to ask if you have any experience in generalizing this approach to triaxial states of stress.

Dr. K. WILLAM

Thank you for this question because it brings up a very important point. We made an extensive study on constitutive models which can be termed non-linear elastic, a name which has also been used by Prof. Zienkiewicz for defining the variable modulus method.

As you know, we describe in this case the non-linear deformation behaviour by scalar functions of stress and/or strain like the modulus of compressibility or the shear modulus. In concrete literature there have been a number of papers on this subject. In our example we compared the predictions of several models with the underlying experimental results.

As long as there is only monotonic and proportional loading, there is no question whatsoever in approximating the non-linearity up to any degree of refinement. This is basically only a problem of curve fitting. Unfortunately, in the actual structure there is no such thing like monotonic loading and proportional loading and I think we are on very shaky ground if we identify our non-linear constitutive model from very simple experiments and apply it subsequently to predict the complex behaviour of actual structures. However, in praxis this simple model is often adopted because it can be implemented neatly within the finite element method through the concept of tangential stiffness. In a matter of fact the variable modulus method is the most straight forward approach for incorporating non-linear material behaviour, but we have to be very cautious in applications. I think, the non-linear elastic model is just too dangerous on the long run, it provides rapidly solutions; for instance, we have presented some results at the 1st International Conference on SMIRT, Berlin, 1971 using a non-linear formulation for triaxial concrete behaviour exactly along that line; but now our viewpoint is that we are on rather shaky grounds, and that we have to restrict applications to monotonic and proportional load regimes which correspond to the test conditions. Besides, for limit load analyses, the non-linearity before failure is of secondary importance and can be neglected in general.

CHAIRMAN

This question is from Mr. Wallushek-Wallfeld: If a concrete element cracks by tensile stress you have the cracking surface perpendicular to the principal stress. But could you tell us anything about the assumption of the surface of an element cracking by compressive stress.

Dr. K. WILLAM

I am wondering about the term compressive cracking, that has been a question already yesterday. One can distinguish certain zones in the failure surface where we hyphothesize that our constitutive model is based on e. g. perfectly brittle or perfectly plastic behaviour. That means, the failure remains surface fixed in principal stress space except for the tension quadrants where it collapses. If you are not happy with that type of thing, and plasticity people have not been happy with that for a long time, one could be introducing strain hardening concepts for modeling local non-linearities during deformation before reaching collapse. Similarly, these hardening concepts also can be used to simulate strain softening effects in the tensile stress regime which corresponds to local instabilities. The failure surface due to cracking is then governed by the tension cut off criterion. On the other hand the elastic-plastic formulation does not provide any information on discrete failure surface orientations in compression, it does not predict any preferred direction of weakening due to micro cracking. Only in the case of Mohr-Coulomb criterion two discrete planes of sliding can be distinguished.

In summary the strain softening elastoplastic constitutive model, as we proposed it, is based on two assumptions: In the compressive range we assume that perfect plasticity governs the post failure behaviour. In the tensile regime which is determined according to the principal stress state, we have two possibilities, at one limit we assume perfect plasticity according to the ductile model, on the other we can also use the concept of ideally brittle behaviour; that means the failure surface in tension collapses upon intersection by the load path to the coordinate planes of the principal stress space.

CHAIRMAN

From Mr. Finzi: Could you please tell us about the possibility of establishing a triaxial strength theory for reinforced non pre-stressed massive concrete structures? Reference is made to concrete reinforced by orthotropic three dimensional mesh or bars.

Dr. K. WILLAM

My presentation was basically restricted to plain concrete and pre-stressed concrete. With regard to reinforced concrete we have adopted till now always the following position: Basically, we have two constituents, reinforcement and concrete. Each behaviour can be dealt with individually by the means which we have shown in principle before. The plain concrete behaviour is modeled by the strain softening elastoplastic formulation, the reinforcement is in contrast rather simple. Normally, we adopt a strain hardening elastoplastic model. The main problem is to discretize the reinforcement in space. In particular the question arises there what scale of observation we should adopt. Either we smear the reinforcement layers by introducing orthotropic material properties in "equivalent" solid elements, or we restrict the scale of observation to smaller samples, then we can represent the reinforcement by discrete layers of bars; these are the two possibilities. Until now we have always adopted that there is full bond between the two constituents and I admit that is a rather questionable assumption, especially near ultimate load.

CHAIRMAN

Prof. Zienkiewicz asks about the nature of a tensile test. Do you want to enlarge on your question?

Prof. O. C. ZIENKIEWICZ

(After having commented on the direct and indirect tensile tests). Such tests apparently show different values of tensile strength, and it would be of interest to consider the reasons for this on the basis of:

a) a strain softening effect after the maximum strain is reached

b) a statistical distribution of material strength.

Dr. K. WILLAM

I agree with Prof. Zienkiewicz, if you look at this specific problem. In general, our viewpoint is as follows: for different types of tensile softening behaviour the ductile and brittle models form limiting bounds.

