Discussion

O. A. Kerensky, A. R. Flint, D. James, H. C. Cotton, H. G. Wolfram, J. E. Roberts, J. H. Van Der Veen, W. L. Mercer, J. D. Harrison, R. R. Barr, G. G. Saunders, R. G. Baker, T. Kanazawa, W. E. Duckworth, T. Gladman, G. P. Tilly, R. Dolby, J. Savas, J. H. Van De Veen, D. W. Smith, J. C. Chapman, A. A. Wells and J. F. Lancaster

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respect to pipes made from welded plate, but it is tempting to speculate on the potential advantages of continuous manufacture of pipe at the point of laying, by winding high strength steel wire on to an extruded polymeric matrix.

The function of this introductory presentation is to extract and emphasize for further discussion those aspects in the papers which reflect the demands on the physical metallurgist arising from engineering requirements. They appear to be summarized as follows:

1. Further provision of understanding of the effects of alloying elements and impurities, and response to heat and deformation, such as will enable the steelmaker to produce products which are both economic and consistent in their mechanical properties, including notch toughness.
2. Further elucidation of the effects of welding thermal cycles and deformations, and their modification by heat treatments, where these are absolutely necessary.
3. Eludication of the surface and interfacial physical and chemical mechanisms involved with oxidation, corrosion and cracking, including environmental effects associated with fatigue, and possibly the surface characteristics for use with adhesive jointing and bonding with polymers.
4. Closer definition of all these properties aimed at securing agreement as to how they are to be measured, and the levels of improvements in magnitude and consistency which might realistically be sought.
5. Consideration should also be given to the propsects for new products, which should include higher strength steels and those such as wire suitable for composite constructions, and new or further developed joining processes for use on site and under water.

Discussion (Chairman: O. A. Kerensky, F.R.S. (Freeman Fox and Partners))

A. R. Flint (*Flint and Neil*)

The designer's primary aims are avoidance of collapse and of unserviceability, generally through excessive deformation. These aims require knowledge of load extension characteristics. Structural strength depends on tangent modulus and deflexion on secant modulus so that premature departures from elasticity and proportionality are of importance. Definition of yield stress by the drop of beam method, while satisfactory for mild steel, may not be so for high yield materials particularly when structural stability is critical. The present definition of yield may have to be replaced by 0.2 % proof stress as has been common for light alloys.

The influence of the limit of proportionality on structural strength has been observed in developing the design rules for plate girders for BS: 153, in the course of which tests undertaken on full-scale samples showed the proportional limit to be as low as 50 % of the nominal yield stress.

The T1 steels in the U.S.A. exhibit major reduction in tangent modulus at about 85 % of their yield stress in compression.

In components unaffected by instability the yield stress at which a plateau of extension occurs is of obvious significance. But advantage may only be taken of this if the structure is not so detailed as to permit local failure before the ductility may be mobilized. In such components, the ultimate tensile strength (u.t.s.) may still have relevance; thus the draft clauses for the new Steel Bridge Code requires that u.t.s. $\geqslant$ yield stress multiplied by 1.25 times the ratio of the gross to the net area at any bolted or rivetted connection. This provision will tend to govern the design of tension elements in materials having a yield stress in excess of that of grade 43. Bolted connections commonly have an efficiency $< 80\%$, and bolted rolled sections commonly suffer a reduction in gross area of about 30 % due to holes. Thus if the yield stress were to be fully utilized, it would be necessary to provide an ultimate stress of $37\frac{1}{2}\%$ greater than the yield stress. This is not achieved by the specifications for grades 50 and 55. The Draft Materials section of the Bridge Code, at present out for comment, permits ultimate strengths no greater than 10 % in excess of the yield stress!

These provisions need only apply to structures where redistribution of forces is to be allowed, and the lower limit for u.t.s. may prove adequate for bridge work where the connections are remote from points of maximum stress in primary members. The lower value of u.t.s. has been considered permissible given assured ductility. A minimum elongation of 16 % is suggested in the Draft Materials Code; this figure having been based on the work at Cambridge into the ductility requirements for plastic design. Furthermore, ductility of this order is desirable to relieve stress concentrations and to permit forming during fabrication.

What is ideally required is an elongaton of 5 % before either work hardening or necking begin, since the former inhibits stress redistribution and the latter is locally damaging.

The designer should ensure that the material properties at a particular part of a particular element have a known statistical probability distribution of achievement. The present so-called 'guaranteed minimum yield strength' represents neither a minimum nor a guarantee which has any significant worth from the viewpoint of providing compensation in the event of non-compliance. The variability of the quality within casts of steel has been shown to be at least as great as between casts and, as a result, there is poor correlation between mill test results and the actual component strength. It is noteworthy that our materials are still as variable as the extreme design loads.

