Transformations: Selected Works of G.B. Olson on Materials, Microstructure, and Design

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- This is just the introductory chapter to Part I.
- Selected works of Greg Olson

INTRODUCTION TO Part I: Martensite

By Harry Bhadeshia

Martensitic transformations in steels are thermodynamically of first order in the Ehrenfest classification. This means that the product and parent can co-exist in a two-phase mixture, a fact that is apparent from the myriad of direct observations. In almost all known circumstances, the nucleation of martensite occurs heterogeneously at defects present in the austenite. The very first paper by Greg was co-authored with Morris Cohen in 1972 (Ref 1) in a festschrift honoring Professor W. G. Burgers, a name surely familiar to every materials scientist in the context of the displacement vector associated with a dislocation. This work identified a clear distinction between stress-assisted and strain-induced nucleation, the former being a simple manifestation of the change in thermodynamic force activating already existing sites, whereas the latter involving the creation of additional nucleation sites by plastic deformation, thus permitting the cascades of nucleation events described generically by the term “autocatalytic nucleation.”

Morris Cohen, Greg’s thesis advisor at MIT, had previously published a number of papers on martensitic nucleation (Ref 2-4), based on classical theory involving heterophase fluctuations, some of which may be quenched in from the austenitic state. However, such fluctuations lead to an inverse functional dependence between the activation energy for nucleation and a power of the driving force, which is not observed experimentally [e.g. (Ref 5)]. By 1976, Olson and Cohen had published a series of seminal papers (Ref 6-8) in which they generalized the dislocation mechanism for martensitic nucleation that previously had been used in the context of the conceptually simpler austenite to epsilon martensitic transformation (Ref 9). In that particular transformation, when the volume change is neglected, the partial dislocations (Shockley partials) that accomplish the lattice deformation involve simple shear by glide on a close-packed plane, so the nucleus has the final crystallography of the visible martensite plate that evolves. However, the austenite lattice cannot be changed into a body-centered one by a shear – the deformation is more complex and leads to a crystallography with irrational parameters, and one that generates a substructure in the final product. Chapter 1 presents a detailed review of Olson and Cohen's dislocation-based martensitic transformation theory. The basis for this dislocation-based theory is outlined in Chapter 2, which presents the hierarchy of interfacial dislocation structures.

Olson and Cohen abandoned some of the features of the macroscopically observed crystallography when proposing the nucleus, arguing that the macroscopic form develops as the nucleus evolves, an assumption that permitted a description of the detailed structure of the dislocations and the nucleus/matrix interface. These ideas, reviewed in Chapters 3 through 6, were implemented in a thermodynamic framework that naturally led to a linear dependence of the (small) activation energy for nucleation on the magnitude of the chemical driving force (pointed out by Magee (Ref 5) in his derivation of the classical Koistinen and Marburger equation (Ref 10)). Classical heterophase fluctuation theory gives, in contrast, an inverse power relationship. I was enthralled by the 1976 publications, which I studied repeatedly. In later years, when I observed a particular dependence of the bainite-start temperature of each alloy on temperature itself, I was able to explain this quantitatively in terms of this linear dependence of activation energy on driving force (Ref 11) (Chapter 7). It was the 1976 papers that nurtured our subsequent collaborations on theory (Ref 12, 13), with some of the work done at the beach in Boston, nourishment provided by Jane in the form of peanut butter and jelly sandwiches. There is also some fascinating unpublished work with Carrie Campbell and Greg which develops the theme further to encompass a wider range of transformations, with the genesis done in Cambridge when Greg spent an extended period of intellectual bliss. There is much more to be said about the nucleation of martensite, a subject reviewed at length in Greg and Morris Cohen's (Ref 14) contribution to Nabarro's epic compilation on dislocations (Chapters 1 and 3). Chapters 1 and 5 include a charming discourse on biological martensitic transformations, work which I quote to the delight of students in my annual lectures for the Natural Sciences Tripos in Cambridge.

I recall a period of gentle confusion in martensite, partly because there was a concerted effort to make the subject interdisciplinary. Soft modes and solitons became fashionable, and the term martensite began to be used in circumstances that were not entirely appropriate. I vividly remember two publications that were seminal in resolving the issues, both in the proceedings of the International Conference on Martensitic Transformations (ICOMAT) '79, a conference organized at MIT by Greg and his co-workers. This was my first ever international conference, indeed, the first overseas trip from the U.K. And, what a wonderful experience it turned out to be, with every single paper that was presented pregnant with meaning, and presented in the presence of giants in the subject. The plenary lecture (Ref 15) by Cohen, Olson, and Clapp demonstrated that the term martensite should be reserved for those transformations dominated by homogeneous deformations that lead to shape deformations with large shear components. This distinguished martensite proper from shuffle dominated transformations and from those where the strain energy is not the determining factor. The second influential paper was by Christian (Ref 16); the idea that the genesis of martensite can be described by a lattice instability in which some elastic constant vanishes (soft mode, strain spinodal, etc.) does not make sense, he argued, because we can observe martensite and austenite to co-exist. The transformation is thermodynamically of first order. To obviate this difficulty, some were arguing for a soft mode to occur at a dislocation strain field, leading to the nucleation of martensite. However, such localization, he said, is not formally different from classical heterophase fluctuation theory because it will involve an interface (albeit a soft one) between the nucleus and matrix (Ref 16).

