Theory of dislocations: pre-war nucleation, post-war crystallization and present growth

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My interest in plastic deformation began in London in August 1939. This was not a proper time to start any research, but it happened in this way. At the end of my first year at the School of Mines in Paris I had to make a study trip abroad in the summer of 1939. I chose England, and while in London, visiting the Royal School of Mines, I spent some time in a laboratory where they were studying rock mechanics and ground movements around mine galleries. I was so interested by these researches, and the way they were attacked at both ends, experimental and theoretical, that I decided I should also carry out some research in the field of plastic deformation of rocks. The war interrupted my visit.

Although at that time in France excellent research was being done on soil deformation, it was in laboratories belonging to the administration of bridges and roads, while I belonged to that of mines and metallurgy, which had no laboratory working on rock plasticity. It happened, some time later, that while I was visiting a steel plant and its laboratories, where interesting studies were made on the creep of alloy steels, it appeared to me that this subject was in the field of plasticity, which interested me, but was easier to study than rocks, with a broader field for experimentation, interpretation and application. Thus I decided to shift my interest to creep in metals and plastic deformation; and when I was entrusted with the task of founding a new Metallurgical Research Centre at the School of Mines, I was very glad that the Council agreed that the first line of research of this Centre would be plastic deformation of metals.

At that time during the war, in the absence of contacts abroad, the best way to start a study of that kind was to rely on bibliography. I was lucky that the library

of the School of Mines was one of the best in Paris for sciences and techniques. I spent hours and days there, and discovered the pioneering work already accomplished in the field of metals plasticity.

To begin with, I read papers about the experimental part of the work. In one sense, it is as old as the civilization of metals, but its scientific development is little less than a century old.

The first to observe slip lines on deformed metal polycrystals were Osmond, Ewing and Rosenhain, about the turn of the century. The first to propose an appropriate law to describe tensile curves of metal samples was Ludwik in 1909; his well known formula,

$$P = P_0 + aL^n,$$

relates load P to elongation L; P_0, a and n are empirical constants ($n < 1$).

On deformed single crystals, the first to see slip lines was Andrade just before World War I; but one has to wait until 1922 and 1923 to find the basic work on plasticity of metal crystals by Polanyi and Schmid on zinc, and by Taylor and Elam on aluminium. They studied carefully the orientation of slip bands, by direct and indirect means. Schmid introduced the notion of 'critical shear stress' and formulated the well known Schmid's law. Later studies in the 1930s brought to light such phenomena as 'easy glide' (the small linear hardening on crystals of metals like Cd, Zn, Mg and, partly, Al), or the rotation of lattices during deformation, and the accompanying curvatures and fragmentations, evidenced by X-ray 'asterism.'

Schmid & Boas's well known book *Kristallplastizität* gave an excellent picture of the state of that science in 1935. Among these observations, two facts were fundamental but puzzling: the low actual values of critical shear stress, compared with the theoretical values for perfect crystals; and the heterogeneous distribution of the strain, concentrated in slip bands separated by seemingly undeformed lamellae. One had also to explain the influence of temperature on the resistance of metals to deformation, more particularly on the critical shear stress. We are thus led to the theoretical side of the work, known at that time as 'theory of dislocations'. Successive steps were as follows:

1. In 1919, Ludwik introduced the general idea that internal stresses (due to deformation, i.e. work-hardening, or to foreign atoms in the case of alloy hardening) were the origin of hardening.

2. In 1925, Becker gave his fundamental formula about thermal activation in crystals to explain the influence of temperature.

3. In 1923, Prandtl, to explain elastic hysteresis, invoked a new type of glissile structural defect.

4. In 1929, Dehlinger invoked similar defects to explain recrystallization of cold-worked metals and called them 'Verhakung'.

5. In 1932, Smekal published a book on structural defects, and introduced the idea of 'structure sensitive' properties (i.e. sensitive to structure defects) and 'structure insensitive' ones, governed by the bulk lattice structure. Smekal con-

sidered these defects as 'internal notches', from where originated glide or fracture; they were related to the 'mosaic structure' much studied at that time.

6. In 1934, came the big start, due independently to Polanyi, G. I. Taylor and Orowan, who gave the first good picture of 'dislocations'. Taylor's positive and negative dislocations were distributed by strain in regular arrays, leading to a power-law (with exponent 0.5) for strain-hardening, which is still observed; Orowan combined Becker's formula for thermal activation to Smekal's internal 'stress-concentration factors', which led him to the well known Becker–Orowan formula, basic for creep and thermally activated plasticity.

7. In 1938, W. G. and J. M. Burgers tried to make a synthesis of the ideas of Smekal, Becker-Orowan and Taylor, by giving a rather complete description of plastic deformation of mosaic crystals. Their theory was completed by Kochendörfer in his book *Plastische Eigenschaften von Kristallen* (1941) and by P. Laurent in 1945, who took Dehlinger's old concept translated into French by 'accrochage' and was the first to give a correct picture of thermal activation, applying the Einstein–Debye theory of thermal activation to describe oscillations of atoms around a defect.

8. In 1939, J. M. Burgers imagined the 'screw-dislocation' and introduced the general description of dislocations, defined by the vector bearing his name.

9. In 1940, Peierls calculated the friction force opposing the movement of dislocations.

This was the state of affairs about the time I started my work on the subject. I had inherited from my grandfather Georges Friedel, the crystallographer, an inclination to always look as carefully as possible with a microscope before risking a theory; and Pierre Chevenard, who led my first steps in metallurgical research, taught me how to record curves of good quality and to interpret them by precise methods.

