T73S04 (R5V2/3) – Session 36: Models for Creep Ductility

Last Update: 30/4/15

Relates to Knowledge & Skills items: 1.4, 1.51

Most of this session is beyond SQEP requirements

BE model for austenitic reheat cracking illustrated by S4 welds; Classic creep failure models: cavity nucleation & growth; Rice & Tracy, Hull & Rimmer, etc.; Vacancy diffusion; Pipe diffusion; Where stress state dependence of ductility comes from; Implicit assumption in ductility exhaustion of correlation between deformation (dislocations) and damage (vacancies); Chevalier's correlation of poor creep ductility of 300-series stainless steels with low Ni content: is it really due to cavities?

Austenitic Reheat Cracking

Qu.: What is reheat cracking?

Reheat cracking is defined as cracking due to creep during the relaxation of welding residual stresses which will occur when the weldment is subjected to temperatures in the creep regime. It is called reheat cracking because it happens when the item is subjected to heating again – i.e., after welding (as opposed to cracking *during* welding). Reheat cracking can occur during post-weld heat treatment (PWHT). However, one hopes that the material and the PWHT procedure would avoid this. In EDF Energy the reheat cracking threat invariably refers to the possibility of cracking in service due to the weldment having entered service without PWHT.

Qu.: What is BE's model for austenitic reheat cracking?

The prediction of the initiation of reheat cracking in austenitic steels according to the BE model essentially follows the R5V2/3 approach. Historically the R5V2/3 procedure had to be augmented to include the key elements of austenitic reheat cracking. These are: (a)that cracking is due predominantly, or solely, to the monotonic relaxation of welding residual stresses, and, (b)that cracking occurs due to the much reduced creep ductility under highly triaxial conditions. The reheat cracking model came first (circa 1996) and R5 was later amended accordingly.

Qu.: How is the reheat cracking model applied?

In practice the reheat cracking model is invariably applied via a finite element analysis which simulates the welding process. This is because the degree of triaxiality of the residual stress field is crucial and cannot be obtained in any other manner. Handbook advice for welding residual stresses generally only give you the first and second principal stresses, not the third, and hence do not provide the crucial hydrostatic stress.

Qu.: What are the key elements of the reheat cracking model?

The key elements are,

- The FE determination of the 3D welding residual stress state;
- Service stresses from FEA, including local stress raisers (notches, weld toes, etc);
- Best estimate creep deformation (e.g., RCC-MR for 316);
- Lower bound creep ductility (e.g., lower shelf of 0.9% for 316H);
- Appropriate Spindler fraction.

Qu.: What is the Spindler fraction?

The Spindler fraction specifies the multi-axial creep ductility as a fraction of the uniaxial ductility, as a function of the three stress invariants: Mises stress, $\bar{\sigma}$, maximum principal stress, σ_1, and hydrostatic stress, σ_H. For example, for austenitic reheat cracking assessments the most commonly used formulation is,

$$\frac{\bar{\varepsilon}_f}{\varepsilon_{f,uni}} = \exp\left\{ p\left(1 - \frac{\sigma_1}{\bar{\sigma}}\right) + q\left(\frac{1}{2} - \frac{3\sigma_H}{2\bar{\sigma}}\right)\right\} \tag{1}$$

R5V2/3 Appendix A1, Section A1.11.1.2 includes two sets of values for the parameters p and q. The more onerous values, $p = 2.38$ and $q = 1.04$, were derived from tests on 304 stainless steel. These larger p and q values are considered to be most appropriate in the transition region, in which region the mechanistic models suggest the ductility will be more sensitive to stress triaxiality (see below).

However, these larger p and q values have been widely used for 316H in the reheat cracking temperature range (see Refs.[1-3]), despite the strain rates putting typical weldments on the lower shelf. There is, therefore, a degree of fudge in the 316H reheat cracking models.

Qu.: How important is the Spindler fraction in reheat cracking?

Crucial.

If it were not for the severe reduction of creep ductility due to triaxiality, reheat cracking would not happen.

This can be seen very simply. The maximum elastic strain that can be recovered from a sample of material stressed to yield is σ_y / E and this is generally around 0.1% for parent and perhaps 0.2% for weld.

The maximum creep strain that can result from monotonic relaxation, starting at the yield stress, is $Z\sigma_y / E$, where Z is the elastic follow-up factor. This is an over-estimate because the stresses will not relax completely, e.g., at steam temperatures the stress in 316H might not drop below ~100 MPa even after tens of years. So the creep strain at long times is more like $Z(\sigma_y / E - 0.065\%)$.

Hence, for typical values of Z in the region of 2 to 3, say, the maximum creep strain is perhaps, very crudely, of order 0.1% to 0.4%.

So, if the lower bound, lower shelf creep ductility is 0.9%, reheat cracking apparently cannot occur. For several years around 1989-1993, when the S4 weld cracking was an escalating problem, this simple observation convinced me that welding residual stresses alone could not be responsible for the cracking. (It did not help that R66 at the time claimed a creep ductility for 316H of 9%, not 0.9%! The latter figure only came later from tests on ex-service HYA/HRA header material – and the realisation that loading strain should be excluded).

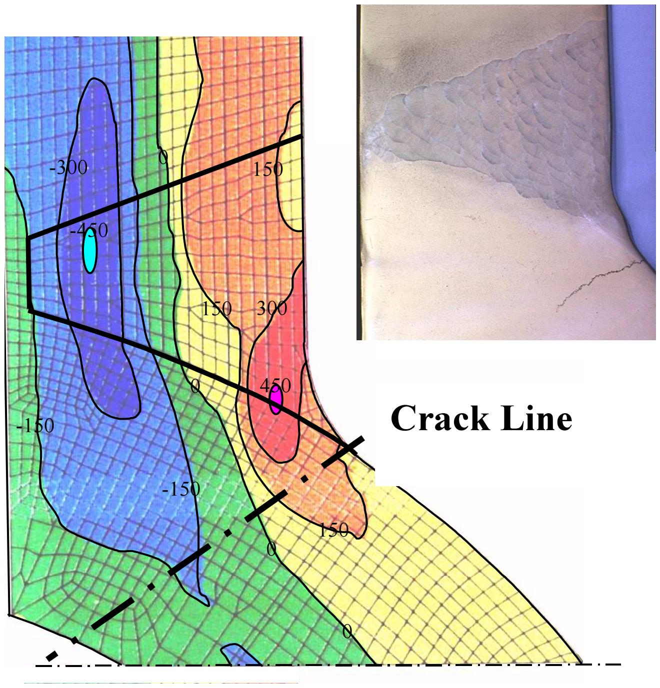
But the Spindler fraction can reduce the creep ductility by up to an order of magnitude for a sufficiently highly constrained weld – and so the triaxial ductility can drop as low as ~0.2% or less, thus causing reheat cracking.

Qu.: What does a typical reheat cracking initiation model look like?

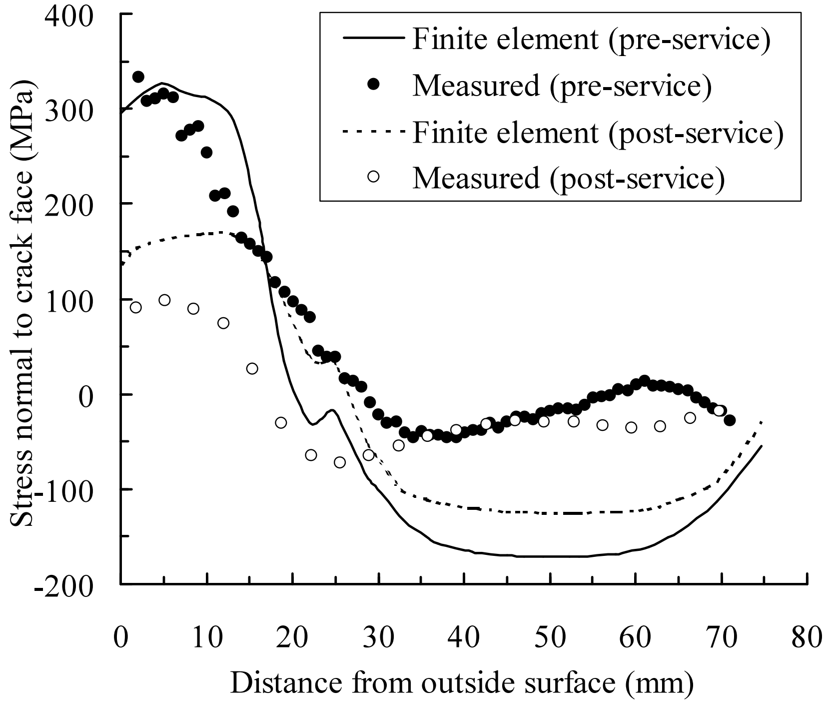
The archetypal examples of austenitic reheat cracking are the S4 welds at HYA/HRA. These welds attach the outlet nozzle to the 316H superheater headers. At least 101 of these headers cracked (out of 128 across the four reactors). Cracks were first discovered in Dec.1988, after about 4 years service (I guess ~20,000 hours). There were major reheat cracking problems on many of the other welds associated with these s/h headers & penetrations (see Refs.[1,3]).

The headers are 64mm thick, as is the nozzle at the weld position, so the set-on branch provides a highly constrained geometry. They were not stress relieved. The FE predicted residual stresses are compared with those measured in Figure 1. The result is high triaxiality which peaks at a depth of ~15mm giving a minimum Spindler fraction of ~0.14 and hence a local triaxial ductility of only ~0.13%. The follow-up factor is 2.06. The damage at this location reaches unity after ~8,000 hours. The damage maps at the initiation time (8,000 hours) and at 250,000 hours are illustrated in Figures 2,3 below.

