PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY A
MATHEMATICAL, PHYSICAL & ENGINEERING SCIENCES

Discussion

D. McLean, F. A. Leckie, N. Hansen, J. T. Barnby, G. M. Boyd, J. Van Der Veen, A. A. Wells, P. B. Hirsch, J. F. Lancaster, A. R. Flint, R. R. Barr, T. Kanazawa, B. F. Dyson, J. D. Harrison, L. P. Pook, L. M. Wyatt, H. G. Wolfram and P. J. E. Forsyth

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Evidently, ductility in the vicinity of stress concentrations is of immense importance, and should perhaps form one of the design criteria. It seems to me unlikely that there will be a simple relation between notch ductility under these circumstances, and ductility in a parallel sided uniaxial test. Perhaps the many uniaxial creep tests on parallel sided specimens should be supplemented by more on specimens with sharp notches in them.

Another point brought out by Branch is the importance of taking the effect of biaxiality and triaxiality into account in design. It is unrealistic to expect that one could ever, or would ever wish to replace all uniaxial creep data with triaxial creep data, but we should at least know how the one relates to the other, and whether there is a unique relation. Can we get such information on properly designed test specimens, waisted or notched to give biaxial or triaxial stresses, or must we undertake the complex and costly testing of structures?

In conclusion, physical metallurgy appears to provide an invaluable basis for conceptual understanding of behaviour, and goals have been defined for materials development. Two factors emerge:

In certain areas, the traditional goals should be modified. Those now defined as important include: improved performance and consistency in yield stress, definition of small scale initiation and propagation tests for brittle fracture, propagation behaviour in fatigue and the improvement of long term and short term notched ductility in creep in biaxial and triaxial systems. In most cases this in no way renders redundant the traditional tests, which will remain economic and invaluable methods of implementing quality assurance.

There is a clear challenge to the material scientist to work with the engineer in evolving constituitive relationships to explain the behaviour of engineering details. Finally, if we are to make the best use of our resources without prejudicing safety, reliability concepts should be introduced to provide a logical basis for design factors, through relating them directly to the reliability required by society for the structures concerned.

Discussion (Chairman – D. McLean (*National Physical Laboratory*))

F. A. Leckie (*University of Leicester*)

I share Baker's opinion (Keynote) that properly formulated constitutive equations form the essential link between material behaviour on the one hand and the laws of mechanics on the other. In this way it is possible to relate material behaviour to the performance of components occurring in engineering practice. The procedure has, in general, been limited to the material mechanical properties and little attempt has been made to include in the constitutive equations, suitable descriptions of the physical mechanisms which take place within the material. There is evidence however, that attempts are now being made, especially in the field of fracture and creep mechanics, to relate in a quantitative way, the physical processes occurring within the material to the behaviour of components with notches.

If, in this way, the physical mechanisms can be related to component performance by quantitative means then it should be possible to design materials to meet engineering requirements in a systematic manner.

N. Hansen (*Danish Atomic Energy Commission*)

A nuclear problem relevant to the theme of this afternoon's session is the design and performance of fuel elements for water reactors. The fuel was designed in the 1950s and materials were developed to fit the design requirements. The fuel elements behaved satisfactorily in-pile except for some difficulties due to lack of control in the manufacturing process. By the end of the 1960s it was necessary for reactors not only to work under base load but also to follow the requirements from the grid. This drastic change in operating conditions led to overstraining of the cladding. The problem has been circumvented by restrictions on operating conditions, an expensive solution costing perhaps U.S. \$5–10 × 10^6 per year for a single station.

The problem was to analyse the behaviour of fuel elements under irradiation and to predict as closely as possible the resistance of the fuel to power cycling. To solve this complex problem mathematicians, computer experts, engineers and metallurgists worked together to develop a fuel element model consisting of a generalized finite element model, including a series of physical sub-models. Parallel to this mathematical and physical modelling, engineering programmes were carried out to test the model predictions.

This model work has had a number of positive results: (*a*) An improved understanding of the relevant problems to be solved in this field, (*b*) A closure of the gap between laboratory tests and what is required in engineering practice and (*c*) With reference to Branch (2.4), it had helped in a rapid transfer of physical metallurgy results into engineering application.