For these two extremes I have shown some results in that very simple thick wall cylinder problem, and you recall that we obtained extremely different results for the limit load. Now, the question is which model describes the actual response after cracking. If we compare the perfectly brittle model with the test results on that pressurized cylinder, we remember that the brittle model predicted an ultimate load of 89 kp/cm^2 against 141 kp/cm^2 observed in the experiment. That indicates that it is not sufficient to express failure simply in terms of a stress criterion. We really have to go further and actually, I showed this example in order to stimulate the discussion. We have to refine the numerical simulation of crack propagation, going into the direction that has been already developed in the context of metal fracture mechanics. There, a stress concentration factor is used which is basically a mechanism to predict crack propagation if the stress gradient (stress concentration) exceeds a limiting value at the crack tip. That was actually all I tried to say with my simple example which was unsatisfactory from a numerical view point because it showed rather poor agreement with test data. But it tells us that we really have to go further for simulating the actual fracture mechanism. It is not sufficient to use failure criteria based only on stress: the stress gradients play also a dominant role in crack propagation.

CHAIRMAN

I'll interpolate a point there. I would agree with that, but also that the fact that you have got two different materials, the stone and the mortar, the soft mortar yields, the stones develop stresses by bridging the mortar and inducing a tension accross the mortar, and that I think is a more powerful factor than the simple Griffith crack effect.

Prof. O. C. ZIENKIEWICZ

Models used in analysis can be either introduced in numerical analysis from the micro scale or in global stress terms. Micro-scale models are very useful to describe certain properties which are not possible to arrive at on the global scale, and Professor Baker has drawn attention to one very useful model here.

CHAIRMAN

Prof. Zienkiewicz we have a question from Prof. Cedolin for you: with reference to the shear retention factor on which, as you said, there is no experimental evidence, do you have numerical experience of its influence on the behaviour of the structure, particularly on the cracking pattern, load deflection curve and dowel action?

Prof. O. C. ZIENKIEWICZ

A substantial amount of numerical evidence was obtained on the influence of the interlocking effect of cracks. A parameter, α, was varied from 0.2 to 1 and this parameter had a very small influence on the actual observed behaviour providing it was made different from 0. Much more experiment is needed to obtain good values here.

CHAIRMAN

A second question from Prof. Cedolin is: from the paper it appears that you used parabolic isoparametric elements, which give a varying state of stress. Can you tell us how you handled an element if it happens to be just partially cracked. Do you think it is possible to do it by just not taking into account in the integration formula the contribution of the gauss points in the cracked region?

Prof. O. C. ZIENKIEWICZ

Whilst in early days of finite element analysis with the simple constant strain element, the introduction of plasticity was apparently conceptually simple as the same strains and therefore stresses occured throughout the element, with more complex ones this was not the case. However, all higher order elements are integrated numerically by sampling at a mesh of integrating points. Each one of such integrating points is treated, in effect, as a simple constant strain element and the plasticity conditions at such a point noted. Despite appearances, where one would suspect that a large number of such integrating points need to be used, it was found that in practice, that with parabolic isoparametric elements only four such points are needed to obtain very excellent results in two dimensions and eight points for three. The regions of cracking are not delineated precisely but are obtained by interpolation between such sampling points. In the examples quoted, which are two dimensional, both 9 and 4 sampling points were used in the elements and equally good results obtained by both.

CHAIRMAN

Prof. Zienkiewicz, you have now a question, one in regard to Mr. Jordan's paper, one in regard to my paper. Would you like to put them now?

Prof. O.C. ZIENKIEWICZ

expressed his interest in Dr. Jordan's contribution and wondered whether a composite model which would describe both creep and failure could be obtained by using non-linear visco-plasticity and visco-elasticity. It appears that the present procedure of dealing quite separately with these two phenomena is not tenable and that further work must be done to produce such a composite.

CHAIRMAN

The idea of the double tetrahedron model is to represent the two-phase effect of a concrete element in which you have the hard stones with a pressure thrusting around soft pockets and I think one must all the time have in mind what is happening with the mortar, i. e. the soft pocket. As the pressure increases the mortar yields a lot more than the stone, in fact at very high pressures it becomes almost fluid and starts to re-shape itself and to enter into crevasses between the stones. The ties in the model around the base of the tetrahedron are very much stiffer than the rest of the model because they represent the area where the stones are in contact and where the value of E is very high indeed, it only needs a small movement to break them, i. e. to start a crack at that point which can happen at a bout 30% of ultimate load. There is much evidence to show that that does occur. In the actual concrete cracks occur in the mortar and along conical surfaces, the base of the cone being restrained by the stones, the stones being very much stiffer than the mortar. So you have really a very complex particle structure which one must always have in mind. The E values and the variation of Poissons' ratio seems to be due to the extention of these microcracks which in the first place are caused by the difference in the E values between the mortar and the stone. They extend, they eventually join up and form the cleavage cracks when the material becomes unstable. At the same time if there is a big confining pressure then the mortar also flows considerably. So those two things, the spreading of the microcracks and the plastic flow of the mortar are the principal causes of the variation in E value which should in a compatibility calculation be taken into account step by step with time. And then of course temperature effects also are significant in the pressure vessel, the higher the temperature, the more rapid the flow of the mortar. The adhesion between the mortar and the stone also begins to break down. So you have really almost a complete metallurgy of concrete which still wants exploring very considerably.