Figure 1: HYA/HRA Superheater Header S4 Welds: Residual Stresses



Predicted nozzle-axial as-welded residual stresses



Predicted & measured stresses normal to crack (Effect of ageing for 55,000 hrs at 525°C)

Figure 2: HYA/HRA Superheater Header S4 Welds: Predicted Reheat Damage

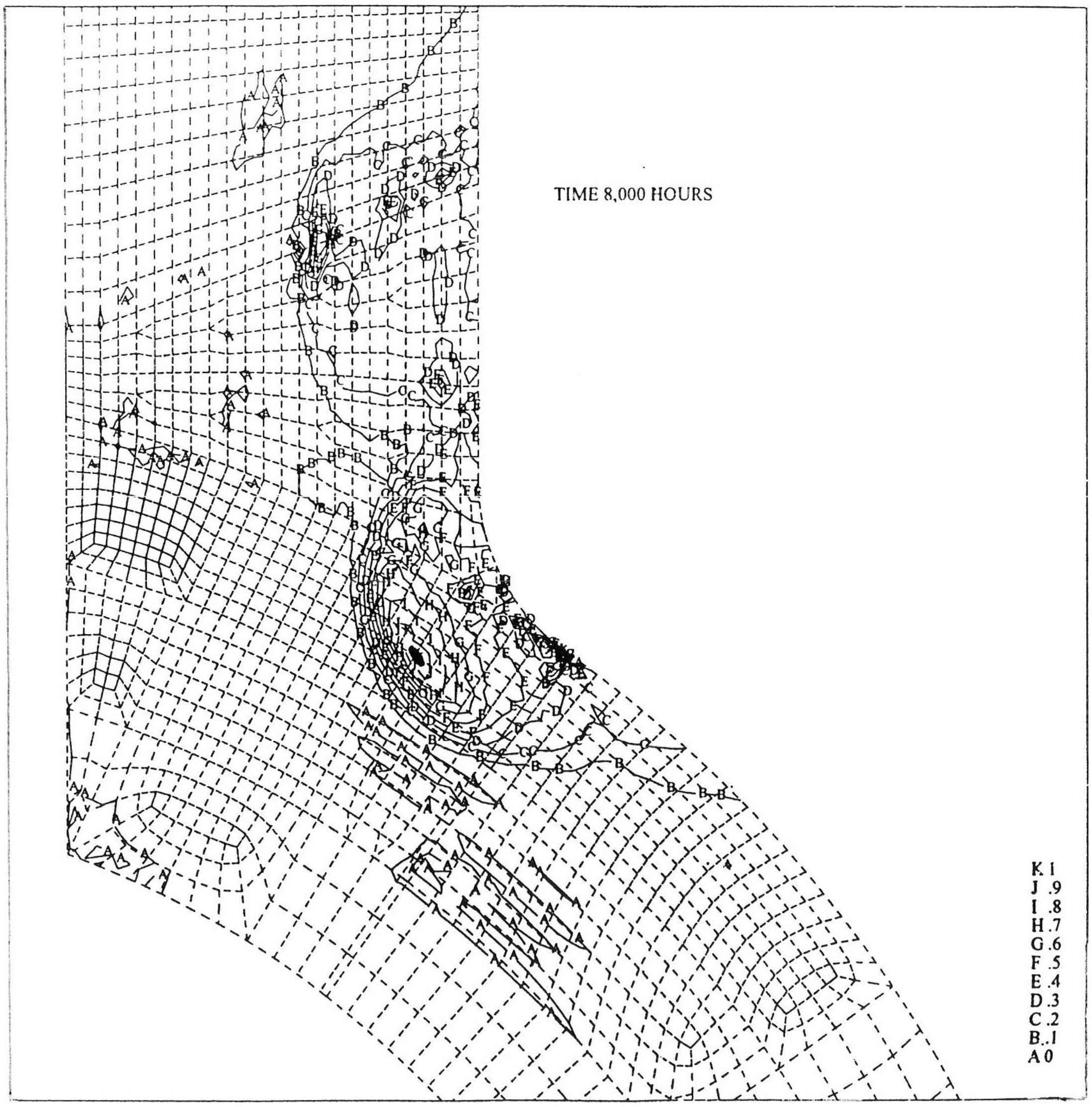
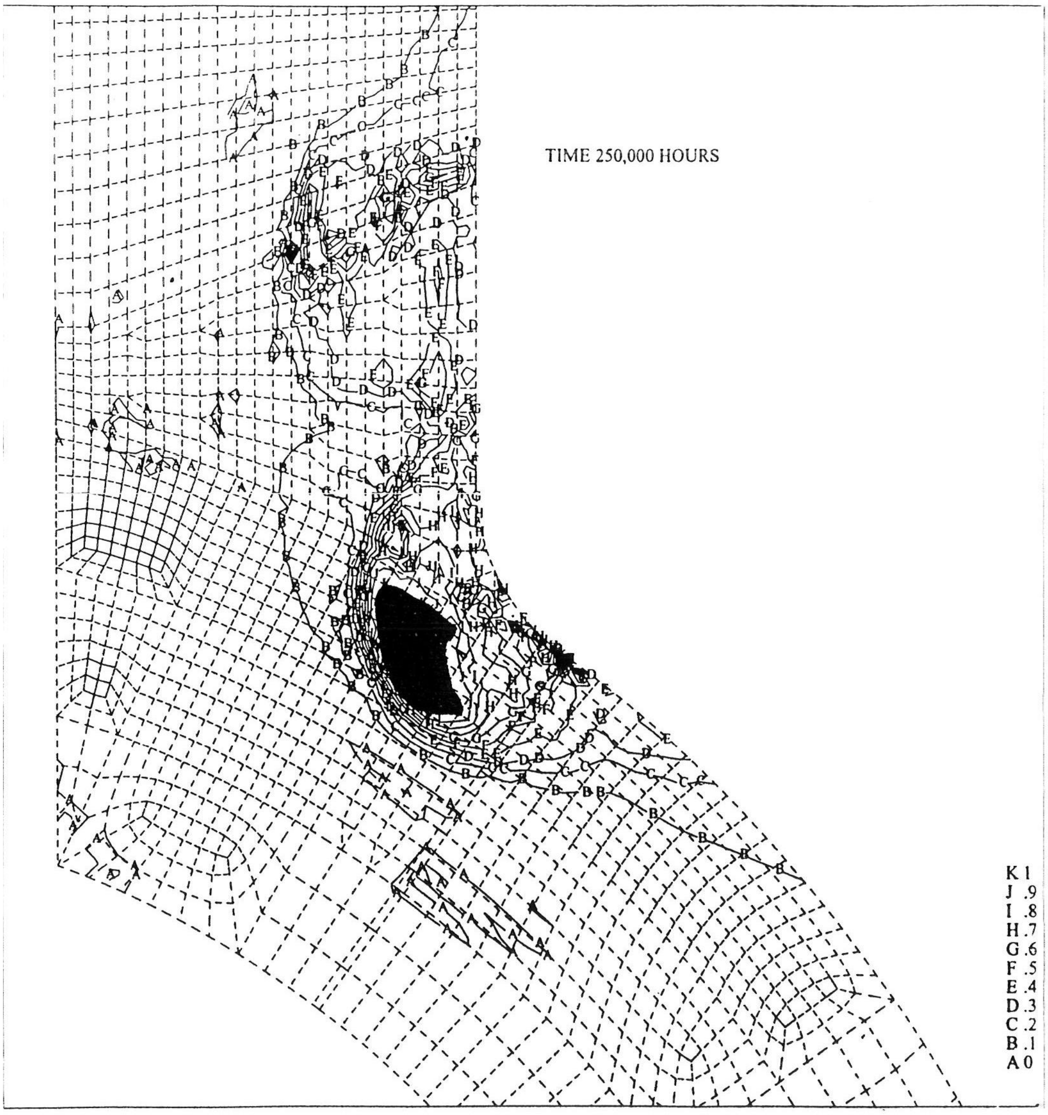


Figure 3: HYA/HRA Superheater Header S4 Welds: Predicted Reheat Damage



The observed crack depths beneath the S4 welds were (from memory) commonly around 15mm and up to 23mm in several cases. The latter figure is in good agreement with the depth of the tensile residual stress zone.

[*Two S4 welds were left with cracks in service a little longer than most and had 35mm deep cracks. Over the last ~12mm the cracks turned direction to be exactly radial to the header, suggesting they had started to grow due to pressure loading. This was disconcerting to me, and I'm glad we replaced all the cracked headers. No further adverse news for the last 14 years as far as I know*].

Qu.: Are "by hand" assessments of reheat crack initiation feasible?

It is possible to assess simply whether reheat cracking is, or is not, likely to be a threat, but only if you know Z and S – which are likely to require FE analysis of the welding process. The following guidance applies to 316H parent welded in the usual manner with a 316 consumable and not subject to PWHT. The reheat crack initiation criterion can be written roughly as $D_c \approx \frac{\varepsilon_c}{S\varepsilon_{f,uni}} = \frac{Z|\Delta\varepsilon_{el}|}{S\varepsilon_{f,uni}}$. For 316H the lower bound, lower shelf creep ductility is usually taken in reheat cracking assessments to be $\varepsilon_{f,uni} \approx 1\%$. The stress drop is likely to be (again roughly) of the order of the proof strength, and so $|\Delta\varepsilon_{el}| = |\Delta\sigma| / E \approx 0.1\%$, because the elastic strain at the 0.2% proof stress is typically about 0.1% for 316H. This gives $D_c \approx \frac{Z \times 0.1\%}{S \times 1\%} = \frac{Z}{10S}$.

Consequently damage reaches unity if $\frac{Z}{S} = 10$. So our crude assessment of the risk of reheat cracking is,

> In susceptible materials, reheat cracking is likely if, at the most onerous point, $\frac{Z}{S}$ exceeds 10. Otherwise reheat cracking will probably not initiate.

For example, for the HYA/HRA s/h header S4 welds, Z/S = 2.06 / 0.14 = 14.7 > 10 and hence reheat cracking is predicted – as indeed it should be!

