Discussion

Session 2, part 1: Structural Modelling for Numerical Analysis

Introduction by Bergan, chairman (Norway); at the beginning of the session.

I am very pleased to have this opportunity of chairing this morning session on structural modelling for numerical analysis. This title indicates that we are now moving from the fundamental mechanisms and concepts in reinforced concrete on to computational techniques that may be used for solving real, practical problems. Coming from Norway I know that there is a great need for more sophisticated methods for non-linear analysis of reinforced concrete structures. I am of course particularly thinking of development of designs of gravity platforms for the North Sea. Already fourteen such gravity platforms have been built, and these structures are really enormous. I think that you have to see one in order to comprehend how enormous they really are. Some of these are nearly 200 m high and contain 500,000 tons of reinforced concrete. And the price for the concrete structure alone is close to one billion (10^9) Norwegian crowns, that is about 200 million U.S. Dollars. Concepts for structures that are supposed to stand in nearly 350 meters of water depth are now being worked out. And if you now consider the linear scale to be doubled you see also that the volume of these new structures will be much, much larger than those that we already have.
Economy and safety aspects are of course essential and it is therefore evident that we need some very powerful and accurate methods of analysis for analysing these structures.
Having said this, I must also add that ten years of research in non-linear finite element analysis of reinforced concrete structures have taught me to be a little bit sceptical as to what can be achieved by these methods. In fact I must admit that three, four years ago I was a little bit more optimistic than I am today. The reason for this is not at all that so little has been achieved during the last few years, but it has become apparent to me that there are still so many questions that are unanswered and it seems that new problems arise all the time. I particularly had this feeling yesterday: each talk seemed to raise more questions than it really answered. This may of course be a good thing for us, who are working in research; we just have to convince our sponsors about all these problems that still are not solved and we will be sure of keeping our jobs for the coming years. But seriously, it is also an indication that our insight is constantly growing and that the interphase to what we do not know is growing at the same time.
Undoubtedly, modelling of reinforced concrete structures is very difficult and a challenging field to be working in. It is somewhat frustrating that you do not have an exact solution to compare with. Of course there are experiments, but if you do the same experiment twice, you are likely to get two different answers. You may think that you have a perfect model that gives exact results, but then somebody comes along and shows that there are some other effects, that you have not considered, and that these effects are of vital importance for the behaviour of the structures. So there are many traps to fall into. And the biggest one among these is to believe that you have achieved a general solution technique when you only have been able to, should we say, post-predict one experimental test by means of adjusting some material parameters in your model. Considering all the important information that we got yesterday, how should we then go about developing our computation models? Is it really necessary to use a general three-dimensional finite element model, that accounts for every detail in the concrete, including pores, aggregate, paste, detailed reinforcement, that follows the development of each individual crack, that accounts for slip, interlocking, friction sliding, temperature, moisture, history effects, aging etc.?

Where will this bring us to? Well, I am sure that in this session we will hear some interesting papers that will point out the way to go, in order to achieve more efficient and better numerical models for analysing real structures. This session begins with an introductory paper by Prof. Christian Meyer of the Columbia University, who will talk about dynamic finite element analysis of reinforced concrete structures. It is a great pleasure to give the word to Prof. Meyer.

For discussion on the Introductory Report by Prof. Meyer see 5 pages further.

DISCUSSION

Session 2, part 1: Structural Modelling for Numerical Analysis

Paper by Cope/Rao, United Kingdom

Crisfield (U.K.): It would be useful to have a break-down between the number of BFGS -iterations, in which updating was applied compared to the number of line searches. Could it be that in some cases the line search, rather than the BFGS, is the more dominant feature?

Cope: I do not have the numerical evidence here, but I can recall that the rate of convergence was in fact most influenced by the line search. Setting CONDMAX as low as 10^3, whereby there would only perhaps be one or two updates of the inverse of the stiffness matrix per increment, gave very similar results to setting CONDMAX equal to 10^5. So the answer is that we feel that the line search is the most important part of the acceleration procedure.

Bazant (U.S.A.): I think it is rather interesting that you found it to be a significant phenomenon that the directions of the principal moments rotate, and therefore that the use of orthotropic models gives better results if you rotate the directions of the axes of these models. However, although you get better results if you do that, it implies rotating the defects within the material. Therefore this formulation is to some extent physically objectionable. This comment also relates to all the so-called "equivalent uniaxial stress" models. These models have the orthotropic form and, by virtue of the fact that they have zero terms connecting normal stress and shear strain, and shear stress and normal strain, they are not invariant with regard to the choice of coordinates. Therefore, these models may lead to significant discrepancies.

