NATIONAL ACADEMY OF SCIENCES

EGON OROWAN

1901—1989

A Biographical Memoir by

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Biographical Memoir

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Egon Orowan

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August 2, 1901–August 3, 1989

BY F. R. N. NABARRO AND A. S. ARGON

EGON OROWAN DIED in the Mount Auburn Hospital in Cambridge, Massachusetts, on 3 August 1989, a day after his 87th birthday. He is buried in the Mount Auburn Cemetery. Together with G.I. Taylor and Michael Polanyi, he was responsible for the introduction of the crystal dislocation into physics as the essential mediator of plastic deformation. Though he occasionally spoke at meetings concerned with science and technology policy, and wrote letters to the press on a number of topics, he was an essentially private person and left no biographical notes. In compiling the present Memoir, FRNN has been principally responsible for the period 1902-1951, which Orowan spent mainly in Europe, and ASA for the period 1951-1989, when Orowan was affiliated with the Massachusetts Institute of Technology.

1. ANCESTRY AND EARLY LIFE

Egon Orowan (Orován Egon in Hungarian) was born in Obuda, a part of Budapest (R.1.)[1] His father, Berthold

Prepared as a Biographical Memoir for the Royal Society of London and the U.S. National Academy of Sciences.

[1]References preceded by the letter *R* refer to numbered papers deposited in the archives of The Royal Society.

Orowan, was a mechanical engineer (R2) and "managed some kind of factory in what is now Rumania" (R3). Berthold's parents were Jakob Orowan and Maria Neubauer, and Jakob was the son of Heinrich Orowan. The origin of the name Orowan is not clear. It sounds Slavonic to Hungarians and Hungarian to Slavs, and, according to family tradition, Heinrich was the first to use it. Egon told one of the writers that Orowan meant "a range of hills", but this meaning does not seem to be familiar to speakers of Hungarian or Czech.

Egon Orowan's mother was Josze (Josephine) Spitzer Ságvári. Her father, Mor Spitzer Ságvári, was originally named Spitzer (R3) but "became bankrupt in an agricultural crisis, went to Budapest and magyarized. Egon Orowan says his name was Mor, but Lorent [his nephew, Lorant Toth of Hungary (R4)] says it was Moris." One of Orowan's cousins, Endre Ságvári, "was an excellent Communist," and a park in Budapest was named for him. To compensate, the Orowans were also related to either Goering or Goebbels (R3). Orowan's wife, Jolan Schonfeld, was a pianist, who studied under Bela Bartok in the Budapest Academy of Music about the year 1919 or 1920. Here she met Egon Orowan, and they became friends, but were not at that time deeply attached. She stayed in Germany until about 1938, then left her work and all her possessions, and fled to her sister in Paris. After a year she found work as a domestic servant in England. She and Egon Orowan met again, and married on 20 January 1941.

According to the biographical note in his Berlin Dr. Ing. thesis (R2), Orowan studied at the Staatsobergymnasium in the IX district of Budapest, taking his Reifeprüfung in June 1920. In the academic years 1920/21 and 1921/22 he studied physics, chemistry, mathematics and astronomy in the University of Vienna. He did practical work in the winter semester of 1922, and began his studies at the Technical University of Berlin in the summer semester of 1928. After initially studying mechanical engineering, then electrical engineering, he transferred to physics under the influence of Professor R. Becker. At the end of 1928 he became Becker's assistant, and underwent his Diplom-Hauptprüfung at the end of the winter semester 1928/29. He began his doctoral research in autumn 1931, and at the time he presented his thesis (1932) he was assistant to Professors M. Volmer and W. Westphal. One of his papers is dated 8 July 1933 and addressed from Berlin-Charlottenburg; another, received 30 August 1933, describes him as "zur Zeit in Budapest". As Orowan explained in his talk at the Sorby Centennial Meeting (R4A): "For a time I could not find employment, and I lived with my mother, rethinking the results of my experiments of the last three years." (Orowan's father died in January 1933).

A letter (R5) from Professor László Bartha, Director of the Research Institute for Technical Physics of the Hungarian Academy of Sciences, says that Orowan worked with the Tungsram Research Laboratory between 1936-1939, under the supervision of Dr. Imre Bródy. According to this letter and to (R6), Bródy invented the krypton-filled light bulb. With the help of Mihály (Michael) Polanyi, he developed a new process for extracting krypton from air. Bartha's letter says that "Orowan was the person, who helped him to verify the large scale separation of krypton from air by fractioned distillation of liquid air. He played an important role at the installation of a pilot plant for krypton manufacturing in a small town—Ajka—about 80 miles from Budapest. I could not find any papers or notes of him from that period."

By 1937, Orowan had moved to Birmingham (R2). The reasons for his move are not clear. According to his daughter (R3) "my understanding (which may not correspond to reality; sometimes things were hidden from me as a child and never rose to the surface) is that after a couple of years managing that tungsten process in the factory, he was offered a job in Birmingham sometime around 1937 and he went there, well before Hitler really started to misbehave." [Hitler had remilitarized the Rheinland in March 1936, occupied Austria in 1938, and Czechoslovakia in 1939].

Experimental Work in Berlin

Orowan's doctoral thesis was not on the topic of crystal plasticity on which he started to work under Richard Becker, although his first published paper (1)[2] and his most outstanding contribution to physics (9) were on this topic. His thesis was on the cleavage of mica. His own account (R4A) is that: "The change of the subject was my fault, not Becker's. I received the problem when I was running across the main court of the Technische Hochschule one day; a fellow student ran along the other diagonal, we came within earshot near the center, and he shouted to me: 'What is the tensile strength of mica?' I shouted back 'I will tell you tomorrow.' This was the start of the doctoral thesis; I informed Becker about it when it was finished . . . in fact I could have done little if I had studied at an efficiently organized university which took care of all the students' time."

In (R4A) Orowan claims that this work "represented the first confirmation of the Griffith theory in the case of a crystalline material." The measured ("technical") tensile strength of a crystal is usually orders of magnitude less than the theoretical tensile strength. Griffith showed that this could be explained by the concentration of the applied

[2]References without the prefix R are to publications of Egon Orowan, numbered according to the bibliography at the end of this Memoir.

stress which occurs at the tip of a pre-existing crack. The question arose whether these cracks (or other centres of weakness) were accidental surface defects or were defects necessarily and systematically present in the real crystal, the so-called "Lockerstellen". The technical strength does not seem to vary greatly from one sample to another, and this fact seems to point to the existence of a systematic array of defects.

The precise lamellar cleavage of mica occurs not so much because the binding energy between sheets is small as because the sheets remain elastic even under large stresses in their plane, as is shown by their flexibility. Orowan had the simple idea of stretching a sheet of mica in its plane, using grips much narrower than the sheet, so that the edges of the sheet were free from stress and cracks in the edges would not lead to fracture. The simple idea was less simple in execution; he had to design complicated self-centering grips which ensured that both edges of the strip were simultaneously free from tensile stress. Nevertheless, the sheets were cleaved from blocks whose edges were cut very gently with a diamond saw. These sheets with unstretched edges had tensile strengths up to ten times those usually measured, showing conclusively that the usual tensile strength is controlled by defects in the edges of the sheets. Sheets with stretched edges had strengths which were of the usual order, but differed systematically between those cleaved from blocks cut with a diamond saw and those cut with shears, again demonstrating that the observed strength is determined by surface defects on the edges of the sheets. Orowan gave a detailed discussion of the fracture process in this exceedingly anisotropic material. The explanation is complicated, depending on the ability of a freshly-cleaved pair of surfaces to come together and heal perfectly. There is a footnote, which is interesting in connection with his later preoccupation with seismology and tectonics, in which he points out that the differing plastic properties of mica and of quartz play an important role in geology.

The most important conclusion is that dangerous defects are extremely rare in mica; a sheet may be reduced to half of its original thickness over a region several millimeters long by the peeling-off of imperfect layers, and yet break in another region where the stress is only half as great. This could not happen if the thinned region contained many dangerous defects.

In a paper submitted soon afterwards (5), Orowan struggled with a number of problems of brittle fracture, the effect of sample size, the effect of grain size and the Joffé-effect that a crystal of rock salt is stronger when it is being dissolved in a liquid. The principal new results are that the grain size is the effective upper limit of the size of a crack, so that, in rough agreement with experiments, the fracture stress is inversely proportional to the square root of the grain size, and that plastic flow increases the fracture stress when glide planes and fracture planes intersect, but decreases the fracture stress when these planes coincide, as for basal glide and fracture in zinc. A passing observation (6) was that a sheet of mica usually makes a sound like cardboard when it is struck; a similar sheet cut carefully with a diamond saw rings like steel. The damping in the former case arises entirely from the friction between cleaved layers at the cut edges.

On Crystal Plasticity

Orowan has given a full personal account (R4A) of the way in which he became involved in crystal plasticity: "My own introduction to dislocations happened on a hot Saturday afternoon in 1928. Until less than a year before that, I studied electrical engineering; I was more interested in physics, but my father, a mechanical engineer, knew that one could not make a living from Physics (this was before the Age of Government Contracts). So we compromised on electrical engineering which provided, at Berlin-Charlottenburg, a thrilling course of lectures on electromagnetic theory by Ernst Orlich, and also the nerve-racking tasks of computing, designing, and drawing a transformer, a motor or generator, and (this was my choice) a reversing rolling mill. Once a week, to soothe nerves and collect energy for another six days, I spent a day in the advanced laboratory course in physics offered by Ferdinand Kurlbaum whom I saw once, across the courtyard during the semesters I worked in his laboratory.

At the beginning and the end of the semester I had to acquire his signature for my roll card; this was given by the laboratory assistant who had the necessary rubber stamp. However, Kurlbaum died in 1927 and his temporary successor, the recently appointed professor of theoretical physics, Richard Becker, did not possess a signature stamp. I had to appear in his presence; he signed the card, asked why I, an electrical engineer, worked in the physical laboratory, and I explained. In the course of the following minute my life was changed by the circumstance that the professor's office was a tremendously large room (it was the room in which Gustav Hertz, Kurlbaum's eventual successor, developed the cyclic gas diffusion apparatus with which he separated the isotopes of neon and which was to play a prominent role in the manufacture of the bomb of Hiroshima). Becker was a shy and hesitating man; but by the time I approached the door of the huge room he struggled through with his decision making, called me back, and asked whether I would be interested in checking experimentally a "little theory of plasticity" he worked out three years before. Plasticity was a prosaic and even humiliating proposition in the age of De

Broglie, Heisenberg, and Schrödinger, but it was better than computing my sixtieth transformer, and I accepted with pleasure. I informed my father that I had changed back to physics; he received the news with stoic resignation...