Note that a "susceptible material" means a material with poor uniaxial creep ductility, less than ~2%. A material with good uniaxial creep ductility is unlikely to reheat crack even at a highly constrained weldment.

Data from detailed FE analyses, Refs.[2,3], Figure 4, confirms that this is a good estimator of the risk of reheat cracking. However this 'rule-of-thumb' does not carry the same authority as a full analysis of the weld in question.

Figure 4: Simple Reheat Cracking Risk Assessment (for 316H Welds)

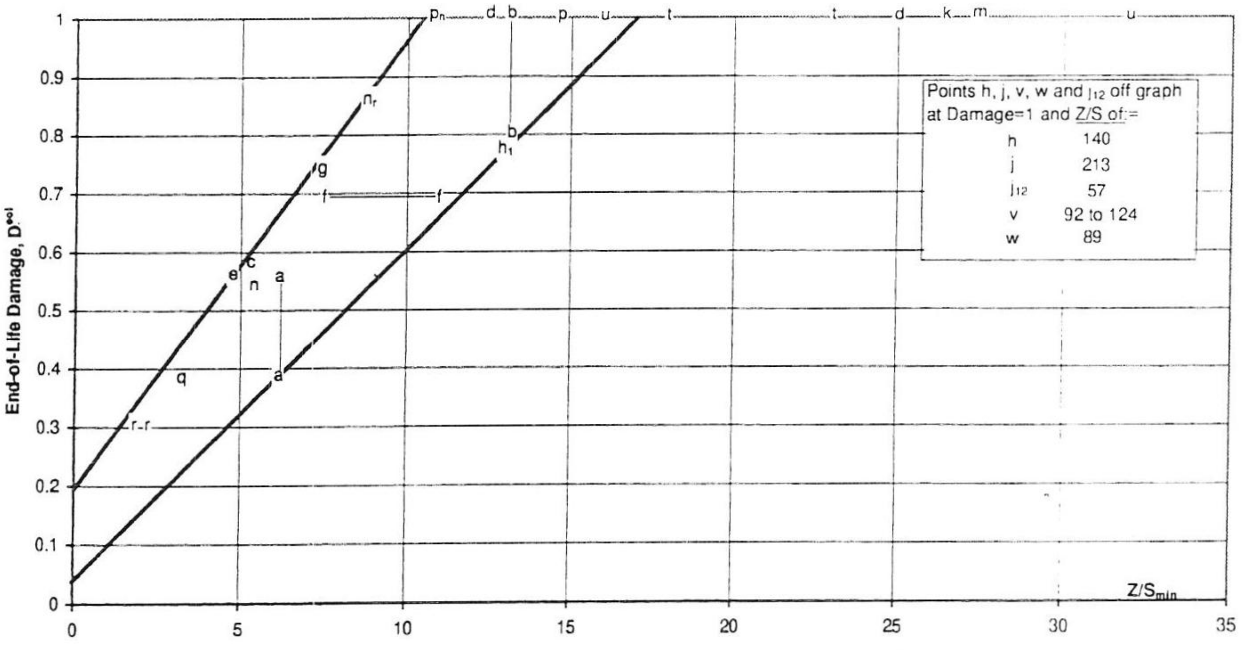


Figure 5: Damage versus Reheat Cracking Parameter, Z/S$_{min}$

Qu.: Is there an even cruder gauge of the likelihood of reheat cracking?

Yes.

Reheat cracking is unlikely if the smallest principal stress (σ_3) is less than 60 MPa.

Conversely, if a weld onto 316H entered service without PWHT and operates in the range (say) 480°C to 550°C, and the minimum principal stress (σ_3) is greater than 60 MPa, then reheat cracking is a very real threat. If the material is susceptible (i.e., if its uniaxial creep ductility is less than ~2%) then reheat cracking is likely under these conditions.

That's an extraordinarily sweeping statement – and is only intended to be a rule-of-thumb. But it's a fairly reliable guide for the 316H reheat cracking models carried out to-date.

It is worth pausing to consider the oddness of this criterion. It depends only upon the *smallest* principal stress. This is why FEA is required to predict reheat cracking. It is very difficult to estimate the smallest principal stress using analytical methods.

The reason why it is the smallest principal stress which matters is that the first and second principal stresses will invariably be of yield magnitude-plus, so it is the third principal stress which discriminates between high and low constraint cases.

Qu.: What are "high constraint" weldment geometries?

- Attachment welds (i.e., fillet welds) are more constrained than butt welds;
- Welds on thick sections are more constrained than on thin sections;
- Geometric stress concentration features generate constraint (notches, severe weld toe features, thickness mismatches between parent items attached by a butt weld);
- Local repair welds can generate high constraint.

Models for Creep Ductility

Qu.: Why is creep ductility sensitive to stress state?

The impression that is often given is that ductility is a material property and hence the stress state is somehow affecting this material property. How come? How would the material know to fail at a different strain because the degree of stress triaxiality is different?

The problem is one of perception. Actually ductility is not really a material property. For this reason it would be better to talk about "strain at failure" rather than "ductility", since it is clearer that the strain at failure is likely to vary according to the conditions.

The use of ductility exhaustion in the R5 procedure is partly responsible for this confusion. It gives the impression that failure occurs *because* some 'critical' strain is reached. But mechanistically this makes no sense since strain is merely dislocations traversing grains – which does not in itself constitute damage.

The mechanistic cause of failure is (probably) something else entirely. The so-called "ductility" is just what the strain happens to be when the true failure criterion is fulfilled. Probably.

Qu.: So what ***is*** the mechanism of creep failure?

There are at least two contenders,

[1] Tertiary creep consists of a softening matrix leading to steeply increasing strain rates and failure results from this rapidly escalating strain rate, possibly at some characteristic strain;

[2] Creep damage consists of cavities forming and growing on grain boundaries which, when they interlink, result in grain decohesion, cracking and failure.

There are other possibilities, such as relative grain sliding causing grain boundary micro-cracking.

Qu.: What arguments are there for and against these two options?

Option [1]

For: In uniaxial creep it is possible to devise mechanistic models of creep deformation using dislocation density as the state variable which successfully 'predict' the primary, secondary and tertiary phases of creep deformation, and hence also 'predict' failure (i.e., when the strain curve goes vertical).

Against: It is not clear that such models can also predict correctly the strain at failure, and especially the stress state and temperature dependence of this 'ductility'.

Option [2]

For: It is the received wisdom that creep cavities on grain boundaries are responsible for creep failures. This is backed up by the direct observation of such cavities in many cases, and cavity density shows a correlation with expected degree of damage. (An example are the cracked, ex-service HPB/HNB bifurcations).

Against: In some cases of creep failure or creep crack growth, cavities have not been seen, e.g., in many laboratory CTS ccg tests. (However, revealing and identifying cavities is notoriously subject to the details of the technique used, and may depend upon the skill of the technician).

For: Cavitation can explain tertiary creep simply.

For: A failure criterion which is mathematically independent of Mises strain is the most obvious way of rationalising why the strain at failure varies with stress state, stress magnitude and temperature. Moreover, models of creep ductility based on the cavitation mechanism, whilst very imperfect, display some of the key qualitative features.

In the brief description of classic models below we shall consider only the grain boundary cavity nucleation and growth mechanism of failure since this has been the most common assumption. But be warned – it may be wrong in some circumstances. So really this is just an illustration of the sort of models which can be developed.

Qu.: So - why ***is*** creep ductility sensitive to stress state?

Suppose there is some known failure mechanism. Call it cavity nucleation and growth if you like, but it doesn't matter what the mechanism may be. However, we suppose that the failure mechanism depends only upon the maximum principle stress, σ_1, but not upon the other stress components. For a given temperature the failure time is thus some function of the maximum principle stress: $t_f = F(\sigma_1)$. Now suppose that the

Mises equivalent creep strain is given by $\bar{\varepsilon}_c = A\bar{\sigma}^n t$, or perhaps by a primary creep law, $\bar{\varepsilon}_c = A\bar{\sigma}^n t^m$, if you prefer, it doesn't matter which. In a uniaxial test, since $\sigma_1 = \bar{\sigma}$, the strain at failure (i.e., the uniaxial ductility) is therefore $\bar{\varepsilon}_{f,uni} = A\sigma_1^n (F(\sigma_1))^m$. Now suppose we apply positive stresses also in the other principal directions, whilst leaving σ_1 unchanged. The effect is to reduce the Mises stress, i.e., we now have $\bar{\sigma} < \sigma_1$. The multi-axial ductility is $\bar{\varepsilon}_f = A\bar{\sigma}^n (F(\sigma_1))^m$, and so the multiaxial:uniaxial ductility ratio is $\frac{\varepsilon_f}{\varepsilon_{f,uni}} = \left(\frac{\bar{\sigma}}{\sigma_1}\right)^n$. Note that the ductility is defined as the Mises strain at failure.

So the assumption that the failure mechanism is controlled by the maximum principal stress automatically leads to a simple expression for the multiaxial:uniaxial ductility ratio. For example, suppose we choose principal stresses of 200, 100 and 50 MPa. The Mises stress is 132 MPa and the multiaxial ductility turns out to be only 0.19 times the uniaxial ductility assuming n = 4. These are quite realistic numbers.

The moral is that the stress state sensitivity of the Mises strain at failure originates from the fact that the creep deformation law depends only upon the Mises stress whereas the failure mechanism may depend upon other stress components. The details of the failure mechanism are irrelevant. All that matters is the stress dependence of the mechanism.

Moreover, the particular sensitivity of the ductility to stress state is due to the typically large exponent n. That is,

The sensitivity of creep strain at failure to the stress state is largely a consequence of the sensitivity of creep strain rate to stress, i.e., the large creep index n

Qu.: What are the classic models for creep ductility?