Having the good fortune of access to an ample supply of high purity aluminium from the Pechiney Co., and to have available the electropolishing technique of Pierre Jacquet, as well as of an excellent tensile and creep 'Chevenard' machine, I started working on the deformation of aluminium single and polycrystals. I looked at electropolished samples through a microscope while they were deformed in different tensile machines, sometimes interrupting the test for repolishing, general or local; I recorded stress–strain curves and deep curves, and analysed them by a differential method.

It is interesting to recall some of the points I was able to establish at that time:

1. Sharp slip bands appear rapidly and stop growing after a certain strain; eventually they can appear in successive jerks, prolonging each other, sometimes not exactly on the same plane. Some slip bands of other type are rather diffuse, and grow slowly and continuously; this type is more frequent at higher temperatures or slow deformation.

2. The repartition of strain is very heterogeneous, even in single crystals, where various types of 'deformation bands' are formed, leading eventually to the forma-

tion of 'kink bands', similar to those discovered in 1942 by Orowan on Zn and Cd. In polycrystals, the grains are fragmented in subgrains and holes are formed (the density decreases).

3. At high temperatures, the slip lines become thicker and wavy; their plane may change: e.g., on aluminium I discovered (110) or (11x) slips. I studied the formation of larger subgrains in creep, with sharp sub-boundaries, and followed their rotation: the latter becomes very strong near boundaries, leading eventually to nucleation and formation of new grains. I showed in 1949 that this subgrain formation in creep, which by the way was discovered in 1935 by Jenkins & Mellor, was actually a case of the 'polygonization' newly discovered by Orowan & Cahn.

4. Stress–strain curves of polycrystals are well represented by parabolic laws of the Ludwik type: $\sigma = \sigma_0 + a\Sigma^n$. But, thanks to the quality of the 'Chevenard' machine and to the differential method used for analysis of their shape, I was able to show in 1949 that actually these curves followed, not a single law of this type, but *two successive parabolic laws*. This work was continued with my collaborator Bernard Jaoul; the 'transition point' or transition zone between these two parabolas was studied and explained, and the whole work has been included in Jaoul's well known book *Étude de la plasticité et application aux métaux*, published after his death.

5. The same differential method applied to creep curves proved that the *transient creep* of many metals (Al, Al-based alloys, Zn, Mg, Ni, Fe, Cu) as well as of glass and plastics, follows a parabolic law: $\epsilon = \epsilon_0\,\phi + a\,t^m$, found actually in 1939 by Esser and by De Lacombe, with m ranging from 0.19 to 0.83 (in a few cases, m is still smaller, e.g. 0.13 or 0, which corresponds to logarithmic creeps), instead of the value $m = \frac{1}{3}$ proposed by Andrade in 1910.

I tried to develop a phenomenological theory based on these observations and using the idea of a distribution of Smekal's 'stress concentration factors': plastic strain was considered as the sum of individual elements, each corresponding to the development of an individual slip band, characterized by a critical stress related to its stress-concentration factor. The stress–strain curve would reflect the distribution law of these factors. From these, by a very simple hypothesis, it was possible to calculate the shape of transient creep. I could show that if stress–strain curves are parabolic, so also should be the transient creep curves, which is true; this theory could account for the variation of the exponent m.

This piece of phenomenology was encouraging. But there came the famous Summer schools in Bristol, in 1947 and 1948, which I had the privilege of attending. This was an exceptional place at that time for metal physics, where, under the guidance of Professor Mott, the most brilliant metal physicists, old and young, but mainly young, used to work, get together and discuss, and create the new theories about dislocations and vacancies. Others will describe the development of this part of physics better than I can. It suffices to mention, *inter alia*, Mott & Nabarro's theory of hardening (a modern version of Ludwik's old idea), the Cottrell effect, Frank's avalanches, Frank–Read sources, Nabarro–Herring creep and Frank's network.

I perceived an approach quite different from my 'phenomenology', ignoring details and going directly to the elemental mechanisms. This approach requires, not so much a detailed analysis of recorded curves, as a type of experimentation where rapid changes in parameters, like temperature, strain rate and loading rate enable one to measure activation energies and activation volumes, and to clarify the mechanisms. This method has been the base of many fruitful studies. But it would not have achieved such a success without the development of electron microscopy: one could now see dislocations. It was very exciting, but also disappointing in some sense, as a mess of curved or forked lines, of tangles and complex systems, obscured the simple, theoretical, schematic structures corresponding to the basic mechanisms. A careful and patient search was necessary to identify the latter.

Grain boundaries were also very difficult to observe by electron microscopy on deformed metals. I was interested in the subject, as, studying creep and the 'equicohesion temperature', I had shown that grain boundaries should act as sources of dislocations at high temperature, and introduced with J. Friedel in 1954 the concept of 'grain boundary dislocations' to explain viscous glide and its interaction with neighbouring grains.

It happens that the study of the condition of formation of these complex structures is related in some way to my old work on fine analysis of the stress–strain or creep curves: it is more of a 'phenomenological' nature than related to the elementary mechanism. For example, the 'transition point' of stress–strain curves is related to the point where subgrains become predominant, or 'cells', as the electron microscopists call them. These cells are largely studied now, as well as the influence of different parameters (temperature, biaxiality of stress, etc.) on their structures. The role of fixed obstacles (such as precipitates) and their relation to cell formation is understood. I have shown that rheological models based on the distribution of such obstacles can explain in many cases the exact shape of transient creep curves (exponent m).

It is worth noting in conclusion that this chapter of metal physics is an illustration of the thermodynamics of an irreversible process, and of Prigogine's 'structuration' principle on dissipative systems.