Prof. I. J. JORDAAN

First of all in the multiaxial field the lack of experimental data for creep at high stress levels has made the analysis difficult. However, it should be possible to extend it, as you suggested, and to add elements which will become active at higher stresses.

Prof. O. C. ZIENKIEWICZ

The heavy work of McHenry reported 1943 and subsequent experiments have confirmed that at low-load levels concrete behaves as a linear visco-plastic material with a certain amount of recoverable creep. More recently Dr. England and Professor Ross have used what is in effect a pure dashpot model with the dashpot properties dependent on total strain and temperature. It appears that such a model gives very adequate results for description of high temperature creep.

I would therefore like to ask Dr. Jordan whether, in his opinion, it was possible to omit the recoverable creep portion without making substantial errors. This would ease considerably the numerical calculations.

Prof. I. J. JORDAAN

The model that I have been talking about reduces to England's model if, as you point out, the recoverable creep is neglected. For basic creep at normal temperatures the recoverable creep is about 30 to 40% of the elastic strain and one wouldn't want to neglect it. If the temperature is increased the dashpot (irrecoverable creep) becomes more important, and the recoverable creep less important, and at high temperatures I would say it is admissible to neglect the reversible creep for engineering purposes. Also, the steady state stresses (in structures without reinforcing) are independent of the recoverable creep.

CHAIRMAN

The main point that you asked about, what happens if you loaded 60% ultimate for a long period. Well, I think one possible explanation is that if you think of the tetrahedral model most of the load in the early stages is going down an axial rod, i.e. going through the mortar pocket, from stone to stone. Now, as time goes on the mortar creeps much more rapidly than the stone, so that you get more and more load transmitted by diagonal rods and increasing the tension component which is holding the element in equilibrium. Eventually that tension becomes too great and the adhesion between the mortar and the stones breaks down. That is a possible explanation.

Prof. R.N. WHITE

I wanted to make a few comments on the topic of shear slip resistance and shear stiffness. This is directly concerned with Prof. Zienkiewicz' paper, for he was making an assumption about the linearity of shear deformations and shear force across the cracks. The problem that we have been studying at Cornell University is the secondary containment vessel of a nuclear reactor subjected to seismic loads. The vessel is a very large structure, perhaps 30 to 50m in diameter and about a meter thick. The design condition deemed critical in our study is an internal pressurization due to some type of accident where there is the rupture of a steam line. Instead of getting the energy out of the reactor it stays inside and there is a large pressure build-up, producing open cracks in the conventionally reinforced concrete containment. Then the structure is subjected to an earthquake, so very large seismic shear forces must be transmitted accross small open cracks. Current design practice in the U.S. (and I think in other countries with seismic problems) is to have orthogonal horizontal and vertical reinforcing for the regular pressure stresses and then to superpose diagonal bars to carry the seismic shears.

We are trying to determine the shear properties of concrete blocks that are reinforced only with the orthogonal steel and the question of transmission of shear across cracks thus becomes very important. We call this interface shear transfer; it is also known as aggregate interlock. It is really two components: one is the interlock of the aggregate and the other is a friction component or an overriding component that develops during the motion. We have been testing very large specimens and looking at the effect of repeated loads. Several types of specimens have been used. First of all, to determine the load-slip relationship for the crack surface itself, we use specimens that have only external reinforcing bars, so we have just the crack surface to transmit shear. The specimen (Fig. 1a) is roughly 3 feet by 2 feet by 1.5 feet, and reversing shearing loads are applied to generate shear across this crack. A second specimen that we have used to study the combination of surface roughness (or interface shear transfer) plus dowel action is shown in Fig. 1b, where we have a bar running through the specimen that can be either unstressed or placed in uniaxial tension. Cycling is done by loading in both directions, first down and then up. The specimen that we are using now is shown in Fig. 1c. It is a bending type specimen with zero shear at the center, where the open crack is located. The specimen is loaded in both directions to produce reversing shear stresses. We are using a single No. 14 U.S. bar which is 2.25 square inches in cross section, and some specimens will be done with the largest bar available in the U.S., a No. 18 (4.0 square inches).

