there was one, was part of a binary system whose other component was an object the size of Jupiter. This led to speculation if the pulsar had indeed broken up and produced this companion. After that single observation in January 1989, no signals were detected again, which left many other astronomers in some doubt.

To explain the properties of this pulsar, astrophysicists came up with new equations of state of neutron star matter and even new theories of pulsar formation and behaviour. But bizarre properties or not, supernova 1987A had tantalized astronomers with the opportunity of observing a pulsar almost in the moment of its creation.

Now those hopes have been belied, at least as far as the observations of Middleditch *et al.* are concerned. In January this year, almost exactly a year after their first report, Middleditch *et al.*, now making their observations at Las Campanas in Chile, again detected the signals, but this time they had the impossible frequency of 7874, almost exactly four times the originally detected frequency of 1968.629! Middleditch *et al.* smelled a rat, and indeed the culprit was found — electrical interference from a TV camera used to record the images focused by the telescope. A similar camera had been used at Cerro Tololo when the observations of January 1989 were made.

The US journal *Science* quoted Middleditch as saying, 'I'm a little bit let down and a little bit disgusted.' It is, however, fortunate that the error was detected, and by the same observers. G. Srinivasan of the Raman Research Institute in Bangalore, who had earlier discussed the implications of the pulsar in SN 1987A (see *Current Science*, 1989, **58**, 280), voiced a similar sentiment. 'But this "pulsar" certainly generated new ideas. It's always useful to stand a theory on its head. After all, that is the way of science.' But what of SN 1987A? Srinivasan is hopeful. 'A pulsar is lurking there somewhere. It will be found eventually.'

RESEARCH NEWS

# Interfaces: structure and properties

*K. A. Padmanabhan*

D. McLean's book *Grain Boundaries in Metals* published in 1957 (Clarendon) represented the coming of age of this topic as a subject of serious study. Yet in 1973, while commenting on Gleiter and Chalmers' review of 'High-angle grain boundaries'[1], R. W. K. Honeycombe lamented about the 'notorious gap' between fundamentals and practice[2]. In his review, Honeycombe lists segregation, diffusion, migration and sliding at grain boundaries as partially understood problems. To this day these problems have not been solved. What then, has been the recent progress in this area? Has the gap between fundamentals and practice reduced? Is the settled pattern of research in this area adequate or is there a need to adopt new strategies? These questions were uppermost in this author's mind when he decided to attend a workshop*.

The workshop was inaugurated by R. Krishnan, Chief Controller, R&D, Defence Research and Development Organization. The keynote lecture was delivered by F. E. Saalfeld, Director, Office of Naval Research, USA. In all there were twenty-four invited papers. Over 140 delegates including 25 foreign scientists participated. On display were interesting posters dealing with diverse topics in metallurgy and materials science. The deliberations, some of which are discussed here, will appear shortly as a book.

From the beginning one could discern two near-parallel streams of thought: One holding forth that understanding thoroughly the structure of an interface is the essential first step and another emphasizing understanding of the interface properties, if necessary by wilfully neglecting certain details (which may later be introduced as secondary effects). It was also evident that over the years the field has widened to cover both intercrystalline and interphase interfaces and 'materials' instead of 'metals'.

V. Vitek described the atomic structure of grain boundaries in ordered and disordered binary alloys using many-body empirical potentials to represent the interatomic forces. It then becomes possible to understand the differences in the intergranular strength and brittleness of alloys and metals. However, as the empirical potentials are generated in the first place using some specific macro-properties of the materials, the procedure can only be regarded as rationalization. Theoretical development of many-body potentials, which will eliminate the semi-empirical nature of the modelling process, very much remains a desirable goal.

S. Ranganathan 'revisited' the coincidence site lattice (CSL) model and went on to explore the relation between the CSL and quasi-lattices. L. A. Bendersky gave a new definition of special orientations (hypertwin) based on the reduction of the number of arithmetically independent lattice vectors. Both confined themselves to describing geometrical relationships obtainable at the grain boundary giving no clues as to the features that are important for understanding the properties of interfaces.

K. H. Westmacott and U. Dahmen acquainted participants with the interesting technique of combining high voltage electron microscopy with the hot stage and a video camera for directly observing boundary migration and the effect of twins on interface mobility. They also considered, among other matters, the significance of the role of microfaceting and the effect of strain, which were interesting in that they provided information of relevance to ledge growth, for example.

An unusual departure from the interatomic potentials and atomic configurational details was represented by the

*The Indo-US Workshop on Interfaces: Structure and Properties was held in Bangalore, 30 November–2 December 1989.

non-discrete, density functional theory (S. Sengupta and T. V. Ramakrishnan) for describing the fcc–bcc interface, central to martensitic nucleation. Although some traditionalists were skeptical of this effort at 'pushing physical metallurgy backwards' the fact remained that an attempt was made to account for interface behaviour. If a 'continuum' can be quantized to obtain useful results, why cannot one make the boundary structure non-discrete for reaching certain desirable goals?