Cope: With slabs the main behaviour is in fact influenced by the steel after cracking, and I do not think that the invariance is particularly important. Although it is obviously not a rigorous analytical model that we are using, we are trying to aim for economy, and to some extent we may have violated a rigorous approach in order to achieve that. I do not know how we would get over it, because with multiple load patterns there is no doubt, that cracking occurs between and inclined to established cracking. Also, where there is inclined cracking at a point, the direction of the applied principle moment determines which cracks dominate the response.
We found in extreme cases, where we have analysed panels subjected first to flexure and then to torsion, that if we fix the material property axes, we then cannot get the stiffness to rotate properly to treat the torsion case.
Yesterday someone mentioned having eight possible crack directions. We tried a similar approach in which material property axes were held in direction until the principal strains rotated by say 30 degrees, then we would allow them to rotate. But this is again, I think, an empirical rather than a rigorous approach.

Blaauwendraad (The Netherlands): Your conclusion that you can omit tensile stresses is correct, but only for this special structure and its typical loading (pure bending). I think that one should be more careful if you would have combinations of shear and bending, which may dominate the failure of the structure.

Cope: The point is accepted: the results were for Kirchhoff plate bending elements, in which transverse shear was not modelled. The Heterosis element, which includes the capability of taking tranverse shear into account, gives very similar results when we do not degrade the transverse shear modulus.
We have not yet started doing studies that involve both flexure and shear cracking. For the sort of slab bridges that we have been looking at, it is not a usual failure criterion.

Paper by Rossi/Bazzi, Switzerland

Collins (Canada): I should like to congratulate you on a really first class paper: I very much enjoyed your presentation. In the comparisons between the predictions of your model and some other predictions for the beam in shear, I noticed that you seemed to be using situations where all of the steel yielded. I am wondering if you attempted to compare the more difficult situation where the steel does not yield, in either one direction or both directions prior to failure.

Rossi: As I mentioned in the presentation, this model till now works just for situations where the steel yields. The adopted yield condition of Drucker-Prager is not very useful for cases with high multiaxial compression, because we will get too high values for the strength. We intend to do some more research in this field.

Bazant (U.S.A.): Dr. P.D. Bhat carried out at Northwestern University a similar investigation of hysteresis, where he used the endochronic theory with layered beam elements, with non-normal cross-sections, and calculated the hysteresis loops. One aspect which he found significant in the response was expansion of the concrete during deformation cycling, which was putting into action the transverse reinforcement. It was a calculation according to the beam theory, where one unknown was also the mean transverse strain in addition to the angle between the cross section and a normal to the beam axis. Have you studied this effect and, if yes, what was your conclusion?

Rossi: So far we are not able to use this shear model for cyclic loading, since it is a plasticity model. We cannot expect good results for cylic loading because we have no possibility to simulate closing and reopening of the cracks; therefore, we have no experience in this case.

Blaauwendraad (The Netherlands): A remark and some questions. You referred to our paper in Copenhagen, which is describing a similar model. The difference is that we used a layered approach. Personally I feel that the difference is not that big, because it does not take much more time to do a correct integration over the cross-section.
Now my questions. We had some difficulties with the distribution of the vertical strains transverse to the bar axis; we started with the same approximation as you, a linear displacement field, resulting in a constant strain field. Specially when you have a compression zone, the vertical strains there will be considerably lower than in the cracked zone in the tensile area.

Did you deal with that? Furthermore, we feel that we should extend this model with a special element for the beam-column connection. Now that we can treat the beams and columns so well, we feel that the weakest part in the calculation of the frame structure is the connection. Do you have proposals for that?

Rossi: With regard to your first question, we had the same problems with the vertical strain distribution and we intend to adopt an approach using different models for each Gaussain point in the vertical direction, for instance three different ones. This means that we would have three more internal unknowns, which we should iterate first. We intend to follow this approach as a next step. To your second question: Until now we have not thought about modelling of the joints.

Paper by Muto/Sugano/Miyashita/Inoue, Japan

Blaauwendraad (The Netherlands): I understood that the intention of this presentation was the simulation of what will happen during earthquakes. If you regard a connection between a column and beam, is this loading case then a relevant one? Should not you consider moments of opposite sign on both ends of the connection? That may result in slip of the reinforcement, and influence the behaviour quite a lot.

Inoue: We had only the intention to obtain results in a short time; we studied the general characteristics, such as the load-deflection relation and the strain of the longitudinal reinforcement and I did not consider all details. In a further study we will extend the scope of the program.

Bergan (Norway): An eight-mode brick element is used, with a 2 by 2 Gaussian integration. It is well known that when you subject this element to bending modes, you get a lot of spurious shear deformations, and when you analyse concrete, that may be rather critical. Would not it be better to use a selective integration of shear strains, like taking the shear strains at the centroid, rather than in the Gaussian points? This choice is rather critical when non-linear material properties for concrete are considered.