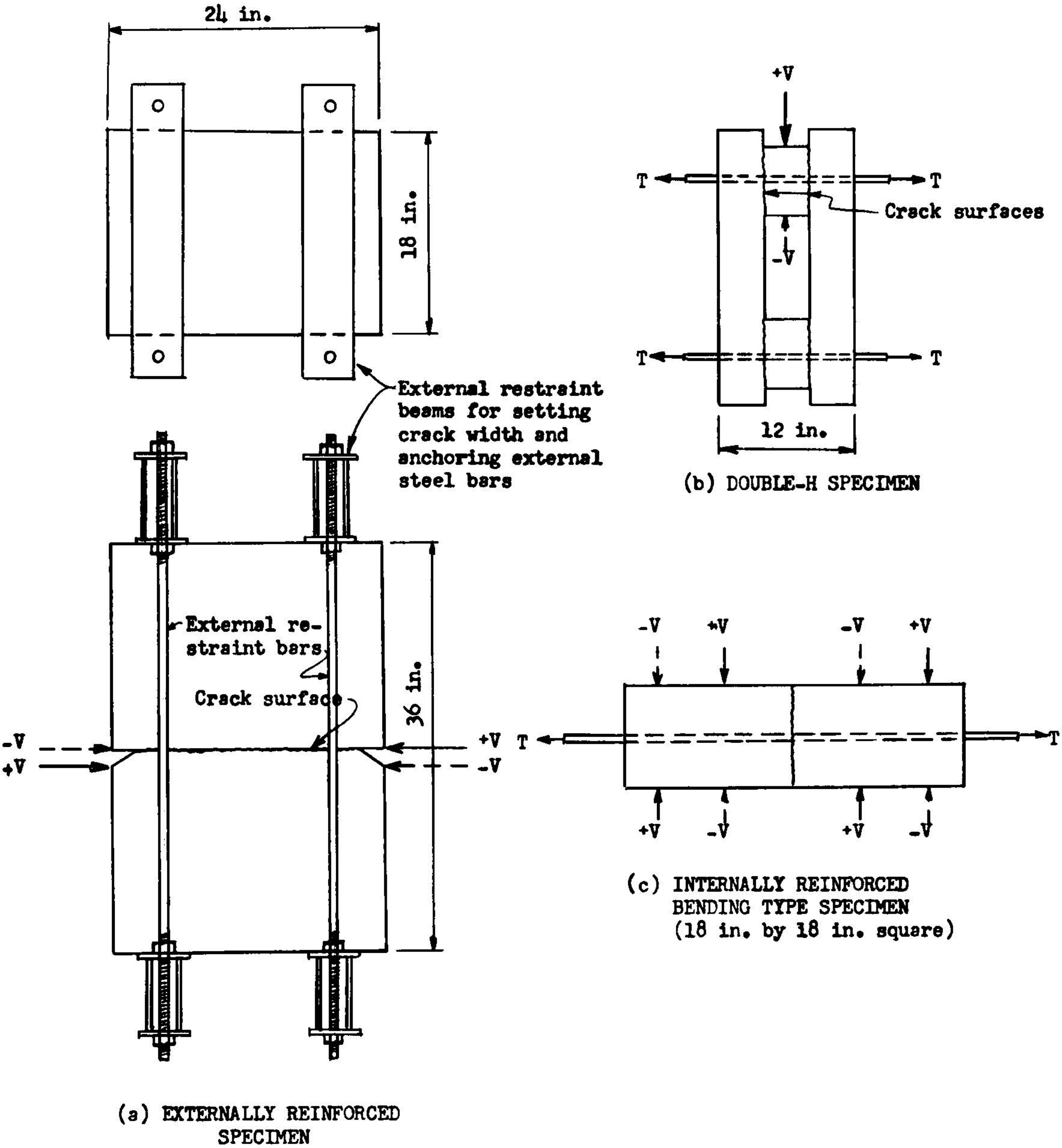


FIG. 1 - SPECIMENS FOR CYCLIC SHEAR TESTING

Now, to get to the behaviour and its implications for any formulation of finite element analysis for repeated loads. The measured variables are the crack width, slip at the crack, and changes in force in the steel across the crack. The clamping stiffness and the unbonded length of the embedded reinforcing are associated with the properties of the reinforcing steel and how much destruction of bond you may get right at the crack. The properties of the concrete, including compressive strength, really do not influence things as much as you might think. Dowel action is most likely a function of bar size; we have thus looked at size effects because it is obviously advantageous to try to do these tests at a smaller scale.

On the first cycle the response is linear for shear stresses up to as high as several hundred psi (Fig. 2a). There is a slight locking effect and upon unloading it is necessary to push the specimen back to zero slip. The first push in the opposite direction is also linear, but successive cycles are non linear, with the second cycle not much different from the 15th except that it has smaller slips. As seen in Fig. 2a, there is a fairly large degradation of stiffness on the second cycle, so some of the integrity of the surface is destroyed on the first push. This happens even at peak shear stresses of 100 psi. Thus the implication for analysis under repeated loads are very important.

The growth of peak slip is a function of cycles and is shown in Fig. 2b. It increases at a decreasing rate. The crack width (Fig. 2c) which in this case started off at 0.03 in., jumps to 0.04 in. during the first cycle. This peak crack width, corresponding to the end of the shear load cycle, does not change very much with repeated cycling.

Fig. 3, compiled from H-specimen data, is quite interesting because it separates the various components. If we look at the aggregate interlock by itself again in the first cycle it is linear. In the second cycle there is a sharply decreased stiffness in the early part of the shear loading, and then an increasing stiffness to a larger total slip. The dowel force by itself was done with a specimen with a smooth cast, greased surface between the two pieces that are being sheared; there is an almost linear load-slip curve for this specimen. Response with the dowel unstressed is shown. If you add tension to the dowel (such as 25 Ksi) then we get a behaviour that is remarkably like the interface shear transfer by itself for first and subsequent cycles. If you put them all together (the interface shear transfer and the stressed dowel action), the assumption of a linear stiffness on the first cycle is still fine. The behaviour is very similar to that of the concrete loaded by itself, with no dowel action, except that the slips are naturally smaller. I think this is all I would like to say on this.

