Acknowledgment

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DISCUSSION

U. F. Kocks[2] and H. Mecking[2]

The recovery creep model developed by Lagneborg and his associates over the years is by far the most detailed available today, and has some very attractive features. As may perhaps be expected of a treatment that breaks new ground, it is, however, non-unique in its predictions at many stages in the derivation. Some of these have been pointed out by the authors, others not. We wish to illustrate this point by a number of examples: their intent is not to propose a better theory, but rather to point out a certain degree of arbitrariness and inconsistency in various assumptions made by the authors.

1 The kinetics of dislocation network coarsening (equation (5)) is described by analogy to the kinetics of grain growth. The applicability of this analogy is not self-evident, for reasons over and above the difference in internal stress state pointed out by the authors. In grain growth, all nodes are supposed to be in equilibrium under the surface tensions; the ensuing curvatures of the boundaries provide a driving force *per unit area* for atomic transport across these boundaries, which leads to a coarsening rate

$$\frac{dl}{dt} \propto \frac{1}{l_{cr}} - \frac{1}{l} \qquad (a)$$

as used by the authors. The same law would hold for dislocation network coarsening only if the rate-controlling step were atomic transport away from (or to) *every element* of dislocation line. Since, however, at least some glide is possible in a dislocation network, the rate-controlling step may here be the displacement of nodes. No quantitative description of this process has yet been proposed.

2 The links in a dislocation network may be thought of as the edges of a set of space-filling polyhedra. The distribution of link lengths is of course constrained by the constancy of the specimen volume. The specific condition (equation (18)) by which this volume constancy is expressed by the authors is, however, a rather special one: it assumes that the shapes of all the abovementioned polyhedra are *similar* (and similar in their orientation). Then,

$$N \cdot \langle l^3 \rangle = \text{const} \qquad (b)$$

(N = total number of links per unit volume). An equally plausible assumption, which has more commonly been made in simple statistical treatments, is that there is *no correlation* between neighboring link lengths. In this case, volume constancy is expressed by

$$N \cdot \langle l \rangle^3 = \text{const} \qquad (c)$$

It can easily be shown that equations (b) and (c) correspond, respectively, to the following expressions for the total length of dislocation per unit volume:

$$\rho = \frac{\langle l \rangle}{\langle l^3 \rangle} \qquad (d)$$

$$\rho = \frac{1}{\langle l \rangle^2} \qquad (e)$$

3 The change of the distribution function through recovery, as assumed by the authors, has a number of curious features. Firstly, no links are supposed to disappear through mutual annihilation (equation (7)). Yet one may well imagine, for example, that an oblong four-sided mesh can disappear through recovery processes: then, the two short sides shrink to zero length, but the two long sides annihilate each other at a finite length. A shrinkage of links to zero length is not precluded in this treatment, in principle. However, the rate of disappearance would be proportional to the slope of the distribution function at the origin (if one uses the coarsening rate specified by equation (a)). This slope appears to be zero in most figures, and it is zero in the (supposedly measured) initial distribution function used in the application (although a finite slope seems to appear with strain, see Fig. 5). Note that even in the régime in which the total number of links is not changed by recovery, the total length of dislocation decreases. Together with the authors' specific condition expressing "volume constancy" (equation (b)), this means that the average link length *decreases* (equation (d)). Apart from the fact that this is possible only when the mesh shape changes (which it was assumed not to in equation (b)), one would intuitively favor a model in which the total number of links as well as the total length of dislocations decrease, whereas the average link length increases.

4 The strain-hardening model used by the authors clearly (and sensibly) predicts an increase in the total number of links with time. When this is not balanced by a decrease through re-

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covery, a steady-state structure cannot be achieved. A fraction of the (net) number of links produced will eventually give rise to further strain; thus, one would expect a steadily increasing strain rate, as in fact was first obtained by the authors (text between equations (8) and (9)). The authors fix up this problem by introducing the notion of an increasing node strength with progressing recovery. While this is possible in principle, it cannot be effective for long, since Orowan bypassing, which is independent of the node strength, would soon take over for these rather strong obstacles.

