EDITORS

B. L. Averbach

Associate Professor of Metallurgy
Massachusetts Institute of Technology

D. K. Felbeck

Executive Director, Committee on Ship Steel
National Academy of Sciences-National Research Council

G. T. Hahn

Research Associate, Metallurgy
Massachusetts Institute of Technology

D. A. Thomas

Assistant Professor of Metallurgy
Massachusetts Institute of Technology

Fracture

Proceedings of an international conference on the atomic mechanisms of fracture held in Swampscott, Massachusetts, April 12–16, 1959.

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Foreword

The principal ideas for the organization and conduct of this Conference on Fracture were developed at a meeting of a small group at M.I.T. in October 1957. Included in this group were research workers participating in the fracture research activities of the Committee on Ship Steel and the Materials Advisory Board of the National Academy of Sciences-National Research Council. Several industrial and academic organizations were also represented.

It seemed evident that theories describing the mechanisms of fracture on an atomic scale had come to a point where a conference in which experimentalists and theorists could come face to face would be fruitful. There had been considerable success in the application of dislocation models to the problems of plastic flow, and it appeared that the neglected field of fracture could benefit from a consolidation of the defect models proposed for various types of fracture. The *ad hoc* planning group broadened the scope of the Conference by including fracture phenomena in metals, ceramics, and polymers, and the mechanisms operating in cleavage, fatigue, ductile, and high-temperature fractures. At the same time the scope of the Conference was limited by excluding the large and important field of engineering fracture mechanics on the basis that this topic could best be considered at a separate conference.

At the request of the preliminary planning group, the National Academy of Sciences-National Research Council, through its Division of Engineering and Industrial Research, established a Committee for the Conference on Fracture with the following membership:

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Generous financial support was obtained from the National Science Foundation, the Office of Naval Research, the Air Force Office of Scientific Research, and the Ship Structure Committee.

The Conference Committee invited twenty-seven papers on various aspects of the atomic mechanisms of fracture. These papers were preprinted and available about a month before the Conference was held.

The Conference was held in two stages at the New Ocean House in Swampscott, Massachusetts. During the period April 12–14, the formal invited papers presented in this volume were read before an audience of approximately four hundred people at sessions open to the scientific public. During the period April 15–16, invited conferees met as a Fracture Research Planning Conference to consider the current status of the theoretical and experimental problems that had been presented during the public sessions of the Conference. The membership of this group is listed at the end of the foreword, and the summarizing reports of the chairman of each of the Conference meetings constitute the first chapter of this volume.

This Conference could certainly not have taken place without the unstinting co-operation of the authors and the other members of the Conference Committee. The Committee and conferees are particularly indebted to D. K. Felbeck, who served as Secretary, and his assistant,

R. W. Rumke, on whom the greatest portion of the burden fell. They managed most of the organizational details and were largely instrumental in preparing the volume of preprinted papers which helped to make this Conference successful. In addition, I must thank my editorial colleagues for their arduous labor in editing the papers and discussions. Without their assistance it is doubtful that this book could have been published in such a short time after the end of the meeting.

B. L. AVERBACH, *Chairman*
Committee for Conference on Fracture
National Academy of Sciences-National
Research Council

Cambridge, Massachusetts
April 21, 1959

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W. A. Wood	*University of Melbourne, Australia*
C. Zener	*Westinghouse Research Laboratories, Pittsburgh, Pennsylvania*

Contents

I. Summary of Current Status and Needs for Future Research

SESSION ON CLEAVAGE FRACTURE

C. S. BARRETT, Chairman
University of Chicago

In this report of the discussion session on cleavage fracture, which has a condensation ratio of about 10 to 1, much of the divergence in opinion among those present tends to be lost. Therefore, let it be stated at the outset that divergence exists, that it is normal among workers in any complex field, and that it extends from the question of whether some stress criterion, some strain or other criterion is fundamental to cleavage all the way to the question of whether fundamental research planning should ever be attempted by committees.

One question is always raised in any conference on fracture: What is wrong with the classical theory which states that brittle fracture will occur if the flow curve intersects a curve representing the critical normal stress for fracture? On the one side of the intersection of the two curves, the yield-stress curve is lower, and therefore plastic deformation occurs; on the other side of the intersection, the fracture stress is lower, and therefore fracture occurs. If it is objected that the fracture stress is a function itself of the amount of plastic flow that has occurred, it is argued that an amplification factor might be applied so that the fracture-stress curve does depend, through the amplification factor, on the amount of prior plastic flow.

It was at the height of the popularity of this concept that Zener proposed the idea of dislocation pile-up at a boundary, which started the succession of theoretical developments to which Mott, Stroh, Cottrell, and others have contributed. These theories reached their highest point in popularity thus far at the present Conference. Orowan asked again, "What is wrong with the classical theory?" and was answered that the classical theory takes only an arbitrary parameter, namely, the fracture stress, and says that, by some mystery, the metal breaks when that stress is reached. A more fundamental understanding cannot be reached in that direction. The atomic mechanisms involved, which must lie at the heart of the true understanding, are bypassed. There is a possibility that the newer theories can lead to quantitative predictions of behavior by starting with fundamental mechanisms and independently measurable material constants. The road is long, dark, and treacherous, but there is hope.