Without trying to list them all, a few of the most notable are,

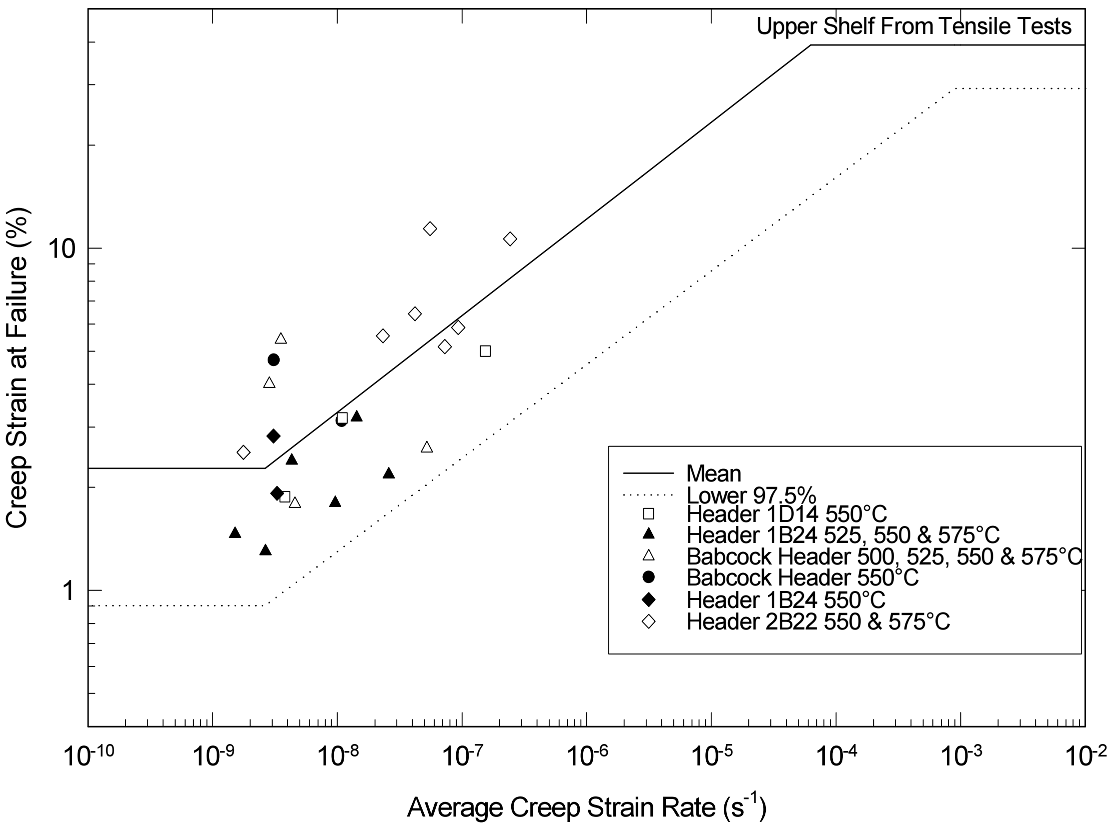
- Hull and Rimmer (1959)
- Speight and Harris (1967)
- Rice and Tracy (1969)
- Skelton (1967, 1975)
- Dyson (1976, 1979)
- Chen and Argon (1981)
- Cocks and Ashby (1980, 1982)
- Much of the above is summarised in the book by Evans (1984)
- Hales and Spindler (1997)

See Refs.[4-17]. In the following paragraphs I try to extract the key points of these sorts of models but without the obscuring details.

Uniaxial Creep Ductility

Recall that creep ductility often displays a strain rate dependence, with a lower shelf, an upper shelf, and a transition region, e.g.,

Figure 5 Ex-HYA S/H Header 316H 525-575°C Creep Ductility (R66)



Note that a reduction in strain rate can only be achieved by either a reduction in stress or a reduction in temperature, so the strain rate dependence of ductility is really the same as there being a stress and/or temperature dependence. There is both.

Moreover, reducing the strain rate, either by reducing stress or by reducing temperature, extends the time to failure, perhaps dramatically. So the strain rate dependency is also a codified way of saying that the longer your test duration, the smaller the creep ductility may eventually prove to be. This is indeed the observed behaviour in general. It is unfortunate because it makes justification of accelerated creep testing problematical.

This observation suggests the existence of a time dependent process which contributes to failure but does not contribute to strain (to the same extent)

Qu.: How do the models distinguish between the lower shelf and transition?

Cavities cannot just grow on grain boundaries independently of what is happening within the grains. The grains must accommodate the increasing strain that growing cavities represent. There are therefore two regimes,

- Unconstrained cavity growth: In this regime the cavity growth rate is controlled by the rate of vacancy diffusion. Whatever mechanisms are required to make the grains conform to the deformation caused by the growth of the cavities is assumed to occur at a sufficiently rapid rate to avoid these mechanisms being the strain rate determining step.
- Constrained cavity growth: In this regime the grains deform or slide more slowly than required to accommodate unfettered cavity growth by vacancy diffusion. The actual cavity growth rate is therefore determined by what the matrix deformation plus grain boundary sliding will allow. It is usually assumed in published models that grain boundary sliding is the relevant rate determining mechanism (but I don't think this is essential to the concept).

Since constrained cavity growth occurs only when the rate of grain deformation plus sliding has dropped below some level, this is the slow strain rate regime – in other words the regime of lower shelf ductility. The models do indeed imply that the creep ductility should be insensitive to strain rate in this regime.

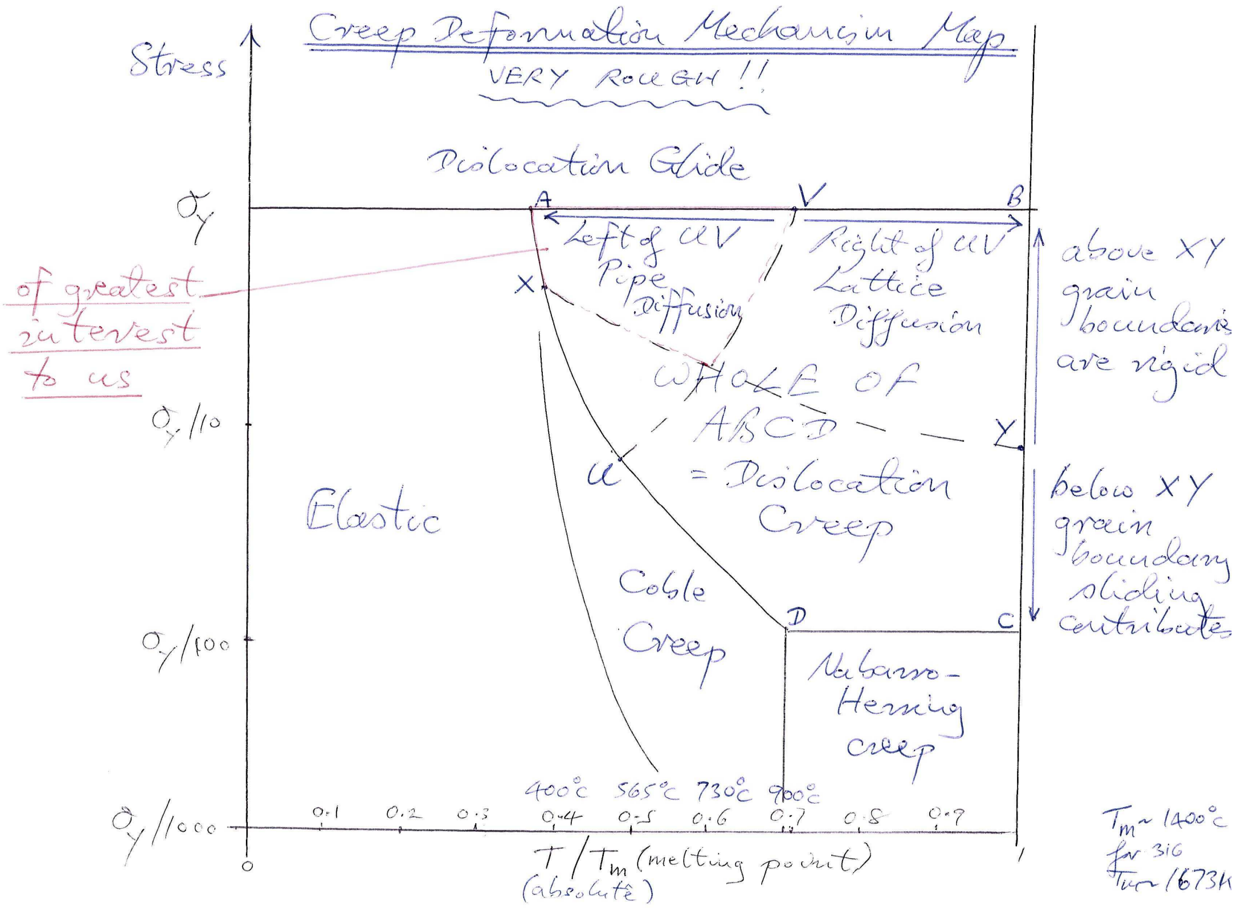
Unconstrained cavity growth applies for faster strain rates and the models do predict that the creep ductility would be expected to increase with increasing strain rate for this mechanism.

Qu.: How does this relate to the mechanism map of Fig.19 in Session 23?

The mechanism map is reproduced below, as Figure 6,

For austenitic steels in the temperature and stress ranges of interest to us, the material will generally be in the region AUV of the mechanism map. This is the regime of 'pipe diffusion', which includes grain boundary diffusion. The curve XY divides the region in two, the region below XY is at lower stress and is the region in which grain boundary sliding and matrix deformation throttle the process. This is the lower shelf region (constrained cavity growth). Conversely above XY grain boundary sliding is sufficiently fast that cavity growth is unconstrained and vacancy diffusion is the rate determining mechanism. This is the transition region (unconstrained cavity growth).