CHAIRMAN

Thank you. There is a little time left, I am quite sure that people who want to ask questions or make points will do so.

Dr. R.D. BROWNE

We heard about a larger number of methods of handling the behaviour of concrete in relation to finite element and other analysis methods. What concerns me is the field of engineering which relates directly to design, where we are looking for very simple methods for handling quite complicated properties. In some ways I am a little worried that it may be necessary for a designer to be linked up to a

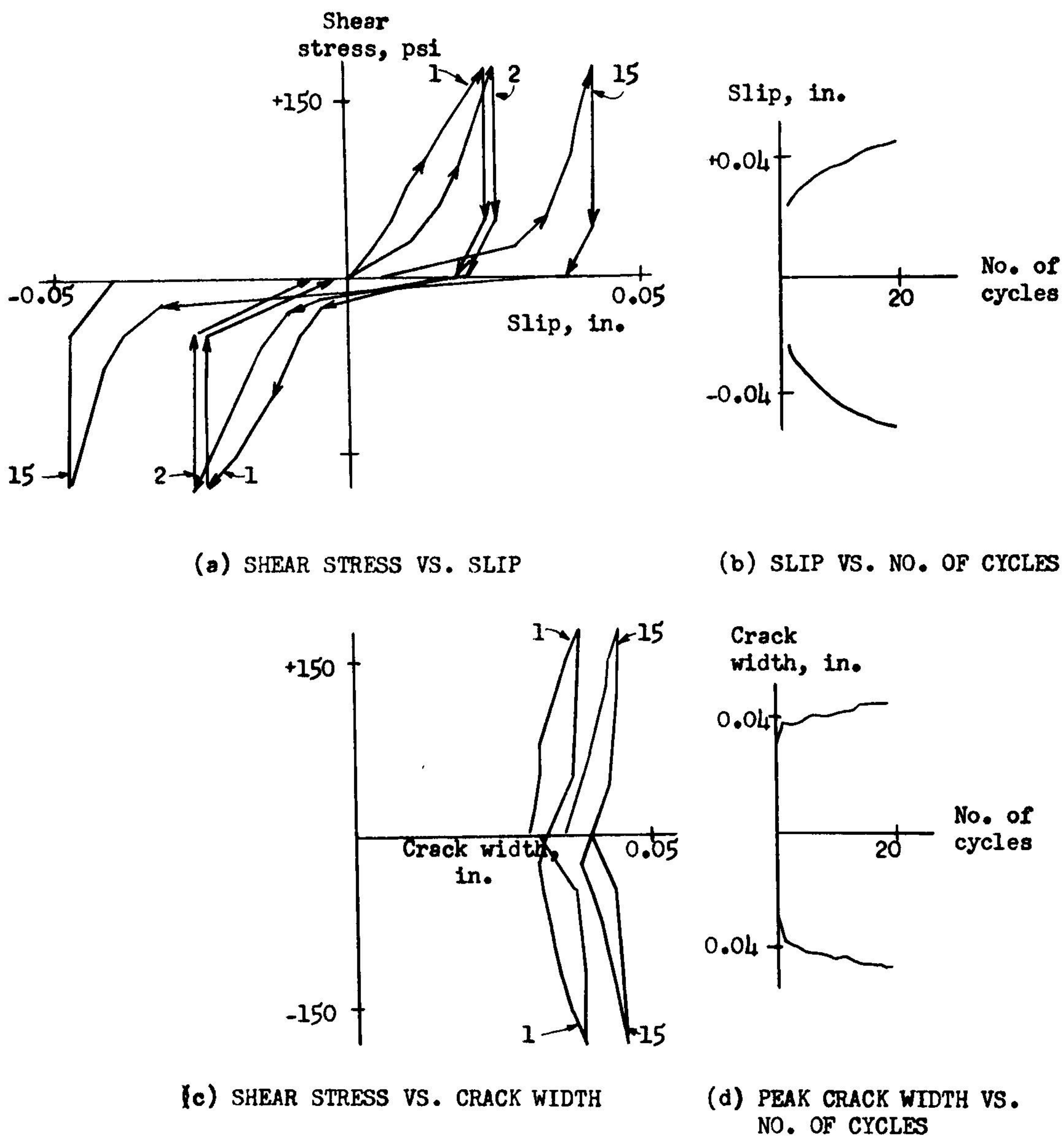
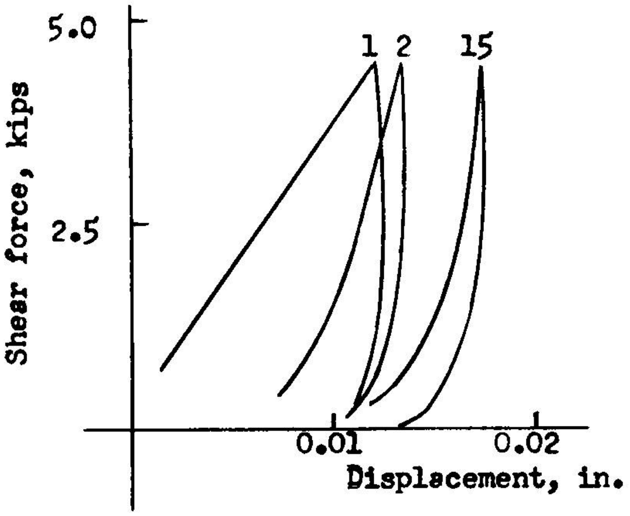
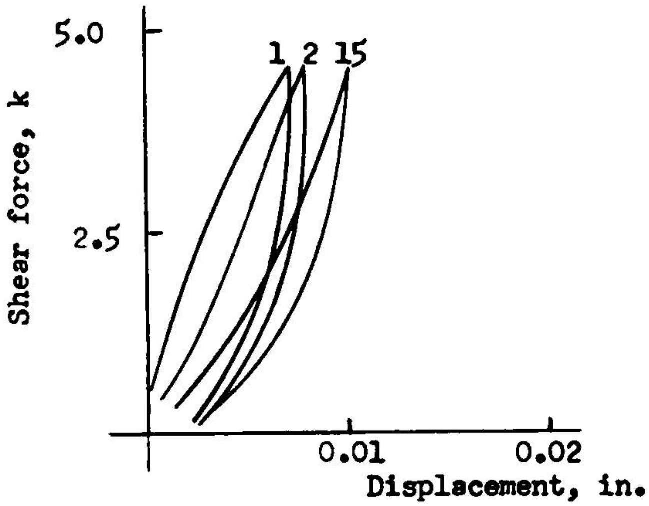