Much of the discussion surrounding current theories of the brittle fracture of metals assumes that there must be a crack forming within a grain (presuming that grain-boundary embrittlement is not involved). The crack supposedly forms by some process connected with glide, since most observers agree that plastic flow, at least in small amounts, precedes the first cracks. It should be possible to understand the one or more mechanisms by which the dislocations causing plastic flow are converted into the cavity dislocations of the crack. The crack nucleus grows to a critical size, and if it exceeds this size, it proceeds onward to a barrier, typically a grain boundary. Now the effective surface energy associated with a unit area of the crack at this point we shall call γ_1, which includes a quantity γ_0, which is the true surface energy of the crack, and the plastic-deformation energy in the nearby surroundings. In mild steel, γ_1 is of the order of 10^4 ergs/cm^2. A second stage occurs when the crack, having reached a first major barrier (typically the first grain boundary), enters the next grain. An effective surface energy γ_2, greater than γ_1, is now involved in subsequent stages. This is of the order of 10^5 or 10^6 ergs/cm^2. Current theories employ a Griffith-type equation and envisage the critical stress for fracture as being proportional to $(E\gamma/c)^{1/2}$, where E is an elastic constant and c is a length parameter such as the crack length.

At ordinary temperatures, the propagation past the first boundary controls the brittle crack behavior; microcracks within individual grains, if formed, do not spread. At lower temperatures where yielding and cracking occur together, the cracking within a grain controls the behavior, for once the first microcrack of critical size forms, it readily propagates through the entire piece. There is a region, however, in which behavior is rather complex and particularly sensitive to metallurgical variables, even in a simple tensile test of steel at low temperatures. In Fig. 1

(from the work at M.I.T.), this region is the one between T_d (the ductility transition) and T_m (the lower limit of temperature at which the microcracks that did not propagate can be seen). At all temperatures above T_m, brittle fracture is controlled by the value of γ_2. Below T_m, the

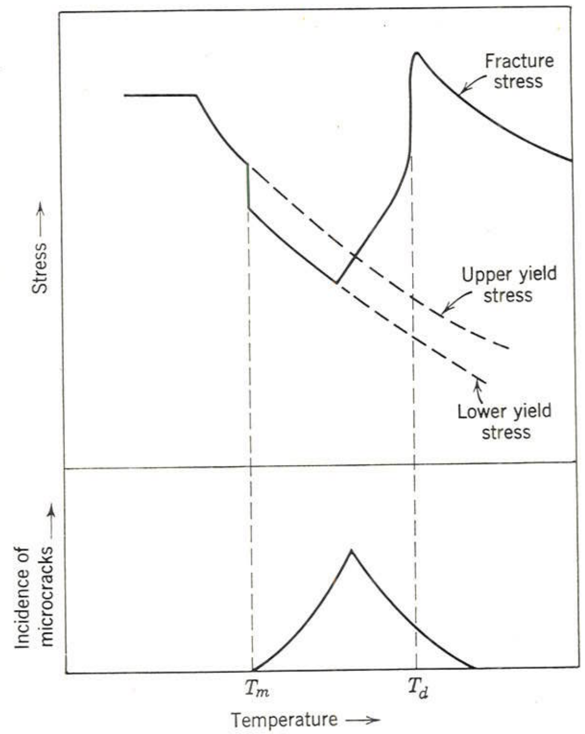

Fig. 1. Schematic representation of variation of fracture stress and incidence of microcracks with temperature for a rimmed steel.

controlling variables are those that permit nucleation of the first crack and its spreading to the first major boundary. At the very lowest temperatures, it appears that twinning is the initiating event (though a few single-crystal experiments on iron may present exceptions).

Below a certain range of temperature, twinning occurs and is increasingly frequent with further reduction in temperature. In some materials and some temperature ranges, it is extremely important; in others, it may not be. It appears that twinning itself requires a critical stress, and this stress is different from the critical stress for the other atomic processes leading to the initiation of a crack and is a different function of temperature.

It is apparent, therefore, that the mechanisms that are controlling the behavior in each temperature range, as well as the various ranges of temperature in which these mechanisms occur, must be explored with care, probably in more detail than has been done thus far. The temperature ranges may shift in many ways with respect to each other, and often it will require experiments spaced closely together in temperature in the neighborhood of T_m and T_d to place these various ranges properly for a given material.

It has long been known that twinning frequently accompanies the spread of a brittle crack in ferrite. It is now clear that twins frequently have small cracks along their interface with the matrix, at least in ferrite, and examples are known of intersecting twins causing crack initiation. It is a matter for determination in each material and temperature range, however, whether or not twins are actually responsible for the initiation of the brittle fracture. It now appears that there are several ultimate mechanisms other than twinning that under some circumstances can initiate fracture. In materials that twin, there must be a very close balance between the stress requirements for twinning and for these alternative mechanisms. The challenging problem, therefore, is to unravel the effects of metallurgical variables on the twinning stress and on the critical stress required by the other mechanisms; a still more challenging problem is to understand why metallurgical variables affect these mechanisms as they do.