Figure 6



Qu.: Is this consistent with empirical evidence on, say, 316ss?

Possibly, yes.

The empirical evidence is that 316 exhibits a mechanism change at around 550°C. This is consistent with the above observations in that we might expect constrained cavity growth below ~550°C.

Qu.: What does this imply for assessing using "ductility exhaustion"?

A conceptual difficulty with defining creep damage as the ratio of Mises creep strain to some creep ductility is that it seems inconsistent with the physical nature of damage – at least if we assume this means cavity nucleation and growth. The reason is that cavities represent a volumetric strain whereas creep deformation evaluated using the usual flow rules occurs at constant volume. Consequently, the calculated Mises creep strain relates (roughly) to shear strain and does not relate to cavities directly at all, i.e., the hydrostatic creep strain is zero. So how can conventional ductility exhaustion possibly be appropriate?

Conventional ductility exhaustion, defined via the ratio of Mises creep strain to ductility, can be appropriate as a measure of cavitation damage only if it is assumed that Mises creep strain is *correlated* with cavitation. In the case of constrained cavity growth (i.e., on the lower shelf) this is perfectly consistent – since constrained cavity growth is, by definition, controlled by the matrix deformation plus the grain boundary sliding (shear).

On the other hand, in the transition region, it is less clear that the Mises creep strain is correlated with the cavity growth since the latter is physically controlled by vacancy diffusion.

Qu.: What is the essence of unconstrained cavity growth models?

These models originated with Hull and Rimmer (1959). They considered how cavities would grow due to diffusion of vacancies along grain boundaries. The key to this is that a vacancy has a lower Gibbs free energy in a position of low stress than in a position of high (tensile) stress. Consequently vacancies diffuse down a stress gradient towards the lowest stress point. (This can also be thought of as atoms diffusing *up* the stress gradient towards a higher stress point).

The other key bit of physics is that the stress at a cavity is relatively low compared with the applied stress. Consequently the vacancies usually diffuse towards the cavity, thus making it grow.

Qu.: Why is the stress low at the surface of a cavity?

Strictly the stress at the surface of a cavity is low only when an equilibrium condition has been achieved as regards the vacancy diffusion (more of this later). However, assuming this equilibrium has been reached, the situation is rather like a bubble. The pressure differential over a spherical surface of radius R is $\Delta P = 2\gamma / R$, where γ is the surface tension[1]. So the radial stress at the surface of a spherical cavity is constrained to be $2\gamma_s / R$, where γ_s is the metal surface energy (=surface tension). This acts as the boundary condition which permits the vacancy diffusion problem to be solved. But it only applies in the steady state – not the transient condition (see later).

For iron, the surface energy is ~2 J/m2, so a cavity of radius 1 micron corresponds to a surface stress, $2\gamma_s / R$, of 4 MPa. This is a low stress, and hence, since applied stresses will generally exceed this, vacancies will tend to diffuse towards an existing 1 micron cavity, causing it to grow.

Qu.: What was Hull & Rimmer's main result?

Hull and Rimmer derived a failure time of,

$$t_f = \frac{a^3}{B}; \qquad \text{where } B \propto (\sigma_1 + \sigma_H - 2\gamma_s / R) \tag{2}$$

Here *a* is the spacing between the cavities on the grain boundaries, σ_1 is the maximum principal stress, σ_H is the hydrostatic stress, γ_s is the energy required to create unit free surface (surface tension) and *R* is the initial cavity radius.

The parameter B was determined explicitly in terms of the absolute temperature, *T*, the atomic volume, Ω, the grain boundary width, δz, and the diffusion constant along the grain boundaries, D_{gb}, and, of course, the stress. Explicitly,

$$B = \frac{16\pi D_{gb} \cdot \delta z \cdot \Omega \cdot (\sigma_1 + \sigma_H - 2\gamma_s / R)}{kT} \tag{2b}$$

Hull & Rimmer actually derived a more general model which also included terms quadratic and linear in the cavity spacing, *a*.

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[1] So the pressure inside a soap bubble is $4\gamma / R$ because there are two air/film surfaces. For cavities there is just one surface and the pressure difference is $2\gamma / R$.

Qu.: What happens to (2) at low stress? Sintering.

According to this model, if the applied stress, specifically, $(\sigma_1 + \sigma_H)$, were less than $2\gamma_s / R$ then the cavities would not grow. Vacancies would not diffuse towards the cavities because it would be energetically disadvantageous to do so. Instead vacancies would "boil off" from the cavities and would diffuse *away* from the cavities. The cavities would shrink. This is "sintering" and is the process whereby cavitated material can be healed by an annealing heat treatment at high temperature (unstressed).

For example, a cavity of radius less than 40 nm will sinter in a stress field less than ~100 MPa.

Qu.: What is the stress dependence of rupture time according to Hull & Rimmer?

Consequently (2) is valid only for $\sigma_1 + \sigma_H > 2\gamma_s / R$. The rupture lifetime is predicted to become infinite according to (2) when $\sigma_1 + \sigma_H \to 2\gamma_s / R$. When $2\gamma_s / R$ is sufficiently small compared with the applied stress we can approximate,

$$t_f \propto \frac{a^3}{\sigma_1 + \sigma_H} \qquad (3)$$

Qu.: Isn't the stress dependence of rupture time in (3) rather odd?

It is immediately clear that (3) provides a very poor representation of the stress dependence of rupture life in steels, which are far more stress sensitive than merely $t_f \propto 1/\sigma$. How well (3) might have worked for copper, or whatever materials Hull and Rimmer had in mind, I do not know. However, the resolution of this issue for steels is…

Qu.: Shouldn't the cavity spacing, *a*, reduce with time?

Equ.(3) can be made more reasonable for steels by accepted that *a*, the cavity spacing, is not a fixed constant but develops over time and is also stress dependent. In other words, new cavities nucleate during the creep process and so the average spacing, *a*, decreases.

Moreover, we may expect that higher stresses cause additional cavities to nucleate (whether immediately due to plasticity or later due to creep does not matter for our purposes).

For sake of argument let us suppose that, at failure, the cavity spacing is given by,

$$a = C(\sigma_1 + \sigma_H)^{-\phi} \qquad (4)$$

There is nothing special about assuming dependence on the sum $(\sigma_1 + \sigma_H)$. This is just a convenience because it is the same dependence on stress as in (2,3). It follows from (3) and (4) that the time to rupture depends upon stress as,

$$t_f \propto (\sigma_1 + \sigma_H)^{-(1+3\phi)} \qquad (5)$$

This has the potential to display a much greater sensitivity of the rupture life to the stress level. But how can we estimate ϕ ?

Qu.: How does continuous cavity nucleation imply strain rate dependent ductility?

It is possible to estimate ϕ without direct observation of cavity nucleation because it turns out that this parameter is crucial to a reasonable representation of the strain rate dependence of the ductility. To see this we assume the minimum (secondary) creep rate is given by a Norton law,

Minimum Creep Rate: $\dot{\varepsilon}_c^{\min} = B\overline{\sigma}^n$ (6)

Crudely we can approximate the ductility as $\varepsilon_{f,uni} \sim \dot{\varepsilon}_c^{\min} \cdot t_{f,uni}$. Using (5) and (6) this gives, in uniaxial stressing,

$$\varepsilon_{f,uni} \propto \sigma^n \cdot \left(\frac{4}{3}\sigma\right)^{-(1+3\phi)} \propto \left(\frac{4}{3}\right)^{-(1+3\phi)} \sigma^{n-(1+3\phi)} \propto \left(\frac{4}{3}\right)^{-(1+3\phi)} \left(\dot{\varepsilon}_c^{\min}\right)^{1-\frac{(1+3\phi)}{n}} \tag{7}$$

The 316H creep ductility versus strain-rate plot of Figure 5, for ex-HYA s/h header material, gives a best estimate value of the exponent in $\varepsilon_{f,uni} \propto \left(\dot{\varepsilon}_c^{\min}\right)^{\lambda}$ of $\lambda \sim 0.28$. If we take $n \sim 4$ this gives $\phi \sim 0.63$.

Consequently the observed strain-rate dependence of the ductility in the transition region can be represented sensibly by this model.

Moreover, the model establishes that, for austenitic steels in this temperature and stress range, continuous nucleation is required. If nucleation were switch off by putting $\phi = 0$ the predicted value of λ would be 0.75 (for n = 4) which is grossly inappropriate. (This development follows Spindler, Hales & Skelton, 1997 & 2001).

Qu.: What is the essence of constrained cavity growth models?

In constrained cavity growth, whilst the cavities still grow by diffusion of vacancies, their rate of growth is throttled by the rate at which the grains can accommodate the required deformation. There are two ways the grains can accommodate the strain,

- The matrix within the grains could deform under creep, or,
- The grains could remain rigid but sliding could occur over the grain boundaries.

The latter possibility is illustrated in Figure 7 below. The first consideration of how compatible grain deformation or sliding constrains cavity growth was due to Dyson (1976).