Bazant (U.S.A.): I just should like to comment in defense of Mr. Inoue's analysis, that this pure shear effect would only be important for very slender members and that the members we are talking about here are very bulky.
So I do not think that this effect could have been of significance in this particular example.

Bergan: I think that it is significant anyhow as long as you consider bending.

Bazant: There is a limit for very slender member.

Bergan: But he is not only considering axial loading on these columns.

Paper by Mang/Floegl, Austria

Gambarova (Italy): What do you think about the minor role of crack spacing, with reference to tension-stiffening effects? I would have expected it to be a major factor.

Mang: It is very difficult to answer this question, having only limited numerical evidence available. What I would say is that consideration of tension-stiffening within the given framework that I presented, acts, as we could say, as a stabilizator for the analysis. It may be not so important what the stabilizing parameters are. This might be an explanation, but I would not be astonished if counter-examples were found, showing a different state of affairs. We hoped, specially with respect to Prof. Bažant's remark about objectivity of some of the constitutive laws used, that analysing large cooling tower structures would give an answer to some of the problems, but this did not happen; even the whole tension-stiffening effect was not important. So also any refinements in the tension-stiffening formulation did not have an influence. Another aspect that was raised by one of the previous speakers refers to the behaviour of slabs; it was stated that the tension-stiffening effect was unimportant. We found, again within the scope of our limited numerical evidence, the same; there was hardly any influence of the tension-stiffening effect on these results. So for these problems of course a variation of the $b_i^{(1)}$ term is unimportant. We hope to develop a rational method, from the standpoint of mechanics, to incorporate the initial crack spacing.

Mehlhorn (F.R.G.): I did not understand how you consider the influence of the angle between reinforcement and the crack. Did you check this influence by tests?

Mang: It is considered in a term containing the direction cosinus between the reinforcement and the crack, such that the special cases of perpendicular intersection would lead to the conventional result, and that the other case, when the reinforcement is parallel to the concrete strut, would result in the lower bound of the tension-stiffening factor, namely 1. There was not time enough to explain this in my lecture. The concept is brought into effect such that in a shell - where you can have the limiting cases of a pure bending state, or a pure membrane state - for the case of a pure membrane state you will arrive at the results for a plane situation. It would be too difficult to explain now all the terms; but it is accounted for.

Bergan (Norway): It is a little bit surprising that your tension-stiffening effect is so important for the ultimate loading; normally one thinks that it is important mainly for lower loading stages, with the initial cracking. Are you absolutely sure that this is correct?

Mang: I expected this question. I may say that we analysed the influence of variations of the meshes and variations of the input parameters, and we found the same thing in all the analysis. I can only be sure as far as correct coding is concerned, but I know that it is a well disputed fact.

Crisfield (U.K.): The stiffness of the material is important if instability effects, such as buckling, are involved in the analysis. Is this the explanation that in the shell example that you presented, tension-stiffening is affecting the ultimate load?

Mang: If the structures would have been built in steel, buckling would be a mechanism to be considered. However, in all typical reinforced concrete structures which we analysed (I mentioned that we analysed large hyperbolic cooling towers), there was no indication of buckling at all.

Collins (Canada): I just like to add to the comments about whether the tension-stiffening affects the ultimate strength. You would be perhaps interested to know that in our panel tests we have found that tension-stiffening does affect the ultimate strength, provided that failure occurs before all of the steel yields. If the steel in both directions fully yields prior to failure, then tension-stiffening has no effect.

Mang: I am glad you made this comment which corroborates our findings.

Bazant (U.S.A.): I would like to come back on the subject of crack spacing, further to what has been said earlier by Dr. Gambarova. Whether or not there is a tension-stiffening effect makes a significant difference, and the influence of the effect should clearly depend on the crack spacing, falling down to zero if the crack spacing becomes infinitely small. You did not find an influence of the crack spacing. This can only be true if the range of crack spacing which is considered is limited. Differences in crack spacing of about 100% are important; differences of 20-30% do not affect the behaviour very much, as can be shown by calculations.

Mang: What we hoped is to show this with a large shell structure, but we did not succeed as I said, because the structure we analysed showed a sudden collapse and the whole matter does not come into the play. What we are looking for now is to find examples in which it would really have some effect.

Discussion on not orally presented papers

Paper by Aguado/Murcia/Mari, Spain

Bergan (Norway): In your paper you gave results for moments. Are you also capable of calculating deformations with your solution procedure?

Mari: The deformations can be calculated only by integration of the curvatures. If the structure is an isostatic structure, the deformations are the addition of the linear deformations and the imposed deformations. But if the structure is hyperstatic, the deformations will be the sum of the linear deformations, the hyperstatic deformations and the imposed deformations.

Paper by Menegotto, Italy

Bergan (Norway): It was not clear from the paper what type of integration you used over the cross-section of the beams. You indicate that you divide the inter-subareas as a basis to find the Culmann-ellipses. Could you comment on how you find these stiffnesses?