Current evidence indicates that the effective surface energy for steel varies from 10^3 or 10^4 ergs/cm^2 in the early stages of the crack at low temperatures and up to 10^6 or 10^7 at higher temperatures. Here is a major variable that appears to be at the heart of the brittle-fracture problem, yet we do not know how to deduce either γ_1 or γ_2 theoretically. We would like to know much more about these effective surface energies as a function of metallurgical variables. We would like a truly independent method of obtaining this effective surface energy, provided it gives results more precise than an order of magnitude. To be sure, the effective γ can be evaluated from experiments by assuming the Griffith crack formula, but it is much more important to have an independent method and one of better accuracy than we now have, a method that does not treat γ as a disposable parameter. There is perhaps a possibility that one may be able to calculate γ_1 or γ_2 from first principles, but if so, we are warned that the calculations can hardly be valid unless they take into account the shape of the crack. There are several factors that blunt the crack, among which are plastic relaxation in front of the crack, which is surely dependent upon the speed of the crack, at least at moderate temperatures, and possibly, the effect of crossing grain boundaries. Both theory and experiment should pay attention to the segregation of impurities at grain boundaries and the role of these impurities in the fracture process. In some experiments there is danger that changing the grain size will change the chemical constitution of the boundaries and thus change the effects of the boundaries on crack propagation. Friedel suggests that, since there are microcracks present near T_d of Fig. 1, the critical question is whether they start propagating or they start widening without propagating. Perhaps T_d should be viewed as the temperature above which they widen without propagating; at T_d the applied stress is sufficiently high with respect to the yield stress that plastic relaxation and widening can occur.

The question is raised by Cottrell as to why, at the upper limit of the notch brittle range of steel, γ_2 seems to be of the order of 10^6 ergs/cm^2, when, in the case of the brittle cracking of a large structure containing a long crack, enough energy seems to be present to make the crack run with γ_2 much larger than this, as it is, for instance, at higher temperatures. In discussing this question, Crussard called attention to an observation that there seems to be a constant state of stress, independent of the distance a crack has propagated, that runs along at the tip of the crack, that is, a sort of steady state moving along at the edge of the crack. (In another connection, Irwin pointed out that photoelastic studies show how the stress field around a crack of increasing length gradually spreads to larger distances; however, this was in a brittle transparent plastic, not a polycrystalline metal.) It is possible that the depth of the cold-worked metal along the propagating crack is related to the grain size of the material and that the grain size therefore limits the plastic work accompanying propagation.

In discussing the relative values of γ_1 and γ_2, Zener makes an interesting suggestion. In the lower temperature range, γ_2 does not seem to control the propagation of a crack; that is, once the crack starts in the first grain, it propagates into the adjacent grains, and the controlling variable is γ_1. It appears as if γ_2 has been lowered to the value of γ_1, since the fracture stress is actually lower in the lower temperature range than in the upper. The difference between γ_1 and γ_2 is presumed to be chiefly the energy involved in tearing the specimen between adjacent cleavage planes that are not coplanar (the tearing producing the river markings). The tearing process involves plastic deformation. The tentative interpretation of the decrease in γ_2 at low temperatures is that the shearing involved in this tearing process becomes adiabatic. The adiabaticity results in a localization of the shear in these tears, and this reduces the required energy to tear. The adiabatic effect should be enhanced at low temperatures because the specific heat is lower at these temperatures, with the result that a given amount of heat has a greater effect on the temperature of the metal. Also, the stress-strain curve at low temperatures is higher, and therefore the heat produced by a given strain in the locality is greater. This concept of adiabatic localization of plastic flow in the tearing process might be subjected to experimental test through studies of the appearance of the tearing surfaces above and below the critical temperature.

Returning to the atomic mechanisms for initiating a brittle crack, what processes other than twinning should be kept in mind? Let us consider Cottrell atmospheres around dislocations, which inhibit slip so that suitable dislocation pile-up can occur. This mechanism for pinning dislocations has been questioned in cases where microcracks have not been seen until after the Lüders front has passed and general plastic flow, which presumably pulls many dislocations from their atmospheres, has occurred. In such cases, do atmospheres no longer play a role? They may still be important, it would seem, if a few critical dislocation sources at critical positions are still pinned, for this permits pile-ups large enough to initiate a crack. One cannot expect every dislocation to be unpinned simultaneously as plastic flow continues. However, other methods of suppressing plastic flow at such critically stressed regions are possible. When multiple glide occurs in a grain (perhaps somewhat behind the Lüders front), dislocation interaction can cause plastic flow to stop. In semibrittle fracture, therefore, it might be said that dislocation locking by this mechanism can replace atmosphere locking. Cottrell has emphasized that what is needed for crack initiation is a falling stress-strain curve, which will provide plenty of dislocations in an avalanche and prevent relaxation from taking place in front of the crack, as discussed by Friedel. Dislocations can be brought together suddenly during the yield drop (whether or not this is caused by deformation twinning).

This brings up the question of the role of dislocation avalanches in the brittle-fracture problem. The general opinion is that more research is needed on this subject. Preyield noises have been heard; a noise suggests an avalanche. Identification of the noises as micro-Lüders lines, twinning, dislocation avalanches, adiabatically localized flow, Cottrell-Lomer lock dissociation, or any other micromechanism should be attempted in view of the importance attached to these mechanisms in brittle-fracture behavior. Current theory regarding cubic metals suggests that without avalanches there would be no brittle cracks, but some investigators wonder if this has been firmly established.