Greg and I had not connected at the time when ICOMAT ’79 was organized, but the conference was formative for my career and in stark contrast to modern meetings that lack depth. But it was the 1979 paper on interphase boundary dislocations (Ref 17), that inspired me to contact Greg and Morris Cohen. This paper clarified for me how single-atom steps can be described as “coherency dislocations,” whose motion accomplishes the lattice change (including dilatation) and yet are glissile! The long-range strain fields due to the coherency dislocations are then accommodated by a set of “anti-coherency” dislocations, which have Burgers vectors that are lattice vectors. Many years later at a meeting in Bariloche, Argentina, Aaronson suggested that the fcc to bcc transformation in brass must involve diffusion because of the volume change. I immediately responded that coherency dislocations can do the job without diffusion. He then asked me how the Burgers vector of the “so-called coherency dislocation” could be described and I was able to explain it in terms of the generalized Burgers circuit defined by Bilby and Christian. Greg was in the audience and I could see him smile via my peripheral vision.

Greg has been impressive in applying his knowledge to the design of strong steels (Ref 18-20) and Chapter 8 lays the foundation for this design approach, outlining the application of transformation plasticity models to predict the mechanical behavior of advanced steels. Charlie Kuehmann discusses the commercial success in creating a design-based industry in the Introduction to Part II of this book, the latest achievement being the blast-resistant steels for the Navy (Ref 21, 22) (Chapters 18-19). I have a feeling, which may or may not be historically accurate, that his design philosophy was kindled by the work on lanthanum additions to rapidly solidified steel (Ref 23), where the purpose was to getter the impurities that exacerbate stress corrosion cracking. Steel design never involves the optimization of a single property; Greg worked hard to educate undergraduates on what design actually means (Ref 24) (Chapter 29), illustrating his ideas with the systems approach that he so loved, inspired apparently by C. S. Smith. At one point, I had dreams about his flow charts. Greg then went on to develop a friendship with first principles calculations, beginning, I think, in the last decade of the last millennium. Iron has the most odd magnetic properties in all its manifestations and the first paper (Ref 25) revealed the stabilities of the ferromagnetic and antiferromagnetic states of austenite. With Krasko, (Ref 26) he published a rather important paper based on ab initio methods, on the role of species such as boron, carbon, and phosphorus on intergranular embrittlement, commenting in particular on the effect of these “‘impurities” on the anisotropy of bonding across the boundary. More detailed studies on this and associated subjects followed later in a close collaboration with Freeman and his team at Northwestern University (Ref 27-29) and are reviewed in Chapters 13-15. I often refer to these papers in my own work on impurity effects (Ref 30) and have mentioned them in an undergraduate text (Ref 31).

There is so much to say about Greg's contributions to science that cannot fit into this Introduction, but a few items must get an honorable mention. Ling and Olson accounted for anisotropic elasticity in dealing with the strain field around martensite (Ref 32). The work on the early stages of tempering (Ref 33) is important in showing that the vast majority of martensite, which in high-M_S steels, is unlikely to contain all of its carbon in the solid solution as defined in thermodynamics. Why is the lattice invariant shear by twinning in some ferrous martensites, and by slip in others? Grujicic, Olson, and Owen (Ref 34) suggested that the mobility of twinned interfaces is much greater than when the lattice invariant deformation involves the glide of dislocations. Therefore, for rapid transformations the twinning mode is favored. Indeed, it is observed experimentally (Ref 35) that when the heat of transformation cannot be dissipated fast enough, the resulting rise in temperature causes the interface to slow down so that a plate that is internally twinned in the center will change to being internally slipped at the peripheries.

I finish with some personal comments. Greg has been a great friend and inspiration, a true scholar and a gentleman. I was able to discuss science with him. I sometimes felt inadequate afterwards, though never disheartened. Greg and Morris Cohen were both there when, unknown to me, I needed support during an era of controversy about bainite. I congratulate him wholeheartedly on his 70th birthday and I am certain that he will continue to surprise and delight. I take the opportunity to send my best wishes to Jane and Elise.

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