Figure 7

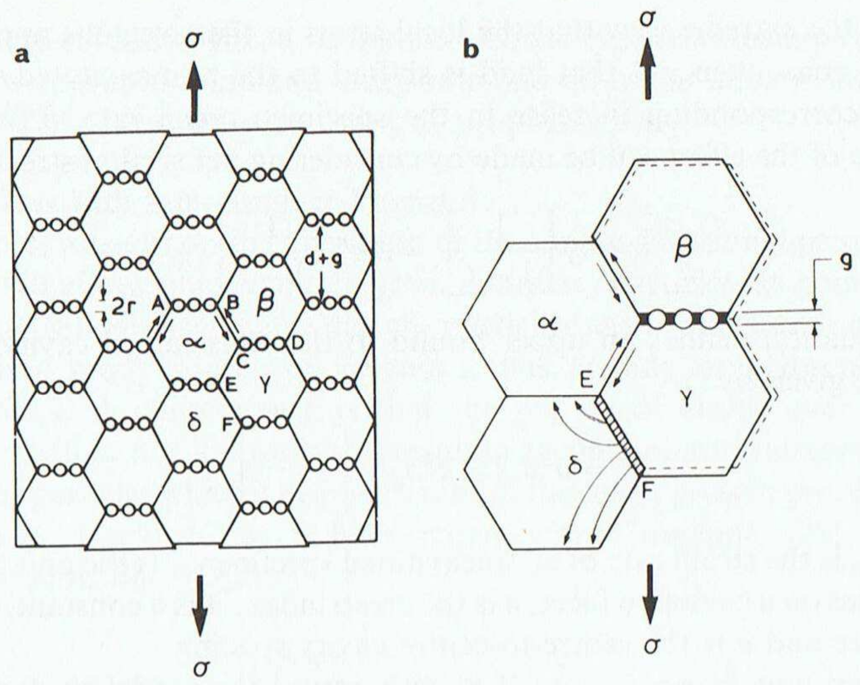


FIG. 5.4. The accommodation of atom plating by (a) grain boundary sliding in a uniformly cavitated structure and (b) grain deformation in a non-uniform structure. (After Dyson [24].)

Simple geometrical considerations are enough to evaluate the contribution of grain boundary sliding to the creep ductility. At failure the cavity diameter equals the cavity spacing, a (the distance between cavity centres, i.e., the nucleation points). Consequently the strain at failure is of order a/d, where d is the grain size. Specifically,

Grain boundary sliding:

$$\varepsilon_{f,uni} = f\frac{a}{d} \tag{8}$$

Different models give $f = \frac{\pi}{6}$ or $f = \frac{2}{3}$. Since stress does not appear explicitly in (8), if the cavity spacing, a, were constant (and independent of stress), then (8) would be a strain-rate independent ductility. Moreover, (8) only applies on the assumption that the rate controlling process (sliding) is slower than unconstrained cavity growth, so it must produce a smaller ductility than unconstrained growth. Hence (8) represents the lower shelf ductility.

Lower shelf ductility occurs for constrained cavity growth, when the strain is dominated by matrix creep or grain boundary sliding, and if further cavity nucleation does not take place during creep.

Qu.: Is the lower shelf really horizontal?

On the other hand, if we allow continuous cavity nucleation in a model whose strain is dominated by grain boundary sliding, then (4) and (8) imply, for uniaxial stressing,

$$\varepsilon_{f,uni} \propto \left(\frac{4}{3}\sigma\right)^{-\phi} \propto \left(\frac{4}{3}\right)^{-\phi} \left(\dot{\varepsilon}_c^{\min}\right)^{-\phi/n} \tag{9}$$

In this case the "lower shelf" would not really be horizontal. Using $\phi = 0.63$ for n =4 (derived above) the ductility would increase with reducing strain rate, but with a relatively gentle gradient on a log-log plot of -0.16. If this were correct there would be a ductility minimum between the transition region (unconstrained cavity growth) and the "lower shelf" (constrained cavity growth). This could potentially explain the "ductility dip" behaviour in certain temperature ranges. However, I do not know how realistic this suggestion may be.

Multi-Axial Creep Ductility

Qu.: Are models for multi-axial ductility so well developed?

No.

In the transition region, the observed sensitivity of the creep ductility to stress triaxiality is predicted reasonably well. But the sensitivity of the lower shelf to stress triaxiality, so crucial in reheat cracking, is harder to explain.

Qu.: What does our toy model say about the transition region?

Substituting Equs.(5) and (6) into $\bar{\varepsilon}_f \sim \dot{\varepsilon}_c^{\min} \cdot t_f$ gives, (10)

$$\bar{\varepsilon}_f \propto \dot{\varepsilon}_c^{\min}\left(\sigma_1 + \sigma_H\right)^{-(1+3\phi)} \propto \dot{\varepsilon}_c^{\min}\bar{\sigma}^{-(1+3\phi)}\left(\frac{\sigma_1 + \sigma_H}{\bar{\sigma}}\right)^{-(1+3\phi)} \propto \left(\dot{\varepsilon}_c^{\min}\right)^{1-\frac{1+3\phi}{n}}\left(\frac{\sigma_1 + \sigma_H}{\bar{\sigma}}\right)^{-(1+3\phi)}$$

Dividing by the uniaxial ductility from (7) gives,

<u>Unconstrained cavity growth</u>:

$$\frac{\bar{\varepsilon}_f}{\varepsilon_{f,uni}} = \left(\frac{3}{4} \cdot \frac{\sigma_1 + \sigma_H}{\bar{\sigma}}\right)^{-(1+3\phi)} \tag{11}$$

As an illustration consider a stress state $\sigma_1 = 300$, $\sigma_2 = 200$, $\sigma_3 = 60$ which gives $\bar{\sigma} = 208.8$ and $\sigma_H = 186.7$. Using $\phi = 0.63$, as derived above, this gives $\frac{\bar{\varepsilon}_f}{\varepsilon_{f,uni}} = 0.2$.

This compares with using the Spindler fraction, Equ.(1), with empirically determined parameters p = 2.38 and q = 1.04, which gives $\frac{\bar{\varepsilon}_f}{\varepsilon_{f,uni}} = 0.15$. Consequently our toy model is not bad. Recall that no empirical input on multi-axial behaviour has been used in the model. The only empirical input was the strain-rate dependence of the uniaxial ductility, which was used to derive $\phi = 0.63$. We conclude that…

The stress state dependence and strain rate dependence of creep ductility in the transition region is explicable in terms of unconstrained cavity growth models together with a model for cavity nucleation expressed by Equ.(4).

Qu.: What does our toy model say about the lower shelf?

In constrained cavity growth we expect the strain at failure to be given by an expression like (8) despite being multi-axial, i.e., $\varepsilon_f = f\frac{a}{d}$. If the cavity spacing, *a*, were stress-state independent, it would follow that the creep ductility on the lower shelf would be stress state independent.

In putting $\varepsilon_f = f\frac{a}{d}$ it is ambiguous whether we mean the Mises strain at failure, i.e., $\bar{\varepsilon}_f = f\frac{a}{d}$ or the maximum principal strain at failure, $\varepsilon_{1f} = f\frac{a}{d}$. We shall assume the former here, though this has not been justified. (Hales & Spindler, 1997, prefer to assume that the maximum principal strain at failure is appropriate).

A stress state dependence of *a* can be introduced via Equ.(4), $a = C(\sigma_1 + \sigma_H)^{-\phi}$. Note that this need not be continuous cavity nucleation during creep. All the cavities may be nucleated on first application of load due to the effects of plastic straining. In this case constrained cavity growth suggests that $\bar{\varepsilon}_f \propto (\sigma_1 + \sigma_H)^{-\phi}$. Hence,

Constrained cavity growth: $$\frac{\bar{\varepsilon}_f}{\varepsilon_{f,uni}} = \left(\frac{3}{4}\cdot\frac{\sigma_1 + \sigma_H}{\bar{\sigma}}\right)^{-\phi} \tag{12}$$

Although this does produce a stress-state dependence on the lower shelf, it is a much reduced sensitivity compared with the transition region. For example, for the same illustration as above, i.e., $\sigma_1 = 300$, $\sigma_2 = 200$, $\sigma_3 = 60$, $\phi = 0.63$ we get $\frac{\bar{\varepsilon}_f}{\varepsilon_{f,uni}} = 0.7$, which is a far smaller reduction than in the transition region.

If we had assumed $\varepsilon_{1f} = f\frac{a}{d}$ was the failure condition instead, then (12) would become, assuming the Mises flow rule,

Constrained cavity growth: $$\frac{\bar{\varepsilon}_f}{\varepsilon_{f,uni}} = \left(\frac{2}{3}\cdot\frac{\bar{\sigma}}{\sigma_1 - \sigma_H}\right)\left(\frac{3}{4}\cdot\frac{\sigma_1 + \sigma_H}{\bar{\sigma}}\right)^{-\phi} \tag{13}$$

For our illustrative example this gives $\frac{\bar{\varepsilon}_f}{\varepsilon_{f,uni}} = 0.86$, and so is even less sensitive to stress state. Hence we conclude..

Constrained cavity growth models imply a much reduced sensitivity to stress state on the lower shelf. But a combination of lower shelf ductility and a stress state sensitivity characteristic of the transition region is required to rationalise reheat cracking in 316H. So there is still something which we do not understand about this.

Qu.: Can we understand lower shelf stress state sensitivity at all?

We can always fall back on the very general, if mechanistically vague, observations made above. Suppose, for a given temperature, the rupture life depends only on, say, the maximum principle stress: $t_f = F(\sigma_1)$. Also assume that the Mises equivalent creep strain is given by $\bar{\varepsilon}_c = A\bar{\sigma}^n t^m$ (perhaps secondary creep in which case $m = 1$).

Hence, $\bar{\varepsilon}_{f,uni} = A\sigma_1^n (F(\sigma_1))^m$ whereas the multi-axial ductility is $\bar{\varepsilon}_f = A\bar{\sigma}^n (F(\sigma_1))^m$.

So the ductility ratio is $\frac{\varepsilon_f}{\varepsilon_{f,uni}} = \left(\frac{\bar{\sigma}}{\sigma_1}\right)^n$. For our illustrative example above, and with $n = 4$, this gives $\frac{\varepsilon_f}{\varepsilon_{f,uni}} = 0.23$, which is not bad compared with the Spindler fraction.

However the assumption that rupture life depends upon the maximum principal stress alone is an assumption which has not been justified by any mechanistic model. In R5 we would calculate creep rupture time using the rupture reference stress – which is a different quantity entirely.

Qu.: What is the stress distribution on the grain boundaries?