The so-called characteristic value introduced under the cloak of limit-state design has not proved particularly helpful, other than by encouraging statistical control of material properties. The reliability of a structure, having a given characteristic strength may be shown to increase with variability in the material. Since it is known that rolled steels available in this country have a wide range for variability, it is clear that the characteristic values as at present defined are misleading. A more rational basis for design requires the definition of both mean and variance of product on a batch rather than a cast basis. A proposal for control procedures aimed to achieve this is included in the draft for comment of the Bridge Materials Standard. It is my view that the term 'characteristic strength' should be forthwith abolished or redefined as the mean and that all design rules should be based on a prescribed combination of mean and variance. In the absence of such a change, designers will continue to be landed with very varied products supplied to meet the same need.

The variability of steel quality seems to arise mainly not due to chemistry but due to metallurgical structure as influenced by rolling. It would seem desirable that materials be developed which are less sensitive to the brutal processes of forming structural components. It is too much to hope that a new family of economical steels will appear which have such properties and minimize residual stresses, perhaps in conjunction with new rolling techniques? If, at the same time, the advantages of improved strength due to rolling can be achieved the product will go far to satisfy the designer.

D. James (*British Aircraft Corporation*)

Flint (this discussion) referred to a new specification for structural steels calling for a minimum elongation of 16 %. In aeronautical structures high strength aluminium alloys with typical elongations of 7 or 8 % are commonly used. These may have short transverse elongations of 2 or 3 %. I am surprised at the very high figure which is being specified for a structural steel. The reasons for this are far from clear to me; it is likely that they are traditional rather than technical.

H. C. Cotton (*British Petroleum*)

Through thickness properties are of special importance when constructing large offshore platforms. This is evident from the many reports of lamellar tearing or what appears to be lamellar tearing experienced during the fabrication of large and thick weldments. It is not possible to simplify such joints as the location at which braces must meet the node are carefully calculated so there is little room for improvement in the geometry of such connections by which welding stresses could be minimized. The question of how to cater for punching shear and other effects presents significant problems and often gives rise to a can so interwoven by longitudinal and radial stiffners requiring full penetration welded attachments that the welder cannot even see the root of the weld. It seemed almost impossible to avoid a lamellar tearing problem under such circumstances especially as no opportunity presented itself under the prevailing conditions to practice special welding sequences. In association with certain steelmakers the cause of lamellar tearing was disclosed to be a basic anisotropic weakness in the steel attributable to inclusion morphology. This was cured and the lamellar tearing problem simply disappeared. In the future there will be many applications for such steel which may supplant the use of forgings for some applications. The use of steel plate with isotropic properties is bound to expand across a wide front of applications.

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

Harper (1.1) has commented on the problems associated with distortion of thin plate structures due to application of heat.

This is one of the fields where close cooperation between metallurgist and engineer can help to overcome the problem. I am referring to heat straightening of stiffened plate structures which has been applied successfully during the fabrication of orthotropic deck panels up to 15 m × 3 m in size.

Before the use of heat straightening, metallurgical advice was obtained to ensure that the mechanical properties of the steel would not be affected adversely by the application of heat followed by air cooling.

However, although the problems of distortion of thin-plate structures can be overcome, I suspect that the future trend in bridge design will be away from thin plate structures towards heavier sections, largely because of:

(*a*) the increasng ratio of labour to material costs, but also because of

(*b*) the high costs associated wih the design of highly complex and sophisticated structures.

It is in this area that the engineer must make some predictions of his future requirements. I see the problems in two areas:

(i) the decline of toughness and weldability with increasing plate thickness,

(ii) the non-homogeneity of steel in the through-thickness direction as manifested by cases of lamellar tearing in cruciform and highly restrained weldments.

J. E. Roberts (*British Steel Corporation*)

Flint (this discussion) mentioned the statistical definition of strength, to recognize the inherent variability of steel plates and sections, and used the term 'characteristic strength'. Some of the problems arise because the metallurgist in the past has misled the engineer into believing that steel was uniform and homogeneous, when it is not. Nevertheless, conventional release test procedures have served adequately for quality control purposes for many years. The 'characteristic strength' concept produces considerable difficulties in application to steel production and clearance of material for dispatch, to say nothing of the question of how to deal with steel already supplied, and perhaps fabricated, should a drift in mean or standard deviation of strength be experienced. Certainly there is continual striving for a more consistent product, but the papers presented in this session all emphasize the need for economy, and so far the economic benefit of applying the characteristic strength concept has not been demonstrated. This is an area where collaboration between engineer and metallurgist is in action, a good deal of discussion having taken place between Flint and his colleagues, and steelmakers (including myself). The best description of the present state of play is 'a draw', but the game is still in progress.