J. T. Barnby (*University of Aston*)

In relation to Forsyth's paper (2.3), I should like to comment on the suggested method of design against fatigue crack initiation by choosing an arbitrary size for the nucleus and counting the number of fatigue cycles to produce such a nucleus. If one does this, there is a practical basis which should be usable for design since number of cycles for initiation, N_i, is related to the stress intensity factor ΔK according to:

$$N_i \propto (1/\Delta K)^m.$$

An initiating notch can also be allowed for. However, I should like to make a distinction between fatigue crack initiation from a large stress concentration, such as some shape factor, and fatigue crack initiation from small surface or internal defects. In the former case, so long as the nucleus size is much less than a critical defect size, then design against 'initiation' is practicable.

In the second case, two problems arise. As we choose a smaller and smaller nucleus size we find that the law above changes in terms of the material properties, namely the coefficient and the power m; though the *form* of the law does not change. This may arise because we are dealing with an extra geometrical factor such as the growth of many small and differently shaped nuclei. The second problem is that, at smaller nucleus sizes, we reach down to the level of inhomogeneity of the structure. If both of these factors are allowed for perhaps we shall have a true capability for design against fatigue crack initiation and perhaps also we shall see that initiation behaviour is directly related to the propagation behaviour which is now quite well known.

Comment by G. M. Boyd (presented by J. van der Veen)

Figure 1 of Wells's paper (2.1), originally appeared in a paper by Hodgson & Boyd (*Trans. Inst. R. Naval Architects* **100**, 1958) and its object was simply to show that a large proportion (75 %) of the steels then available in the world would comply with a certain criterion, i.e., minimum energy 47 J (35 ft lbf) and 30 % fibrous fracture at 0 °C.

Since the steels surveyed were taken virtually at random from all over the world, at a time when there were no specification requirements for Charpy values, it is not surprising that they conform to a Gaussian distribution with a fairly wide dispersion. This is even more probable considering that the values plotted were at a particular, arbitrarily chosen, temperature, i.e., 0 °C.

When the values were plotted separately for each individual steelworks (as stated in the original paper) the individual scatters were much smaller. The points tended to form 'clouds' in the field of the diagram, and the positions of these 'clouds' clearly reflected the steelmaking practices of the individual steelmakers.

Since then (1955–58), specifications have included Charpy requirements, and the 'clouds' now tend to be confined to the region II of the diagram.

J. van der Veen (*Hoogovens, Netherlands*)

In Wells's paper (2.1) reference is made to design habits for bridges, in which loading density increases with increase of span. The necessity for long bridges to accept a hypothetical queue of (heavy) vehicles is rejected. I think this can sometimes be a dangerous method since in heavily populated areas, such as Holland, and especially around Rotterdam, almost every day, queues of heavy traffic occur during peak hours on rather long span bridges.

A. A. Wells (*The Queens' University of Belfast*)

In a reappraisal of the synthesis of equation (1) in my paper (2.1), with regard to the probability of interaction of an actual crack length and a critical crack length, it is necessary to draw attention to two objections. Firstly, the interaction will also occur if the critical crack length is less than the actual defect length, as well as when the two are equal. This is a question of the most suitable modelling for the physical event. However, there is also a second objection, which is fundamental, that the probability P_x should have been expressed as $P_x\,\mathrm{d}x$. The preferred alternative expression, due to Sir Peter Hirsch (unpublished communication) is

$$P = \int_0^\infty \left(\int_0^x P_\alpha\,\mathrm{d}\alpha\right) N_x\,\mathrm{d}x. \tag{1a}$$

It is quite fortuitous, but nonetheless fortunate that the evaluation of the original equation, detailed in the Appendix, is the means of showing that equation (1*a*) may also be calculated to a good approximation in closed form, leading to a result that differs little from equation (2). Use is made (*a*) of the series expansion,

$$\int_x^\infty \mathrm{e}^{-\xi^2}\,\mathrm{d}\xi = \frac{\mathrm{e}^{-x^2}}{2x}\left(1-\frac{1}{2x^2}+\frac{3}{4x^4}-\frac{5}{8x^6}\cdots\right)$$

and (*b*) of the observation that interaction following equation 1 is confined to the narrow band about $d = ac^2/(b^2+c^2)$, with standard deviation $e = bc/\sqrt{(b^2+c^2)}$. It is then sufficient for the purpose of the second integration to observe that the average value of $1/2x$ is $(b^2+c^2)/2ab$, which is usually significantly less than unity so that only the first term of the expansion is needed. On this revised basis the recalculated probability,

$$P = [(1+n^2)/\pi]^{\frac{1}{2}}\,\tfrac{1}{2}(1+\operatorname{erf} nm)\,\mathrm{e}^{-m^2} \tag{2a}$$

differs from equation (2) only by the multiplying factor $(b^2+c^2)/2ab$. The values of m, n, d and e remain as before, and the trend lines of figure 2 are lowered and somewhat steepened.