Menegotto: The interntal forces are integrated over the section just by multiplying the stresses by the areas and summing up. The areas are small layers of concrete and spot steel areas. The point is that there can be an infinite number of definitions of the stiffness for a step of loading. Among these, that one is chosen which is deemed to be the most correct one, i.e. the one related to the local linearization of the stress-strain paths corresponding to the assumed loading step. In that sense I would like also to discuss Mr. Rossi's and Bazzi's paper, who find a non-symmetric stiffness matrix for the section. I think that among the infinite possibilities the symmetric matrix which one can find would be better.

Blaauwendraad (The Netherlands): I appreciated your paper very much. You are speaking in terms of a "very general approach". Is your procedure so general, that you can extend it to cases in which you have not only combined bending but also torsion?

Menegotto: Up to now it is only referred to combined normal forces and skew bending, but I think that the stiffness criterion can be generalized.

Introductory Report by Meyer, U.S.A.

Blaauwendraad (The Netherlands): I understood from you that you use a combination of integration in time, material non-linearity and maybe geometrical non-linearity. I am wondering how many hours computer time you need to process a real structure of the size you showed us, and what type of computer you have available to do such jobs.

Meyer: That was one of the reasons why I illustrated the various levels of sophistication. If you endeavour to analyse a multi-story building you cannot afford to break up every beam and column into hundreds of finite elements and integrate them all through the time domain. That is why I personally prefer the full member approach, at least for analizing buildings, although the layered or semi-finite element models may be also possible. About the last examples that I showed at the end, the 3-dimensional models for blast loading, which I got from Dr. J. Isenberg, I can add that these calculations were carried out for the Defense Department of the United States, and, as you know, money is of little concern to them.

Blaauwendraad (The Netherlands): What is the tendency in the U.S.: should we think of super-computers, such as Cray 1, or should we think in terms of "Super-Vax" in combination with an Array Processor? How will people handle the job in practice?

Meyer: It really depends on what kind of structure you are talking about. For example, for a simple slab cover over a missile tube, you can perform a full dynamic analysis on man common computers without requiring execessive funds. But on the other hand, in the case of some of the examples I have been involved with - some very complicated internal structures in a submarine, for example - you may speak of several hours of computer time on the Cray-machine. And if you know what an hour on the Cray costs, you can imagine what that means. But for common civil applications we really have to be concerned about simplifying our models. Otherwise calculations are simply not feasible, definitely not in engineering practice, for example on the civil engineering profession, where the final objective is to design and build structures, not to spend all the money analyzing them. You just have to draw lines somewhere. I consider all these approaches which have been discussed here only as a means of understanding concrete behaviour, that should help us ultimately to come up with simplified models.

General discussion

Crisfield (U.K.): I just want to show very briefly something relating to what Dr. Cope said this morning about various "rogue" solutions with the BFGS-method. He said one should look out for those rogue solutions. But they are still equilibrium states. He rejected them because he had the experiments, but you do not always have the experiments before the case.

I would like to talk about some of these alternative equilibrium states that can arise and relate this to the issue of tension-stiffening that has already been talked about, strain localization, discussed earlier by Bažant, and mesh dependency.
The mechanism of cracking and its effect can be represented in a way as shown in Fig. A. If a crack occurs at constant strain the stress falls down to a lower level. If the stress is then increased again a new ascending branch is followed until new cracking occurs.

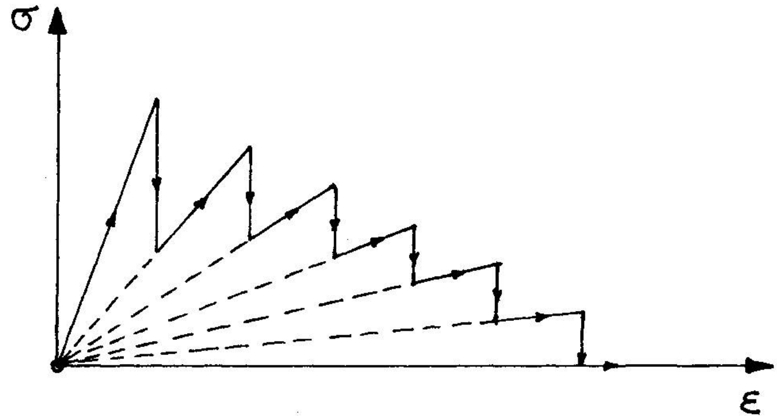

Fig. A

In fact these jumps are dynamic. Most of our analysis techniques involve pseudo-static methods of analysing these things. In general we nowadays replace such a discontinuous relation by a single line (Fig. B) at the element level.