J. H. Van der Veen (*Hoogovens*)

Ultimate strength is not a physical property of the steel but the point of instability in a particular test, the tensile test with parallel faces. As a steelmaker, I have no objection to a criterion on 'reserve ductility or strength beyond yield point' but please let it be a proven scientific criterion, such as, if justified, the strain hardening coefficient m.

W. L. Mercer (*British Gas Corporation*)

Could I comment on the need for a proper balance in development studies leading to higher strength materials. I think it is important that we look at all the required criteria together because only in this way can one be sure of covering any really critical factor important in a particular application. I think phenomena like corrosion, fatigue, etc. will be limiting in the majority of cases. For example, in the case of pipelines, one can say that there is obviously an ecnomic advantage in utilizing steels of 690 MPa (100000 lbf/in^2) yield strength. I have no doubt that it would be easy to provide steels with adequate levels of toughness to match these high levels of strength. However, I think the things which are going to trip us up in the end are corrosion behaviour, particularly stress-assisted corrosion processes and hydrogen embrittlement. Metallurgical development work needs to recognize these parallel potentially limiting factors and to incorporate them into the programme from the outset.

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

Is there any justification for requiring pipeline weldments to have sufficient toughness to arrest propagating cracks? This does not seem to be required by the British Gas Corporation but is still called for by some pipeline customers and is referred to as a requirement by Barr & Burdekin (2.2). The accident envisaged by Mercer (1.2) is breaking of the pipe by earthmoving equipment. A crack started in this way could not conceivably run for more than one pipe length in the weldment. The weldment should have sufficient toughness to prevent fracture initiation under normal service conditions. The specification by less enlightened customers of an arresting property in pipeline weldments can be a serious limitation on the materials and procedures used in construction.

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

I agree with Cotton (1.3) that the c.o.d. concept is not cut and dried, and in the paper by Burdekin and myself (2.2) we have discussed in detail some of the problems associated with the testing of weldments and the interpretation of the results. In these circumstances it is important to be able to rely on the results from wide plate tests and I would like to discuss further one or two points related to stored energy as referred to in Cotton's paper.

Cotton makes reference to the Robertson test and the question of stored energy and compares this to the wide plate test situation. I would like to point out that the essential difference between the wide plate and Robertson tests is that the former is a crack initiation test while the latter is a crack propagation test.

However, taking up the point about short crack initiation in the wide plate test, we consider that the stored energy in the B.S.C. test system is sufficiently high to drive a crack once initiated. I would like to quote some preliminary results from a test using a T-welded configuration where an attempt has been made to assess the dynamic response of the test frame. The notch was a surface notch sharpened by fatigue (55 mm long × 11 mm deep) and located in a m.m.a. weld metal (C–Mn) the steel being 25 mm thick, BS 4360:55E. Strain gauges were placed on a line parallel to the expected progress of the fracture and some 50 mm from the weld, running transverse to the plate as shown in figure 1. The gauges were placed at intervals of 75 mm from the centre line of the plate, but only one was placed on the opposite side from the weld. We recorded the response of these gauges during the course of the subsequent fracture at -30 °C, the fracture initiation stress being 528 $\mathrm{N/mm^2}$.

The recording system proved to be capable of resolving the dynamic effects which occurred during fracture and this is illustrated in figure 1, where the signal voltages are illustrated over a time period of 8 ms. The gauge nearest to the plate centre line shows a sharp fall in strain as the fracture is initiated and propagates. The defect incidentally initiated and travelled through the transverse weld but arrested on the other side of the centre line, as a result of the increased propagation resistance of the parent plate. Moving out from the centre of the plate to the gauges nearest the edge of the plate, these show large transient rise of strain followed by a fall to the permanent plastic strain, indicating that there was sufficient stored energy to maintain the load on the remaining ligament giving a net increase in stress and hence the transient increased strain measurement.

The fracture arrested in the parent plate as mentioned previously and the gauge in this region showed a very large strain measurement being within a large plastic zone accompanying the arrest of the fracture.

In conclusion, these preliminary observations together with calculations on stored energy associated with the measured deflexion of the loading points, etc., should help to retain some confidence in the wide plate test as an initiation test in the circumstance described in Cotton's paper where there is a small zone of low toughness. This work will be continued and it is our intention to publish more details in the near future.