The main effect of the correction on the worked example is to cause the observed failure rates for oil storage tanks and ships to be consistent with defect populations which are 4 times larger, i.e. $\frac{4}{3}$ defect per structure for tanks, and 0.53 defect per structure for ships. The relevance of the comparison is unimparied, bearing in mind that defect rates for modern welded structures are shown by non-destructive methods to be somewhat lower than these figures. The contention is strengthened, that quite small improvements in the r.m.s. scatter in the range of interest are likely to lead to much reduced failue probabilities.

Van der Veen is right to draw attention (this discussion), as a result of his discussion with Boyd, that figure 1 substantially broadens the scatter band likely to have emanated from the products of any one steelworks because it represents a world-wide sample. Nevertheless, it is necessary to make use of such a broad sample, in order to be consistent with the world-wide population of casualites, which would have been associated with the collective products of these steelworks, and possibly more. Another demonstration then arising from the analysis is the powerful effect that would have resulted from unification of the notch ductility rules of the world grouping of Classification Societies, by means of which the proportional scatter was first reduced during that period. This in itself is a tribute to the work of Van der Veen and Boyd, and others that they would wish to be remembered. It is fitting that the debt of society to this distinguished group should be openly acknowledged.

P. B. Hirsch, F.R.S. (*University of Oxford*)

I should like to comment on the use of failure probability analysis of cracked structures. There are at least two reasons for using an analysis of this type: (1) It is desirable to have some way of quantitatively estimating the failure probability of a structure or vessel, rather than relying entirely on experience and safety factors. One simple way in which fracture mechanics is used by the designer is that for a given value of toughness and stress a critical crack size is worked out, and if this is larger than any cracks which might possibly exist or could be detected, one is reasonably happy about the reliability of the structure. But there is a spread in values of yield stress and toughness in a given material, this may lead to weaker spots being present in the structure with unacceptably high probability.

In principle, given that the spread of toughness values and of stresses are known, and given that the distribution of defects is also known, the failure probability could be estimated using the analysis of the type developed by Becker & Pederson, and also used by Wells (2.1). The variation of the failure probability with time may be evaluated given data on fatigue crack growth rates under operating conditions. If n.d.t. is used to determine defect distributions, the probability of missing defects must also be taken into account. The estimates are of course only as good as the input data, and at present data on defect distributions and on the variability of materials parameters are lacking. It seems desirable to obtain such data and to characterize materials not only by mean values of parameters such as toughness, but also with distribution curves.

The second reason for this type of approach is that it serves as a sensitivity analysis. It shows up the importance of particular parameters, e.g. how important the problem of fatigue crack growth is and the bearing it might have on inspection programmes, what the effect of the spread of values of toughness is, etc. In principle this kind of analysis could lead to a 'sharpening-up' of design parameters and safety factors. I do not underestimate the difficulty of obtaining the necessary data, and the complexity of the problem, e.g. there are alternative failure modes, but I do feel that some effort should be made in developing this approach.