It is possible to determine the nature, distribution and concentration of stacking faults using diffraction analysis, as was demonstrated by D. Pandey. His warning that metallurgists should not get obsessed with a few odd details was well-meant and in order.

S. Mahajan considered both homo- and hetero-interfaces in electronic materials. By analysing domain contrast in ordered layers he brought home the fact that tubes of disordered material, caused by steps on the surface of the underlying substrate, could simultaneously exist. Interfaces in electronic materials is a nascent field. A lot remains to be understood.

Faceting in two- and three-dimensional quasi-crystals (D. Levine) and the characterization and energetics of dislocations and grain boundaries in quasi-crystals (Sriram Ramaswamy) were adequately discussed. Is Pauling's cat finally dead?

Bi-crystals of high melting point bcc metals grown using a floating zone technique (Y. T. Chou and A. Das Gupta) could be used to show that the (electrical) flux pinning force depends on the orientation of the tilt axis but is insensitive to grain boundary misorientation. Evidently, these results are of relevance to type II superconductors.

H. I. Aaronson pointed out that the phenomenological theory of martensitic transformation (based on shear) is able to predict the interfaces and the direction in such interfaces along which ledge formation (and possibly also ledge growth) occurs least readily and which are therefore retained preferentially during growth. Such interfaces then become the broad faces of Widmanstatten plates and the sides of Widmanstatten needles. It is only because of this that the phenomenological theory is useful in understanding many important aspects of diffusional phase transformations. Perspectives of this type are interesting because, in the absence of a comparison between the macro and micro levels of analysis, the above conclusion could not have been reached.

The effect of interfaces in microstructural development in Zr–Nb alloys during Widmanstatten precipitation, tempering of martensite and static and dynamic recrystallization was discussed by S. Banerjee. Grain nucleation during recrystallization and isothermal grain growth was considered by C. S. Pande and B. B. Rath as a stochastic process for predicting grain growth and grain size distribution with and without particles. S. Mishra, on the other hand, traced the Goss texture in iron–silicon alloys to surface shear in hot rolling. He pointed out that the removal of the Goss texture could inhibit grain growth. These were neat studies that employed established procedures.

One learnt (from C. A. Handwerker) of a metal reference line technique and of the determination of the crystallographic orientations and grain misorientations in polycrystals from electron back scattering patterns generated in a scanning electron microscope for characterizing the energies and the misorientations of grain boundaries present in fine grained materials useful experimental procedures.

D. A. Smith noted that a dislocation model of interfaces may account for misfit accommodation and energy and can also address qualitatively the mechanisms of point defect emission and boundary sliding. But the model fails to account quantitatively for grain boundary mobility and more importantly the general physical relevance of the model itself is uncertain. His was the dilemma of an honest intellectual—what if one had been looking at the 'wrong' features of the interfaces?

The role of heat treatment and alloy chemistry and the use of grain boundary segregation isotherms and interaction maps in the control of boundary segregation with a view to improving the toughness were described by R. D. K. Misra—a study that has immediate practical use.

According to D. Banerjee the interphase interfaces, in conjunction with the specific nature of slip, play a critical role in determining the location and density of failure initiation sites and their subsequent linkages in tension and creep of Ti and $Ti_3Al$ base alloys. His very apt comment about the 'black box' that exists between the structure of the interfaces and their properties was contested by H. I. Aaronson, but realizing the limitations of one's knowledge is important.

P. M. Hazzledine treated two-dimensional Zener pinning by analogy with the Friedel or Labusch models of solution hardening. (However, for the three-dimensional case the results of computer simulation disagreed with the predictions of the Friedel–Zener theory.) Zener pinning is of major consequence in Hall–Petch effect and superplasticity and is the main factor causing grain coarsening resistance in the commercially successful (superplastic) 'Supral' alloys.

Advocating the decisive use of 'Occam's razor' in problems of analysis (towards which end the sanction of Bertrand Russell and Stephen Hawking was invoked), K. A. Padmanabhan stressed that for successful theoretical development the essentials have to be separated from a host of extraneous details. Judgment no doubt plays a part, but without this exercise a physical model may lose its significance through the introduction of many empirical constants/parameters. He argued that unequivocal evidence during optimal superplastic flow existed only for the occurrence of grain/interphase boundary sliding–diffusion coupled flow and grain rotation. The implications of this conclusion for theory were also discussed.

T. G. Langdon discussed the role of boundary migration and sliding in high temperature, low cycle fatigue. He showed that boundaries of the coincidence type do not migrate and that while in a pure metal cavities form only after a large number of cycles, in a solid solution alloy microcavities nucleate in the very early stages of a test. No attempt was made by him to fit the results into a theoretical framework.

