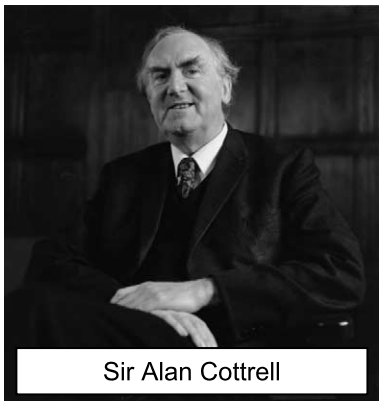
Strength, Fracture and Complexity 8 (2013/2014) 71–79
DOI 10.3233/SFC-140158
IOS Press

Sir Alan Cottrell: Fracture and structural integrity

John Knott *
University of Birmingham, Birmingham, UK

Received 3 April 2014
Accepted 8 April 2014


Sir Alan Cottrell

Sir Peter Hirsch's tribute to Sir Alan's many achievements in Academic circles and National "Corridors of Power" appeared in the *FESI Bulletin* **6**(1) (2012). Chief Scientific Advisor to the Government; Master of Jesus College; Vice-Chancellor of Cambridge University; Writer of seminal Monographs; an inspiration to hundreds, if not thousands, of Metallurgists, Materials Scientists and Materials Engineers the World over, Peter regards him as the most outstanding and influential Physical Metallurgist of the 20th Century: the only one to be awarded the Copley Medal of the Royal Society. In my *Materials Science and Technology* editorial of July 2009, written to celebrate Sir Alan's 90th birthday, I was moved to write "If anyone can be said to 'bestride the World like a Colossus' it is Sir Alan". In this piece, I hope to highlight some of the contributions that he made to Fracture and to Structural Integrity, and to recall some personal memories.

I first became aware of Professor Cottrell, as the author of *Theoretical Structural Metallurgy* (1948) and of *Dislocations and Plastic Flow in Crystals* (1953), when I was an undergraduate at Sheffield: probably in my second year, 1957/1958. In the summer of 1958, I had a vacation job at the Royal Aircraft Establishment, Farnborough and, perhaps strangely, in the light of all the interest in fatigue following

*Address for correspondence: John Knott, University of Birmingham, Birmingham, UK. Tel.: +44 121 414 6729; Fax: +44 121 414 7468; E-mail: J.F.KNOTT@bham.ac.uk.

1567-2069/13/14/$27.50 © 2013/2014 – IOS Press and the authors. All rights reserved

the *Comet* disasters, the project that I was given involved the hydrogen embrittlement of mild steel. I became fascinated with the subject of brittle fracture, and it was in 1958 that Cottrell's magnificent paper [1] on cleavage fracture in steel hit the press. He postulated that Intersecting slip bands could produce cleavage crack nuclei on the observed {001} cleavage fracture planes and calculated that, if this occurred with decrease in energy, fracture would be *propagation-controlled*. This enabled him to provide an explanation for effects of notches on cleavage fracture, which the previous Zener/Stroh theories of fracture had failed to do. I read this paper over and over again, trying to get to grips with all the details. As chance befell, the Central Electricity Generating Board (CEGB) offered me a bursary to support research for a PhD. At the final interview, when I said that I wanted to do research on the brittle fracture of steel, I was asked if I would like to go to Cambridge to be supervised by Cottrell. I did not take long to reply!