J. F. LANCASTER (*Kellogg*)

Boyd's paper on ship steel was a very memorable one because it represented one of the first good correlations between large scale failure experience and a small scale test, in that case the Charpy-V-notch test. 47 J (35 ft lbf) was the figure that Boyd concluded was the Charpy value required for the arrest of a brittle crack in the ship plate. This figure was built into Lloyd's specifications and subsequently the rate of failures went down quite dramatically. I think this though, should not make us as metallurgists, too ready to predict failure from small scale tests. May I give you an example? In our particular line of business we design plant operating at 900 °C and under internal pressures of 3.5 MPa (500 lbf/in^2). The basis for design is the stress for rupture in 10^5 hours and although it is a perfectly true statement people take it that because this is the basis for design the life of the tube will be 10^5 h say 5–10 years; after that it is finished and you have to throw it away. In fact when you look closely at what the designer does, it is not quite that way. He takes the average stress for rupture in 10^5 h at a given temperature, but then he applies a temperature safety factor to his. This in effect multiplies the life of his design by a factor of 10. He also uses other approximations including a mean diameter formula which biaxial tests have shown to be very conservative. Thus, in practice, he may actually be designing the tube for a life of between 100 and 1000 years while nominally according it a 10 year life. This is a case where small scale testing is being projected to determine the life characteristics of equipment in an unduly conservative way. I feel that much more work needs to be done in objectively surveying the actual rates, and modes of failure in service in order that we can determine what is the reliability of equipment and in order that theoretical formulae may be correlated with real behaviour. I was very glad to see that Wells (2.1) had made a useful and interesting beginning in this endeavour.

A. R. FLINT (*Flint & Neill*)

Wells (2.1) has referred to the need to allocate the best material to the most demanding structures and Baker (Keynote) asked whether it was advantageous to reduce scatter of yield strengths. It is evident from application of reliability theory that the 'best' British steels from the designers' viewpoint are those having maximum variability for these provide the greatest reliability (least risk of failure) for a structure. This arises as a result of the practice at the mills of ensuring that the rejection rate of non-compliance with the guaranteed minimum yield strength is similar for different mills which results in the use of higher mean strengths in those which produce the more variable product. It may be simply shown that in such circumstances the nominal reliability increases with the coefficient of variation. There is thus no incentive to reduce scatter with the present definition of 'guaranteed' values related to the grades of steel. However the rationalization of safety factors for structual design which is in prospect *will* have the effect of penalizing the steels with low mean strengths and hence those mills providing the best control will be encouraged to raise the levels. It is hoped that this may be done with economic advantage.

R. R. BARR (*British Steel Corporation*)

The limitations to the application of crack opening displacement for general structural steels should be recognized if the concept is not to be misused. In the linear elastic range the conditions for unstable fracture initiation are fairly rigorously defined by K_{Ic}, but there is a need to clarify the useful range over which the c.o.d. concept can be used to characterize initiation and to calculate tolerable defect sizes. At temperatures up to the transition range, with limited plasticity accompanied by cleavage fracture, the c.o.d. can be used as a measure of tolerance for defects. Beyond this region (i.e. on the upper shelf) with a fully fibrous fracture, it is not possible to measure a c.o.d. value which has any relevance to the conditions at the crack tip. The general yielding fracture mechanics theory relies upon the c.o.d. measurement fully characterizing localized conditions of strain acting in the vicinity of the crack tip. The c.o.d. at onset of fibrous crack extension, δ_1, does not relate to conditions for fracture in a large structure

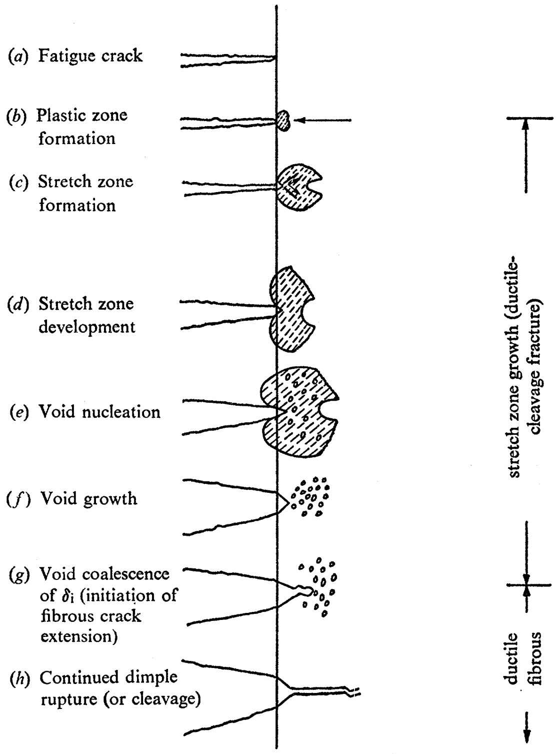

Figure 1. Successive stages in crack extension of a fatigue crack in a low strength steel by 3 point bending, (for clarity, the plastic zone is omitted in (*f*)–(*h*)).

and, if used, gives a very significant under-prediction of acceptable defect size. In the circumstances where a δm value, the c.o.d. at maximum load, is measured this is a function of test piece size and machine stiffness, and fracture is controlled by tensile instability. Whenever a maximum load type of failure is observed in a full thickness c.o.d. test conducted at the appropriate loading rate and temperature, the most realistic failure parameter is one of plastic collapse based on the attainment of the flow stress (or yield stress) in the remaining uncracked section of the structure.