The question was raised whether in iron and steel the intersection of slip bands populated with many dislocations, which is the dislocation interaction found to be so important in magnesium oxide, also occurs.

If this is an important type of interaction in iron and steel, how does it happen that extensive pile-ups have not yet been identified in these materials? It has been maintained that, since grain-size effects are still present in flow after the lower yield point elongation is completed, some pile-up or locking mechanism must still be important. Gilman's work with lithium fluoride suggests that a mechanism of dislocation multiplication that produces avalanches consists of the cross slipping of segments of the first gliding dislocations. As these segments cross slip out of the original slip plane, they become potential sources of further dislocation loops.

Anderson pointed out that it was evident from the whole tone of the Conference that the Griffith criterion is here to stay; the question is, how quantitative can it be made? Can it account for the dynamic effects in fracture propagation and how well? In applying the Griffith hypothesis to the speed of propagation of a fast-moving crack in glass and considering the assumptions that go into derivation of the fracture velocity, the Griffith hypothesis is handsomely verified to a first approximation. As to the Griffith formula for slow-moving cracks, one must look at time effects. The surface energy may change with time or the modulus; or it may be that reactions at the surface produce a weaker material or a changed surface energy. Any or all of these things can account for time effects. However, to go from a first approximation theory to a second-order approximation theory, one would have to find experiments that would distinguish between these various mechanisms. A fully developed theory should also be able to predict from first principles the variation of crack velocity with temperature. There is conflicting experimental evidence in this realm.

Irwin points out that in different plastics the effective surface energy of a crack is affected to different degrees by the noncoplanar crack origins that must join each other as a crack spreads. Cases can be cited in which the effective energy is a much smaller multiple of γ_0 than is the case in metals.

Considerable work has been done in glass fibers with regard to size effects. Whether a size effect in the strength of fibers is observed or not seems to depend critically on precisely the way the fibers are drawn. By changing the drawing conditions slightly, one can vary the strengths greatly. The density of flaws varies from one fiber to another, a fact which makes it difficult or impossible to draw conclusions from a statistical analysis of fibers of different size. Very recently, it appears, the flaws themselves have been seen — they seem to be about 1000 Å deep, 200 Å wide, and 50 μ long.

In summary, the Griffith flaw theory seems perfectly satisfactory as a first approximation, but much work must be done before it can become more quantitative. Statistical analysis of the effect of indenters on glass plates and attempts to interpret these in terms of the distribution of flaws run into the disturbing and confusing fact that the deviation in the measurements does not change with the size of the indenter in spite of the fact that it should.

D. S. Wood illustrated the usefulness of microscopic continuum mechanics connected with fracture by mentioning his work done with Hendrickson and Clark on elastic-plastic stress analysis in notched specimens. With a knowledge of the yield stress at different temperatures, this analysis indicated that brittle fracture was initiated if, and only if, a certain critical tensile stress was reached in the material before plastic instability occurred. Furthermore, this critical fracture stress was independent of the temperature and rate of loading; that is, temperature and rate of loading influence the initiation entirely through the variation of the yield stress with temperature and rate of loading.

However, a discrepancy appears if one takes the same steel in the same metallurgical condition and makes an unnotched tensile bar of it; the stress that produces brittle fracture at liquid nitrogen temperature is much smaller than the maximum stress calculated for the notched-bar specimen. Wood suggests the possibility that the value of the peak stress calculated for the notched bar (a stress existing over a small volume near the root of the notch) may not be the correct criterion for the initiation of the fracture. Perhaps the criterion must involve the average stress existing over a volume that contains a certain minimum number of grains. Despite this particular discrepancy, the application of a similar analysis to the problem of the transition temperature in the Izod Impact Test was quite successful, and the prediction of the transition temperature was close to the observed one.

Wood also questioned whether the values of any of the stresses that are obtained in tensile tests at low temperatures are physically significant. At these temperatures, where fracture is initiated while a Lüders band is starting and propagating across the cross section, there is a very inhomogeneous distribution of plastic strain. The stress distribution may be anything but simple, and the true stress at the point where the brittle fracture starts may be quite different from the applied load divided by the area of the specimen.

In the case of the fast propagation of brittle cracks or semibrittle cracks, the characteristics of the plastic waves originating in the region around the tip of the fast-moving crack govern the amount of energy absorbed in plastic flow and hence, γ_2. For example, materials that have low rates of work hardening tend to have low velocities of plastic wave propagation and low γ_2.

SESSION ON FATIGUE AND DUCTILE FRACTURE

R. W. K. HONEYCOMBE, Chairman

Department of Metallurgy
University of Sheffield, England

There seems general agreement that in all but special cases fatigue damage commences at or near the surface of the specimen regardless of whether the specimen is stressed by bending, by push-pull, or by some other means. The relatively few experiments that have been done on surface removal show quite conclusively that during the greater part of the fatigue test such treatment enhances the life of the material. Now that more detailed microscopic observations on the depth of penetration of surface damage have proved fruitful, it seems desirable to carry out further work on effect of surface removal on fatigue life, paying particular attention to the stage of the process at which surface removal is carried out and determining the exact depth of material that is removed. Annealing during the fatigue test is much less effective in prolonging fatigue life, but further examination of this point is needed. For example, annealing should have a big effect in the first 5 to 10% of fatigue life, but this effect should diminish beyond this point if current ideas about the onset of permanent damage in the form of fissures are correct.