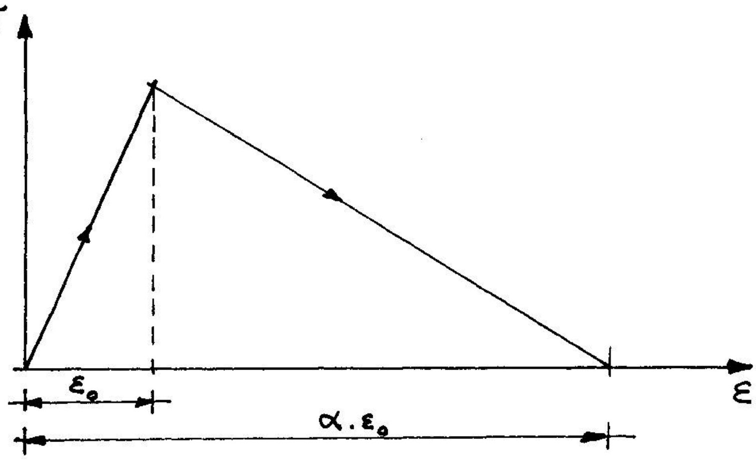

Fig. B

However, as the mesh gets finer, the stepped response of fig. A may be inevitable at the structural level, because of the strain localization effects. Even worse, pseudo-static analyses can result in nasty responses as shown in Fig. C.

If the inclination of the softening branch, represented in Fig. B, is small (so α is large), no problems will occur, but if this branch is steep, this may result in trouble. This is illustrated in Fig. D, displaying the load-deflection curve of a one-way slab, subjected to two concentrated loads.

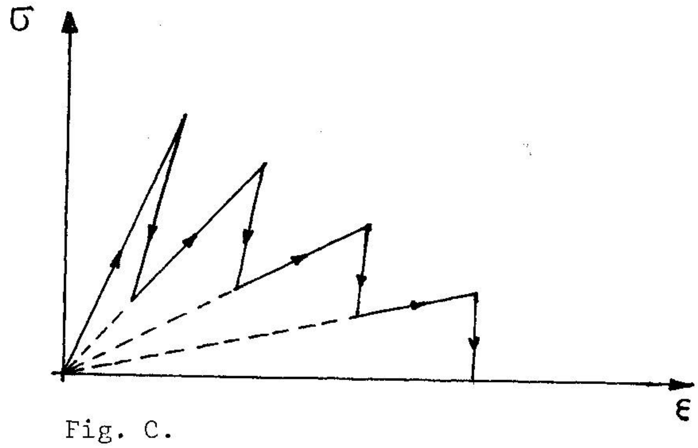

Fig. C.