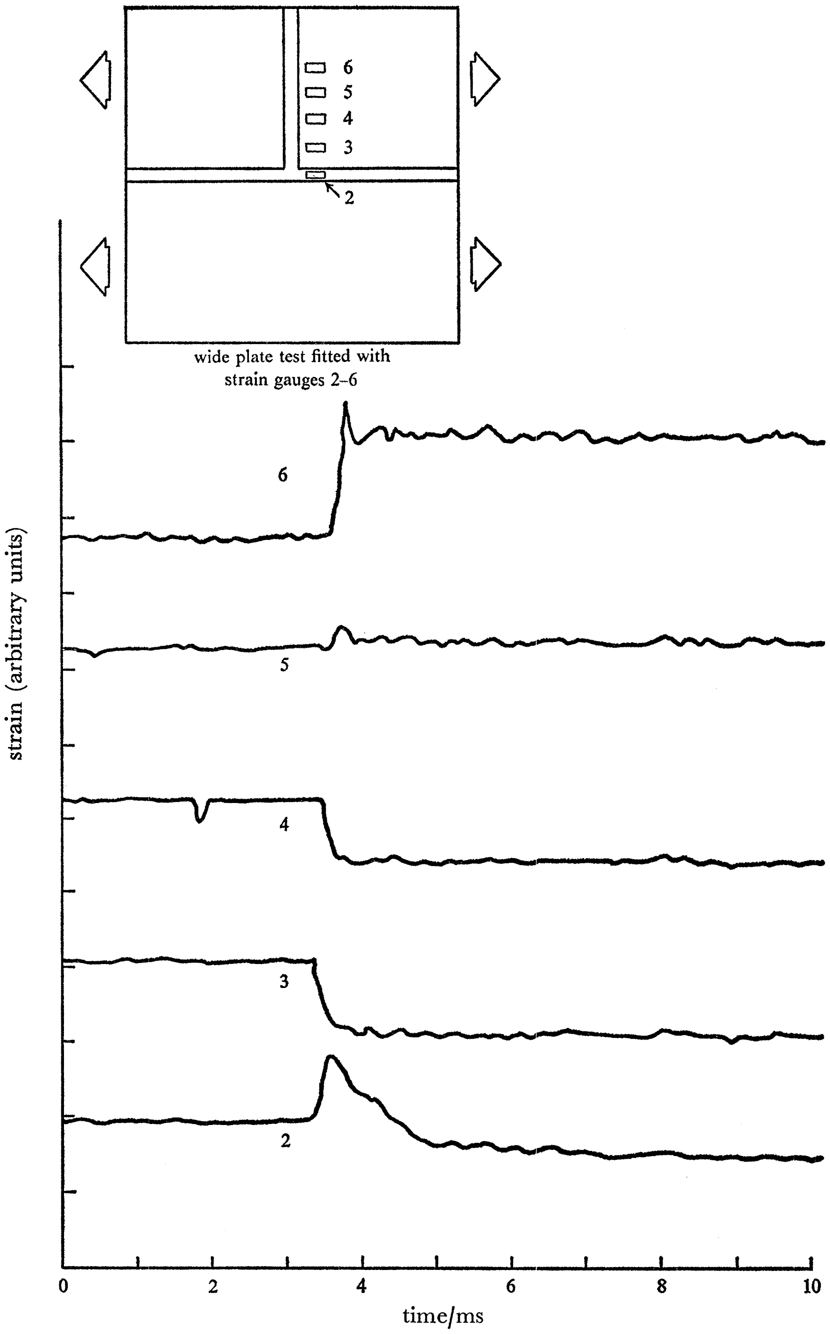

FIGURE 1. Strain against time records during passage of failure crack in wide plate test.

G. G. SAUNDERS (*Whessoe Ltd*)

I wish to raise two points. The first is directed towards Flint (this discussion). I am amazed that in 1975 engineers are still requesting information for design purposes, derived from the uniaxial tensile test. It must be clear from the papers to this conference that there are far more important material characteristics relevant to structural behaviour that cannot be obtained from such tests.

The second follows Harrison's comment (this discussion) concerning dynamic fracture toughness requirements in weld metals. It is already exceedingly difficult to achieve adequate static fracture toughness levels in weldments, particularly for joining high strength steels. The prospect of meeting dynamic toughness requirements is a daunting one in these circumstances.

W. L. Mercer (*British Gas Corporation*)

I wish to reply to the suggestions by Harrison and Saunders (this discussion) that unrealistic crack arrest criteria may be required for weld metal in pipelines. I am not sure where this idea originated – I certainly hope there is no such implication in my own paper.

The fracture arrest requirement in British Gas and other specifications applies to plate material only. This arises directly from the fact that the main cause of pipeline failure is mechanical damage caused by earth moving machinery. Thus, a super-critical defect can suddenly appear and a fracture initiate. By specifying toughness in terms of realistic arrest criteria, the fracture so started is quickly arrested. Normal practice is to weld pipes together to form a pipeline with the longitudinal submerged arc welds out of line with those in adjacent pipes. If a fracture is initiated in a submerged arc weld, it runs into tough plate material in the two adjacent pipes, that is – if it does not run away from the weld line into the parent plate of the initiator pipe before it reaches the adjacent pipes. The same basic philosophy is applied in the case of pipeline fittings, i.e. the requirement is to prevent brittle *initiation.*

I would like to take this opportunity to add that it is all too easy to concentrate one's attention on the pipe properties alone. Experience indicates that more emphasis is required on the specification, design and manufacture of critical fittings such as valve bodies, flanges, etc. While it has become relatively easy to obtain high levels of strength, toughness and weldability in line pipe through use of micro-alloyed steels and controlled rolling techniques, there has been no equivalent development in materials for fittings.