5 The strain-hardening model illustrated in Fig. 2 suggests that the total length of dislocation lines does not change at all (if one subtracts links dissolved by breaking away of the mobile dislocation A-B). Equation (12), which describes this process, is puzzling at first sight, since the formation of new links should be *conditional* upon the breaking away of a previous link, and thus should be multiplied by the right-hand side of equation (9). Instead, this condition is introduced indirectly by making $C(t)$ proportional to the strain rate. At this point, a scaling is introduced which forces the dislocation density to increase by the law (15): this is the real crux of the strain-hardening behavior. Equation (15) is arbitrary to some extent, in that it specifies a very particular average length ($\rho^{-1/2}$) in the network to which the mean-free path is supposed to be proportional. It is not really known whether that is better than, for example, assuming a proportionality to $< l >$ or some other quantity. Since the authors do make a clear distinction between these different averages in other respects, it is important here also.

6 The mean-free path derived by fitting the theory to some experiments comes out very short indeed: less than an average link length. While this order of magnitude makes some sense in connection with the present model, it seems to be far out of line with observed behavior. For example, the strain-hardening rate would then be of the same order as the shear modulus; but it is commonly observed to be at most 1/100 of this value. It seems likely that the gross disagreement between the calculated and experimental curves of dislocation density versus time in Fig. 6 is due to this same effect. It may be, however, that the particular experiments analyzed in the paper reflect anelastic behavior rather than a hardening-and-recovery transient, in which case a "hardening" coefficient of the order of the shear modulus would be expected. The small amount of strain in the observed transient (Fig. 4) is certainly compatible with such an interpretation. How does the derived value of the "mobility" M compare with what would be expected from a dislocation climb mechanism on the one hand or an anelastic mechanism on the other?

7 Finally, the authors have found a dependence of the steady-state creep rate on the initial conditions, which they deem unimportant. However, their initial strain rate was varied by only $\pm 50\%$; one should expect initial strain rates of different order of magnitude to give rise to the same steady-state behavior.

With all these qualifications in mind, it is evident that the increase in sophistication proposed in the present paper has not as yet led to an increase in the predictive capabilities of the recovery creep model over and above, say, the simple form developed by Lagneborg earlier (reference [13]). It is satisfying to find, however, that the elaboration of the theory in considerable detail by these authors has not uncovered any fundamental inconsistencies in the recovery creep model itself.

Authors' Closure

In their discussion of "A Recovery-Athermal Glide Creep Model" U. F. Kocks and H. Mecking present seven paragraphs where they criticize different details in the model. We will discuss these one by one. However, what we consider their main criticism is their final conclusion that this model has no "predictive capabilities" over and above a simple recovery creep model [13]. This is not true and we will therefore start discussing some "new predictive capabilities" of the model. It is regrettable that these capabilities were not included in full extent in the paper due to lack of space.

In this model we assume that there is one athermal obstacle associated to every dislocation link. The shear stress needed for a dislocation link to surmount its obstacle is given by $\tau_i = \alpha Gb/l$, equation (1). The local effective stress for glide is defined by $\tau_e = \tau_a - \tau_i$, equation (2), where τ_a is the applied stress. For all arrested links τ_e is negative. For all moving links τ_e is positive. This can be illustrated in an $l\alpha$-plane defined by $l \geqslant 0$, $\alpha \geqslant 0$, Fig. 9. The line $\alpha = \tau_a l/Gb$ corresponds to $\tau_e = 0$. We also assume that the velocity of the gliding dislocations is very high and therefore the creep process can be characterized by a dislocation network in which practically all dislocations are at rest, i.e. $\tau_e < 0$. By recovery some of the arrested dislocation links are growing and some are shrinking. When a growing link reaches the line $\alpha = \tau_a l/Gb$ it surmounts its obstacle, expands into a loop by glide, and is eventually arrested at obstacles where $\tau_e < 0$, Fig. 9.