ONE-WAY SLAB

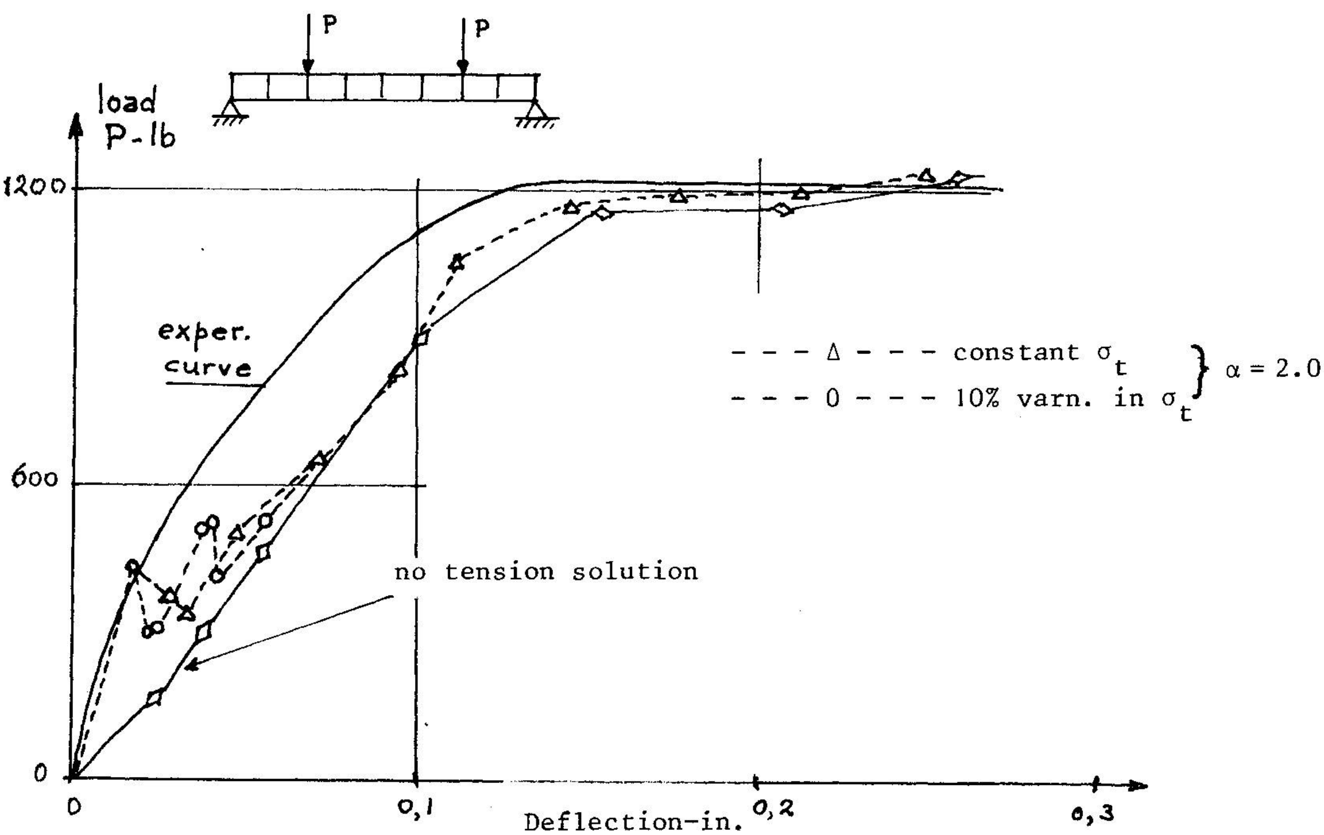

Fig. D

The upper, solid, line represents the experimental relation. The lower line shows the solution, obtained with a no-tension approach. In addition, two finite element solutions are given, both based on a steeply descending softening branch, with $\alpha = 2$. One of the lines is found, assuming that the tensile strength of the concrete σ_t in the area between the loads, is constant. After a local maximum in the curve, at first cracking, the load decreases and then three negative pivot points for the tangent stiffness matrix are obtained; however, convergence still occurs to an equilibrium state.

The reason that there are three negative pivots is because there are three excess Gauss-points, going down the strain softening path, where elastic unloading could occur as a result of the strain localization. So this situation is unstable. The second line which is represented, is obtained with the assumption that the tensile strength in one of the elements between the loads is 10% less than in the adjoining elements. Hence an alternative equilibrium path is obtained; after the first local maximum the load is reduced to a minimum, which is still an unstable situation because one extra Gauss-point is now "redundant". Subsequently again a local maximum, followed by a local minimum is obtained. This is definitely a very mesh-dependent situation.
If we took the variation in tensile strength (which in theory could be as small as 1%) between Gauss-points, we could even obtain a "snap-back" (as in fig. C) with many local load-maxima, depending on the mesh size in the constant moment zone. It may be not very practical but it is important to realize that these problems exist and that our solution codes are often trying to trace some very complicated things like this.

Bažant (U.S.A.): In several papers the use of orthotropic or equivalent uniaxial strain models was mentioned. I would like to call attention to the lack of invariance of these models with regard to rotation of the coordinates. I mean models the incremental stress-strain relation of which is characterized by zero terms connecting normal stress to shear strain and shear stress to normal strain:

$$\begin{Bmatrix} \Delta\sigma_x \\ \Delta\sigma_y \\ \Delta\tau_{xy} \end{Bmatrix} = \begin{bmatrix} x & x & 0 \\ x & x & 0 \\ 0 & 0 & x \end{bmatrix} \begin{Bmatrix} \Delta\varepsilon_x \\ \Delta\varepsilon_y \\ \Delta\gamma_{xy} \end{Bmatrix}$$

Such a model can principally be used in two ways. The first way is represented in Fig. 1. According to the standard rules the coordinate axes can be chosen arbitrarily with regard to the material in the initial state. However, when the element starts deforming, the coordinate axes have to be kept attached to the material. So if initially a uniaxial stress is applied, as in Fig. 1a, and subsequently an increment of a uniaxial stress that is oblique, is applied, (as in Fig. 1b), we get a combination of hydrostatic pressure and shear; however, in spite of this combination, in a vertical plane no shear deformation can occur.

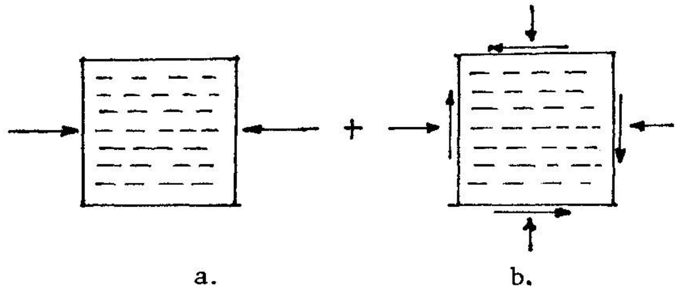

Fig. 1 Coordinate axis fixed

The second way is represented in Fig. 2a. An oblique orientation of the coordinate axes is adopted. Now the second increment in Fig. 2b, similar to that applied in Fig. 1b, is a uniaxial stress. However, now a shear deformation increment $\Delta\gamma$ is obtained as a result of a normal stress increment. If in both cases the first stress is 80% of the ultimate stress, the difference between the values of the maximum strain in both cases can be shown to be up to 50%, which can be rather significant. So there is no objectivity in this sense.