I suggest this is a suitable practical topic for further examination today to illustrate the value of close working links between the engineer and the physical metallurgist.

R. G. Baker (*National Physical Laboratory*)

Surely the *initiation* of cracking is independent of the stored energy in the wide plate test rig, provided cracks are small in relation to plate width. Barr (this discussion) suggested that perhaps this was not so. Perhaps Kanazawa would like to comment on relevant experience in the Wells–Kihara and deep notch tests, which have widely different notch depth to plate width ratios.

May I also suggest that redistribution of strength depends on local ductility adjacent to points of stress concentration. No rise of strength is necessary while deformation occurs. Surely then, notch ductility is more important than overall ductility and a positive work hardening coefficient, however small, is more relevant than y.s./u.t.s. ratio.

T. Kanazawa (*Tokyo University*)

I agree with Baker. There are many ways of testing materials for both crack initiation and arrest. Also the fracture process depends not only on the nature of the material but also on the test specimen sizes. In other words the arrest of a long running crack is different from short crack arrest. I think it is very difficult to analyse such phenomena in detail.

H. C. Cotton (*British Petroleum*)

Sometimes for light reading I keep in my bag a copy of *Welding in the World*; recently in volume 13 I read the final report of a working group on brittle fracture tests for weld metal. I am glad it was the final report because it seems from a cursory reading that confusion has been compounded as time has gone by. There is a tacit assumption I think by certain groups in Britain, that crack opening displacement is the only valid way to assess fracture initiation in yielding materials and no other methods are really worth considering yet, strangely enough, when one goes to the U.S.A. for example, one finds that almost nobody gives any thought to c.o.d., other techniques being used (K_{Ic}, d.w.t.t. etc.) to estimate low stress fracture risk.

Before we convince ourselves that we know everything that is to be known about crack initiation let us bear in mind that there are many other techniques in vogue which have received scant attention in this country. Furthermore the presently accepted criteria for c.o.d. assessment may be unduly conservative and hence costly. Because a weld shows transient brittleness in the c.o.d. test at a low applied stress it does not follow that the surrounding metal might not be able to arrest the small crack very quickly after initiation.

On the other hand in Wells cross welded wide plate tests, after a fracture has run for a short distance, it often arrests in more notch ductile material. Whether or not this is significant depends upon whether the load is still being applied as it would in a real structure where the actual load could be progressively rising rather than decaying as it does in many experimental arrangements.

In regard to the question of arresting a fracture which is propagating at some length in a large pipe line it is our conclusion that this is just not possible in the size of pipe and at the pressures and stresses that are commonly used for transmission pipelines. I believe that a Canadian Study Group has abandoned all ideas of arresting running cracks by means of metallurgical effects and are now resigned to using mechanical crack toppers, such as we use at B.P. We have some confidence in the wide plate test, we have some confidence in crack opening displacement, it might even be that we have a degree of confidence in the Nibbering test, but we think it prudent to keep all our options open at this time pending a clearer understanding of the mechanics of fracture initiation.

W. E. Duckworth (*Fulmer Research Inst.*)

May I comment on Baker's reference (this discussion) to tensile strength. Some time ago I would have supported the view of its irrelevance, but recent work at Fulmer suggests that the point of instability may be where the first microvoids, which ultimately coalesce into failure, may start. If this is confirmed then it may be an important measure of the ability of metals to withstand the onset of fracture in the regions of local stress mentioned by Baker. Thus, the instincts of the engineer in clinging to u.t.s. may be justified.

T. Gladman (*British Steel Corporation*)

May I endorse the view expressed by Van der Veen (this discussion) concerning the inadequacy of the u.t.s. as a design criterion. A high tensile strength is seen as a safety measure to guard against overstressing. The elongations associated with stresses above the yield are significant and would prove unacceptable in major structures. A safer approach might be to further increase the yield strength. If any plastic distortion is experienced, this is usually in some region of stress concentration where fracture, i.e. cracking, is far more likely to produce failure than any concept of plastic instability on a macroscale such as is inferred by tensile strength. Cavity development, although important in connection with ductile failure, has little to do with plastic instability. Experience points to the fact that cracking rather than plastic instability would cause failure, and the modern approaches to fracture should be followed through; this aspect will of course be dealt with in a later session.

A. R. Flint (*Flint and Neil*)

The strength of bolted or riveted connections in bridges are directly correlated with the ultimate tensile stress as obtained from the normal type of test, and if we are to make the best use of the yield stress that our steel produces, we do not want our connections to be weaker than the remainder of the structure, and we want to develop ductility in the structure remote from the connections, and it is for these reasons alone that I think we still need to consider the ultimate tensile stress.