The simple recovery creep model referred to by Kocks and Mecking regarded the glide process as a thermally activated process. There is now convincing evidence that glide is athermal in high temperature creep and in accordance with this glide is treated as an athermal event in the present model. The first "new predictive capability" of this model to be pointed out is that it is capable to predict strain transients after stress changes, e.g. primary creep, and at the same time treat glide as an athermal process. Despite large efforts we have not found it possible for simple theories based on average quantities, like dislocation density, to produce such strain transients and at the same time be athermal.

Another "new predictive capability" of the model is its ability to predict that a stress reduction, even the smallest one, is followed by an incubation period during which the strain rate is zero. Instantaneously after the stress reduction there will be no arrested links in the region given by $l > \alpha Gb/\tau_a$, i.e. there are no links that are able to surmount their obstacles. During the time it will take for the links that are growing by recovery to reach the line $\alpha = (\tau_a - \Delta\tau_a)l/Gb$ the strain rate will be zero, Fig. 10(a, b). For single crystals incubation periods are observed for all stress reductions, $0 < \Delta\tau_a/\tau_a \leqslant 1$. For polycrystals they are observed for $0 < \Delta\tau_a/\tau_a \lesssim 0.3$ [7].

Still another "new predictive capability" of the model is its ability to describe a cumulative stress reduction experiment. In this experiment the stress is decreased by $\Delta\tau_a$. When the incubation period has ended a further $\Delta\tau_a$ is removed, and so on. From Fig. 10(a) it is easily seen that the length Δl increases for every new stress reduction. The growth rate dl/dt will also decrease as a consequence of the decreasing dislocation density. This means that a plot of the cumulative stress reduction versus the cumulative incubation period will have the qualitative behavior given in Fig. 10(d). This is in qualitative agreement with experiments [7]. The existence of the incubation period and the qualitative behavior of the cumulative stress reduction experiment can of course be understood by use of a mean link length and a mean obstacle strength. However, in this case it is not possible to simulate the primary stage in the creep curve.

We will now discuss the seven paragraphs one by one.

1 Friedel has shown by a simple theoretical reasoning that the average mesh size ($\bar{R}$) should grow according to the expression $d\bar{R}/dt = MT/\bar{R}$ when the dislocation structure is subjected to high temperature recovery. In addition experimental measurements of the dislocation density in specimens subjected to recovery comply with the Friedel relation [4]. This shows that there is quite a close resemblance between the growth process of dislocation meshes in recovery and grain growth both with re-

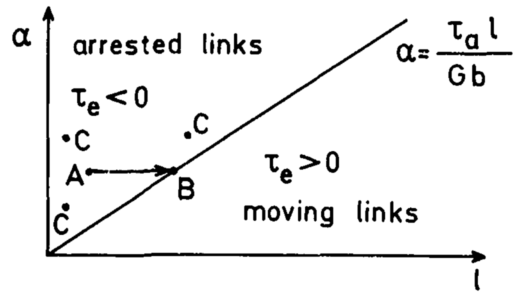

Fig. 9 α **is the obstacle strength and** l **the dislocation link length. A dislocation link at A grows by recovery. It surmounts its obstacle at B expands quickly into a loop and is eventually arrested at the points C.**

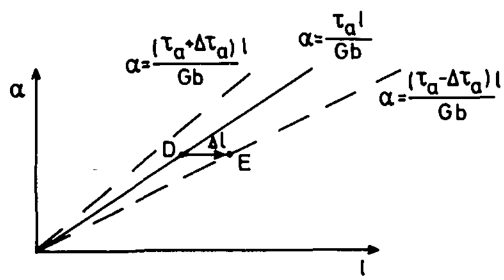

Fig. 10(a) Before the stress reduction the link at D **will surmount its obstacle by an infinitesimal growth. After the stress reduction it must grow until it reaches** E **before it can surmount its obstacle. After the stress increase all links with** $l > \alpha Gb/(\tau_a + \Delta\tau_a)$ **will start to glide instantaneously.**