These comments do not refer to the high strength materials where unstable fracture can occur by microvoid coalescence, but to the structural steels where fracture is accompanied by significant amounts of plastic deformation.

The sequential events which occur at the crack tip have been examined in detail at Swinden Laboratories (see figure 1). Stretch zone formation is the consequence of localized yielding and shear ahead of the crack tip. Finally, there is void nucleation, void coalescence and crack extension.

Thus c.o.d. should only be used over a limited, but usually important, temperature range to measure the significance of defects.

I wish to refer now to the statement that sufficient notch toughness is required in parent plate to provide a reserve of toughness to account for the reduction in toughness in the h.a.z. due to the thermal effects of welding. This means that at normal design conditions the fracture initiation toughness of the parent plate is usually well in excess of what is required. Attention has therefore to be focused on weld metal and h.a.z. toughness. Interpretation of the c.o.d. measured in fracture toughness tests can become a problem when examining these regions and these problems are discussed in our paper.

It is important to consider what level of weld metal toughness is really necessary in a particular structure and not merely to assume that it should meet the same minimum levels as for parent plate. This is an area which requires further investigation because the selection of weld metal toughness levels whether it is specified by Charpy V-notch impact strength or c.o.d. can affect the economics of fabrication.

Also low toughness in the weld root can occur in, for example, a vertical-up weld using a full weave as illustrated in figure 11 of our paper (2.2). The conditions for occurrence of low root toughness have been examined in detail at B.S.C., General Steels Division Laboratories. The main conclusions reached are that the toughness in the weld metal is controlled by dynamic strain ageing due to the thermomechanical cycle. The toughness is therefore dependent on the level of interstitial elements such as carbon and nitrogen and the parent plate composition does not affect the toughness to any marked extent.

In these circumstances it is difficult to control toughness by electrode composition but it can be controlled by weld heat input, and can be improved by stress relief. The existence of low root toughness is believed to be widespread and is not just confined to quenched and tempered steels or to British Steels. It is a function of the weld process and the welding consumables. The interpretation of its significance is difficult. Practical experience indicates that it is not a serious problem otherwise there would have been a series of brittle failures associated with low root toughness. Wide plate tests carried out at our Motherwell laboratories support the view that one should not assume that the toughness in the weld root is the sole criterion describing the integrity of the weldment. Detailed information on these aspects will be published shortly.

T. Kanazawa (*University of Tokyo*)

I would like to introduce the testing method widely used in Japan to evaluate the fracture toughness of materials under the condition of large scale yielding.

Using various size specimens for bend and tensile tests, a comprehensive cooperative research work has been conducted by the T.M. committee of the Japan Welding Enginering Society. It has concluded that the fracture stress may be predicted from the critical c.o.d. value for an appropriate size bending specimen based on c.o.d. concept.†

Taking account of this fact, we have no standard testing method and specimen size at present, but a c.o.d. bend specimen with only a mechanical notch of 0.2 mm width is generally accepted in Japan, to avoid the difficulties for producing a notch by fatigue for the practical and industrial test.

† T. Kanazawa, S. Machida & T. Miyata 1974. Present status on the evaluation criteria of fracture toughness for structural steels and their melted joint in Japan. Conference on the Prospects of Advanced Fracture Mechanics, Delft.

Of course, we know there are some differences in the critical c.o.d. values between the specimens with and without a fatigue notch. But at present we feel that such differences should be included in the safety margin, deduced from the statistical data based on the experimental results, for bend specimens with and without fatigue notches.

When specially designed machines are available to produce notches by fatigue faster, easier and cheaper, we shall not hesitate to adopt c.o.d. specimens with fatigue notches.