G. P. Tilly (*Transport and Road Research Laboratory*)

One of the problems faced by designers of bridges and other steel structures is to assess collapse behaviour under compressive loading. It is now possible, by using advanced elasto-plastic analyses, to determine the post-buckling behaviour of steel plates having different degrees of initial imperfection, residual stress, and simple stiffening (Crisfield 1975). However, in making predictions it is necessary to use tensile stress–strain curves for the as-received material. In practice it is suspected that stress–strain curves, particularly in the heat affected zone, will be very different from the available data. In addition it is now known that compressive yield stresses are approximately 5 % higher than tensile yield stresses. It is therefore essential to have more accurate data to enable satisfactory comparisons to be made between analysis and experimental behaviour of structures. This is an area which requires research into material behaviour to aid the further development of analysis and improved design rules.

Another area in structural engineering requiring more information is that of connections made by high-strength friction-grip bolts. Such bolts are usually plated and can be susceptible to hydrogen embrittlement. There have been examples of delayed failures that have occurred up to four years after installation (Ito & Matsuyama 1970). In contrast laboratory testing has been conducted for much shorter durations and it has usually been considered that fractures due to hydrogen embrittlement occur relatively soon after tightening of the bolts; clearly this is not always the case and long term laboratory data are required to elucidate this situation. Current research is being conducted to determine behaviour of faying surfaces in joints being exposed for up to 10 years (T.R.R.L. 1974).

In steel bridges fatigue damage can occur and there have been several reported instances, such as those in the United States (Heins & Galambos 1974), of cracks particularly in designs having girders with cover plates welded to the flanges. One of the difficulties in assessing such fatigue is that bridges are designed for up to 120 years usage. During this service life, between 100 million and 1000 million fatigue cycles may be imposed by traffic and there may also be damage due to wind induced vibrations. Unfortunately laboratory data are usually for tests halted at endurances up to approximately ten million cycles and it is therefore necessary to extrapolate lives by approximately an order of magnitude in order to assess behaviour in design lifetimes. Fatigue cracking can also occur in welded regions subjected to pulsating compression.

An example of such behaviour occurred in the stiffener to cross beam welds of an orthotropic steel bridge panel subject to normal traffic at a site near Denham (Nunn 1974).

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R. DOLBY (*Welding Institute*)

I would like to refer to the use of quenched and tempered steels and in particular their low usage in this country even with the long experience in the United States and Japan. It seems from our position at the Welding Institute that one of the reasons for the low usage of the micro-alloyed quenched and tempered steels for tanks construction and other fabrications is one particular aspect of weldability. There are no cracking problems. Hydrogen induced cracking, hot cracking in the weld metals, lamellar tearing and so on, are no more likely to occur in this type of steel than in controlled rolled or normalized steels. The one stumbling block at the moment appears to be low weld metal toughness. There is a dilemma here because low carbon micro-alloyed steels are being used in Japan and are apparently satisfactory for these applications, but our experience is that the toughness of the weld metal regions can be very low. I think there would be wider usage of quenched and tempered steels in the U.K. if this particular problem could be resolved.

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

To refer to Dolby's remarks (this discussion) on quenched and tempered steels, low toughness at the root of a m.m.a. weld is apparently a problem when welding in the vertical-up position using a full weave. This situation is more widespread than may be realized and it is not confined to micro-alloyed quenched and tempered steels, nor is it associated only with British steels. It is a weld metal problem related to the thermal effects during welding and the fact that information has not been published elsewhere is probably due to the fact that it has not been detected in laboratory weld trials.

Referring to one of the points raised by Baker (this discussion), I did not mean to imply that the stored energy in a test rig would exert any control on the conditions for fracture initiation. I was commenting on the situation, referred to by Cotton, where a fracture might occur locally, perhaps within a region of low root toughness. I was trying to illustrate that, in the event of a short fracture being initiated, there is sufficient stored energy to maintain the load on the wide plate test and I hope that when we have additional data we can satisfy Cotton on this point.

J. SAVAS (*Republic Steel Corporation*)

At our Research Centre, we have found an interesting correlation between hardness of the weld metal and the heat affected zone area and toughness. We have found with some of the steels that if they are too highly alloyed you get exceptionally high hardness, and you have to resort to subsequent tempering of the heat affected weld zone. Thus, you can overcome the problem of using quenched and tempered steels in the welded condition. It all depends on the particular compositions you are looking at. We are not in a position to reveal our alloy developments in these areas, but it is suggested that it might be worth looking at these considerations if there is a problem with the use of quench and tempered steels in some applications.

Chairman – Is it a metallurgists' problem?