...The assignment was to make single crystals of zinc, tin, etc., and to find out whether they had a trace of plasticity left at the temperature of liquid air: Becker's theory demanded complete brittleness at very low temperatures. Whatever Becker's theory might imply, Polanyi, Meissner and Schmid showed in 1930, before Orowan's equipment and crystals were ready, "that these metals were almost as ductile in liquid air as at room temperature." This was odd, "because the papers of Polanyi and Schmid contained the stereotyped remark that their metal crystals were drawn from the melt and then broken into pieces of suitable lengths in liquid air. When I asked Polanyi about this, he replied "Metal crystals broke in liquid air in those days: today they don't."

Though Orowan was not able to complete his experiments before the work of Meissner, Polanyi and Schmid became known, they formed the basis of his Diplomarbeit in February 1929 and of his first publication. One Saturday afternoon he had only one zinc crystal available. He dropped it on the floor, found it bent, straightened it, left it to anneal for some time, and tried a practice run. To his surprise, it extended with sharp jerks instead of flowing smoothly. From this observation, often repeated, he drew a surprising amount of information and was "led, almost unavoidably, to the concept of dislocation." It must also have led to his interest in the problem of the strain aging of steel. His paper with Becker (1) poses two questions, which are fundamental:-

1. How does local gliding begin and what determines the number of glide processes which initiate every second?

2. How does the local gliding grow into an elementary act of gliding, and what determines the development (rate of gliding and extent of the individual act) of the elementary glide act?

One clear observation was that, in a stress relaxation experiment, the average size of the glide steps remained constant, while their frequency fell as the stress became less.

It is interesting to notice that this purely experimental paper on an unfashionable branch of physics was addressed from the Institute for Theoretical Physics of the Technische Hochschule, Berlin - Charlottenburg.

The real development of this work came only when Orowan returned to Budapest and stayed at home, unemployed and thinking. It led to the papers *Zur Kristallplastizität* **I-V** (7, 8, 9, 13, 14), and to several other papers (15, 16, 17, 19) in which the work is extended or applied to the observations of other workers.

Paper I begins by considering Becker's formula that the rate of deformation u of a crystal gliding under a stress s is given by

$$u = C\exp\left[-\frac{V(S-s)^2}{2GkT}\right]. \tag{1}$$

Here C is an undetermined constant related to question (2) above, S is a stress which should be of the order of the theoretical yield stress of the crystal, and therefore perhaps 1/30 of the shear modulus G. By analyzing this formula, Orowan arrived at the important conclusion that the phenomena of crystal plasticity cannot be explained by thermal fluctuations alone or by the presence of stress concentrations alone; both factors play an important role.

Becker had shown that the ratio $p = S/s$ could be determined by analyzing experimental results. The value of p turned out to be about 2.5. Yet it was well known that for pure single crystals the ratio of the theoretical shear strength S to the observed flow stress s was of order 10^2-10^4. The only solution seemed to be that glide was initiated in small regions where the local stress s was not the applied stress σ, but was enhanced by a stress concentration factor q to the value $s = q\sigma$. Orowan also pointed out that, although Becker's formula (1) led to a rate of deformation which would become unobservably slow if the applied stress was held constant, in practice one adjusts the applied stress to obtain a convenient rate of flow. The consequences of this are developed further in (17). Becker's formula, with or without Orowan's stress concentration factor q, then shows that the flow stress σ at temperature T is related to flow stress σ_0 at zero temperature by the simple formula

$$\sigma = \sigma_0 - B\sqrt{tT}. \qquad (2)$$

This formula fitted the observations for zinc and cadmium rather well. Later arguments have modified the formula, but the basic ideas underlying it remain valid.

Paper II, which is concerned with the theory of creep, is densely argued. It sets out to show that the "static" theory of creep, in which steady-state creep results from a balance between the rate of work hardening and the rate of recovery by softening, must be replaced by a "dynamical" theory based on modifications of Becker's formula. Orowan began by showing that the static theory leads to what has become known as the Baiey-Orowan equation. If the flow stress is σ, the elongation x, and time t, then a steady state is reached when

$$d\sigma = \frac{\partial \sigma}{\partial x} dx + \frac{\partial \sigma}{\partial t} dt = 0. \tag{3}$$

Here $\partial\sigma/\partial x$ is the rate of work hardening, $-\partial\sigma/\partial t$ the rate of recovery. Equation (3) leads to a steady-state creep rate u given by

$$u = \frac{dx}{dt} = -\frac{\partial \sigma}{\partial t} \Big/ \frac{\partial \sigma}{\partial x}. \tag{4}$$

This Bailey-Orowan equation has a remarkable history. Although Bailey developed the physical idea on which the equation is based, it seems that he did not publish the equation itself. Orowan published it, but only in order to show that it is not valid. However, he did not claim to have developed it, but attributed it to Polanyi. A further complication is that, in the form

$$\left(\frac{\partial x}{\partial t}\right)_{\sigma} = -\left(\frac{\partial \sigma}{\partial t}\right)_{x} \Big/ \left(\frac{\partial \sigma}{\partial x}\right)_{t}. \tag{5}$$

it is a mathematical identity. How, then, can it not be valid? (The answer, of course, (13), is that (5) applies only when σ is a unique function of x and t, which is not the case in the actual experiments). Orowan's arguments are quantitative, but essentially demonstrate that at low temperatures $\partial\sigma/\partial t$ is negligibly small, $\partial\sigma/\partial x$ is finite, and $\partial x/\partial t$ is not necessarily small. (Surprisingly, in 1938 Orowan noted (19) that in a number of cases the activation energy for secondary creep is independent of stress, which "means that secondary creep is a flow by strain hardening recovery (thermal softening)". It seems that the mechanisms of "secondary" or "steady-state" creep may be quite different at high and at low temperatures.)

Assuming the rate of work hardening to be constant, Orowan further modified Becker's formula to read

$$u = C \exp\left[-\frac{V(S - q\sigma + bx)^2}{2GkT} \right]. \tag{6}$$

This formula is mathematically inconvenient, and, in applying it, Orowan replaced (6) by the function

$$\left.\begin{array}{ll} u = 0 & \text{for } 0 < \sigma < c \\ u = a(\sigma - c - bx) & \text{for } \sigma > c \end{array}\right\} \tag{7}$$

(It seems that c should be replaced by $c + bx$ in both conditions defining the ranges of σ). Using (7), Orowan was able to show that under usual testing conditions an apparent stress-strain curve of the conventional form would be obtained even in the total absence of work hardening. Consider, for example, a test with a constant rate of increase of stress. When the critical stress is first exceeded, the crystal begins to extend very slowly, while the rate of increase of stress is finite. Thus $d\sigma/dx$ is large. At high stresses the crystal flows rapidly, and $d\sigma/dx$ is small. The same argument explains the observed reductions of the flow stress when a crystal is suddenly unloaded and then reloaded at a finite rate after a brief interval. The crystal begins to flow slowly under a stress lower than that at which it was previously deforming at a finite rate, even if no recovery has occurred. This observation may be misinterpreted as a very rapid rate of recovery.

Paper III is one of the famous group of three papers, one by Orowan, one by G I Taylor and one by Polanyi, in which the idea of the dislocation as the carrier of plastic deformation was first introduced. Figures 1 (a) and (b) are taken from this paper. While the dislocations in edge orientation in Figure 1 (a) are the same as those introduced by Taylor and by Polanyi, Figure 1 (b) is unique in showing a glide zone bounded by a dislocation which takes all orientations, edge, screw and mixed. The paper is explicit in defining the dislocation as the boundary between those regions of the glide plane over which glide has or has not taken place. Orowan's description of the stresses near an edge dislocation does not seem entirely clear. The dislocation acts as a "stress concentrator" in the sense that the shear stresses it exerts on the glide plane are of opposite sign in front of it and behind it. The applied stress increases the numerical value of one of these stresses and decreases the numerical value of the other. An activation process of Becker's type will first occur on the side where the total stress is numerically greatest. Orowan clearly states that the (screw component) produces "sideways" shear stresses in planes lying perpendicular to the glide plane. The stress concentration produced by the stress concentrators in the crystal propagates in the glide plane in the form of dislocations.

Historically, it is clear that Orowan and Taylor developed the idea of dislocations as the carriers of plasticity independently. According to (R4A) "Soon after the appearance of the papers I received a letter with an enclosed galley-proof from Taylor; he wrote that he came to a similar picture, and his paper would soon appear in the *Proceedings of the*

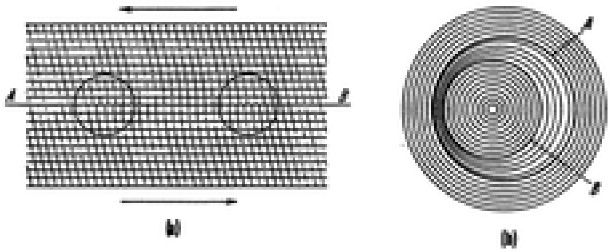

Figure 1: (a) Schematic picture of a local gliding; section in the glide direction perpendicular to the glide plane. The lattice was linear and orthogonal before loading; the dislocation zones are circled. The lattice does not allow the high shear stresses in the glide plane within the dislocation zones to be observed. (b) Schematic picture of a local gliding; view of the boundary of a glide plane. Before loading, the circles were concentric. The dislocation zone lies between the circles A and B.

Royal Society. In fact, he submitted his manuscript several weeks before Polanyi and I sent off ours; but the *Zeitschrift* published faster, and so our papers came out first". It is equally clear that Orowan and Polanyi were in fairly close touch. In Paper I, Orowan states clearly "The plasticity-inducing action of such `dislocations' was recognized by Polanyi several years ago", and in (R4A) Orowan remarks that "Polanyi's term for a dislocation, for several years, had been 'vernier'; in his first publication about it he changed it to 'Versetzung', a term which I also adopted." In fact Orowan recognized (13) that the germ of the idea lay in a model invented by Prandtl before 1913 to explain the elastic after-effect. But the development of the idea into a physical theory was due to Taylor and Orowan. Orowan recorded (R4A) that during his time in Budapest "Slowly I realized that dislocations were important enough to warrant a publication, and I wrote to Polanyi, with whom I discussed them several times, suggesting a joint paper. He replied that it was my bird and I should publish it; finally we agreed that we would send separate papers to Professor Scheel, editor of the *Zeitschrift für Physik*, and ask him to print them side by side. This he did".