During the summer vacation 1959, when I was working in the Central Electricity Research Laboratories (CERL) at Leatherhead (a job that I had "wangled" by playing on my CEGB bursary to-be), I received a letter from Cottrell, saying that he thought that a good project for my research would be to use transmission electron microscopy (TEM) to look for cleavage-crack nuclei in α-iron (formed by the newly-postulated Cottrell mechanism), under the supervision of Dr. Nutting. He advised that I should prepare for this by reading-up on electron microscopy. Of course, I did what I was told, but I must confess to being disappointed that I was not going to be supervised by the great man himself. Again, however, fate played its part. It turned out that Nutting was about to leave Cambridge, to take up the Chair at Leeds, and when I arrived at Cambridge to start my research, I was welcomed by Cottrell, who told me that he was, after all, going to be my supervisor. He immediately confronted me with stress-concentrations, notches, slip-line-field theory and the embryonic Bilby–Cottrell–Swinden (BCS) theory. This represented a crack under Mode III loading (anti-plane shear) in terms of distributions ("inverse pile-ups") of screw dislocations: "virtual" within the crack; real outside the crack. The TEM research was forgotten, which is probably as well, because the observation of Cottrell cleavage crack nuclei has proved to be extremely elusive. A major feature of the BCS theory was that crack advance was predicted to occur when a *critical crack-tip* (*sliding*) *displacement* was attained. Much of my PhD was spent trying to measure notch-tip strains and displacements in both opening and sliding modes. My thesis, submitted in 1962, had to explain (extremely tactfully) that although a critical displacement might *characterise* the onset of fracture at a given temperature, the *criterion* for fracture in the cleavage regime was associated with the *local tensile stress* ahead of a notch. Although going along with this in our joint paper [2], Sir Alan was not really comfortable with the concept until later, in 1966, I demonstrated that the critical value of tensile stress could be related to his cleavage fracture model. (Later still, it was to be re-interpreted in terms of micro-crack nuclei in grain-boundary carbides, intra-granular carbides or other brittle particles, such as alumino-silicate inclusions in welds.)

Alan Wells introduced his concept of the "crack-opening-dislocation" at the Cranfield Crack Propagation Symposium in September 1961 and I think that it is of value to trace the historical sequence of events, to address any confusion as to whether Cottrell or Wells had the original idea for the critical displacement criterion. It was clearly in Cottrell's mind when I started my research in October 1959, and early BCS calculations had been made, although the final paper was not published until 1963 [3]. This year also saw the publication of his Bakerian Lecture on "Fracture" [4]. From 30 November to 2 December 1960, the Iron and Steel Institute (ISI) held a meeting on "Steels for Reactor Pressure Circuits" at which Sir Alan presented a paper that outlined the critical displacement criterion and gave sample calculations [5]. The Proceedings were not published until 1961 (see pp. 281–296 for Cottrell's paper: in particular, pp. 292–293), but Wells was present at the ISI meeting in 1960 (account of oral discussion,

p. 133; written discussion p. 428). Wells then went off to the NRL in Washington for a period, to work with George Irwin, on "engineering" fracture mechanics, and memories of Cottrell's paper in his mind would have been, at best, subliminal, re-surfacing perhaps when he prepared his Cranfield paper.

I do not think that Wells was present at the conference held to mark the opening of the Berkeley Nuclear Laboratories (BNL), 30 May–2 June 1961, when Cottrell again introduced the critical displacement calculations (again, pre-dating the Cranfield Conference), but I recall another point of interest. Towards the bottom of page 12 of the Proceedings of that Conference [6], there is an innocent-looking statement: "A recent analysis (Cottrell 1961 b) of low temperature notch bend tests (Allen 1961)". This recalls a memory of the discussion to Cottrell's ISI paper, at which Dr. N.P. Allen (Head of Metallurgy at the National Physical Laboratory and a man of more than somewhat uncompromising disposition) damned the displacement concept because it associated plastic deformation with brittle fracture and then collected his papers and marched out of the hall before Cottrell had time to reply! Sir Alan was *not* pleased and my subsequent task to demonstrate that fractures, even in notched bars at 77 K, were preceded by plastic deformation was given a sense of urgency. John Griffiths, who succeeded me as Sir Alan's research student, spent most of his PhD on the demonstration of this, using 3% silicon–iron, which could be etch-pitted to reveal plastic deformation at the micro scale [7].

Cottrell, as a supervisor, more than lived up to my expectations. He was full of ideas and enthusiasm, and was prepared to give me a pretty free hand in what I did. He also showed encouraging interest in any new results that I obtained and was always ready to discuss their implications with me. On the N.P. Allen issues there was clearly more than the usual encouragement. He also encouraged me to discuss my work with others and to go to relevant lectures and meetings (expenses paid, courtesy of CEGB). One memorable highlight (for an ever-hungry research student) was an Institution of Metallurgists "Refresher Course" on brittle fracture, held at the Grand Hotel, Eastbourne, complete with five-course dinner. As a result of his great eminence and a protective secretary, I had to devise stratagems if I wanted to speak to him at short notice. He regularly took coffee and tea in the communal Departmental tea-room, and what worked well was to lie in wait just before the appointed hour and "accidentally" run in to him *en route* from his office. I also developed the practice of writing down on paper all the points that I wanted to discuss when I had meetings with him. He had a tendency to expound at length on the first topic and assume that this comprised the totality of our discussion. With great temerity, I would then look at my list and say "Could you please give me some guidance on this next point, please?". His musical interests came through when he gave me advice on how to write my thesis. "It should be like an opera: a developing story, but a pause for an aria every now and then".