During the Conference, much interest centered around extrusions and intrusions, which, it has become realized, are very widespread in their occurrence during fatigue. The general opinion seemed to be that extrusions and intrusions should occur in most cases where fatigue failure eventually takes place. However, this point needs further attention, particularly in materials that exhibit a fatigue limit; the microstructural behavior below and above this limit should be carefully examined. Taper-section micrographs suggest that the fissures are closely related to the intrusions and extrusions and that the fatigue cracks originate from the fissures, although the subsequent path is not fully understood. This work is clearly very valuable, and much more should be done over a wide range of testing conditions, preferably on single-crystal material. A more continuous record of the state of the surface could be obtained by the application of precision interferometric methods which should give a direct quantitative idea of the development of extrusions and to a lesser degree, fissures. This work should be carefully correlated with the results of surface removal experiments.

While much time was spent on assessing whether these phenomena in the vicinity of the surface were directly responsible for the initiation and propagation of a fatigue crack, a great deal of attention was given to various possible mechanisms for the formation of extrusions and intrusions. At least six mechanisms were examined, some of which are dealt with in the papers, while others are treated in the discussions. The majority of these models require special features which should be looked for by experiment; for example, cross slip is needed in some cases, while another mechanism requires slip on two systems in the vicinity of the surface. Mechanisms that give only extrusions are clearly inadequate in the light of evidence presented at the Conference and elsewhere. While it is unlikely that only one mechanism is responsible for all the defects observed, it should be remembered that extrusions have been detected during fatigue at 4°K, a temperature at which processes requiring thermal activation are not possible. There was some lack of agreement on whether extrusions can occur suddenly and whether they are randomly distributed across metal grains. These points and others could be cleared up by application of metallographic and interferometric techniques. Mechanisms could be noted by comparative examination of, for example, zinc and lead crystals, aluminum and magnesium, or even by examination of one metal at different temperatures.

It was mentioned that pits, the origin of which is as yet unknown, also occur along the slip striae. In view of the observations that cavities develop during ductile fracture, study of this phenomenon may help to link the two types of fracture.

There was some discussion of whether cracks could nucleate below the surfaee of a material. More work is needed on this point, for the only cases of subsurface nucleation appear to be in macroscopically inhomogeneous materials, such as nitrided and carburized steels. While focusing of sound waves seems impracticable as a means of causing internal cracking, it may be possible, by means of stresses set up by heating and cooling anisotropic materials, to achieve subsurface nucleation of cracks in the absence of singularities of the material. It is felt that deformation commences and continues in the vicinity of the surface because the effective source lengths are longer. Experiments of the type where solute is diffused into the surface of single crystals that are subsequently deformed in tension have shown that the deformation still commences in the surface region. It would be very interesting to carry out similar experiments in fatigue through selection of solute metals that will harden the surface of the specimen substantially.

Attention was drawn to recent Russian work on the effect of environment on fatigue of steels. There is a reduction in fatigue life in the presence of long organic molecules and even with cutting oils. This is

clearly of technological importance, but it is felt that at the same time experiments should be done on systems where the possible mechanisms can be more easily explored. For example, effect of environment on the fatigue of single crystals would provide interesting information. There is already much evidence that atmospheres play an important role in fatigue behavior. Fatigue in inert liquids would minimize this difficulty, and the effect of active additions could then be examined. At the same time, comments on chemical contamination made during the Conference lead to the view that fatigue experiments, performed in very high vacuum on materials of extreme chemical cleanliness (exclusion of gases and grease), should be attempted.

The foregoing remarks suggest that the emphasis in the immediate future should be on metallographic features of fatigue closely correlated with mechanical behavior. There seems little doubt that the methods now available to us should allow us to test some of the theories, particularly of extrusions and intrusions, put forward at the Conference. However, there are many variables in fatigue that should be given individual attention in any such studies. They are the following:

1. *Temperature.* Tests at normal temperatures should be carefully compared to tests at very low temperatures on the same material.

2. *Strain level.* Work discussed at the Conference showed that damage at large strain amplitudes can be very different from that occurring at small strain amplitudes.

3. *Frequency.* This may have considerable bearing on the development of extrusions. There is already some evidence that in certain circumstances extrusions develop extremely rapidly at some stage during the test.

4. *Environment.* This includes atmospheres and possible liquid media that probably do not alter basic processes but certainly influence the rate at which failure occurs.

5. *Variation of stress program.* Most of the structural observations have been made on specimens of fatigue at constant stress or strain amplitudes. The structural effects of overstressing and "coaxing" need much more examination. Macroscopic measurements such as the energy absorbed during a cycle at various stages of the tests are extremely useful in this connection and should be correlated with the structural changes.

6. *The fatigue limit.* It should be definitely established whether extrusions and intrusions occur below the fatigue limit in those materials that exhibit one. If they do, then the reasons why fatigue cracks do not form and propagate must be examined.