Just 50 years after the event, Orowan wrote in a letter (R8):

When my 1934 papers appeared, I received from Taylor a letter with galley-proofs of his papers soon to appear. I did not know who Taylor was, and his seniority, and wrote him that, unfortunately, his theory was all wrong; he replied that I was unable to follow a mathematical argument. However, soon he was convinced, and I spent a night in his house in Cambridge.

The fact was that his theory was no theory at all: he assumed that the crystal had built-in obstacles, and calculated how these would lead to a stress-strain curve *if* there were no pile-ups. He assumed *one* set of slip-planes, and obtained a parabolic stress-strain curve as given by cubic crystals, and the order of magnitude of hardening of cubic metals with intersecting slip.

Since Taylor was an engineer, he assumed that dislocations would be produced abundantly by thermal activation (Griffith's calculation to the contrary is mentioned in your book).

A large part of the paper is devoted to a discussion of the mechanism of jerky extension. This jerky motion is most marked if the crystal has been previously bent and straightened. In translation "One can aptly equate this 'glide hindering' with the barriers to nucleation which appear in phase changes. . . . The 'hardening' which the second curve [taken some time after the first curve] shows in comparison with the first curve therefore does not consist in a real increase in the resistance to glide; it is a sort of *nucleation barrier*, which opposes the generation of the 'glide nucleus', the local gliding." The phenomena of jerky flow is evidence of the blocking of glide sources, and so indirect, but strong, evidence for the existence of glide sources. Orowan could not explain the mechanism of this blocking. It was not until 1947 that Cottrell explained the blocking as being produced by the segregation of solute atoms in the strain field of the dislocation. This mechanism is generally accepted, but it seems that Orowan was never convinced by it.

Paper IV is concerned with showing by quantitative arguments that the "dynamical" theory represents the reality of plastic deformation much better than does the "static" theory with the superposition of recovery. It begins with the analogy between the static theory and Aristotelian mechanics on the one hand and the dynamical theory and the Newtonian dynamics of conservative systems on the other hand, while work hardening in the dynamical theory is analogous to dissipation in Newtonian dynamics. Orowan then gives a formal theory of deformation in the presence of work hardening and recovery on the assumptions that:

(a) In the absence of recovery the flow stress σ is a unique function of the deformation x.

(b) Recovery returns the material to an earlier "state of damage"; further deformation after recovery therefore follows a curve displaced along the x axis from the original curve $\sigma(x)$.

A footnote says that (b) is equivalent to the statement that the totality of all possible states of damage forms a one-dimensional manifold mapped by the value of the flow stress σ, a concept that was developed extensively by E. W. Hart in 1970 and later years.

The result of the analysis is that recovery deforms the stress-strain curve by an inhomogeneous shear parallel to the *x* axis, so that critical stresses such as the flow stress are not altered. Moreover, the distortion of the stress-strain curve in a typical case is by a factor of 1.00002. The temperature dependence of the flow stress cannot be determined by recovery; for low-melting metals there is no recovery below about -20°C and "instantaneous" recovery about 100 - 150°C, whereas the temperature dependence of the flow stress is smooth from very low temperatures to the melting point. For tungsten, the discrepancy between the temperature dependence of the flow stress measured by Becker and that predicted by extrapolation of the rate of recovery at very high temperatures is "several dozen powers of ten". Similarly, recovery fails to explain the rate of the elastic after-effect by many powers of ten.

Paper V "completes" the rate formulae (1) and (6) by allowing for jumps in the direction opposed to that favoured by the applied stress. A term with $-\sigma$ replaced by σ is subtracted from the expressions shown.

The Controversy with F. Zwicky

On the same day (28 October 1932) that the *Zeitschrift für Physik* received Orowan's first paper (1), written jointly with Richard Becker, it received a much longer paper (2) by Orowan alone, entitled (in translation): Comment on the Works of F. Zwicky on the Structure of Real Crystals. The abstract shows a directness and self-confidence unusual in the first independent publication of a young scientist (Orowan was just 30 years old). Again in translation: "Of the two approaches which Zwicky has made to a theoretical foundation for his 'secondary structure hypothesis', one contains an error of calculation, and his assumed effect disappears when it is corrected; the second on the other hand is based on assumptions which are not fulfilled in the majority of crystals".

Zwicky had attempted to explain the structure-sensitive properties of crystals, such as mechanical and electrical breakdown, by showing that the ideal crystal structure represented only a local minimum of the energy, and that the lowest energy state contained regions with different lattice constants, which could act as nuclei of failure or of concentration of the elastic or electrical fields. His first arguments were based on the theory of ionic lattices, with allowance for polarization. They involved approximations. Orowan showed that more reasonable approximations would allow Zwicky's regions of irregularity to occur only in crystals having a dielectric polarizability much greater than any then known, and added that such polarizable ions would not form an imperfect structure of the sodium chloride type, but a perfect structure of a different type. Zwicky's second arguments concerned crystals of the sodium chloride type in which the repulsive forces between neighbouring ions decreased with their separation r more slowly than r^{-6}. Such crystals would have a ferroelectric domain structure, and the domain boundaries would act as nuclei of mechanical or electrical breakdown. Orowan pointed out that most crystals are not ferroelectric at usual temperatures, so that Zwicky's second model does not approach the general problem of structure sensitivity.

Zwicky was not slow to publish his reply in *Helvetica Physica Acta* **6**, 210 (1932). The abstract claims that Orowan's criticism is unfounded (Haltlos), and a footnote to the abstract records that Orowan had sent Zwicky the draft of his paper, Zwicky had responded with his objections, and Orowan had published a revised version without informing Zwicky. The reply points out that neither Zwicky's nor Orowan's calculations are exact, and that (as one would say in more modern terms) a ferroelectric structure with immobile domain walls would not be easily distinguished from a perfect non-ferroelectric crystal. Many other interesting issues are raised, such as the need to distinguish between an accidental mosaic structure (of higher energy than the perfect crystal) and a systematic secondary structure (of lower energy than the perfect crystal), but these have little bearing on Orowan's basic criticisms.

Orowan's reply, also in *Helvetica Physica Acta* (4), has a footnote by the editor and a footnote to the abstract. The first says that the reply is published at the express wish of Dr. Orowan, that it is followed by a response from Dr. Zwicky, that the discussion is then closed, and that the editor takes no responsibility for the content of either paper. The second, by Orowan, says that Zwicky's comments on his original draft contained no objections (*keinerlei Einwände*), but suggestions for improvements if Orowan published his work. After a general discussion of Zwicky's arguments, Orowan explains that the aim of his earlier criticism was exclusively to warn experimentalists against an undue reverence for theory by demonstrating the untenability of the "secondary structure hypothesis". In his reply, which is very moderate in tone, Zwicky leans heavily on calculations by Evjen, and then concentrates on the question of the apparent dielectric constant of a multidomain ferroelectric.

Orowan's final contribution (9) is entitled (in translation): *Comments on a Polemical Work by F. Zwicky.* The title has a footnote of 26 lines, complaining that Zwicky published his reply to Orowan in *Helvetica Physica Acta*, which was not open to Orowan as a non-Swiss. Zwicky had written to Orowan that he had chosen this medium "just in order to avoid further completely useless polemics. In this way I gave you the advantage of the much greater circulation of the *Zeitschrift für Physik*". Orowan began by claiming that "*nichts zu finden ist*" of the calculations on which Zwicky leans in the papers to which he refers. He then proceeded to a detailed refutation of the arguments in Zwicky's paper.

There is no doubt that Orowan understood the physics of the situation and Zwicky did not. It is surprising that the *Zeitschrift für Physik* allowed Orowan to express his criticisms in such a forthright style; perhaps he was the spokesman for some more senior physicists.

Birmingham 1937-1939

Orowan spent the years 1937-1939 in the physics department of the University of Birmingham; of which Oliphant was the head (R7). This was before the days in which the department of metallurgy had acquired its great reputation in the science of metals with the work of Cottrell, Raynor and others. His main contact seems to have been with Peierls, but he also thanks Moon for valuable discussion in the major paper (20) that he wrote in Birmingham.

Although (21) he constructed a "soft" tensile testing machine, his main interest was in a theory of fatigue. He began by assuming that a homogeneous sample such as a single crystal would suffer cumulative work hardening in a fatigue test, however small the applied stress range. (The remarkable ability of single crystals to "self-organize" into inhomogeneous structures was not then known.) Now suppose that there are "soft" regions (e.g. regions of stress concentration or favourably oriented grains) which deform plastically, while the applied alternating stress is too low to cause plastic deformation in the matrix. The stress amplitude in such a region falls below that in the matrix by an amount which is proportional to the plastic strain amplitude in the soft region. This plastic strain amplitude, and the amount of work hardening it produces, are in turn proportional to the excess of the effective stress amplitude in the region above the current flow stress of the "soft" region, which is continually getting harder. All of these changes decrease by a constant factor from each cycle to the next. The total change after infinitely many cycles is the finite sum of a geometrical series. If the original flow stress plus the sum of all the work-hardening increments of flow stress exceeds the fracture stress, the applied stress amplitude is above the safe limit. Developing the argument, Orowan predicted a relation between the stress amplitude A and the number of cycles to failure N very similar to that observed. The discrepancy was that the slope of the graph of log S against log N was -1, whereas the observed slope usually lies between –0.1 and –0.5. Orowan explained that at small stress amplitudes the Bauschinger effect would be important; small reverse strains occur quasi-elastically, without causing work hardening. (Oddly, Orowan did not mention the obvious interpretation of this effect in terms of dislocations.) The model correctly predicted the general observations that the safe stress range is approximately proportional to the static ultimate strength, but does not correlate with ductility, and depends little on the mean (bias) stress in an unsymmetrical stress cycle.