B. F. Dyson (*National Physical Laboratory*)

I would like to take issue with Branch over his conclusion (2.4) that the physical metallurgist has given insufficient attention to creep ductility. This surely is incorrect since the reduction in creep ductility by intergranular cavitation has been the reason for most of the physical metallurgical interest in this field over the last 20 years.

I want to take up two areas of research that Branch raised in his paper. The first concerns the influence of extraneous factors, which Baker also brought up in his keynote lecture, on the service performance of materials. One such extraneous factor is plastic deformation. This can occur during fabrication or during plant 'start-up' or 'shut-down' when it is then termed thermal fatigue. In Nimonic 80A, plastic deformation has a deleterious effect on the subsequent creep ductility due to the creation of intergranular cavities during plastic deformation (1). These cavities are created at the intersection of slip bands with grain boundaries (2) and cause much earlier intergranular cracking during subsequent creep. The design of plant on the basis of uniaxial rupture data is plainly inadequate if certain critical parts of the structure are subject to static or cyclic plastic strains and the material responds in a similar manner to Nimonic 80A. The second point is in answer to Branch's request for experimental information on the component of the stress tensor which correctly describes the deformation resistance at very low creep rates. In Nimonic 80A and at stresses which give lives up to 10000 h the von Mises criterion adequately describes the minimum creep rate and is therefore in agreement with most of the published high strain rate data.

References

(1) Dyson, B. F. & Rodgers, M. J. 1974 *Metal Sci.* **8**, 261.
(2) Dyson, B. F., Loveday, M. S. & Rodgers, M. J. Submitted *Proc. R. Soc. Lond.*

The following five contributions were received after the meeting.

J. D. Harrison (*The Welding Institute*)

I am sure that Wells (2.1) is right and that one of the most important requirements in the future will be to develop a probabilistic approach to fracture mechanics. It seems to me that currently the most difficult problem in this field is the prediction of the probability distribution of the size of pre-existing defects. This assumes great importance when we consider that we are usually concerned with extreme value statistics involving the overlapping of the tails of two distributions, defect size and toughness. Wells has assumed that the defect size is distributed as one half of a normal distribution. Becker & Pederson, referred to by Wells, assumed an exponential distribution; but distributions of size are usually assumed to be log-normal. The data available to choose between these distributions are almost non-existent. This may seem a minor point but changes in the assumed distribution of defect size could change the apparent risk of failure by orders of magnitude. I would plead for a major effort to study the actual distributions of defect size in real structures as part of a more general attack on probabilistic fracture mechanics.

Secondly I would like to dispute a point made by Forsyth (2.3). We find that the vast majority of fatigue failures studied by us are caused by the presence of the weld itself rather than by weld defects in the normally accepted sense. Of course any weld could be considered as a blemish on the unsullied product of the material manufacturer; but on the whole fatigue of welded structures appears to be a problem of design. In disagreement with Dr Forsyth we do find that fatigue strength reduction factors can be assigned to welded joints and this is the basis of current design in welded bridges and other types of structure.

L. P. Pook (*National Engineering Laboratory*)

It is surprising to see it suggested by Forsyth (2.3) that fracture mechanics are not applicable at short crack lengths, surely what is meant is that it is difficult to get the applied mechanics right in the situations of interest.

Fracture mechanics, especially the stress intensity factor, has contributed a great deal in that it has provided a clear view of fatigue crack growth. Unfortunately after about 25 years active work there is no agreed standard method of concluding a fatigue crack growth test under constant amplitude loading never mind variable amplitude loading. This is relatively unimportant when the mean life of a structure has to be predicted, perhaps for fracture analysis purposes. However it becomes serious when making a safety case in that it is not possible to make worthwhile estimates of scatter in structure life from the scatter observed in fatigue crack growth tests.

L. M. Wyatt (*Central Electricity Generating Board*)

I would like to augment statements by Baker (2.5) and Branch (2.4) on crack propagation in creep. They rightly describe some of the factors which make this difficult to assess, but there in one important factor they do not mention. This is that the law which governs the propagation of a crack in creep is very dependent on the method of creep deformation. In ductile materials which deform by disolocation movement, creep deformation quickly blunts the tip of the crack and it is probably safe to consider the area of the residual section when calculating the time to failure. On the other hand, when grain boundary diffusion is the dominant mode there is a very strong tendency to form cavities of the kind shown by Dyson (this discussion) ahead of the creep crack and creep propagation is proportional to a very high exponent of the stress intensity at the tip of a crack. The mode of creep deformation is very dependent on stress. In addition, however, such factors as high solution temperatures undergone in a heat affected zone or the presence of relatively large contents of tramp elements tend to promote the tendency to deform by grain boundary diffusion. By avoiding both these conditions the danger of failure by creep propagation can be very much reduced.