Yes, it is metallurgical due partly to the heat treatment that is required subsequent to welding to covercome the general problem of toughness. We have found that we can exactly double our toughness in experiments which use the implant test. If you are familiar with that test, you will appreciate that there is a thread in the sample that is welded over and that presents a sharp notch right in the heat affected zone. Thus, it is quite a critical test. Our results show that in most cases, depending on composition, you will require a post weld heat treatment.

Chairman – Is the cost of all this viable?

Well this has to be taken into consideration along with strength level.

One can work with steels having yield strengths of 690 $\mathrm{MN/m^2}$ (100 000 $\mathrm{lbf/in^2}$) or higher. I believe even 1000 $\mathrm{MN/m^2}$ (150 000 $\mathrm{lbf/in^2}$) yield strengths can be used in structures.

J. H. Van de Veen (*Hoogovens*)

In talking about sufficient stored energy in tests on brittle crack propagation, I would like to draw attention to the question of whether sufficiently high stored energy is really 'available' at the right time and place for continuing crack propagation. Energy can travel no faster than the velocity of sound and this is of comparable magnitude in relation to brittle crack speed.

D. W. Smith (*Mott Hay & Anderson and University of Dundee*)

Fatigue and jointing methods. Cotton (1.3) has rightly drawn attention to the fact that welding reduces all structural steels to the same fatigue strength, and this tends to inhibit the use of high strength steels. New methods of joining are consequently desirable.

Epoxy adhesives can join steel to steel and steel to concrete with a strength greater than the shear strength of concrete; and, with proper selection, they are remarkably tolerant of quite significant rust (but not grease) which may be on the surface before their application.

It would be of interest to know whether there is a metallurgical implication. Is the selection of suitable adhesive affected by the metallurgy of the steel? Is it necessary to repeat all the adhesive bond testing every time a change of trace element is made in the steel mix?

Another point is that adhesives may favour a return from thin plates to heavy steel sections. An adhesive joint is not as strong as the steel, and may therefore have to be wider than the steel part, as, for example, the flange of an I-section is wider than the web thickness.

J. C. Chapman (*Wimpey*)

We are talking about the role of metallurgy in engineering practice. I work at the rough end of engineering, so perhaps you will not mind if I talk about metallurgical practitioners and their role in engineering practice. The practicing metallurgist has tremendous experience in diagnosing failures and recommending modifications. This experience, I think, equips him to contribute much more than perhaps he does at the moment to design, material specification, inspection, application and protection. He often has I think, an important role to play in the education of engineers, who need to know something about the practical behaviour of materials. In their training, to my mind, engineers are not taught enough about the practical behaviour of materials; they tend to get watered down courses on metallurgy – I think they want a more superficial but practical treatment of the actual behaviour of materials, and metallurgists have an important role to play in this. Where appropriate, practicing metallurgists should form part of the design team to advise on the matters I have mentioned; they should not simply be involved in the diagnosis of failures.

I think more attention needs to be given to the role of the metallurgist in bringing existing knowledge to bear on engineering problems. Naturally, we are talking mostly today about unusual problems, but most of the failures which occur are failures which could have been foreseen if enough previous attention had been given to them within the scope of existing knowledge. Equally in material development, high priority should be given to reliably and expeditiously producing materials to the existing specifications.

Perhaps I could just mention one or two examples where the practicing metallurgist has a most important role to play. Corrosion protection, at the design stage, is a very important area for the metallurgist. We talk a lot about the corrosion of steel but we think that when it is protected by concrete there is no problem. However, in my experience, when steel protected by concrete corrodes you are in even bigger trouble than when the steel is not so covered. Criteria need to be established for an adequate degree of protection in terms of aggregate purity, thickness of cover, crack width, exposure and environment.

In the choice of materials and design of details to prevent stress concentrations and fatigue, the metallurgist comes into his own because he is looking at these failures every day of his working life and we should be seeking his advice at an early stage. Ship structures are a very important area for the practicing metallurgist; failures may be induced by propeller induced vibration, wave forces and machinery vibration, and may occur in the machinery itself.

Could I just ask one question on development of new materials: is there a greater role for ductile iron castings? We have heard a lot about the diffiuclty of making connections, particularly welded connections in tubular members. Ductile iron has developed a lot in recent years; so far as I know there is a considerable difficulty in welding but is there any hope that it will be weldable in the future? Can we look forward to ductile irons being a tool at the disposal of both the engineer and the architect?

A. A. Wells (*Queens University*) (*Keynote Speaker*)

I simply want to put forward one thought in the remaining time. In the discussion this morning on two occasions the question arose as to the definition and development of a test. It came up in terms of Flint's point concerning the importance of the departure from linearity in the stress–strain curve, particularly with regard to compressive loading, and I think Tilly took that point up later. Certainly when Tilly was speaking, it came home to me that it was rather strange that this particular effect was not being qualified by a compressive test since one was dealing with something to prevent compressive failure. Then again, several speakers took up the question of the significance of the loading conditions in the wide plate test and in crack opening displacement tests, so it produced this thought which I think is important enough to put before you.