The other paper dating from this period (21) is largely an introduction to *Zur Kristallplastizität* for English-speakers, but it contains one elegant experimental demonstration and one partly successful theoretical prediction. The first consists of bending wires of copper and of iron around a finger. Copper, which work hardens, forms a smooth curve; iron, which shows a yield point, forms a polygon. The second considers the dissipation of energy by a moving dislocation by the analogy of a ball moving down a corrugated slope. "With the energy acquired the ball would be able to continue its movement without further help if the board were rigid . . . as the stress increases, the mechanism of the propagation of gliding will change over from the thermal activation mechanism . . . to this momentum transfer mechanism". Orowan also remarks that "stress-strain curves cannot be used for processes like rolling where the deformation takes place in a less time than the first period of extension", showing that he was already preoccupied with the theory of rolling which was to occupy him for most of the war years.

A short discussion (19) is remarkable for the statement that the activation energy of secondary creep is independent of stress, and "the constancy of the activation energy means that secondary creep is a flow by strain hardening recovery and thermal softening". This approach, amplified in (27), is in apparent conflict with the arguments of *Zur Kristallplastizität*. The boundary between the two processes involved is still not clear (33).

Probably the most important consequence of Orowan's stay in Birmingham was that he introduced Peierls to the problem of the structure of a dislocation core and the stress required to move a dislocation through the lattice, a problem which Peierls solved with characteristic elegance.

Cambridge 1939-1950

Orowan's first few years in Cambridge produced several ingenious ideas. The spacing between slip bands could well be determined by the distance at which two dislocations could just pass one another under the applied stress (23). Kinking, a mechanism of deformation new in metals though long known in minerals, was described and analyzed (26). The presence of W. L. Bragg seemed to stimulate Orowan's interest in X-ray techniques. If a spot in a rotation photograph of a deformed crystal corresponding to a reflection *g* is unusually sharp because the curvature of the lattice planes focuses the beam on to the film, then the spot *-g* is unusually drawn out. This was illustrated by a remarkable photograph from a cadmium crystal (24). If a fine wire grid is placed over the film and rotated through an angle proportional to the rotation of the crystal, each diffraction spot is crossed by fine parallel lines, and their inclination shows the angle of rotation of the crystal at which these planes come to satisfy the Bragg condition. These short contributions continued over many years: "static fatigue" in glass is attributed to reduction of the surface energy (28) and, with M. S. Paterson, X-ray line broadening is analyzed in metals deformed at different temperatures or with a change in temperature (38).

But Orowan's main interest at this time was devoted to the technology of munitions production. It led to a paper *The Calculation of Roll Pressure in Hot and Cold Flat Rolling* (27) which occupied 28 large pages of small print, and led to ten pages of printed discussion. It is a formidable sustained effort of applied mechanics. Orowan began by listing six physical approximations which had been introduced by previous workers. Disagreements with the only available experimental observations, those of Siebel and Lueg, could be as serious as a factor of four. "It is not clear which of the numerous simplifying assumptions and approximations is responsible. . . . In fact, Siebel's theory with its crude mathematical simplifications was often found to agree with roll pressure measurements much better than the theory put forward by Kármán who, with the same physical assumptions, had used far better mathematical approximations". Orowan set out "to attempt, first, a sufficiently general and accurate treatment of the problem, without respect to whether the method is simple enough for everyday use . . . from which simplified methods of calculation, valid for special cases of rolling, can be evolved". He did not seem perturbed by a discussant's report that "The rolling loads obtained . . . using a 10-inch slide rule . . . did not differ by more than 2.0 per cent from the accurate computations calculated on the Brunsviga machine". His reply to the discussion pointed out that: "Many rolling mill engineers could tell of unsuccessful experiments with rolling mills where hundreds of thousands of pounds could have been saved if a sufficiently accurate method had been available for calculating in advance whether the advantages were worth the expense".

The paper would be very difficult to read if Orowan did not lead the reader with the skill in exposition for which he became well known in his later years at MIT. He found the main errors of earlier calculations to be the assumptions that sections of the sheet normal to the rolling direction were homogeneously strained and that there was a constant coefficient of friction between the rolls and the strip. The latter assumption failed because it would lead to shear stresses in the strip several times larger than the flow stress of the material. There must be regions of the arc of contact where the tangential stress is determined by the coefficient of friction and regions where it is limited by the flow stress. Orowan replaced the former assumption by flow patters derived by Prandtl and by Nádai. Prandtl considered plastic compression between parallel plates, while Nádai considered plastic material flowing towards the apex of a wedge. Nádai's solution is appropriate for the material on the exit side of the rolls. For the entry side, Orowan made an intuitive change in the formulae; in the discussion, E. H. Lee showed the validity of this approach. As in problems of indentation, there must be a region in the strip near the line of maximum roll pressure where the stress in the strip is close to a hydrostatic pressure, and there is no plastic deformation.

The investigation involved some experimental work as well as plasticity theory. Sir Lawrence Bragg had raised the question "how a rolled bar is able to become longer even if its surface cannot slip on the rolls (i.e. in the case of complete sticking)." The solution was found by rolling laminated plasticine strips with their layers perpendicular to the roll direction. The surface layers extend suddenly as the bar enters the rolls. The deformation then propagates towards the middle of the strip as it passes through the roll.

A subsequent paper (32) by Orowan and Pascoe gives simplified formulae for roll pressure and power consumption for hot rolling where the flow stress is low (and assumed to be a linear function of the strain rate), and there is no slipping between the rolls and the sheet. Simplifying geometrical assumptions are also made, but these allow the calculation to be extended for the first time to stock of finite width.

A problem which became of great importance around 1944 was the catastrophic failure of welded "Liberty" ships by brittle fracture. The combination of physics and engineering involved was ideally suited to Orowan's gifts, and, with J. F. Nye and W. J. Cairns, he made some major contributions. Professor Nye has written the following account of the life and work of Orowan's group at that time:-

I was Orowan's first research student. Ernst Sondheimer and I had taken our Finals in December 1943 (how it came about that we were the only physics students to do so at that time is another story) and both Orowan and Randall (of magnetron fame), working in the Cavendish, needed a student to help them. Randall was the senior and had first pick and so got Sondheimer; Orowan got me. I came to the Cavendish in January 1944 to report to Sir Lawrence Bragg as head of the laboratory, and I remember asking him tentatively what kind of work it was to be, as I had been given no inkling before. This was wartime and one did not always expect to be told much. "Shatter phenomena," said Bragg firmly, and that was that. He took me down to the basement of the Cavendish where I met Orowan. On the left side of the corridor there was a rolling mill, with a conspicuous car-type gearbox with a gear lever attached to it, and there was also a testing machine. It was on an engineering scale and rather larger than the kind of apparatus I had met as a physics student. Orowan had his desk and typewriter in a large room on the other side of the corridor, which was also occupied by Captain J. Los, a Polish exile, who was working on transient creep, and Dr. Hof from Austria, who was concerned with measurements on the rolling mill. This comprised the whole group. Soon after we were joined part-time by Warren Cairns.

There was a new contract with the Armament Research Department at Fort Halstead in Kent (under the Ministry of Supply) to study notch brittleness, and I was to help in this. The problem was to do with the fact that many of the new welded ships, used to bring supplies across the Atlantic, were cracking, some of them to such an extent that they were sinking. With the traditional method of construction, where the plates were rivetted together, any crack would run into a rivet hole or to the edge of the plate and not spread further, but, with the new method of welding the plates together, once a crack began to spread there was little to stop it. Some ships cracked completely in half. The problem was being studied from several different angles and we were to look at the fundamentals of the fracture process. Our final report (33) came out in July 1945, too late to help in the War. I remember a meeting in the Engineering Laboratory in Cambridge (26 October 1945) at which a number of the people concerned came together to report progress. G. I. Taylor described some impact experiments. Many of the participants thought they had the answer, but it was different for each one. The metallurgists were confident that the problem arose from the molybdenum in the steel; reduce the molybdenum content and there would be no cracking. The ship architects pointed out that the cracks typically started at the hatch copings, and therefore these should be made of a better grade of steel. Orowan (30) observed that, if the cleavage strength of the steel was less than about three times the yield stress, then notch brittleness was only to be expected. The mariners noted that the casualties were mostly in the North Atlantic, and therefore the ships should take care to take more southerly courses in warmer waters. The shipbuilders said the trouble was that it was American steel; British steel would not have that problem. Perhaps they were all correct.

Orowan would sit at his typewriter on one side of his desk and I would sit facing him. At first I had to follow up a number of references to examples of different kinds of fractures published in technical magazines that Orowan received, and which I was a little scornful of because they were not scientific journals. When I asked him why he bothered with such stuff he replied, with his quaint and precise characteristic enunciation, "It is a kind of hobby". My role, apart from the experiments, was to read the many drafts he wrote and, and besides trying to be critical, to correct the English. He was fond of exploiting the eccentricities of the language in ways that would never occur to a native speaker. The draft he happened to be working on was always the "semi-final" draft; there were many, many semi-final drafts. When I went down to Fort Halstead to see Professor Mott, who was in charge of the project, Mott asked me how the work was going and I had to tell him that, alas, it was at a standstill, This clearly was not what he was expecting to hear. I explained that Orowan was fully occupied with writing the quarterly report. "He mustn't bother with that," said Mott, and I hurried back to Cambridge where Orowan sat at his typewriter to bring him the good news. After a while we moved upstairs to the first floor of the Cavendish where I still shared a room with him (the same room that Max Perutz later occupied). This did not mean that Cairns and I could always get his attention, because he had many visitors, and would always, in his politeness, deal with the latest one to join the queue, often to the dismay of the one he was talking to. As an extension of this principle, he would give the telephone absolute priority. We learned this, and when the need was urgent would take care to ring him on the internal phone. I never managed to get through a door behind him. I think he found my public school ways as odd, at first, as I found his Hungarian manner, but I found very soon that my puzzled amusement gave way to both respect and affection.