B. Cantor proposed an 'interfacial adsorption model' of the solid–liquid interface that is useful in predicting liquid undercooling before the onset of heterogeneous nucleation. This effort is commendable because of its applicability to solidification processing, although it was stated by a few that the description of the interface was somewhat sketchy.

K. T. Jacob explained that enhanced carrier concentration is present in the diffuse space charge layer surrounding the dispersoids, which leads to stable microstructures of high conductivity in dispersed, crystalline solid electrolytes. Reasons were given for the discrepancy between the theoretical predictions and the experimental results concerning the defect concentration profiles.

This workshop was organized by the Department of Metallurgy, Indian Institute of Science, Bangalore and was supported by the Materials Research Society of India and the Indian Institute of Metals (Metal Sciences Division and Bangalore Chapter). The Department of Science and Technology, Government of India and the Office of Naval Research and the American Institute of Biological Studies, USA, sponsored the workshop. By considering both the fundamentals and applications, the workshop brought out clearly the distinction between 'metallurgy and materials science' and 'solid state physics and chemistry'.

1. Gleiter and Chalmers, in *Progress in Materials Science*, Pergamon, 1972, vol. 16.
2. Honeycombe, R. W. K., *Metals and Materials*, 1973, 7, 298.

*K. A. Padmanabhan is in the Department of Metallurgical Engineering, Indian Institute of Technology, Madras 600 036.*

# Plant breeding and molecular biology: whither shall the twain meet?

*J. Gowrishankar*

It is for the better part of a century now that the plant breeder has exploited the principles of Mendelian genetics in developing and evaluating new plant varieties for agricultural use. There is no quarrel over the fact that his efforts over the years have been enormously successful, nor over the one that he will continue to play the major role in all attempts at crop improvement for some time to come. (I have deliberately used the male personification here, for the gender distribution amongst this class of people is more biased than is the case in most other sciences.) Recent advances in plant molecular biology, particularly with regard to the identification, mapping and transfer of genes and traits from heterologous organisms into plants, nevertheless offer new approaches that merit the breeder's consideration. A symposium* was organized with the very intention of bringing the two communities of workers (the breeders and the molecular biologists) together, and the focus was upon the potential for molecular genetic approaches in the task of breeding for tolerance to abiotic and biotic stresses in crop plants.

Looking back upon the deliberations of the symposium, one could clearly discern the new gene *technology* in all its glamour: Agrobacterium-mediated transfer of a variety of genes into tobacco (a method matter-of-factly alluded to by so many speakers that I had constantly to remind myself that it did not exist eight years ago); successful gene transfer into rice protoplasts and regeneration of fertile plants therefrom (independently achieved and described by several groups); development and use of probes for restriction-fragment length polymorphisms (RFLPs); and other *in vitro* methods (with associated jargon!) routinely used by this tribe, such as pulsed-field-gel electrophoresis, linker scanning analysis, run-on transcription assays, subtraction hybridization, homology searches, gel-retardation experiments, particle-gun bombardment, and the like. Also in evidence were reports on the successful application of this technology in answering some questions of basic interest—on, for example, the biology of rice tungrovirus, or coat protein-mediated resistance to virus infection, or the regulation of gene expression during plant development or under varying environmental conditions such as anaerobiosis, salinity stress, or pathogen attack.

But there was little in the symposium to excite the bread-and-butter interests of the plant breeders in attendance. In part, this is because many of the fruits of the new technology are not immediately at hand, but rather remain largely as promises to be fulfilled in the coming years; to a greater degree, however, it reflects the lack of depth as exists at present in the molecular biologist's approach: although it is conceptually feasible (now or in the near future) to incorporate a gene of interest from *any* living creature into transgenic plants, the number of such candidate genes or strategies that have so far been identified from the standpoint of utility remains extremely limited (as typified by the fact that, of six independent presentations on the topic of genetic engineering for resistance to insect pests, as many as four dealt with the use of a protease inhibitor gene, and the remaining two with that of the insecticidal protein gene of *Bacillus thuringiensis*). Where important traits are controlled by multiple genes—a phenomenon which is more the rule than the exception in plant genetics—the application of the molecular biological approach for making transgenic plants becomes progressively more difficult.

The experienced breeder is also justifiably sceptical of the approach of 'single gene-engineering' of traits such as pest resistance or herbicide tolerance in new crop varieties, because the forces of natural selection operating in the field conspire to limit the timespan over which such traits retain their utility. This point, or even the ecological factors to be considered before the release of transgenic plants for use by the farmer, was only fleetingly touched upon in the final round-table discussions. On the other hand, the complexities inherent in the genetics of and breeding for resistance to diseases and insects, and for tolerance to water-, salinity- or

*International symposium on Molecular and Genetic Approaches to Plant Stress, organized by the International Centre for Genetic Engineering and Biotechnology (ICGEB), New Delhi, 14–17 February 1990.