It was mentioned above that the boundary condition at the surface of a cavity is generally taken in cavity growth models based on vacancy diffusion as being a radial stress of $\sigma_r = 2\gamma_s / R$, where γ_s is the metal surface energy (surface tension). Moreover this is a very low stress if the cavities are approaching a micron in size, or larger. (It becomes a large stress only for cavities of the order of tens of nanometres). The stress between cavities is much larger, because the average stress must equilibrate with the gross continuum stress. It is this steep stress gradient which drives the vacancy diffusion towards the cavities.

However, it is worth pausing to consider how odd this stress distribution appears from the perspective of a structural analyst. It is illustrated in Figure 8, below,

Figure 8

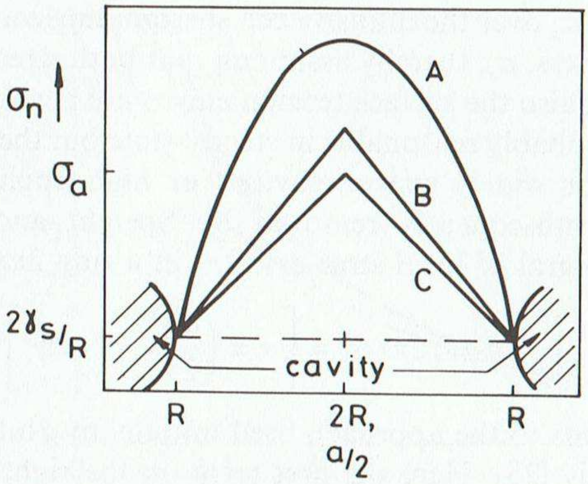


FIG. 4.8. Predicted steady-state stress distributions along the grain boundary for closely spaced cavities ($R = a/4$). A, Speight and Beeré [60]; B, Hull–Rimmer as modified by Vitovec [35] (eqn. (4.40)); C, Hull and Rimmer [23], simplified model.

A stress analyst would expect a stress concentration at the surface of the spherical cavities, with an initial elastic SCF of ~3 for uniaxial stressing, and a stress *minimum* midway between the cavities. This is the exact opposite of what is implied by vacancy diffusion models. Whilst creep will relax the initial elastic distribution, there will remain an SCF near the cavities. Continuum creep flow cannot explain how the stress near the cavities becomes smaller than the mean stress.

The received wisdom is that there is a transient during which the stress elevation near the cavities is eliminated by vacancy diffusion. The stress distribution near a cavity surface is assumed to evolve as illustrated by Figure 9 below,

Figure 9

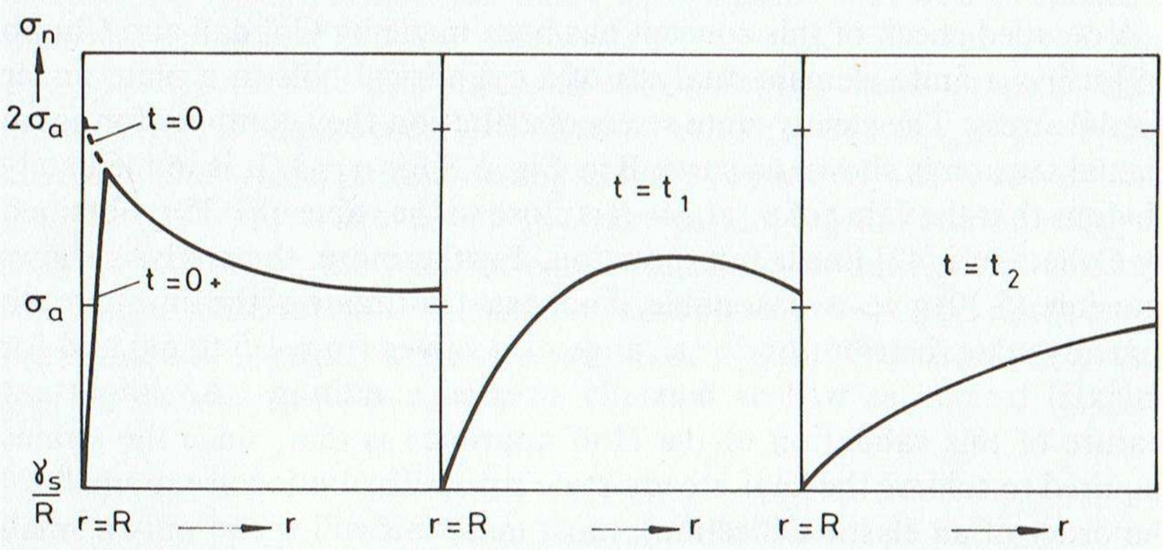


FIG. 5.9. Schematic representation of diffusional relaxation of elastic stress distribution tangential to the cavity surface ($r = R$); $t_2 > t_1 > (t = 0+) > (t = 0)$.

Crude estimates suggest that the time, t_s, to establish the steady state distribution due to vacancy diffusion, i.e., Figure 9, is given as a fraction of the creep rupture life as,

$$\frac{t_s}{t_f} \sim 2\frac{\sigma}{E} \qquad (14)$$

If this is true, then it is reasonable to ignore this transient period, because the RHS of (14) will generally be less than about 0.002.

Qu.: Is it treacherous to use conventional stress analysis at the microscopic level?

Yes.

Figures 8 and 9 illustrate why. Vacancy diffusion is just not included in conventional engineering analysis based on equilibrium, compatibility and a continuum formulation of the material's constitutive behaviour.

Qu.: How important is material composition to creep ductility?

One might reasonably guess that creep ductility would be sensitive to compositional details. In the context of 316H, Marc Chevalier has made the discovery that nickel-equivalent content seems to be the key to its cracking behaviour (Refs.[18] and [19] – the latter is in draft only at May'13).

The discovery was initially made in the context of HPB/HNB s/h bifurcations, Ref.[18]. It was noticed that casts from a certain supplier (Brown-Firth) were (massively) over-represented in the cracked bifurcations removed from the boilers. These casts turn out to have low nickel-equivalent content compared with the other bifurcation casts – see Figure 10.

Marc has now added to this Schaeffler diagram some of our old favourite items which have suffered from reheat cracking (S4 welds, R07 forging). They also lie at very low nickel-equivalent. On the other hand a highly constrained geometry, the HPB s/h pintle-tubeplate welds, were inspected at HPB R3 in 2009 without discovering any cracks. (HPB R4 is to be inspected in 2011). This material lies at higher nickel-equivalent levels. So the plant evidence is crudely consistent that creep cracking problems in 316H align with low nickel-equivalent content. However the story is not completely clean – there are exceptions.

Qu.: Why does low nickel-equivalent cause cracking?

Low nickel-equivalent is expected to correlate with a higher percentage of delta ferrite. Evidence now accumulating is that cavities tend to form preferentially on ferrite, or ferrite/austenite, grain boundaries. Naively, you would expect the ferritic phase to be less creep resistant.

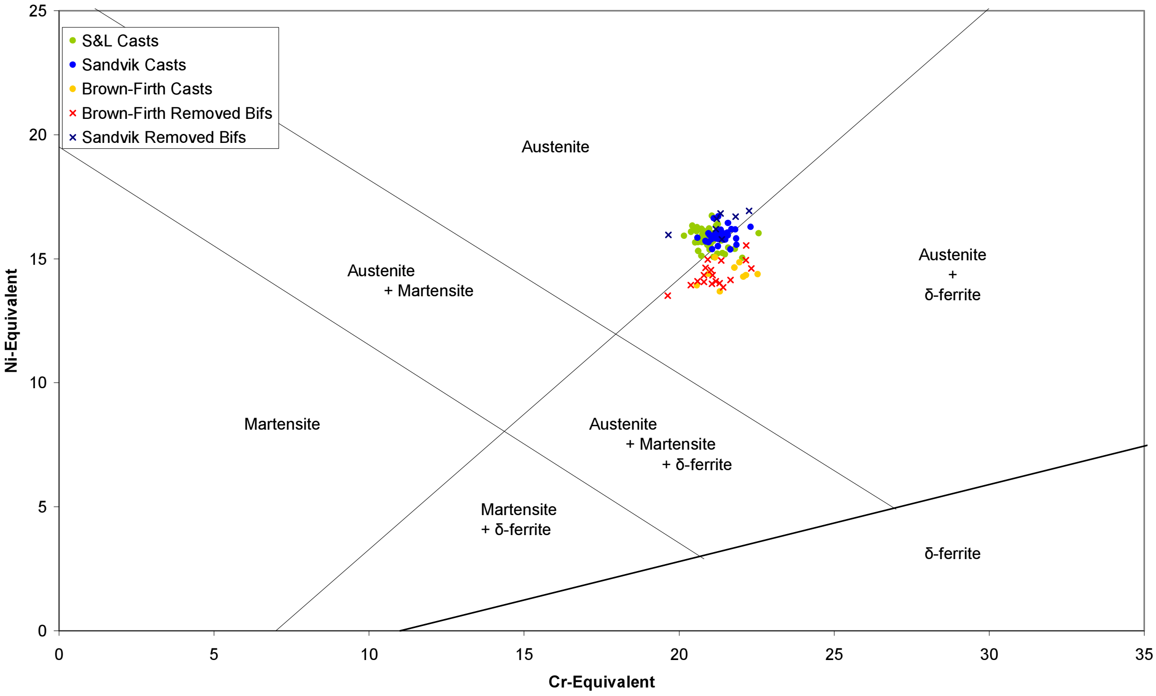
Qu.: What is "nickel-equivalent"?

In 300 series stainless steels some elements act to encourage the formation or stabilisation of austenite phase (as opposed to ferritic phase). The principal "austenitising" element is nickel, but other elements also have this effect, e.g., carbon. There is an empirically determined weighted sum of the concentrations of the austenitising elements which is called the "nickel equivalent", i.e., equivalent to nickel in terms of stabilising austenite.