The ideas that Orowan developed while working on the notch brittleness contract were mostly included in the review article (39) on fracture that he wrote for Reports on Progress in Physics several years later. He was always slow to publish, and established something of a reputation for publishing important work in obscure places. This was partly gained because he published the work on notch brittleness in the Transactions of the Institution of Engineers and Shipbuilders in Scotland (31). In the same vein, new ideas on mechanical testing were published in a Report of the General Conference of the British Rheologists' Club (42). However, at about the time that I joined him he had just published his paper on "A simplified method...[for calculating the power needed by a rolling mill]", which had attracted much attention and had received an award from the Institute of Mechanical Engineers. His observations on kinking in cadmium, published in *Nature* (26), had been done a few years earlier. However, I do not think he published the related X-ray pictures. These he had in a drawer, and they showed how the streaks of asterism became spotty on annealing, due to polygonization. Later he set Robert Cahn to work on that problem when he arrived, and that was how polygonization came to be studied. The experimental work on transient creep in polycrystals was begun by Capt. Los; later it was continued by Eric Hall and C. L. Smith. I think logarithmic creep came from Hall's work, but I am not absolutely certain. Others who worked in the group included Robert Honeycombe (recrystallization), Norman Petch, Rodney Hill (Mathematical plasticity), F. H. Scott, Geoffrey Greenough (internal stresses from X-ray line broadening), Peter Pratt, and then there was Mr. Charter the laboratory assistant. (Mick Lomer was officially under Orowan but was "lent" to Bragg, and John Glen, who worked closely with us, was under Perutz.) We worked fairly closely with the crystallography group under Will Taylor. There were also two researchers we saw little of, who needed their own lathe because the metal they were working with was radio-active. I asked Orowan what they were working with, and I remember his diplomatic reply, "It is an element". This was before the bomb was dropped. Only later did I realize that it was uranium, and, if I had known, I would not have realized its significance.

Orowan was especially clever at bringing to bear on problems very simple ideas of stress analysis. For example, in the work on notch brittleness he appreciated the connection between the stress enhancement in a tensile specimen containing a deep notch and the problem of indentation by a circular punch; it was a matter of changing the sign of the stresses in the punch problem and so turning compression into tension. In a similar way he most elegantly explained how it is that a single crystal of aluminium, when it necks down towards the end of a tensile test, can develop a hole passing right through the centre of the neck. He would do his own meticulous mechanical drawings for the apparatus that was to be built in the workshop, and he spared time to teach me some of the tricks of mechanical drawing too. He was skillful with his hands and fingers and was keen on microscopy. We bought a polarizing microscope, which I used, under his supervision, to study the photoelastic effect of dislocations in silver chloride ('transparent metal'). It was he who had the idea for this work, made possible by the availability of rolled sheets of silver chloride from the Harshaw Chemical Company. He was generous in sharing his ideas and, as a supervisor, he taught by example. To learn from him, especially when his group was still a small one, was a delight.

On the topic of dislocations, an idea which at that time was simply a theoretical hypothesis, Orowan told me that he had played a small role in the genesis of Sir Lawrence Bragg's bubble model of a metal. Bragg had noticed, while mixing the fuel for his motor mower, that a number of equal-sized bubbles were produced, which clung together on the surface to form a regular pattern like a crystal. He came in to the lab and asked his assistant Crowe to set up a small glass nozzle in a soap solution and blow air through it to reproduce the effect. Crowe was having no success; the bubbles were coming out with different sizes. At this point Orowan happened to be passing, looked to see what was happening and suggested to Crowe that he turn the nozzle so that it was pointing upwards. The tube being straight, the nozzle was naturally pointing down and the bubbles were getting in the way of each other. Crowe put a bend in the tube so that the nozzle pointed upwards and there was never any problem after that.

I mentioned that Orowan had many visitors, for he was much in demand for consultation. One such meeting was particularly fruitful, because it marked the beginning of modern glaciology. Vaughan Lewis, Lecturer in the Geography Department, was interested in cirque (or corrie) glaciers. He wanted to explain how they eroded their characteristically shaped basins; he had an idea that he called rotational slip and to work it out he needed to consider the mechanics - how the couple due to the weight of the ice mass was resisted by the friction of the bed. So, very wisely, he came to consult Orowan. I sat across the desk from them and listened with interest. The outcome was that Orowan became a main contributor to a joint meeting of the British Glaciological Society, the British Rheologists' Club, and the Institute of Metals, held at the Institute of Metals on April 29 1948. He emphasized that ice, like crystalline solids in general, is not linearly viscous but a plastic material.

He brought into glaciology for the first time the notion that creep, as studied in metals by Andrade many years before, was the basic mechanism of glacier flow. He then, characteristically, suggested the approximation of perfect plasticity with a constant yield stress and introduced three very simple models. The first was a rectangular block of ice on a slope. He showed that for a given slope there was a critical thickness for flow. The second model was a tall slender column; he showed that there was a critical height (about 20 m) beyond which the column would squeeze out at the base (in the discussion that followed W H Ward related this critical height to the depth of crevasses). The Greenland ice sheet was much thicker than

20 m (about 3000 m)and this led to the third model, which was of a wide mass of ice spreading out on a rough horizontal base; Orowan showed that from the known height and width of Greenland one could calculate a yield stress for ice, and the figure was of the correct order of magnitude. The brilliant simplicity of these models set the scene for a new era of glaciology, with the mechanics firmly based on the physical flow properties of ice as measured in the laboratory. This glaciological work was never published as a paper, but there is a full account of the meeting in the *Journal of Glaciology* (40). Having attended that meeting I was asked to write a report on it for *Nature* (J. F. Nye, the flow of glaciers, *Nature* **161**, 819 (1948)); thus began my own interest in glaciology.

Vaughan Lewis later persuaded Orowan to join him in a tour of Swiss glaciers with Professor Hollingworth, the geologist of University College, London (on a grant from the Royal Society). Orowan was already interested in rock mechanics (Nádai's book on plasticity was influential here) and this, together with the glaciology, led on later to his interest in earthquake mechanisms, flow in the Earth's mantle and his model of a convection cell in the mantle based on perfect plasticity. It was after he left Cambridge that he acquired a reputation for always doing something different from what he was employed to do (geophysics at Boeing, and economics at M.I.T.), but that was after I lost close touch with him.

I think he never felt at home in England (or perhaps anywhere). He was always the detached quizzical observer, always the foreigner. He had dining rights and later a Fellowship at Caius College, but college life did not interest him; he would have lunch in the town restaurant at the Corn Exchange (terrible sandwiches) rather than in college. He struck me as largely oblivious of his surroundings. He was fond of music. He and his wife were friends with the Diracs, and I believe with the Rideals also, but I doubt if there were any strong social ties to keep him in Cambridge. Perhaps the transition to the United States would not have seemed to him any great upheaval.

As Nye records, much of this work was first published in rather inaccessible places (29, 30, 31). It was only after a delay of several years that Orowan summarized the work in a long, but still very condensed, review paper (39), which ranges from Thomas Young's theory of the cohesive strength (published in 1805) to current unsolved problems. His criticisms of earlier work are expressed forcibly. Of the statistical theory of cracks he says “The space available for the present Report does not admit of even the briefest review of the results obtained; however, a few typical difficulties, not all of which have received due attention in the literature, must be mentioned”. There follows a brilliant exposition of the observation that the brittle strength in compression is eight times that in tension, and of the interaction between tensile and hydrostatic stresses. His discussion of the “true” tensile strength ends: “A brief reflection shows that the ‘true’ strength, even if it existed and if it could be measured correctly, would have no practical importance for applications in engineering.” On notch brittleness, to which he and his collaborators made such important contributions, he says “it is a much discussed question what types of specimen and test are best suited to give an accurate measurement of the tendency to notch brittleness.... These data can be obtained without any testing machine, by means of a vice, a hammer and a refrigerator or solid CO_2.” The discussion on notch brittleness depends on a modification of calculations by Hencky and Prandtl of the indentation of an ideally plastic solid by a rigid punch. The modification shows that if the yield stress is Y, the maximum stress in a notched sample cannot exceed about $3Y$. Let the brittle strength of the material be B. Then, in a test at room temperature,

“if $B < Y$, the material is brittle;
if $Y < B < 3Y$, the material is ductile in the tensile test but notch-brittle;
If $B > 3Y$, the material is fully ductile (not notch-brittle)”

Now take the material to low temperatures. The brittle strength B is hardly altered, but the flow stress Y is roughly trebled. A material notch-brittle at room temperature will be brittle at low temperatures. Finally, the very elegant results of (29) for a series of samples with notches of different depths are discussed briefly. It remains to be explained why there is a size effect in notch brittleness on the scale of centimetres. A square rod of side 0.5 cm with a machined notch may bend in an entirely ductile manner, while a geometrically similar sample 10 cm on a side breaks explosively in a bending test. The suggested explanation (43) is that, when a reasonable quantitative allowance is made for the work of plastic deformation on the surface of a nominally "brittle" fracture, the critical size of a Griffith crack is enhanced to about 1 mm. Such cracks will not be present in the virgin specimen, but must grow in a ductile manner during the test; there will be a size effect in notch brittleness in all specimens which are not much larger than the enhanced Griffith crack size.

One of Orowan's major contributions occurs only in the report of a discussion (36). He explained that a theory of the process of precipitation hardening was emerging, but that it could not account for the fall of flow stress on overaging. He showed that when the particles of precipitate become large and widely separated, a dislocation will "bulge forward into the gaps between the particles... and finally detaches itself from the obstacles, leaving them encircled by small closed dislocation lines". These are now universally called Orowan loops. In a letter (R8) concerning a talk that Orowan intended to give at a meeting in London celebrating the 50th anniversary of the introduction of dislocations into the theory of crystal plasticity, Orowan wrote:

> Your suggestion that I should incorporate any addenda in "my talk" in London can hardly be carried out: according to the program I received two

days ago I shall have only 15 or 20 minutes. My first experience in this field was that in Detroit 20 years ago, at the Sorby Symposium: I was asked to give a resumé of Taylor's contribution (he could not come), and I believed that each contribution was allotted 10 minutes. I gave 10 minutes to Taylor's paper and then started my talk, but I was interrupted after two sentences: it turned out that 10 minutes were the allotment of a speaker, not of a paper. On the 18th September in Detroit I began by mentioning that I would now continue my talk of 20 years ago; but again I misjudged the timing and was stopped after 17 or 18 minutes. - On one occasion I escaped this fate, thanks to Mott's intervention (in 1948): when I wanted to discuss briefly the condition of the extrusion of a dislocation between two obstacles, the chairman finally gave me 5 or 10 minutes for it in the discussion after Mott pressed him.

In another "discussion", this time "invited" (40), Orowan was able to explain a number of the curious features of glacier flow by considering ice to approximate to an ideally plastic solid rather than to a very viscous fluid, and thereby, as Nye writes, "set the scene for a new era of glaciology".