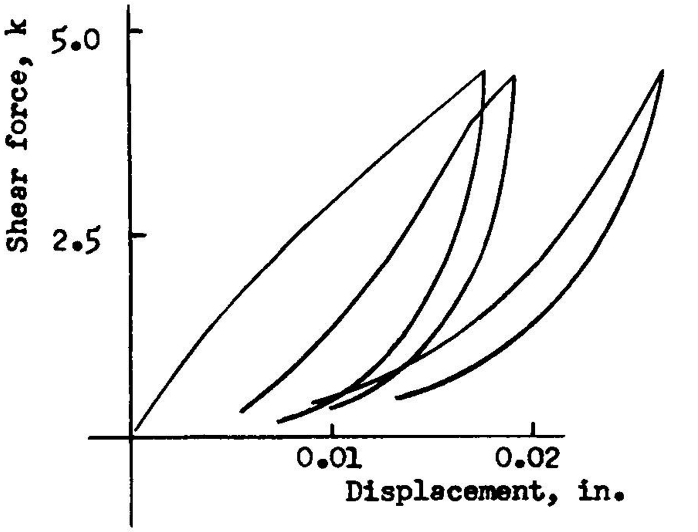
FIG. 2 - TYPICAL BEHAVIOR OF EXTERNALLY REINFORCED SPECIMEN (TYPE 1)



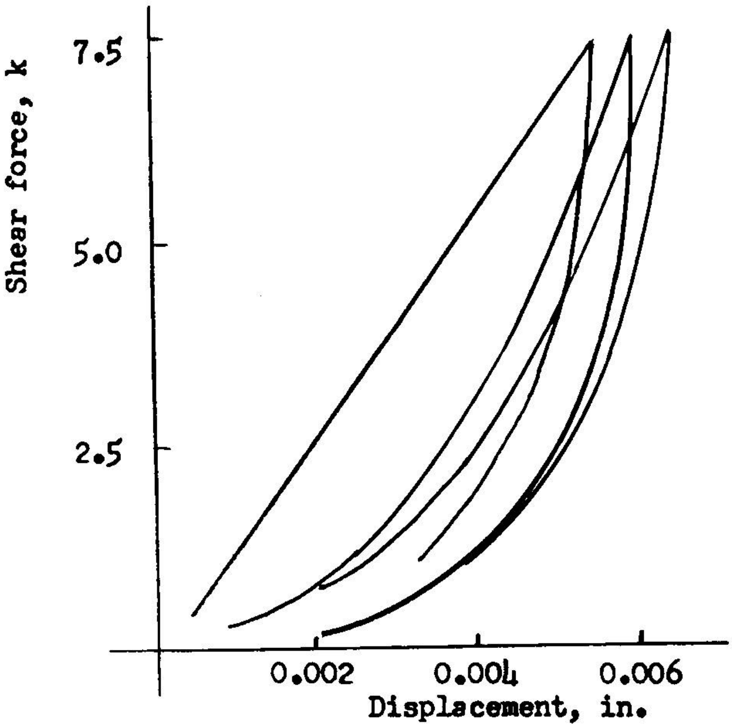
(a) INTERFACE SHEAR TRANSFER



(b) DOWEL ACTION (NO APPLIED AXIAL FORCE)



(c) DOWEL ACTION (WITH DOWEL STRESSED IN TENSION)



(d) COMBINED INTERFACE SHEAR TRANSFER AND DOWEL ACTION

NOTE: Shear area = 15 in^2; 3 kips load gives shear stress of 200 psi.