On the other hand, some elements inhibit austenite, the principal example being chromium. (Chromium is present to imbue the steel which its stainless characteristic, i.e., corrosion resistance). Again, there is an empirically determined weighted sum of the concentrations of the austenite inhibiting elements which is called the "chromium equivalent". The Schaeffler diagram is a plot of nickel-equivalent against chromium-equivalent, not simply nickel versus chromium.

From memory, the materials which Marc considered which had low nickel-equivalent did tend to have low nickel, but many (especially the Brown-Firth bifurcation casts) also had low silicon. So it's not just low nickel which may be to blame.

Figure 10: Schaeffler diagram for HPB/HNB s/h bifurcation material compositions. The line positions denote the phase changes based upon material at room temperature



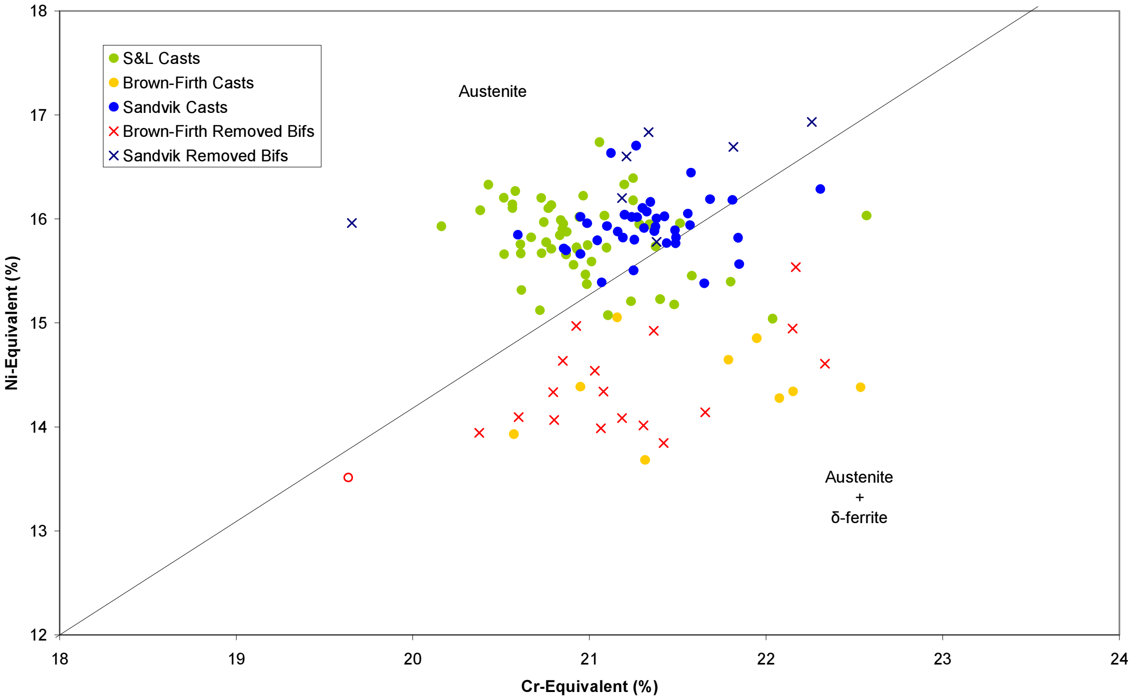
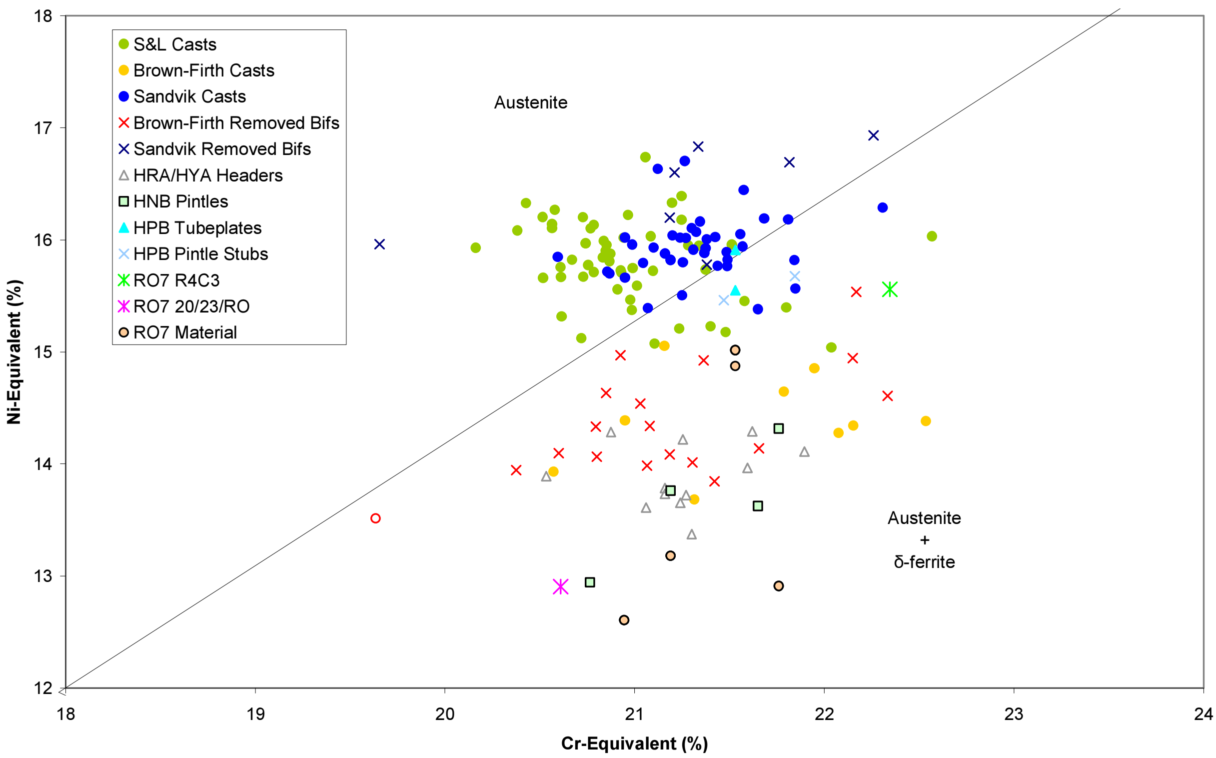


Figure 11 Schaeffler diagram including data for other plant components



References

[1] B.L.Baikie, R.A.W.Bradford, R.Hales, D.A.Miller, M.W.Spindler and R.A.Stevens, “Reheat Cracking in Austenitic Stainless Steels – Current Status of Understanding”, EPD/AGR/REP/0346/97 (Issue 1 dated November 1997): Examples of reheat cracking].

[2] R.A.W.Bradford, "Finite Element Modelling of Reheat Cracking Initiation in Austenitic Weldments", Institute of Mechanical Engineers Conference Transactions, International Conference on "Assuring It's Safe", 18-19 May 1998, Heriot-Watt University, Edinburgh, UK, paper C535/023/98, pp 287-295.

[3] R.A.W.Bradford, “A Summary of Residual Stress Analyses and Crack Initiation Models Completed To-Date under the Generic Reheat Cracking Programme”, EPD/AGR/REP/0328/97 (October 1997).

[4] D.Hull, D.E.Rimmer, *Philos.Mag.*, 1959, **4**, 673.

[5] M.V.Speight, J.E.Harris, *Metal.Sci.J.*, 1967, **1**, 83.

[6] J.R.Rice, D.M.Tracey, “On the ductile enlargement of voids in triaxial stress fields”, J.Mech.Phys.Solids, 1969, **17**, 201-217.

[7] R.P.Skelton, “Note on the effect of grain boundary cavitation on ductility”, *Phil.Mag.*, 1967, **15**, 405-409.

[8] R.P.Skelton, “Diffusional creep strain due to void nucleation and growth”, *Metal.Sci.*, 1975, **9**, 192-194.

[9] B.F.Dyson, “Constraints on diffusional cavity growth rates”, *Metal.Sci.*, 1976, **10**, 349-353.

[10] B.F.Dyson, “Constrained cavity growth, its use in quantifying recent fracture results”, *Canad.Metall.Quart.*, 1979, **18**, 31-38.

[11] I-W.Chen, A.S.Argon, “Diffusive growth of grain boundary cavities”, *Acta.Metall.*, 1981, **29**, 1759-1768.

[12] A.C.F.Cocks, M.F.Ashby, “Intergranular fracture during power law creep under multiaxial stresses”, *Metal.Sci.*, 1980, **14**, 395-402.

[13] A.C.F.Cocks, M.F.Ashby, “Creep fracture by coupled power-law creep and diffusion under multiaxial stress”, *Metal.Sci.*, 1982, **16**, 465-474.

[14] R.P.Skelton, I.W.Goodall, “Review of Cavitation and Multiaxial Creep Ductility in 316 Steel”, Imperial College Consultants Ltd, report ME/199/05, January 2000.

[15] H.E.Evans, “Mechanisms of Creep Fracture”, Elsevier Applied Science Publishers, 1984.

[16] R.Hales, M.W.Spindler, “Interim Results on the Multiaxial Creep Ductility of Ex-Heysham 1 Superheater Header Type 316H”, EPD/GEN/REP/0222/97, November 1997.

[17] M.W.Spindler, R.Hales, R.P.Skelton, “The Multiaxial Creep Ductility of an Ex-Service Type 316H Stainless Steel”, Proc.CFEMS9 Conference, Swansea, 2-4 April 2001.

[18] M.Chevalier, “HPB/HNB: Analysis of Material Compositions of Cracked

Superheater Bifurcations Compared with the Total Bifurcation Population", E/REP/BBGB/0055/AGR/09 February 2010.

[19] M.Chevalier, "Type 316 Stainless Steel Components and their Propensity to Failure based upon Cast and Chemical Composition", E/REP/BBGB/0077/AGR/11, *still not issued at May 2013 – feel free to harangue March mercilessly to do so.*