THE THEORY OF THE YIELD POINT

Orowan's first paper was on jerky extension, the paper in which he introduced the idea of a dislocation was largely devoted to the same effect, and he remained interested in the topic for another fifty years. In 1949 he wrote with W. Sylwestrowicz a paper (43) for The British Iron and Steel Research Association in which a single Lüders band was caused to run along an iron wire at a controlled speed. If straining was stopped and the sample was aged, a new band initiated in one of the grips ran through the old band at a higher stress, but the stress dropped to the original lower yield stress when the new band ran into the underformed part of the wire. This work was developed and later published by Sylwestrowicz and E. O. Hall. Later work at MIT led to a paper by R. A. Elliott, Egon Orowan and Teruyoshi Udoguchi (R9) and a paper (R10) by Orowan alone, written largely when he was on sabbatical as a guest of the Boeing Scientific Research Laboratories. They were never published. Years later he wrote (R8):

There is a very dark spot in my plasticity-career. In 1966-1967 I wanted to prove or disprove work of Mrs. Tipper and Polakowski, and carried out a thorough investigation with Mr. R. A. Elliott and Professor T. Udoguchi; I sent the paper, and its theoretical evaluation, in 1967 to *Phil. Mag.* The Editor sent them to Cottrell who, it seems, was sorry about them; not wanting to embark on a discussion, I postponed the reply until the affair froze. The two MSS may still be in the *Phil. Mag.* — I am enclosing the MS of the first. Since I have not followed the literature for years, I do not know whether the results have been published by somebody in the meantime. They show that, while a strain-aged steel shows the sharp yield point if it is strained in the same direction as before, it shows no trace of a yield point if the direction of straining is reversed. There are other remarkable phenomena also; you will see them in the MS [R9]. Of course, the theory of the *Y P* requires extensive additions after these. What is needed ought to have been obvious already from my and Becker's 1932 paper and my 1934 papers, but the human brain is inefficient. I would also defend myself by the circumstance that in 1967 I switched over to my present field of work, and big-game hunting makes varmint-shooting less exciting.

The matter was clearly still on Orowan's mind in 1984. At the memorial service in the MIT Chapel on 15 September 1989 (R12), Walter Owen said:

The last time that I saw Egon Orowan was not so long ago. With Ali Argon, we had lunch in Walker, where we met to try to help Egon with his arrangements to go to London to celebrate the events of 1934 at a meeting of the British Institute of Metals and the Royal Society, where he was to appear on a platform with Mott and Cottrell and others. It seemed to me that Egon wasn't very keen to go, but he did allow himself to be persuaded by Ali and myself in a fairly short space of time, and I had the impression that he allowed himself to be persuaded simply because he wanted to talk to both of us about the Cottrell explanation of the strain aging of steel which he considered to be wrong. Now he'd considered this to be wrong for thirty-five years at least and whenever he saw me he was reminded of thirty-five plus years ago when he for the first time thought this was wrong. So, periodically throughout these years he had continued this attack on Cottrell's idea of strain aging. But, during all this period of time he never published a word on any of this. In fact what there was written down in these famous pieces of paper on his desk, that have already been alluded to, were various versions of this attack that he added to from time to time. So I think he agreed to go to London because he wanted to present this attack on Cottrell's strain aging theory, and that he didn't really care too much about the British celebration of the discovery of dislocations. Unfortunately he didn't go to London because Mrs. Orowan was ill and he couldn't leave her. I think this was a pity because certainly he would have left the British scientific establishment with something to think about in no uncertain terms.

The paper R9 describes a series of experiments in which a thin-walled iron cylinder is tested in torsion. The stress system favours circumferential shear and axial shear equally; circumferential shear is induced by a circumferential groove, and then persists during the deformation. The unique feature of this design is that the direction of deformation can be reversed. If the tube is twisted plastically, and allowed to age, it will deform again in the same direction only under an increased stress, and shows a marked yield point. However, if, after aging, the tube is twisted in the opposite direction, there is a strong Bauschinger effect, plastic deformation setting in under a stress numerically less than that under which forward deformation was occurring, and there is no sign of a yield point. The second paper considers the interaction between internal stresses and the stress required to unlock a dislocation from an atmosphere, and concludes that the locking process must affect not only the mobile dislocations but also the obstacles which impede their motion. There was a long correspondence between Orowan and Cottrell on these papers; much of it is (R11) preserved in the MIT Archives. In reply to a letter of Orowan's dated January 25, 1968, Cottrell wrote "I am sorry you could not find my address. It is in fact the Cabinet Office. . . . Thank you also for the copies of your manuscripts. . . . In fact I saw these two papers briefly in January, when W. H. Taylor invited me to referee them. I felt, however, that I was not in a good position to do so and I believe that he has now sent them to someone else." In the last letter of the series, dated April 2, 1968, Orowan wrote "pinning is quite unnecessary for preventing a general outbreak of plasticity . . . you can get no significant deformation without multiplication; the grain boundaries act, not as 'firebreaks' of deformation, but as multiplication-stoppers." He continued "Benefiting from our conversation I have re-written the Abstract (enclosed); I shall make alterations in the paper and acknowledge my debt to you, in a form that can not be interpreted as implying your agreement with the views given. "Unfortunately, he did not do this.

The Decision to go to M.I.T.

In many ways Orowan's stay in Cambridge was a successful one. It was scientifically productive. He led a very effective group; what may have been a farewell tribute to him (R13) reads "To Dr. Orowan with best wishes from J.F. Alder, R.W. Cahn, W.J. Cairns, S.D. Charter, P.T. Davies, J.W. Glen, G.B. Greenough, E.O. Hall, R. Hill, R.W.K. Honeycombe, W.M. Lomer, D. Humphreys, J. Los, J.F. Nye, K.J. Pascoe, N.S. Paterson, N.J. Petch, V.A. Phillips, P.L. Pratt, C.L. Smith, E.M. Stokes, W. Sylwestrowicz, O.H. Wyatt, E. Yoffe." He was not without honour: Thomas Hawksley Gold Medal (1944, Fellow of Gonville and Caius College 1949 (R7), FRS 1947, University Reader 1947. Yet by 1950 it was well known that he was looking for a move. There were offers in plenty. The MIT archives contain a letter from E.P. Wigner inviting him to Princeton, one from F. Seitz inviting him to Illinois, a letter from D.B. Copland, Vice-Chancellor of the Australian National University, regretting that Orowan had not accepted their offer, and one from T.E. Allibone asking him to change his mind and join the AEI Research Laboratories in Aldermaston. But Orowan accepted the invitation of C. Richard Soderberg, Professor in Charge of the Department of Mechanical Engineering at MIT, to come for a trial period with a view to a permanent appointment. Chadwick wrote to Orowan during this trial period, outlining what had been done to stabilize Orowan's position in Cambridge, which was clearly not firmly assured. "I am not dissatisfied with what has been done. It is not all you wish for, but the rest may follow, after your return" (R14). It seems clear that Orowan's discontent was not solely with his uncertainty of tenure, but its real nature is not clear. D.B. Carpenter wrote "I do hope you will find in Massachusetts the freedom from administrative care you have been seeking", yet it seems that in some ways Orowan was seeking a more public rather than a more private position. Chadwick's letter has a paragraph "on the subject of Higher Technological Education", and the draft of a letter from Orowan to Soderberg says "the admirable Bulletin...of the MIT...is on loan to a high standing personality actively engaged in initiating reforms of the engineering education at British universities". His worries were clearly well known. Andrade wrote from the Royal Institution "It seemed to me that the course which you have taken was almost inevitable", D.A. Oliver, Director of Research, the BSA Group, wrote "I do know that certain Professorships did not seem entirely suitable and it may well be that time has run out on opportunities in the British Isles", while Mott wrote from Bristol "I . . . feel that in your case there was really no alternative". Rather more direct was Miss E. Simpson, Assistant Secretary of the Society for Visiting Scientists, who wrote "you did indicate your reasons for having to go to the United States, and I am very unhappy and distressed that this should be so. You will realize that your story is repeated often enough in other fields to make some of us very concerned, especially as we see no prospect of an amelioration", while Sir Charles Goodeve, Director of the British Iron and Steel Research Association, wrote "Cambridge has had its great days during the Rutherford period and it is suffering from a reaction." In a similar vein, Peierls wrote "(he moved to Cambridge, but never got a permanent job there, for which he blamed Bragg)" (R15), to which Mott commented (R16) "Blaming Bragg was a hobby...I'm afraid I did sometimes — but not after having experienced the pressures on a head of the Cav. and the choices he has to make." Walter Owen in his memorial address at MIT said "While he was at Cambridge he did some work on the rolling of steel sheet which was greeted with cries of complete incomprehension by the physicists in the Cavendish, who considered of course that engineering was not really the proper job for an English gentleman, at any rate". In fact, Bragg himself had contributed to this work, and Bragg was succeeded by Mott, who had done equally ungentlemanly work during the War years, and wrote "I was enthusiastic about Orowan's work" (R16).

Cambridge, Massachusetts 1950-1989

Orowan joined the Massachusetts Institute of Technology in the summer of 1950 as the George Westinghouse Professor in the Mechanical Engineering Department, after a three month trial period in the spring of that year with a title of Visiting Professor. Upon the resignation of Charles MacGregor, he became the head of the materials division. While the Department of Mechanical Engineering had traditionally a strong materials division, Orowan brought a fresh new mechanistic point of view to the teaching and research of mechanical behavior of materials.

During the period beginning in 1950 and extending to his formal retirement in 1968, Orowan continued to occupy himself with many of the same problems that he so successfully started to investigate while in England, namely: mechanisms of crystal plasticity, brittle and ductile fracture, fatigue, and the application of these to geology. After his retirement he added to these considerations other more philosophical questions of: the stability of the Western industrial economies, aging of societies, problems of higher education etc.—all of which occupied his attention up to the time of his death. The last effort which Orowan entitled “socionomy”, that began in the early 60’s as he started losing interest in the mechanical properties of engineering materials, and even in their applications to geology, has remained unpublished. At the time of his death this work, intended initially as a book of about 500 pages, was left behind in the form of 42 volumes of neatly arranged looseleaf note books, covering the same subject matter in a number of different unfinished variations.