7. *Crack propagation.* During the Conference, attention was usually fixed on crack initiation. The propagation of a crack is equally, if not more important, and much attention should be given in the future to

the condition under which cracks become nonpropagating in specimens subjected to uniform macroscopic stresses.

One final general comment should perhaps be made. Now that careful structural observations have been shown to be very useful and some correlations are being attempted, it would seem advisable to carry out more work on single crystals where the stress conditions can be more accurately determined and the slip reactions more closely worked out. At the same time, the need for detailed information on dislocation movement and interactions should encourage work on fatigue within thin metal films, which can be subjected to electron microscopic examination, and on materials where etching or other techniques reveal the dislocation distribution at different stages during the fatigue test.

It is perhaps a measure of our slow progress in the fundamental solution of the fatigue problem that we are still largely concerned with the behavior of pure metals. In the near future, we must also extend fundamental experiments to the study of simple alloys, to determine whether, for example, elements in solid solution have any profound effect on fatigue behavior. This would logically lead to the study of more complicated systems in which different phase distributions occur. While we are beginning to understand some of the principles involved in the development of creep resistance, we are still a long way behind in determining the factors that promote fatigue resistance in alloys.

Knowledge of ductile fracture is still very limited; however, several new ideas and facts came out during the Conference. Microstructural examination of the neck just prior to cup-and-cone fracture has revealed that one of the main features of the central fibrous zone is the formation of pores which gradually open up and result in a type of rupture that cannot strictly be described as a propagating crack. The view has been put forward that these pores nucleate at impurity particles almost always present in ordinary metals and alloys. This explanation is supported by the fact that the purer a metal becomes, the greater is the reduction in area in the metal prior to ductile fracture; this leads to the assumption that all metals, if very pure, will not show a ductile crack but will merely rupture with 100% reduction in area. While this may be true of pure metals, there is some doubt as to whether it would hold for a hard solid-solution alloy free from second-phase impurities. The ductile-fracture characteristics of such alloys will provide a useful field for study.

Alternatively, the cavities could be nucleated from small cracks formed from piled-up dislocations. The difficulty here is that pile-ups have not been observed in metals such as copper and aluminum. However, this does not exclude the possibility that they are present in heavily worked regions of the neck or that they occur in alloy materials. Some difficulty

was encountered during the Conference in defining precisely what a ductile crack is and how it can be distinguished from a brittle crack. It is obvious that the crack must form after substantial plastic deformation, but in addition, it seems that the material in front of the crack must first be plastically deformed so that a fracture criterion can be fulfilled. Work on alloy single crystals indicates that the resolved shear stress on the slip plane is a criterion for initiation of a ductile crack. Such work suggests that cavities are not important within individual grains and that a definite crack propagation occurs independent of the cavities. It would be useful to carry out some ductile-fracture experiments with fairly coarse-grained material to determine whether the cavities are formed primarily within the grains or if they are formed when dislocations pile up against the grain boundaries.

Work reported at the Conference showed that a study of fracture surfaces using the electron microscope was very useful in determining how the voids formed in polycrystalline metals were distorted in the subsequent propagation of the ductile fracture. Furthermore, information was reported on the final stage of ductile failure, namely, the formation of the cone which, it has been suggested, is a part of intense localized plastic shear where the material is softened by the heat of plastic working. However, the geometry of the cone does not allow a simple sliding off of the metal. The surfaces must also move away from each other by a process that appears to be the reverse of the adhesion phenomenon occurring when two metal surfaces slide over each other.

The ductile behavior of a tensile specimen is very dependent on the orientation of the specimen relative to the bar from which it is machined. While a specimen taken parallel to the direction of working may give a normal cup-and-cone fracture, a specimen taken at right angles to this will give a ductile fibrous fracture, often with very little necking. This appears to arise from the anisotropic distribution of small, second-phase impurities in the material. These are normally drawn out in the direction of fabrication and will usually lie parallel to the tension axis; as has already been pointed out, these particles can initiate ductile failure by causing the formation of pores. However, if the orientation at right angles is taken, then the stringers run across the specimen normal to the tension axis; in this direction, tiny channels develop which grow together by plastic deformation. Little or no necking occurs, and no hydrostatic stress is set up in the sample. Such differences result in variations of as much as 50% in the fracture stress, while the total strain to fracture may vary to an even greater degree. When the fibers run normal to the tension axis, the fracture process begins very soon after yielding.

Grain size is another important microstructural variable. It has been shown that the fracture stress increases linearly with the inverse square root of the grain diameter, when the fracture stress is taken to mean the peak stress just prior to ductile failure. Here again, the slope of the curve depends on the orientation of the specimen relative to the direction of working; in mild-steel plate, the slope is three times greater in the rolling direction than in the thickness direction.

To summarize:

1. The main problem at present in ductile fracture appears to be whether inclusions or second-phase distributions are responsible for the propagation of a ductile crack. Experiments on very clean metals are needed to determine whether, in the absence of such impurities, 100% reduction in area occurs in the necked zone. If this were the case, the rupture would then be caused simply by prolonged displacement on the slip planes and possibly, grain-boundary shear. Experiments should not be limited to pure metals, but should include solid-solution alloys, which may have less intrinsic ductility.