FIG. 3 - RESULTS FROM DOUBLE H-SPECIMEN (ONLY + CYCLES SHOWN)

computer most of his working day. There is not a lot of evidence from the work so far produced here that people are working towards very elementary methods of handling this phenomenon called concrete.

In Professor Baker's paper, he described one engineer's method, based on Poisson strain limit. Certainly we used this type of approach quite successfully in one examination for high load columns involving wire winding, where a critical limiting strain in the third direction was employed. It does mean that you can get your answer out on a piece of paper very rapidly.

Professor Zienkiewicz' paper, it seems, is based on limited biaxial data on concrete behaviour produced in the early 1960's. I wonder why he hasn't tried to incorporate well known later work, for example, that John Newman, has carried out at the Imperial College, where a considerable amount of information on volumetric strain behaviour has been available for a number of years. Professor White's description of his work on the reaction between beams and columns, should be linked with Prof. Zienkiewicz' analysis of the column head, to begin to formulate what is going on in a building, in total.

One sees, as we briefly mentioned in our own paper yesterday, that we understand very little about the total interaction of components in a building and one is perhaps drastically overdesigning structures. The designer is not inter-relating the components within the building in a global manner.

CHAIRMAN

I would just like to make two points with regard to what Mr. Browne said. First of all, it is possible, it may be found that some simplification can be made in regard to the compatibility calculations. Instead of doing them three dimensionally with precision in regard to the distribution of strains across thick walls, it may be more important to take account of triaxial strength and its possible weakening by creep and temperature effects over a long period. I think it would be very interesting to see what sort of deviations one would get in calculations, because one of the critical parts really of a pressure vessel is the cap and the shear strength of unreinforced concrete deviates enormously with identical concrete. Therefore, is there any point in trying to be so absolutely precise with the computer calculations? It may be much simpler and much better to take a limiting shear resistance and do a much simpler calculation, just bending in one plane, but using the three dimensional effect to take advantage of the extra strength you get in these massive concrete structures.

Prof. O. C. ZIENKIEWICZ

Prof. Finzi raises the question of the the transmission of research information to the designers hands. As soon as a certain degree of sophistication comes into the design difficulties arise "on the shop floor" where it is not easy to transmit them into a simple form. For instance, standard elastic analysis such as one finds in Timoshenko's books or elsewhere, and later elastic analysis of slabs has produced many difficulties in the translation of the bending moment information into the distribution of reinforcing rods in concrete. Here completely new problems have arisen and some answers to this have been provided by Wood and others in U.K. and have been adopted for design purposes of concrete bridges.

When applying the much more complex description and theories discussed at this conference to practical engineering a big gap will have to be bridged, and indeed it is important to focus the attention on the most important features of the material non-linearity. As I already mentioned elsewhere, it seems that the tensile behaviour of the concrete is its primary characteristic of importance in reinforced concrete structures and one can obtain very good results which correlate with experiment by introducing purely such a tension cut-off. Early work done in Swansea by several of my students has indicated extremely good correlation between experiment and theory without taking into account any compression non-linearity. There are, however, other situations where the compression non-linearities are of primary importance and the designer has to use his intelligence and intuition in interpreting and specifying the details of analysis.

CHAIRMAN

Prof. Zienkiewicz, could you produce a computer programme in which you could put this double tetrahedron model with the correct stiffnesses of the rods and so forth at the different stresses and temperatures. Would that be possible or is it too elaborate?

Prof. O. C. ZIENKIEWICZ

It certainly would be possible to adapt your models to the finite element analysis, at least on the micro-scale. Indeed some similar work of this kind has been done at Cornell by Prof. Nielson. The question still remains - would it be possible to analyse the whole structure using a finite element model based on the micro behaviour? This is something which has to be seriously considered.

I would also make a comment that when using macro descriptions of behaviour it is essential to distinguish between associated and non-associated plasticity with the normality condition. Clearly cracking is a case of non-associated behaviour.

Dr. K. WILLAM

May I just make a comment to your last remark, Prof. Zienkiewicz, that the associated flow rule would not be appropriate. That is exactly what is normally assumed in the case of tensile cracking. In my opinion, the stress transfer corresponding to brittle fracture is equivalent to the associated flow rule of a strain softening elastoplastic solid in which the direction of the inelastic strain due to cracking is governed by the direction of the principal stress exceeding the tension cut-off criterion.

Actually, I wanted to make a comment, because I think Prof. Jordaan presented an excellent paper on concrete creep. At the moment there is a lot of discussion going on in different committees which kind of creep laws and creep formulations shouldbe used. I think the formulation of Prof. Jordaan is very flexible, since it contains on the one side the flow theory and on the other side the delayed-elastic theory. Recently we have done some work trying to develop from general principles of mechanics a constitutive model based on internal state variables which encompasses those two phenomena as function of age and temperature. Basically, if we look at creep from a rheological standpoint the theory corresponds to a generalized Burgers model similar to what was proposed by Freudenthal in 1958 or something like that.

I have one question to Prof. Jordaan, that is an argument which is always raised by Bazant and people who propose the delayed elastic concept. What is happening if we load an old concrete? Normally we underestimate the creep deformations if we use the rate of creep method and similarily if we use the rate of flow method with small delayed elastic effects.