Orowan had remarkable talents as a teacher in clarifying complex concepts in mechanical behavior by simple and penetrating developments based almost exclusively on his own research. What made his lectures so memorable to both undergraduate and graduate students was his dramatizations that often began with the statement that “such and such a phenomenon was not at all well understood until some fateful moment rather recently when things suddenly became very clear”. The identity of who eventually was responsible in the creation of the new clarity was evident to the experienced student. In fact, many discerning graduate students who also followed the literature separately, quickly became aware that they had been treated to a special dose of Orowania, even so, the coherent picture sketched out was greatly appreciated.

While always cordial in his interactions with his research students or collaborators, Orowan maintained a working relationship that resembled more that between a master craftsman and his apprentices rather than between a senior researcher and his junior collaborators. His research students probably learned more from him through the demonstrations of uncompromising logic in interpreting results and planning new steps in research than through any collection of facts or formal methodology. Orowan was capable of complex mathematical analysis but rarely engaged in it in the development of theory. He preferred logical qualitative arguments and simple experiments that demonstrated the validity of one mechanism over another, reinforced, if necessary, by an order of magnitude estimate. With the exception of his tour-de-force on the sheet rolling problem discussed in some detail above, most of his analysis did not go beyond the penetrating simple statements that captured general trends as in the case of the well known Orowan stress for overcoming precipitate obstacles. His students remember his justification for this uncomplicated approach from his statement that he felt it was more important to "supply vitamins rather than calories".

Fracture and the fracture instability

An interest which Orowan brought with him from England was the brittle fracture of ship steel. With his first doctoral student David Felbeck, Orowan (48) showed that the fracture instability in steels with very little ductility could be described adequately by the well known Griffith condition in which the specific surface energy term $\propto$ of the purely brittle material had to be re-interpreted as the superficial plastic fracture work per unit area, p, that can be associated with the two new surfaces of fracture. Through fracture experiments on large edge-cracked plates of the tanker Panaganset that broke up in 1947 in Boston Harbor, they estimated the value of p to be of the order of 3.5×10^6

erg/cm^{-2}. To Orowan this was another successful demonstration of the important work of Griffith which he considered to be of epoch-making magnitude and wrote often on how much it influenced his thinking, not only on fracture but in the conceptualization of dislocations and their origin in stress concentrations. In a long private letter to Cyril S. Smith recalling his early career, he stated his views about the work of Griffith as "I need not dwell on Griffith, whose work has been among the most consequential in physics in this century; it has opened up a new chapter well-known to all except the members of the Nobel Committee" (R21). In spite of this deep admiration, Orowan was well aware of the limitation of the Griffith conditions in problems of fracture with more pervasive plasticity, and furnished clear examples of inapplicable cases in a paper on the energy criteria of fracture (49), and one on the conditions for high velocity ductile fracture (50). Apart from such observations, however, Orowan did not pursue the study of ductile fracture and its mechanisms, and had very little liking for the formal developments in fracture mechanics initiated by Irwin, that were revolutionizing the study of fracture in engineering. From the early 50's to the early 70's Orowan continued with his interest in fracture mechanisms with studies in a random collection of materials. One such study was the statistics of strength of plate glass surfaces probed by the Hertzian fracture experiment (51), stimulated by his industrial consulting arrangement with the Pittsburgh Plate Glass Company, in connection with their new float-glass process for the production of plate glass. Other studies of this period included the fracture of crazable glassy polymers with Michael Doyle and others, where propagating cracks are preceded by a narrow plastic zone in the form of a craze (52), and fracture in adhesive joints (53). In the latter Orowan considered basic requirements for achieving tough joints with brittle cements and clarified the important role of residual stresses in the adhesive layer that can retard the growth of a crack in the layer by repeatedly diverting its propagation direction away from the median plane of the joint into the adherent. In connection with this study on adhesion he revisited the well known Young equation of capillarity and provided an elegant proof of it as applied to capillary equilibrium between liquids and solids, including a precise discussion of the important differences between the notions of surface energy, surface tension, and surface stress (54).

Crystal Plasticity

In hindsight it is now clear that at the time of his move to the U.S.A. the mechanisms of crystal plasticity no longer occupied central stage to Orowan. Nevertheless, in a number of noteworthy publications he summarized his points of view. The first of these was a comprehensive position paper associated with a main lecture on creep in metallic and non-metallic materials (55) delivered at the First U.S. National Congress of Applied Mechanics. In this he provided a far reaching discussion of the rate mechanisms of plasticity as they manifest themselves in creep behavior. The discussion included anelastic creep (in which a finite concentration of activable deformation units are mechanically polarized); Newtonian, linear viscous creep (in which thermally activable deformation units are re-created at the same rate as they are polarized); non-Newtonian creep (in which the deformation work during a thermally activated event is no longer small when compared with kT); creep by diffusional flow and by grain boundary sliding; and finally, mechanistic reasons for the non-existence of a mechanical equation of state for plastic deformation. A small section discussing the possible mechanism of plasticity in both simple atomic and chain polymeric glasses admirably anticipated much of the confusion that beset the study of these phenomena by future researchers who unfortunately were unaware of Orowan's observations.

Another remarkably comprehensive summary of Orowan's views on crystal plasticity appeared as a longish chapter on Dislocations and Mechanical Properties, in a special publication edited by Morris Cohen, resulting from a special symposium on dislocations held in 1951 during an AIME meeting (56). In this chapter, which Orowan also used in his graduate classes on Physics of Strength and Plasticity at MIT as a reference for supplementary reading, he discussed in very simple terms important characteristics of dislocations, including their topological features, stress fields, energies, how they give rise to plastic strain through their motion, and how they multiply during straining. He then discussed, again in very simple terms, by resorting only to order-of-magnitude estimates, important phenomena such as the lattice resistance to dislocation motion, precipitation strengthening, work hardening, the yield phenomenon in low carbon steel, and the nucleation controlled processes such as twinning, martensitic shear transformations, recovery, and re-crystallization—all with a thorough historical perspective which was his penchant. The discussions of the Taylor theory of work hardening and the Cottrell theories of the yield point in low carbon steels include perceptive and sharp criticisms but offer little in the form of quantitative alternatives. The discussion on twinning and re-crystallization make a very clear case for the need of embryos or for topological mechanisms that can build up discrete interfaces gradually rather than nucleation of fully formed saddle-point configurations.

During the early 50's Orowan, together with his co-workers Bragaw, Sylwestrowicz, and Torti conducted extensive transient creep, strain-rate change and temperature jump experiments on polycrystalline aluminium, copper, and some solid solution alloys such as alpha brass and monel metal to explore the rate mechanism in crystal plasticity. In these experiments, which were never published beyond brief reports to the funding agencies (or doctoral theses deposited with the MIT Library), the emphasis was on the time law, i.e. whether logarithmic (no recovery) or Andradian (with recovery), with little or no mechanistic interpretation—beyond what was in the mind of Orowan. It is quite likely that during the course of these studies Orowan became convinced that the old Becker-Orowan formalism of nucleation controlled plasticity could not deal with these experiments, but apparently also remained unconvinced that the new developments on forest-cutting, advanced by Cottrell and co-workers or Seeger and co-workers were appropriate. In fact Orowan returned to this conflict of nucleation vs. propagation control many times in later life by recalling that in the German school in the 30's, of which he was a part, the task was to explain why plastic flow occurred at a level so low in comparison with the ideal shear strength. In comparison, in the English school pioneered by Taylor, dislocations were assumed to be easily created and had little resistance to their motion, requiring dislocation interactions to explain a finite plastic resistance.

Geology

As remarked in the recollections of Nye, quoted above, while still in England, Orowan developed a deep interest in plasticity and fracture problems related to glaciology which he later in the U.S.A. broadened to problems of seismology, tectonics, continental drift, rifts on the ocean floor, and the like. Here he found a fertile field in which he could exercise his inventiveness and apply knowledge of mechanisms of inelastic deformation and fracture on a grand scale, in his preferred style of the semi-quantitative order-of-magnitude approach. The re-emergence of interest in this field seems to coincide with a three month leave of absence in 1958, spent at the California Institute of Technology upon the invitation of B. Gütenberg. In a series of well structured and unpublished tutorial notes prepared for lectures at CalTech, Orowan presents a specialized application of the physics of deformation and fracture to problems of geology.

Throughout the 60's in a series of papers (57-64) Orowan considered key mechanisms involved in continental drift, and the associated problems of convection in the mantle, formation of mid-oceanic rifts, ocean floor spreading, orogenesis, and volcanism. In these carefully reasoned papers Orowan considers the impossibility of fully developed deep convection cells in the mantle on the basis of evidence of insufficient fluidity of the deep mantle and dismisses the alternative shallow convection model limited to the asthenosphere (a term which he does not like and proposes that it be replaced with "low hardness layer") as having too high a drag. Part of the problem associated with convection becomes rectified by considering incomplete cells in which the upward motion of rising material in the mid-oceanic rises is considered to obey a plastic plug-like flow, rather than Newtonian viscous and that the loop is closed by a slight rigid body motion of part of the mantle and adjustment in the fluid core—in a pattern that he entitles "transvection". In the discussion there are many instances where more precise material behavior in the form of plasticity, Andradian power law creep, fracture, and concepts such as plasticization of rocks are introduced to replace simple elastic or Newtonian viscous behavior. In a popular article in Scientific American (64) Orowan gives an overall synthesis and speculates on an alternative to radioactivity as the main heat source to drive convection, in the form of a large (100 km diameter) asteroid impacting the earth and setting off a long term transient convection process. In support he points out that this would be of a magnitude similar to those responsible for the formation of the large maria on the visible side of the moon. Some of the later papers of this period were finished during a stay of Orowan at the Boeing Scientific Research laboratories in Seattle in 1966-67. In another exercise on the subject of processes on the grand scale Orowan considered the origin of the surface features of the moon (65). Based on the results of the lunar expeditions of the late 60's and early 70's and on examination of high resolution photographs brought back by the lunar orbiters and the Apollo missions, Orowan considered that all lunar features are explainable by meteorite and asteroid impacts, leaving no room for volcanism.

A final, and very long paper on the "Mechanics of Continental Drift" was submitted to the Royal Society in 1978 for publication in the Proceedings. The fate of this paper and how the Royal Society dealt with some of the consequences of its rejection is discussed below.