2. The transition from single-crystal fracture to polycrystalline fracture should be studied using very coarse-grained materials that may reveal in more detail the way in which voids are formed. By suitable selection of metal or alloy, it should be possible to distinguish between formation of voids at second-phase impurities, at grain boundaries, or by other mechanisms.

3. Further work on single crystals would be useful. In particular, tests at very low temperatures on solid-solution crystals free from second-phase impurities would be valuable. There is, for example, some evidence that, even in pure copper, a marked ductile fracture zone can occur with little necking at 4°K.

4. In all the above experiments, examination of the fracture surfaces should be made by means of fracture replicas, for they have already proved very informative. However, further work is needed on their interpretation.

5. Finally, the view that the cup-and-cone region is a part of intense localized plastic shear where the material is softened by adiabatic deformation should be closely examined. This could possibly be tested by fracturing a metallurgically unstable alloy such as an age-hardenable alloy in the solution-treated condition. There is some evidence that plastics may provide a better opportunity to demonstrate adiabatic heating in shear. It should, however, be borne in mind that the cup-and-cone fracture is only one type of a number of ductile-fracture phenomena, and therefore, it is not necessarily characteristic.

SESSION ON HIGH-TEMPERATURE FRACTURE

N. J. GRANT, Chairman

Department of Metallurgy

Massachusetts Institute of Technology

Even though the primary concern of this session of the Conference was with ductile, high-temperature fracture, the point was made that there is insufficient information regarding the initiation of fracture at the grain boundaries. A reasonable number of models for grain-boundary sliding have been proposed, and while some of these models are satisfactory in a few respects, none of them is as yet capable of predicting either the point of fracture or how it originates.

It is fairly obvious that blocking points must exist in a sliding grain boundary in order to provide sites for either the initiation of a crack, or for vacancy condensation, or for the combination of these two items to be effective. It is also quite certain that an unknown minimum amount of grain-boundary shear must take place to initiate fracture and cause it to propagate. Grain-boundary migration tends to diminish or eliminate intercrystalline cracking, whereas subgrain formation tends to minimize the degree or amount of such cracking.

There was considerable discussion regarding the significance of the measurements made by Rachinger of the contribution of grain-boundary sliding to total deformation. Because Rachinger's results indicate that the contribution of grain-boundary sliding in the interior of the specimen to the total elongation may be of the order of 95%, whereas surface measurements indicate a contribution of the order of 10 to 20%, questions were raised, not only about the experimental method used in making the measurements, but also about the interpretation of the measurements.

It appears difficult to reconcile these large differences in grain-boundary elongation to total strain without some evidence of major structural changes in grain size or grain shape. Thus the question also arose as to the significance of grain geometry on measurements of grain-boundary shear. Could grain growth, either preferentially or volumetrically, be interpreted incorrectly as contributing to grain-boundary deformation? If one were to suppose that the surface measurements and internal measurements were both correct, would this not indicate that there ought to be a very significant difference in grain size or grain shape on the surface of the specimen in contrast to that noted in the interior of the specimen? Such differences in grain size and shape have not been reported by most of the investigators.

Some question was also raised as to why such exceedingly large values of grain-boundary contribution to total strain have been reported for a number of the hexagonal metals. There appears to be a need to determine whether these large grain-boundary contributions are the result of preferred orientations and grain geometries, or whether the grain boundaries in hexagonal metals actually contribute more to the total elongation than is possible in the cubic systems.

Everyone seemed quite satisfied that there are adequate vacancies present to permit vacancy condensation (whether or not the crack was initiated by such condensation) and that such vacancies can diffuse at exceedingly high rates to preferred sites to permit growth or adjustment of cracks by further vacancy condensation.

Crussard discussed some of the interesting aspects of the structure of the grain boundary, particularly when the metal is not of exceedingly high purity. Photomicrographs were presented which showed the distribution of carbide precipitates and their effect on grain boundaries and fracture, and of thin oxide films which had a major effect on fracture. The work described by Crussard was concerned with low-temperature fractures, however, which are easier to examine than are those produced at high temperatures. In the case of intercrystalline fractures that are produced at high temperatures, the heavy deformation accompanying the fracture results in recrystallization. This makes it impossible to determine whether the fracture face resides in either grain, has facets of both grains, or is of a composite nature involving the grain boundary and one of the grains. Laue back-reflection photographs made on the surfaces of high-temperature fractures inevitably show complete Debye rings, which thus indicates a finely recrystallized structure that prevents the identification of the grain in which the fracture might have occurred. Considerable emphasis was placed on the importance of the form and amount of the precipitate along the grain boundaries, since this would have a large effect on the nature and rate of cracking.

Experimental techniques that aim at providing more information about the intercrystalline fracture path are needed. It is possible that intercrystalline fractures that have been initiated at high temperatures but completed at very much lower temperatures might offer a clue regarding the position of the fracture with respect to the two grains that constitute the grain boundary.