If you want to fine-down the actual point at which engineers and metallurgists react most with one another, it is at this point where tests have to be defined and developed. This job is characteristically done by task groups. When we have a meeting of the kind that we have this morning, we realize that there are so many raw topics that no one discussion can ever have sufficient time to go through to finality. This is the sort of thing that can be complemented by smaller groups of people working together in task groups when they do have a definite job to do; they are asked to come up with the specification of a test.

Some people will know exactly what I have in mind having seen the E24 committee of the A.S.T.M. in action. In the United Kingdom, in years gone by, we had the Navy Department Advisory Committee on structural steel that provided the umbrella for quite a number of years for this kind of group of people to get together. May will recollect his committee, which was concerned with the high strength steel. It is my understanding at the moment that this kind of activity has suffered somewhat by the depression conditions which we met and hope we are going to get out of, and the thought I would like to leave is that we should look to the importance of task groups in this field.

The following two contributions were received after the meeting:

J. F. Lancaster (*Kellogg International Corporation*)

I think the embrittlement of steel in service deserves more attention than it receives at present. One is hampered in discussing this subject because of a lack of good quantitative information from the field. In particular, there is often a legal fog surrounding pressure vessel failures which makes it impossible to obtain other than under-the-counter information. Nevertheless, it is clear that catastrophic brittle failures of pressure vessels and other equipment continue to occur in process plants. This situation may be contrasted with that in shipping where brittle failure, which caused many losses at sea during the forties and early fifties, is no longer a serious problem. One possible reason for the relative lack of success in controlling brittle failure of pressure vessels is that modes of embrittlement in service are insufficiently understood. Two years ago there was a catastrophic brittle failure of a liquid ammonia storage tank in South Africa. There was a major escape of ammonia, as a result of which eleven men died. Investigation of the failure showed that the material of construction – BS 1501–151–28A – was chemically within the specification, but that one plate had a very high impact transition temperature and poor tensile ductility. There was no reason to doubt the as-delivered quality of the plate, and the investigators of the accident considered that a major contributory cause was strain-age embrittlement of the material. Clearly it is important to control susceptibility to this type of embrittlement. Nevertheless no such means of control exists in either British or American codes and standards. Moreover, there is very little information in the metallurgical literature on strain-ageing of pressure vessel steels, and virtually none on their ability to maintain notch-ductility over a period of time.

Strain-age embrittlement is only one of a number of possible embrittlement mechanisms that may afflict metallic equipment over a period of time. The risk of such embrittlement is well known in the hydrocarbon processing industry and not many operators would be happy to repeat a full hydrostatic test after a period of service. This is a problem that deserves study by the steel industry, bearing in mind that the quality of a material is best measured by how it behaves in service and not merely by its ability to pass the standard specification tests.

In presenting this viewpoint, I have been concerned to give examples of how the metallurgical profession can contribute to safety and reliability in process plants. In the future, both user requirements and government legislation will call for higher standards in this field, and it behoves metallurgists to anticipate such requirements and to be in a position to offer the improvement in both education and in materials that the development of our new-found resources will demand.

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

The theme of the conference implies cooperative efforts by metallurgists and engineers. I think we should explore the avenues of cooperation in a little more detail. Just as there are metallurgists who are involved in research, or teaching or testing, or consulting, so there are engineers who are involved in various branches of their own discipline. I think it becomes necessary, therefore, to discuss also in more specific terms the stages at which metallurgists and engineers should be cooperatively involved and the scope of their association.

In my own branch of structural engineering, and particularly with reference to steel bridges, buildings and pipelines, experience has shown that for best results it is not only desirable, but also essential, for this cooperation to be established at the start of a project and to be continued through all its stages.

An early understanding by the metallurgist of the engineer's requirements for a particular structure with respect to strength, ductility, weldability and fatigue, will enable the metallurgist to advise the engineer on suitability and availability of steels for the particular purpose and to cooperate with him in the preparation of appropriate specifications for materials and workmanship, testing and standards of acceptance.

This early involvement of the metallurgist in the particular project provides him with the necessary background to enable him to act as adviser and consultant during later stages of fabrication, when inevitably metallurgical or welding problems arise which require expert attention.

Conversely, the anticipation of industry's future requirements enables metallurgists engaged in research and steel production to channel their efforts into productive fields.

I would suggest therefore, that as well as specialization and expertise in specific areas of metallurgy and engineering and cooperation between experts in these disciplines, we require broadly based cooperative efforts between metallurgists and engineers on a continuing basis for particular projects.

I am sure that this Rosenhain Centenary Conference will help greatly to achieve these objectives and would like to congratulate the organizers of the conference on their efforts.