Socionomy[3]

In 1962 Orowan accepted an invitation to spend a year at the Carnegie Institute of Technology as a Visiting Institute Professor with the understanding that he could spend his time on problems of evolution of societies and economics—subjects that occupied his attention with ever increasing dedication until the end of his life. In the Fall of 1972 he accepted another appointment as Alcoa Visiting Profes-

[3]A word coined by Orowan to refer to the interrelationship between sociology and economics.

sor at the University of Pittsburgh for the same purpose, where he intensified his involvement in this subject and actually gave a lecture course for which he prepared detailed notes. In an interlude of yet another one-year leave in 1965-1966 at the Boeing Scientific Research Laboratory he spent some of his time also on this subject. Between this period in the 60's and the time of his death in 1989, barring a few solitary diversions, he spent his time almost entirely on writing on this subject.

Orowan picks up his theme from the fourteenth century Tunisian Arab historian Ibn-Khaldun who studied in some detail the rise, maturation and senescence of successive North African tribes from lean dynamic beginnings to rich and decadent ends, when they are replaced by a new wave of dynamic invaders and so on. Identifying these cycles as *surges* Orowan finds many parallels to these in modern Western societies where however the role of economics becomes of central importance. Tracing the evolution of thought on the modern Western economies from the early trend setters of Adam Smith and Malthus, Orowan identifies one of the fundamental causes of the present weakness of the economic structure of advanced Western Societies to lie in the problem of overproduction resulting from ever increasing productivity which replaces the old crafts that employed many highly skilled craftsmen with automated industries requiring ever fewer people. The attendant problems of chronic unemployment then require establishment of government-charity in the form of production of armaments and establishment of government contracts for work and research not needed by society. In his writings, on this subject Orowan engages in many interesting but inessential diversions on historical facts and anecdotes for the purpose of focusing on the failings of many of the well known economists, historians and social scientists of recent times — making fascinating reading but too often diverting the attention of the reader from the main thesis. His daughter Susan Martin is considering publishing one or more special summaries of this work.

In his writings on the subjects of higher education and professional life Orowan advocated more attention on development of creativity and permitting creative people to continue in their field of expertise as individual professionals rather than directing them too early into management positions. (66).

Other activities

Throughout his professional life, and particularly during that part in the U.S.A. Orowan filed many patent applications on inventions occurring to him in the course of his research in his consulting practice and in private life. One of the few patents actually granted (U.S. Patent No. 3,100,488) (67) is for an ileostomy appliance that introduced substantial improvements over models existing on the market at that time, which often created severe problems of secondary skin infections and extreme discomfort for its wearers. This appliance that has been described in the literature (68) has made major impact on the quality of life of all the users who could be equipped with it from pilot production. There are numerous letters in the Orowan collection, in the MIT Archives, from exceedingly grateful patients, attesting to the success of the appliance. Unfortunately, an attempt to commercialize the appliance through the formation of a specialized company did not succeed.

Between the mid 50's, and until 1980 Orowan has had many consulting arrangements with industry. Two of these, with Pittsburgh Plate Glass Company and with E.I. du Pont de Nemours were of more major type and of longer duration. From the extensive notes left behind by Orowan and from statements made by scientists in these companies it is clear that he was a very effective industrial consultant. Throughout his life Orowan maintained a keen interest in all modern social and technological developments and often commented on major happenings through letters to editors. These include comments on the real causes of the disastrous Scott expedition to Antarctica; effect of possible valve malfunction due to a design inadequacy in the Three Mile Island nuclear power station accident in 1979; and a particularly detailed back and forth correspondence with the presidential commission investigating the ill-fated Challenger Shuttle disaster of 1986, on the possible "real" cause of it which Orowan thought was due to a short-transverse brittleness in the main rocket casing, bringing in eventually into the debate a sizable group of NASA scientists and Senator Patrick Moynihan, the Senate overseer of the commission.

During his years in the U.S.A. Orowan continued to receive honors and awards that were, however, more in recognition of his earlier work in Europe and less for his activities in the U.S.A. These included membership in the American Academy of Arts and Sciences (1951), the U.S. National Academy of Science (1969), corresponding membership in the Göttingen Academy of Sciences (1972), an honorary doctor of engineering degree from the Technical University of Berlin (his former alma mater) (1965), the Eugene Bingham medal of the American Society of Rheology (1959), the Gauss medal of the Braunschweiger Wissenschaftliche Gesellschaft (1968), the Paul Bergse Medal of the Danish Metallurgical Society (1973), the Acta Metallurgica gold medal (1985), and the Vincent Bendix gold medal of the American Society of Engineering Education (1971).

In his professional activities (and his intellectual hobbies) Orowan went for the unusual and hidden explanations of things, often drawing support from forgotten historical facts, and clever anecdotal quotations or overlooked aspects of phenomena, partly for their shock value and partly to impress. His early education in engineering and his eventual practice as an applied physicist (or chemist, or metallurgist) was both a source of strength in his professional life and a cause of a split personality. He lectured to engineers on the merits of the scientist's approach and to the scientists that of the engineer. While he criticized some scientists of whom he did not approve as ". . . Oh well! he was only an engineer . . .", he took great pride in associating himself with prominent chief engineers.

Family life and personal characteristics

His daughter Susan has written "My mother was probably his best friend, although I don't think they realized it until the last couple of years. He looked after her full-time at home from 1984 until she went to a nursing home in 1986. . . . When she died, in October 1986, I think he started to believe that he didn't want to live any more". He had many other friends. Susan remembers Peierls, Shoenberg, Dirac, Perutz, Bragg and Besicovich from England, and especially Laszlo Tisza and Leo Gross in America. Susan, after majoring in languages at Tufts, had a distinguished career in librarianship, and is presently University Librarian at Georgetown University. Her husband, Dr. David Martin, is Dean of the School of Education at Gallaudet University, D.C.

Susan's memories of Egon's private life are very clear. . . . "He liked plants, flowers, trees. He and Laci (Tisza) used to go to Jamaica Plain to the botanical gardens as one of their favourite outings". David Tabor has noted similarly (R17): "One aspect of life in the U S A that he prized above many others was the size, scale and openness of the American

National Parks". Susan continues: "Modern culture, though, and particularly 'American' things: popular music, foods, recent clothing fashions, and computers were among the things he looked down on".

Many of the comments at the MIT Memorial Service are revealing. Ali Argon learned from Orowan: "Problems had to be solved completely. . . . Your first reaction should be extreme skepticism, including of course on your own work". This skepticism could sometimes be expressed almost too forcibly. Tabor recalls: "His powerful intellect was most evident at the seminars or lectures that he attended. If a particular point caught his interest he would continuously interrupt... Experienced lecturers could take these interruptions with good humour and sometimes with enjoyment, but junior speakers would often feel frustrated. . . . Orowan, who in private life, as David Shoenberg recounts, was a warm and friendly person, appeared only to understand the intellectual aspect of his interjections, and to be quite oblivious of their effect on the lecturer and the audience". In fact in private life he could be almost excessively gentle. Peierls recalls (R15): "He had strong views about most matters and about people, which he would express clearly, but always with a veneer of politeness. We [Peierls and his wife] used to tease him about this, and once introduced him to a woman who was not only unattractive to look at, but also with a very unpleasant manner, and rather boring conversation. We wondered how he would express his comments about her. When asked, he said 'She is very cerebral (durchgeistig)'." There was another "very dark spot" in Orowan's "plasticity-career", which throws an interesting light both on his thinking and on that of the Royal Society. He submitted a paper "Mechanics of continental drift" (and probably a companion paper) to the Royal Society. It did not meet with approval from the referees, but the Royal Society does not lightly reject the work of a senior and distinguished Fellow, and it went to further referees. Sir Charles Frank wrote (R18) “I was involved as third or fourth referee”. Orowan’s approach from first principles could lead him to neglect the work of others, and Frank found “extensive recitation of ideas . . . without due reference to past work . . . and a degree of ignorance of the ‘plate-tectonics’ revolution. . . . I tried (unsuccessfully) to get him to salvage from it a shorter paper”. Orowan ceased to pay his subscription to the Royal Society, which leads automatically to expulsion. Mr. Neville le Grand, who was Finance Officer of the Royal Society at the time, has explained (R19). “On the first occasion of Orowan’s lapse, I wrote the usual reminder letters but we then realized the possible reason for this. So far as I can recall I spoke with [Sir] David [Martin] (and I believe Flect or Menter whoever was the Treasurer at the time) and we decided on a somewhat phony ground to make the transfer from one of the R.S.’s own funds to cover subsequent payments”.

There are many stories illustrating Egon Orowan’s approach to life. Sir Alan Cottrell recalls (R20) one he told of his student days in Berlin:—

> Sometime, in the 1930’s, the German education authorities changed the rules for matriculation, which required all candidates thereafter to pass an examination in the physical sciences. This set a problem for a nearby convent, where the nuns made a modest income by teaching. None of them knew any science, of course. And so, one of them was sent to Orowan to ‘learn physics’. This caused difficulties both for Orowan and the unfortunate one so chosen, due to the vast chasm between the religious and scientific outlooks. After one long session, Orowan finally felt that he was breaking through, on the subject of atmospheric pressure. And so he pointed to a barometer on the wall and said ‘tell me, why does the mercury, in that, stay up?’ She thought for a moment and said, in a perfect demonstration of the dogmatic approach, ‘Oh, because it is a barometer’.

His daughter Susan has many memories (R3):— "He was a highly skilled amateur photographer. . . . He particularly liked clouds, and had a terrible fondness for obese people, which always embarrassed me". At the MIT ceremony she reminded the audience of some of Orowan's characteristic phrases: "It's very simple", and "I understood it a month ago, but now . . ." She also quoted one of his more intimate remarks: "Let me close by telling you that in my early teens I was at one stage terrified by the thought of dying. I confided this in Daddy who immediately resolved the problem, the business about the essence of the simplicity. He said, 'Do you remember what it was like before you were born?', and I said, 'No' And he said, 'Well, that's what it'll be like after you die'."

In the preparation of this memoir we have been helped by many people and organizations, particularly Professor Orowan's daughter, Dr. Susan Martin, The American Institute of Physics Niels Bohr Library, Professor Lázló Bartha, Sir Alan Cottrell FRS, Mr. J. Deakin, Dr. M. Doyle, Sir Charles Frank FRS, Mr. M. le Grand, Dr. P. Hoch, Dr. S. Keith, the MIT Archives, Sir Neville Mott FRS, Sir James Menter FRS, Professor J. F. Nye FRS, Sir Rudolf Peierls FRS, Professor D. Shoenberg FRS, Professor D. Tabor FRS and Dr. D. Tichy.

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