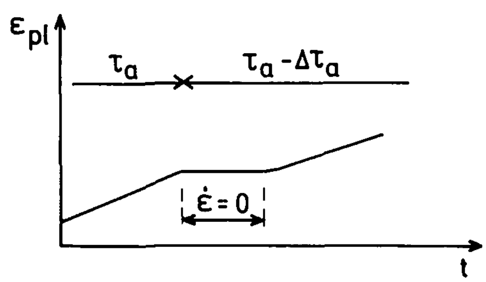

Fig. 10(b) The plastic strain versus time after a small stress reduction

spect to their basic physical features and the mathematical laws describing them. When it comes to the description of the growth of the individual dislocation meshes (or radius R) it is therefore natural to adopt the expression describing the growth of individual grains, equation (5). However, recovery is an extremely complex process and it is admitted that equation (5) in no way can describe it to its full extent. For instance the process may comprise the annihilation of dislocations and glide events driven by long-range elastic interaction between dislocations. Clearly, such processes cannot be accounted for adequately by equation (5). However, the fact remains that equation (5) is able to describe the observed changes of the average mesh size as well as of the distribution of their sizes quite well [4]. Therefore awaiting more refined theories of recovery it seems relevant to apply it.

2 We agree with Kocks and Mecking that both conditions of constant volume are equally plausible. However, we feel sure that the results depend only in a minor way on which condition of constant volume we choose. The important thing is that a condition of constant volume is used. The good behavior of the model, e.g. the steady state, depends on the use of a condition of constant volume and the way it is applied to the model.

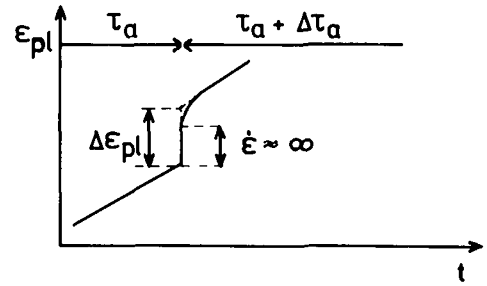

Fig. 10(c) The plastic strain versus time after a small stress increase

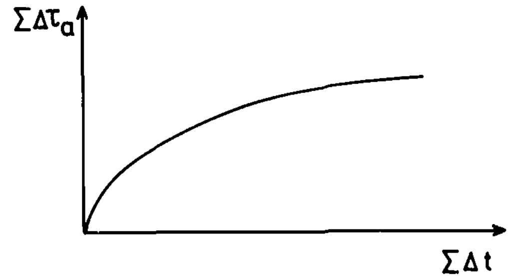

Fig. 10(d) The cumulative stress reduction versus the cumulative incubation period

If the dislocation density is defined as the total dislocation length per unit volume it is always given by equation (4). This is a consequence of the definition of ϕ.

3 For a discussion of the annihilation processes we refer to paragraph (1).

The discussion in the remainder of this paragraph is based on erroneous observations by Kocks and Mecking. For $t > 0$ the slope of the distribution function at the origin is never zero. This slope is entirely determined by the model itself. For curve B in Fig. 5 the slope at the origin is $-1.1 \cdot 10^{32}\ m^{-5}$ and $\left.\frac{dN}{dt}\right|_r / N = -4.4 \cdot 10^{-4}\ s^{-1}$ $\left(\left.\frac{dN}{dt}\right|_r\right.$ is the rate of disappearance as a result of recovery$\left.\right)$. For curve D in Fig. 5 the slope at the origin is $-6.3 \cdot 10^{33}\ m^{-5}$ and $\left.\frac{dN}{dt}\right|_r / N = -7.5 \cdot 10^{-3}\ s^{-1}$. For $t = 0$ the slope at the origin is zero. This is a consequence of the choice of initial function (which in this sense is a bad choice). However, the results are very independent of the analytical expression of the initial function. According to these results the total number of links decreases. Also, the total length of dislocations decreases and consequently the average link length increases. This is the behavior intuitively favored by Kocks and Mecking.