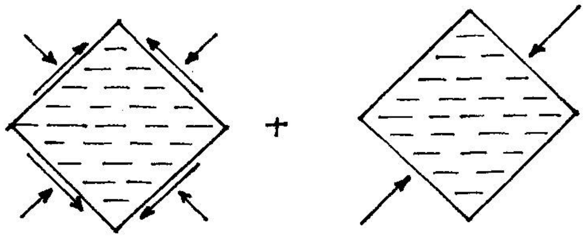

Fig. 2 Coordinate axes rotated against material

a. b.

Another possibility of the use of such models is that the coordinate axes are rotated in such a way that they always coincide with the direction of the principal stresses. In this case the model is invariant, but this is physically objectionable, because it implies that we get some oriented defects, e.g. microcracks, when we rotate them with the axes (Fig. 3).
A crack, however, cannot be rotated in the material.

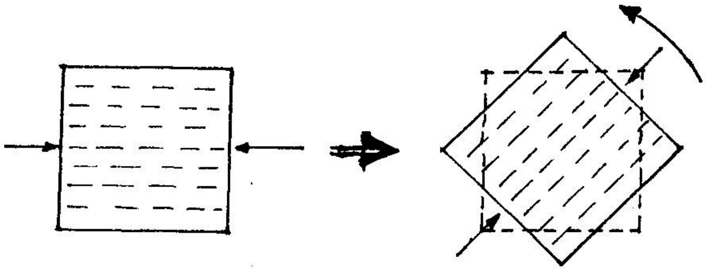

Fig.3. Rotation of cracks against the material is physically unacceptable

Of course it may be said that there is trouble with every model; we have no perfect model and a number of objections can be raised against plastic, endochromic or plastic-fracturing models. However, it should be pointed out that there are two kinds of troubles, those which are so complex that we do not know how to deal with them and those which can easily be avoided.
The trouble which has been discussed here can be easily avoided: therefore such errors should not be commited, because there are other models which are free of these errors.

Simplified Calculations of Concrete Problems

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The method given here is based on the fission(splitting)of deformations into an active part,which causes further rheological phenomena,and a passive part,which remains unchanged,as is the case for materials without creep.The fission is executed at the moment in time"i"(Fig.1/a)so that the active part D_{ai} of the deformation D_{fi},caused by a brought(planted)deformation D_{bo} at the moment "0" determines by itself the creep as though this active deformation had been brought(planted)in the structure at the moment "i" (Fig. 1/b),which has been chosen for the fission.The passive part D_{pi} of the deformation D_{fi} remains unchanged from this moment "i" onward,i.e.it maintains a constant value.These two parts,i.e.the active part D_{ai} and the passive part D_{pi},together,completely substitute the deformations D_{Ti} from the moment "i" onward,caused by the brought deformation D_{bo} at the moment "0"(Fig.1/a).In such a way the influence of the active part D_{ai} and that of the passive part D_{pi} together provide a complete substitute for the influence of the real brought deformation D_{bo} from the moment "i" onward,and by introducing them,we can cut off the whole rheological history from the moment "0" up until the moment "i".