One of the benefits of being associated with such a famous figure was that there was a steady stream of visitors coming through the Department, from all over the World, wishing to pay their respects. Often, they would get passed on to me for half-an-hour or so, to be told about notches, brittle fracture and size effects. I was able to meet and have discussions with many internationally recognised experts. There is a story to the paying of respects to Cottrell. Just before I arrived, in the summer of 1959, a certain research group had contrived to cause quite a large fire in the buildings. When Cottrell's office was finally refurbished, it was carpeted with a rubber-backed carpet, such that visitors, walking across to shake his hand, and later, to take their leave, tended to build up a substantial static electrical charge. On reaching for the brass door handle to leave the office, discharge occurred and a spark flew. They assumed that it was the handshake with the great man that had charged them up, and who were we to disabuse them? The effect was quite discomforting for Cottrell himself, who had to leave his office every day, and, sadly from the point of view of preserving the legend, a remedy had to be sought. This was provided by his

secretary, who ensured that there was always a bowl of flowers in water in the room, to prevent the air from becoming too dry.

Towards the end of my first year, there was a second fire, caused by the same research group, but with slightly less devastating circumstances. Following this, the said group was re-located in a new extension, at the top of the Pembroke Street building: the theory being, I suppose, that if a third fire were to be ignited, loss of property would occur in a steady, controlled manner, from the top down. What happened, however, was that, shortly after the decorators had finished covering-up fire damage and left the premises, two members of the group contrived to flood the building. Gallon upon gallon of water flowed down through all the newly decorated rooms. In later life, equanimity restored, Sir Alan was wont to refer to these incidents in terms of the tribulations of Egypt in biblical times "fire, flood: we were waiting for frogs, flies, locusts, plague and pestilence", but, at the time, equanimity had certainly deserted him, as I heard at first-hand from the two research students responsible. They had been given an extremely severe dressing-down, by a near apoplectic Sir Alan. It was clear that, in the future, they would never again confuse gas-tubing with water-tubing.

In the late summer of 1961, Cottrell asked what I had thought of doing after my PhD research. I hadn't given much thought to this (I was only just coming to the end of my second year). I began to reply "I had wondered about going to the States . . .". When I detected a clearly disapproving look in my supervisor's eye, and so finished ". . . but I think that there are some good possibilities in the UK: perhaps the Berkeley laboratories?" It was then that he told me that Trevor Broom had become Head of the Materials Division at CERL and that Ted Smith would be heading up a group to study brittle fracture. He felt that this might be a good place for me to go and suggested that I meet up with Ted at the Cranfield Symposium in September. The rest, as they say, is history. Ted, never one to miss a scoring point, is rumoured to have explained my choice to "colleagues" (i.e. rivals) at Berkeley in more graphic terms "Of course, he's got more sense than to work with you lot in the hot cells and ruin his marriage prospects!".

Amongst the delights of working at CERL, some of the most memorable are those when Sir Alan came, in his role as Consultant to the CEGB, to discuss our research. Ted was working on extensions of the BCS theory and allied topics, and I was studying fracture mechanisms in both mild steel and an alloy steel used for low-pressure turbine discs. To my shame, the more memorable parts of the days were not necessarily the details of the scientific discussions, but the fact that the visit of such an eminent personage entailed CERL's most splendid lunches, at the Burford Bridge Hotel, on the Leatherhead/Dorking Road. I was asked to join Sir Alan, Trevor Broom and Ted Smith on these occasions, causing much envy amongst my colleagues. Unfortunately, Sir Alan had to step down as CEGB Consultant when he joined the MoD in 1965, and the best that I could ever manage afterwards was "The Bull" in Leatherhead. His appointment as Deputy to Sir Solly Zuckermann was accompanied by an interesting piece in *The Times*. This was headed "Not a Tadpole", with reference to Zuckermann's zoological background. In the article, Alan was claimed to smoke cheroots and to have a fondness for fly-fishing. The former may have been a passing phase, but Sir Alan's son, Geoffrey, has confirmed the love of fishing, recalling times spent with his mother, camped out in unforgiving parts of Northern Britain, munching cheese baps and desperately trying to keep warm, whilst Sir Alan was standing in some beck or burn, casting for trout. Geoffrey and his son Edward were both taught how to cast by Sir Alan, on the back lawn. Although Sir Alan was an enthusiastic fisherman, it appears, however, that he was not a particularly successful one. This gives the rest of us hope: there was something at which he did not excel, even though he put his mind to it.