The role of the atmosphere in high-temperature testing deserves much more attention than it has received in the past. While it has often been thought that a strongly oxidizing atmosphere might be damaging to the rupture life, creep rate, and ductility of a specimen, recent tests with an 80% nickel–20% chromium alloy indicate that the role of an oxidizing atmosphere might be much more beneficial than damaging in most instances. Tests on such an alloy, performed in air and in purified argon at 1500° and 1800°F, show that the creep curves are coincident with each other up to the point of the initiation of the first intercrystalline cracks; after this, the specimen tested in air undergoes internal oxidation and nitrogen solution at the crack interfaces and therefore yields a considerably stronger structure in this area. Because the fracture process, as well as the deformation process, is cyclic on a microscale, a crack is expected to stop growing after a period of time until the stresses are redistributed throughout the specimen. At some later time, owing to stress redistribution and thermal annealing, the cracks may continue to grow. During the period of time when the crack has stopped growing, however, oxygen is dissolved into the structure at the newly formed interfaces and produces chromium oxides or nickel-chromium spinels which strengthen the metal in the vicinity of the crack and thereby prevent or delay further propagation of the crack. As a result, the life of the specimen tested in air may increase by a factor of 10 or more; and the ductility, instead of suffering, may improve by a factor of 2 or 3 compared to the test in the argon atmosphere.

These results, as well as other recent data that have appeared in the literature, indicate the need for greater attention to the test atmosphere, for it is obvious that the effects of the atmosphere can be exceedingly large, depending on the temperature and the rupture time for the test.

Machlin brought up the interesting relationship, reported by several different investigators, between rupture time and minimum creep rate. In its simplest form, the relationship takes the form: rupture time $\times$ (minimum creep rate)$^m = K$. It appears that this relationship holds for tests leading to either ductile intercrystalline or transcrystalline fracture behavior. The value of the exponent m is sometimes equal to 1.0 but actually varies from about 0.8 to 1.2 for a large variety of metals and alloys.

The product of rupture time and minimum creep rate is a constant of unknown significance but may be related in a complex way to the ductility of the metal or alloy under the test conditions. Considerable work has recently been initiated to determine in what way K varies for different classes of materials and for different temperatures.

It has been shown, in fact, that if one separates the data for tests in which ductile transgranular fractures are observed from data for tests in which ductile intercrystalline fractures are observed, one then finds that all of the test points fit into a narrow band on a plot of log rupture time versus log minimum creep rate. In view of the fact that this relationship holds over as many as seven to ten logarithmic cycles of either rupture time or creep rate, a spread of values within the data band no greater than ± 0.5 logarithmic cycle of time or creep rate is not disturbing. Closer examination of the test points within the band for any one temperature indicates that the tests that yielded higher ductility values tend to fall on the high side of the band, whereas the test points with lower ductilities tend to fall on the lower side of the band. If, for any reason, unusual embrittlement occurs during the course of the test, one can expect such points not only to fall on the lower side of the band but possibly to fall below the band. The correct determination of m permits calculation of K. These values of K are not only proportional to measured ductility values in creep rupture tests, but often they are good approximations of the actual values.

These observations lead Cottrell to the thought that although stress and temperature affect the rate at which flow processes go on, the fracture process is set up by a prefixed geometry within the grain boundary. This geometry is concerned with inclusions and other phases that lead to a certain amount of necking around each particle. Such necking can then lead to the dimples observed in other ductile fractures, followed by coalescence of the dimples, which leads to fracture. Such behavior might suggest an exhaustion type of concept for available deformation prior to fracture. Since there is either no work hardening or a slight amount of work hardening that is recoverable in the area of the local necked regions, the grain boundary is plastically unstable, and fracture could progress quite rapidly.

Some evidence was presented regarding extreme embrittlement which takes place in refractory metals at or near the solidus temperature. It was not determined whether there exists a quite different type of deformation at these exceedingly high temperatures, or whether this is a question of the need for a more precise determination of the solidus temperature. Interest in embrittlement at these exceedingly high temperatures would be motivated by the desire to know more about what takes place at forging temperatures when one begins to approach the solidus temperature.

Deformation and fracture studies of polymers and plastics have indicated some interesting relationships between the structure and the degree of embrittlement that can occur. Subtle changes in structure produced either by temperature or by time-and-temperature effects have led to vastly different fracture phenomena. Theory and experimental results are apparently in good agreement regarding fracture. While a dislocation picture has not been applicable, it has not been determined whether

the Griffith crack concept can be useful in explaining fractures in polymers and plastics. The size of a Griffith crack in the case of plastics would be hard to define but might conceivably be no larger than the break in a bond in any one molecular chain. Heterogeneous structures involving fillers in which the fillers can vary in size, shape, and amount have not received nearly as much attention as plastics, and the deformation and fracture characteristics are not as readily definable or predictable, based on the current state of knowledge.

In the over-all area of high-temperature fracture, it is quite obvious that most attention has been paid to single crystals, bicrystals, and coarse-grained structures in pure metals. Considerable engineering work has been done with the more complex commercial materials, but little effort has been made to apply theoretical concepts to deformation and fracture in such instances. Fracture in somewhat more complex structures than pure metals has received exceedingly little attention. There is undoubtedly great need for further studies with polycrystalline materials, and especially with multiphase structures, to determine how the fracture characteristics would differ in these more brittle structures from what has been observed in the relatively ductile, high-purity metals. The ability to pin a grain boundary more effectively should be of great experimental value, for it will permit studies of fracture under conditions where extensive grain-boundary migration cannot take place.