With the greatest respect I cannot endorse the principles of materials selection advocated by Flint (this discussion) and I would like to bring to the attention of the Conference the reason why his proposal can be potentially very dangerous. This is that the 'guaranteed minimum value' that he mentions is not the minimum value that can occur within the material or within the parcels of material supplied. It is the minimum value obtained by testing a limited number of specimens. When this is borne in mind it becomes clear that the minimum value actually existing in the material is likely to be much lower than the guaranteed minimum value if there is a wide scatter in material properties. If there is a small scatter the chances of a low value are very much reduced. Moreover the lowest value may in the former instance be well below that allowed for by the design safety factor. Therefore, the chances of failure are much greater with a wide scatter of material properties than with a small scatter.

H. G. Wolfram (*West Gate Bridge Authority*)

Wells (2.1) covers a wide spectrum of structural design philosophy in relation to materials properties. I should like to comment briefly on only one aspect of this paper.

I was struck particularly by the statement:

'While it is the function of mechanical testing to identify the limits of safe operating conditions in structures, it is no advantage in the final synthesis to overemphasize one property, particularly when this is at the expense of other properties.'

I come from a part of the world where in the last 15 years three major bridges collapsed; one during construction and two in service. One collapse was caused by brittle fracture, one through a combination of circumstances and one through navigational mishap.

I think the converse of Wells's statement is also true, namely that one must not underemphasize any one property.

I think we have reached the stage in structural design and knowledge of the behaviour of structures that, provided we make full use of the knowledge of the properties of materials and the tools of structural analysis, we can have a reasonable degree of confidence in the safety of our structures.

I am thinking particularly of the developments in the fields of brittle fracture and weldability, the refined methods of structural analysis and the developments of non-destructive testing.

But there is one area where knowledge is either insufficient or the available knowledge has not yet penetrated to the design engineer, that is in the understanding of the phenomenon known as 'lamellar tearing'.

I was once involved in the design of a fully-welded multi-storey frame, where cracks occurred in highly restrained beam-column connections. Fortunately, the metallurgist whom I consulted was able to identify the cracks, and identified them as 'lamellar tearing'. This was some 12 or 13 years ago when 'lamellar tearing' was a new term to structural engineers.

More recently, we found lamellar tearing in the saddle of a tower for a cable-stayed bridge. In both cases the problem was resolved by gouging and rewelding and, in the second case, this was followed by stress-relieving.

I have been told that the best way to avoid lamellar tearing is by clever designs which do not result in highly restrained T-joints or cruciform joints. Maybe we engineers are not ingeneous enough, but it is not always possible to avoid these problems purely by clever design.

The yield capacity of highly restrained joints can be improved by buttering up the joints with low yield strength weld metal. This also has its limitations. What is needed, is a deeper understanding of the mechanism of failure by lamellar tearing and methods of quantitative analysis of behaviour of joints where the through-thickness properties of the material are important.

If these methods do not yet exist then I would suggest that this is a property which urgently requires full investigation. If the methods do exist then there is perhaps scope for wider dissemination of the information.

P. J. E. Forsyth (*Royal Aircraft Establishment*)

In reply to comments by Harrison (this discussion) I would say that I did mention the fact that fatigue strength reduction factors could be assigned to welded joints; I questioned only the precision of such procedures.

Pook (this discussion) has objected to my rejection of fracture mechanics for the treatment of small cracks. I would still maintain that what I said is substantially correct. The stage I crack extends by the joining of slip plane damage and is formed in the first place by the action of the plastic deformation itself. It frequently lies in a surface zone where the spread of plastic deformation is considerably greater than the dimensions of the crack. Under these circumstances I cannot see how one is justified in claiming that fracture mechanics could be made to work, Even the small stage II crack may lie in a zone of plastic deformation and the same objections would apply. As a metallurgist, and a failures investigator, I eagerly await any advances in applied mechanics that would help to make service life predictions on components containing short cracks.