Between 1965, at the Royal Society's meeting on "Mechanics of Fracture in Large Structures" [8], and 1974, when he returned to Cambridge as Master of Jesus College, I saw very little of Sir Alan: we met on less than a handful of occasions. He was fully supportive of my return to Cambridge as a

Lecturer in 1967. I regularly sent him copies of my papers and he invariably responded with kind words and perceptive comments, being especially responsive to *Fundamentals of Fracture Mechanics* (1973) saying that it was most helpful with respect to his considerations on the safety of nuclear pressure vessels. It was in 1974 that he appeared before the Parliamentary Select Committee and voiced his concerns on PWR pressure vessel safety, leading to the setting-up of the Marshall/Hirsch Study Group (see *FESI Bulletin* **5**(2) (2011)). The reports issued by this group addressed the concerns in detail, and as Sir Peter's tribute makes clear, Sir Alan expressed himself content in the early 1980s.

In my July 2009 *MST* Editorial, I recalled an (apocryphal) story from his time as Master of Jesus College relating to a 'phone call made to the College, and answered by Alan's son Geoffrey as "Jesus here". After finding that the caller wanted to speak to Sir Alan, his words were "I'll get my father". My comment was "What more need be said?". Without actually confirming or denying the accuracy of this story, Alan told me of a variation on the theme. One Christmas day, his wife, Jean, answered the 'phone, and when she replied "Yes" to the question "Is that Jesus?" she was treated to a rendering of "Happy Birthday to You". Not that Jean would have been thrown off her stride in the slightest by this. When Sir Alan was Vice-Chancellor of the University, Prince Philip was Chancellor, and he and other members of the Royal Family stayed, from time to time, at the Jesus College Master's Lodge. Jean applied herself to organising hospitality for them with skill, charm and a certain amount of (barely disguised) irreverence. As Sir Peter Hirsch's tribute makes clear, Alan was devoted to Jean. When she fell ill in 1996, he made it his main priority to care for her. This took its toll physically over the next three years, and when she died in 1999, he was devastated, gradually recovering with the help of family and friends.

A major aspect of Sir Alan's concerns with RPV safety related to the presence of initial defects in the vessels, introduced by forming and fabrication processes, these could conceivably grow to critical sizes during the operational life of a vessel, by mechanisms such as fatigue or environmentally-assisted cracking. A "belt and braces" approach was to be adopted, involving both close control of all manufacturing processes and thorough non-destructive inspection (NDI). A variety of "quality-control" inspections was applied at each stage of manufacture and fabrication, but there was also a major ultrasonic (U/S) inspection after the hydraulic over-pressure "proof" test, to serve as a "fingerprint", to be compared with periodic U/S inspections during service. These would have to be made remotely, involving custom-built inspection assemblies. All the proposed inspection procedures, equipment and personnel had to be validated, and this was achieved through an Inspection Validation Centre (IVC), located at AEA Risley. To provide further reassurance, the IVC instituted an independent Management Advisory Committee (MAC), of which Sir Alan was the Chairman, from 1983 to 1993. I was asked to serve on this committee, together with Michael Burdekin and several other independent members. Journeys to Risley from Cambridge were initially by AEA car, which entailed conversations, both there and back, of some 2–3 hours duration. Staying bright and lively and appearing to be intelligent, particularly on the return journey, was quite a strain! Later, we travelled by Suckling Airways, which flew from (Marshalls) Cambridge Airport to Manchester Ringway, returning in the evening. It was a small plane with limited headroom. A petite stewardess treated us to fresh sandwiches (lovingly prepared by a consortium of Ipswich housewives), and, if we wanted, a glass of wine (poured from a bottle). From my point of view, in addition to the TLC, the much reduced journey time was also something of a blessing.