Prof. I. J. JORDAAN

Your question has to do with mature concrete. If you use unloading tests, as we have, there are errors in the prediction of creep for mature virgin specimens, but you can't have it both ways with a linear law. Models which predict the creep of virgin specimens correctly overestimate significantly the response of (mature) virgin specimens to stress changes (positive or negative). I believe that our formulation is more acceptable, and is the best way to use data in tests initiated at about the time that it is expected loading to be applied. If that is done, the accuracy is impressive but for virgin specimens loaded at later ages, there is certainly some error, but this is hardly of practical importance.

Dr. R. D. BROWNE

Professor Baker, I think this conference to my mind will be associated with the first major discussion on compressive shear failure. As regards this subject, if you are looking at the slip between two layers of cracked concrete, then the aggregate itself will play an important role in the slip after cracking for high lateral compressive stresses. If you take for example, a soft lightweight concrete aggregate, you may in fact improve the cracking performance of concrete. One must perhaps bear this in mind when comparing results for different concretes.

CHAIRMAN

In regard to the point Mr. Browne made about the interlocking aggregate, I would have thought that it is a pretty indefinite sort of extra shear resistance really. The final shear strength depends on how far a crack extends and that depends on what is happening partly at the tip of the crack and partly on the reinforcement if any that is restraining the crack from extending. I am thinking of the shear in the cap of a pressure vessel, when it breaks up into sectors and you have a shear crack starting through bending. The main forces controlling the extension of that crack, are the external pressure, and the local resistances at the tip of the crack which varies enormously from concrete to concrete. The aggregate interlock, is a little bit vague. I wouldn't like to depend too much on it.

Prof. R. N. WHITE

In the specimens that we have examined at Cornell University (about 60 specimens) we found that the shear transfer is perhaps more influenced by the properties of the cement - sand fraction of the concrete. During initial slips, you simply don't have the aggregate bearing against the surface as much as you do have the mortar surfaces. We have tested specimens with different sizes of aggregate and the different degrees of hardness of the aggregate. If you go from half inch maximum aggregate to an inch and a half maximum aggregate you will find that the stiffness is increased on the first cycle by perhaps 30%. But then, as you have more and more load cycles, the large or hard aggregate tends to become more beneficial, because you gradually wear away the mortar interlocking surfaces. But for one or two load cycles the strength and size of the aggregate is not really an important variable.

CHAIRMAN

One imagines that on either side of the crack the concrete is moving relative to the other side in the direction of the crack. This is not the case with the crack opening as though hinged at the tip.

Prof. A. D. ROSS

My question is addressed to Prof. White who showed us some fascinating diagrams of the result he has had from aggregate-interlock experiments. But I wasn't quite sure what maximum level of stress was achieved. In an ultimate load calculation, you want to know the maximum benefit that might be derived from aggregate interlock. What were the maximum shear stresses?

Prof. R. N. WHITE

Tyipical shear vs. slip response for 25 cycles at about 160 psi is shown in Fig. 4. Then we pushed some of the specimens up to as much as 400 or 500 psi and there was no well defined failure point; we got the response shown, with the curve still going up. The test had to be stopped because the loading angles that were used to put the shear into the blocks actually punched into the concrete at shear stresses of around 450 psi and that was the end of the test. The final slips were very large, on the order of about 0. 3 in., but this was with a very small reinforcing bar stiffness across the crack surface. I think if you had normal reinforcing bars running through the crack line (in the tests these were exterior bars, simply holding the blocks together) you could go up to 400 psi or perhaps higher, with very small slips.

Dr. R. D. BROWNE

Professor White's data referred to shear tests with very low shear stresses applied to the specimens (i. e. 200 psi).

In the shear work done in our laboratories for pressure vessels shear states, nominal stresses of the order of 4000 psi or more were obtained at failure. It is in these high slip states that aggregate interlock and aggregate properties may dominate rather than, as suggested, the mortar properties.

Prof. R. N. WHITE

I agree with you, you are talking of very small cracks and we are talking about relatively large cracks and relatively low stresses.

Dr. F. GARAS

A question to Prof. White: have you tried to compare your results with Dr. Taylor's and Fenwick's in Australia?

Prof. R. N. WHITE

Yes, we have done some comparisons with Fenwick's work; I think there is some size effect here. Fenwick's specimens were about 5" x 5" square and he had a controlled crack width. He was testing with a very stiff system that kept the crack width constant, so that we couldn't make a direct comparison, but the general trends of behaviour are very similar. Also, we have done some four inch square specimens, and were able to get essentially the same sort of behaviour as Fenwick. In our test the crack is allowed to open because of the flexibility of the transverse reinforcing steel. So we start off with a 0.020 in. crack width and we are actually testing with a 0.030 in., which can then be compared to Fenwick's fixed crack width specimen.

CHAIRMAN

We have really gone a bit past our time and I think it just remains for me to say what a splendid set of informative papers we have had and to thank all the members who gave the papers and all those who participated in a very interesting and informative discussion. Thank you very much indeed.