4 The quantity l which forces the strain rate to decrease during the primary stage is not clearly defined in the present model. In a development of this model which will be published in a near future these problems will be discussed in more detail. In this refined model we have introduced a minimum obstacle strength α_{min}, i.e. $\alpha_{min} \leqslant \alpha \leqslant \alpha_{max}$. $\bar{l}$ is now clearly defined as $\bar{l} = \alpha_{min} Gb/\tau_a$. The notion of an increasing node strength is not used in the refined model.

5 In this paragraph Kocks and Mecking present a number of erroneous statements. Firstly, Fig. 2 does not illustrate the strain hardening model. It is clearly described in the paper that the strain hardening process consists of three subsequent events: (i) a link surmounts its obstacle, (ii) The link expands into a loop by glide. This subprocess gives rise to the increase in dislocation density given by equation (15), (iii) The expanded

loop is arrested in the network. This subprocess is illustrated in Fig. 2. Secondly, if equation (12) is multiplied by the right-hand side of equation (9) this would mean that the supply of new links in the interval $(l, l + \Delta l)$ only depends on links in the same interval which break their junctions. This is not true. Every expanding dislocation loop can give rise to new links in the interval $(l, l + \Delta l)$ independent of the length of the original dislocation which broke away from its junction. Thirdly, $C(t)$ is not proportional to the strain rate. By use of equation (13) and (15) it is easily shown that $C(t)$ is proportional to the total length per unit time of the expanded dislocation loops. This result is easily understood in view of the definition of $C(t)$: The probability that a link of length l is hit during Δt is equal to $C(t) \cdot l \cdot \Delta t$.

6 By fitting the theory to experiments the mean free path comes out very short. This means that the strain hardening rate, $d\sigma/d\epsilon$, would then be of the same order as the shear modulus. Kocks and Mecking state that $d\sigma/d\epsilon$ is commonly observed to be at most 1/100 of this value. However, there are reasons to believe that the strain hardening rate is very large during creep conditions. In Fig. 10(a) it is shown that if the stress is increased by $\Delta\tau_a$ all links with $l > \alpha Gb/(\tau_a + \Delta\tau_a)$ will immediately surmount their obstacles and expand into loops. This gives rise to an instantaneous plastic strain which is probably followed by a small "primary stage," Fig. 10(c). By measurement of the plastic strain increment $\Delta\epsilon_{pl}$ in Fig. 10(c) the strain hardening rate has been estimated to be 4G for the 20 percent Cr − 35 percent Ni steel used to test the model [14].[3] Similar results have been obtained by other authors [15, 16, 17]. By use of equation (15) and (25) and the Taylor factor, 3.1, it is easily shown that $L = (3.1)^2\alpha' G/2(d\sigma/d\epsilon) \approx 2G/(d\sigma/d\epsilon)$. We obtain $L \approx 0.5$ in close agreement with $L = 0.65$ obtained in the paper. The stress dependence of L can be obtained from the stress dependence of $d\sigma/d\epsilon$. For Cu $d\sigma/d\epsilon \sim \sigma^{-1}$ [7] and for Al $d\sigma/d\epsilon \sim \sigma^{-1.1}$ [15]. Both values are in close agreement with $L \sim \sigma^{1.1}$ obtained in the paper.

[3]Numbers 14-17 in brackets designate Additional References at end of Closure.

The derived value of the "mobility" M is about 30 times greater than what would be expected from a dislocation climb mechanism [1].

7 If the initial creep rate is varied by a factor 3 the steady state creep rate is changed a factor 1.12. It is the relation between these two factors that have made us to state that the steady state creep rate is approximately independent of the initial creep rate.

Acknowledgment

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