Sir Alan became a member of TAGSI (The Technical Advisory Group on Structural Integrity) when it was set up in 1988 and contributed energetically to its deliberations for over a decade. He retired after the 32nd meeting of the main Committee on 21 June 1999, and I quote from the minutes of that meeting. "The Chairman (Sir Peter Hirsch) noted that Sir Alan Cottrell had announced his decision to retire from TAGSI and that this would be his last meeting. On behalf of TAGSI, the Chairman said that everyone

Fig. 1. Sir Alan presiding over a selection of the MAC (The reader can observe (a slimmer version of) me on Sir Alan's right and Michael Burdekin on Sir Alan's left. Directly behind Sir Alan is Chris Waites, Director of the IVC). (Colors are visible in the online version of the article; http://dx.doi.org/10.3233/SFC-140158.)

Fig. 2. Sir Alan on site at Sizewell 'B' 5 June 1989. (Colors are visible in the online version of the article; http://dx.doi.org/10.3233/SFC-140158.)

had been extremely sorry to learn of Sir Alan's decision, although the reasons for it were appreciated. Sir Alan had been a member of TAGSI since its inception in 1988, during which time he had been TAGSI's most influential member. Indeed, the very existence of TAGSI could be traced to Sir Alan's concerns about the safety of PWRs that he had first expressed in 1973. The Chairman noted that Sir Alan's guiding influences had had an enormous impact on reactor safety within the UK and elsewhere. This unique contribution would be sadly missed". One interesting output from his interest in TAGSI issues was a short paper on warm pre-stressing [9].

Sir Alan's interests in structural integrity were not confined to the nuclear industry. From 1988 to 1995, he was Chairman of the Rolls-Royce Materials and Process Advisory Board (MPAB). Throughout this time, the other permanent members were Tony Kelly and myself; we sought the help of subject experts when needed for specific issues. Although the topics mainly concerned the manufacturing of components and the properties of materials, assurance of fitness for purpose throughout service entailed a number of "lifing" studies: for turbine discs, blades and thermal barrier coatings. Consideration was also given to the properties of organic matrix fibre composites and of ceramic matrix composites. Sir Alan laid the foundations for what is now the Materials, Manufacturing and Structures Advisory Board, which spans the whole range of structural integrity issues. The advice given over the last 25 years has been much appreciated by the Company, and has underpinned many decisions on the introduction of new materials into aero-engines.

Sir Alan was a prolific author of books, including *How Safe is Nuclear Energy* [10], but, for Metal Physicists and Material Scientists, *the* classic is *Dislocations and Plastic Flow of Crystals* [11], first published in 1953. Fifty years later, in 2003, Professor L.M. (Mick) Brown FRS organised, in Cambridge,

a splendid one-day set of "readings" from this book, accompanied by discussions on how the various subject areas had progressed over time. It was a highly convivial occasion, and Sir Alan was clearly very pleased to be able to celebrate in the company of many of his friends and colleagues over the years. A nice touch was that, at the end of the day, a number of us went to the front with our own copy of the book (often, the original 1953 edition) for Alan to sign.

On learning of Sir Alan's death, two people whom I immediately contacted were John Griffiths, my successor as Sir Alan's research student (see above and Ref. [7]), and Bill Tyson, who collaborated with Alan and Tony Kelly on a classic paper [12], relating to the competition between brittle crack extension and crack tip blunting by plastic flow. Bill's reply said "I was a graduate student at Cambridge from 1961–1965, and still remember Prof. Cottrell's lectures as outstanding examples of brilliant insight and clarity. It was an honour to be associated with him during that time. Our discipline has lost a leading light". John echoed these sentiments, but also recalled Sir Alan's love of the occasional pun. "Medal Fatigue" was referred to in Sir Peter's tribute and John produced another, from a time when he and Sir Alan were looking at a recent paper by Peter Forsyth (of fatigue fame). Sir Alan's comment was "I see that we are to be treated to another instalment of "The Forsyth Saga". It so happened that John (who lives in Australia) was just about to visit Bill in Canada, to go north to the Great Slave Lake to view the Northern Lights. Doug Boyd (another Cambridge contemporary) joined them in Yellowknife and, for the occasion, Professor J.D. (David) Embury produced the following (with just a small amendment from me in the penultimate line):

There are strange things done 'neath the midnight sun
By the men who things deform
And the Arctic trails are full of tales
Of the tests that they perform
The Northern Lights have seen queer sights
But the queerest they ever did see
Was the funeral wake at Great Slave Lake
When we toasted Sir Alan C

The words are adapted from a work by the Canadian poet, Robert Service, referring to Lake Lafarge and a certain Sam McGee, but I think that there is something rather splendid in the thought of these old friends from far-flung parts of the world, who first encountered Sir Alan fifty years ago, when they were research students, now sitting under the Northern Lights: dramatic atmospheric displays overhead; recalling their own experiences of Sir Alan, and raising a glass to his memory.