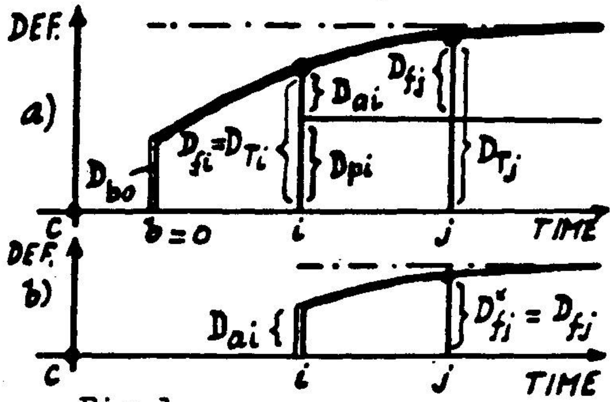

Fig.1

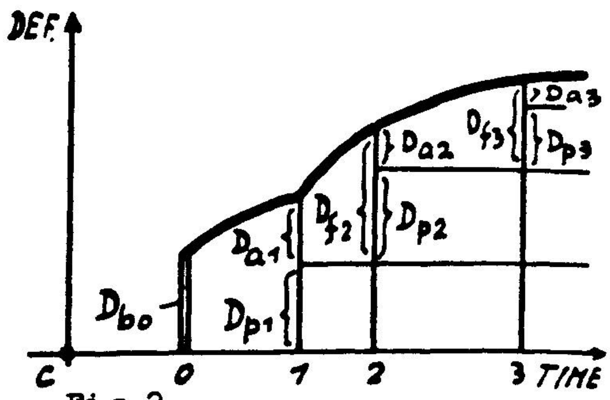

Fig.2

In the case of pure creep this procedure makes it possible to consider one climate in the first interval 0-1 (Fig.2),and then in the second interval 1-2 another climate which is decisive for the development(growing) of the deformation D_{a1}.At the moment "2" the climate can change again.In this case the fission of the"fissionable" deformation D_{f2},-caused by the active deformation D_{a1} at the moment"1"-is carried out,into active part D_{a2} and the passive part D_{p2},and for the development of the active part D_{a2} the climate in the interval 2-3 is considered. In this simple manner the creep resulting from an inconstant climate can be predicted.

The method further permits that the loading is changed at the moment of fission.For example at moment "1"(Fig.3)the deformation

D_{b1} is brought and this brought deformation is added to the active deformation D_{a1}, and from then on the common "working" deformation $D_{W1}=D_{b1}+D_{a1}$ for the further development of creep in the interval 1-2 is considered. The additional deformation can be an unloading, too, e.g. a deformation D_{b2} at the moment "2". At the moments "1", "2" etc., the climate can change, too. In this case the development of the working deformation D_{W1} in the interval 1-2 is subjected to another climate as the development of the working deformation $D_{W2}= D_{a2}-D_{b2}$ in the interval 2-3.

If, using this method, the total deformation D_{T1} ($=D_{f1}$), given by the first brought deformation D_{bo}, is diminished by means of a brought deformation D_{b1} so that at the moment "1" the initial deformation D_{bo} is reobtained (Fig.4), and if the same operation is carried out at moments "2", "3" etc., too, then at the moments "1", "2", "3" etc. a constant deformation D_{bo} is obtained. And if the intervals 0-1, 1-2, 2-3 etc. are very short, then by means of this procedure the phenomen of relaxation is obtained.

The fission of deformations must be always carried out at the moment when the structure changes, e.g. when a second span (part) is to be built into a first span, and a simply-supported beam turns into a continuous beam over two spans. Because of creep-phenomena, here a rheological redistribution of bending moments will occur here. This rheological redistribution can be easily predicted by the described method, and a simultaneously changing climate can be

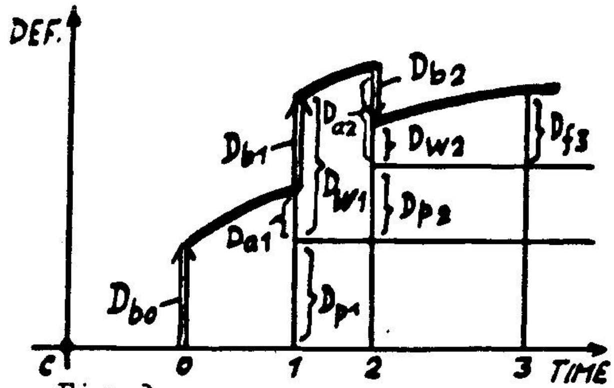

Fig.3

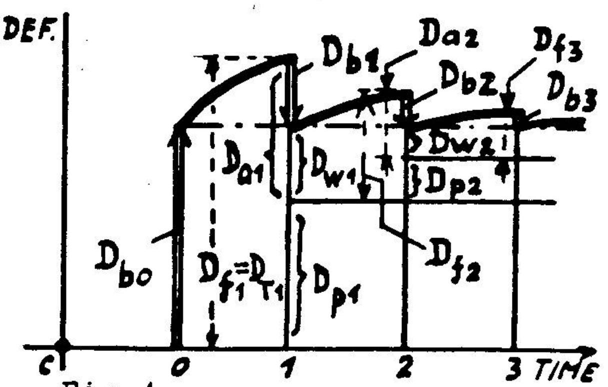

Fig.4

considered, too. Naturally, a fission of deformations must be arranged when a third span is built into in the second one, etc.

In the case of rheological redistribution, too, infinitely small intervals are used. That is in the case when the structure changes in a continuous manner. Such a case occurs if the second span is not a precast member (part), but is concreted directly onto the first span. In this case the stiffness $(E.I)_{II}$ grows from zero to the end-value [$\doteq (E.I)_{I}$]. (First span.. $(E.I)_{I}$, second span.. $(E.I)_{II}$)

The method is confirmed by experimental results obtained at the "Institute for Research of Materials and Structures" (=ZRMK) in Ljubljana. These results and numerical comparisons with results of other authors confirm a good accuracy for the technical practice.