In my *MST* Editorial of July 2009, mentioned at the beginning of this article, I speculated on a suitable material to celebrate a 90th birthday, and settled on Thorium (Atomic Number 90), noting that; "Thorium is named after Thor, the second god in Scandinavian mythology, and equated to Vulcan in Roman mythology: the god of fire and the working of metals!". I also observed that it provided an alternative material for nuclear power generation and was hence related to Sir Alan's interests. It may be of some interest for those who set store by such things that Sir Alan died at age 92 and that 92 is the Atomic Number of Uranium, with which he was very closely involved for much of his life. Uranium is named after Uranus, the Greek personification of Heaven, married to Ge, or Gaia, (Earth) and the father of the Titans: a super-God if ever there were one. Recalling also Geoffrey's "Jesus here – I'll get my Father" story, one can suppose that Sir Alan is destined to sit with the Gods on high, whether of Scandinavian, Greek or Christian persuasion. But how to get there? Elijah was sucked up by a whirlwind and a chariot

Fig. 3. Sir Alan and his "chariot of fire". (Colors are visible in the online version of the article; http://dx.doi.org/10.3233/SFC-140158.)

Fig. 4. *En route* to Apotheosis? (Colors are visible in the online version of the article; http://dx.doi.org/10.3233/SFC-140158.)

of fire; Grizabella in "Cats" joined the "Heaviside Layer", accompanied by that haunting tune, "Memory". In later years, Sir Alan acquired a "chariot of fire" (actually, an electric bicycle) – see Fig. 3. This picture appeared at the back of the order of service for his funeral. We all have our own memories of Sir Alan, and will recall them, like Bill, John and Doug at Great Slave Lake. For me, an evocative memory of Sir Alan is shown in Fig. 4. It is from that day of 5 June 1989 when we visited Sizewell 'B', but it could be seen as an image of Sir Alan ascending to the heavens, leaving behind him the nuclear power station now functioning efficiently and, above all, safely, thanks to his persistent efforts over the decades. A job well done, and now, time to go! Memories of Sir Alan Cottrell remain strong, powerful and, above all, heart-felt: he was a great man, and is sorely missed.

References

[1] A.H. Cottrell, Theory of brittle fracture in steels and similar metals, *Trans. Am. Inst. Min. Metall. Petrol Engrs* **202** (1958), 192.
[2] J.F. Knott and A.H. Cottrell, Notch brittleness in mild steel, *J. Iron and Steel Inst.* **201** (1963), 249–260.
[3] B.A. Bilby, A.H. Cottrell and K.H. Swinden, The spread of plastic yield from a notch, *Proc. Roy. Soc.* **A272** (1963), 304–315.
[4] A.H. Cottrell, Fracture, *Proc. Roy. Soc.* **A276** (1963), 1–18.
[5] A.H. Cottrell, Theoretical aspects of radiation damage and brittle fracture in steel pressure vessels, Special Report 69, The Iron and Steel Institute, 1961, pp. 281–296.
[6] A.H. Cottrell, Fracture, in: *Properties of Reactor Materials and the Effects of Radiation Damage*, D.J. Littler, ed., Butterworths, 1961, pp. 5–14.
[7] J.R. Griffiths and A.H. Cottrell, Elastic failure at notches in silicon steel, *J. Mech. Phys. Solids* **11** (1965), 135–140.
[8] A.H. Cottrell, Mechanics of fracture in large structures, *Proc. Roy. Soc.* **A285** (1965), 10–21.

[9] A.H. Cottrell, Warm pre-stressing and fractures in structural steel, *Eng. Fract. Mech.* **28** (1987), 481–483.
[10] A.H. Cottrell, *How Safe is Nuclear Energy?*, Heinemann, 1981.
[11] A.H. Cottrell, *Dislocations and Plastic Flow in Crystals*, Oxford Univ. Press, 1953.
[12] A. Kelly, W.R. Tyson and A.H. Cottrell, Ductile and brittle crystals, *Philos. Mag.* **15** (1967), 567.