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# Remembrances of Clarence Zener*

We, in the field of internal friction, think mainly of Clarence Zener as the father of our field. Indeed, he did tremendously advance the study of energy loss in crystalline solids, both by his own research and by his influence on senior colleagues and students. However, many other facets of his professional career may not be as well known to many of us. In this brief comment, I recount a little of the work by which we know him, but also cite features of other work for which he is even more widely known.

The career of Clarence spanned more than 60 years. His published work dates to the late 1920s, when he was one of a small number of physicists to publish papers on aspects of wave mechanics in crystalline solids. He had just completed an undergraduate degree at Stanford and a graduate degree at Harvard. Two papers from that period are particularly notable – both published in the *Proceedings of the Royal Society* while he was a Fellow at the University of Bristol. The first, titled 'Non-adiabatic crossing of energy levels', is a short paper, some six small journal pages [1]. Yet, even after 60 years, it is heavily cited – some 50 citations per year. The second, 'A theory of the electrical breakdown of solid dielectrics', is also only a short six page article [2]. Yet it is after this paper that a particular p–n junction, the Zener diode, is named. For many scientists these papers would be a career, but Zener was only starting his long professional life.

Clarence returned to the United States to take a position at Washington University in St. Louis, where he continued his research in the theoretical aspects of photoelectric emission from solid surfaces [3]; and X-ray diffraction in crystals [4]. He also turned to other aspects of the study of solids – internal dissipation of energy in vibrating solids, the study for which we most know him. Of the two dozen papers he published on this subject before 1946, two groups of papers are particularly notable: energy dissipation by thermal currents [5] and the energy loss in substitutional solid solutions, a phenomenon we now call the Zener effect [6, 7].

Clarence said later that he "left" theoretical physics in the 1930s to study things of a more practical nature because he felt that he could not support his family as a theorist. Remember that this was the depths of the great world-wide depression, and the prominence of the papers cited earlier had not yet been established. However, this trait – boldly connecting theory with technological practice – was a major feature of his entire career.

Clarence spent the years of World War II as Principal Physicist at the Watertown Arsenal. He later mentioned a little of the work into penetration of steel armor plate by heavy projectiles. Whatever the success of that work, it must have resulted in him learning of the technological problems in the hardening of steels, for he later published a landmark metallurgical paper titled 'Kinetics of the decomposition of austenite' [8]. That paper described the kinetics of the formation from austenite, of product phases such as pearlite and bainite. Perhaps his longest paper (some 45 journal pages), it provoked a great deal of discussion – 12 pages of critical discussion and comment follow the formal paper. Reading his replies to comments and criticisms from the most prominent steel-metallurgists of the time shows another aspect of his style – always gracious, never critical or angry. He both maintained his position when he felt he was right and accepted corrections when features of steels new to him were pointed out (metallurgists, in common with all competent technologists, have a memory bank of countless facts). The reader may grasp the sense of that paper, and others in metallurgy, from the paper by Mats Hillert contained in a symposium honoring Zener's 80th birthday [9].

Zener joined Cyril Smith (and others) in founding the Institute for the Study of Metals at the University of Chicago in 1945. The group of physicists, metallurgists and chemists formed perhaps the first interdisciplinary materials research institute in the nation. The Institute included such well known members as C.S. Barrett (metallurgy), A.W. Lawson (low temperature physics), E. Long, L. Meyer and N. Nachtreib (Chemistry) as well as Smith and Zener. The University had other prominent faculty – among other senior faculty were E. Fermi and E. Teller (nuclear), W. Zachariason (X-rays), G. Wentzel (quantum physics), R.S. Mullikan (atomic physics), W. Libby, J. Mayer and H. Urey (chemistry), and M.-G. Mayer (physics).

It was there that Zener formed a group of young researchers in Internal Friction. T.S. Kê came from Berkeley, L. Dijkstra from the group of Snoek at Philips-Eindhoven, A. Nowick from Columbia and C. Wert from the

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University of Iowa. I need not describe their work in detail except to state this: Clarence had so many ideas, and shared them so freely that his colleagues advanced the field rapidly. If there was a hallmark of the work it was that a physical model should be developed—just as was true for his earlier work on thermal currents and the Zener Effect.

One trait which Clarence had learned 20 years earlier at Harvard was the use of dimensional analysis. Bridgman was a proponent of its use and Clarence applied it to both his austenite decomposition analysis and to the formation of precipitates from iron–nitrogen and iron–carbon solid solutions (investigated by Dijkstra and by Wert). Thus, he proposed that a sphere should grow as $t^{3/2}$, a cylindrical rod growing radially and lengthwise as $t^2$ and a flat plate growing edgewise in two dimensions and thickening in the third dimension as $t^{5/2}$. He then modified these time-dependencies to take account of impingement as the precipitates grew large enough for the diffusion zones to interfere with each other. Yet the first notion came from dimensional analysis. Hillert describes in more detail Zener's many attempts to model precipitate nucleation and growth.

His book, *Elasticity and Anelasticity of Metals*, was published at that time [10]. However, he was not fully occupied with internal friction. He proposed a mechanism of diffusion based on rotation of rings of atoms—ring-diffusion [11]. It happens that energetically this mechanism is not preferred over diffusion by motion of vacancies in metals, but attendees of meetings of the American Physical Society well recall the spirited discussions which took place. It is characteristic of Zener's style that this paper would be published as a companion paper to one of his friend, Fred Seitz, who argued in his paper for his long-held preference of the vacancy mechanism.

Clarence had long been interested in magnetism, and in this period he also published on the origin of ferromagnetism in transition metals [12]. He argued that the direct interaction of unfilled d-shells was not the only interaction of importance; interactions through the conduction electrons were also important. Again, vigorous discussions took place at national meetings.

Two other phenomena are worth mention. The first came from his intimate knowledge of elasticity of crystals. Using published data, Zener came to the understanding that some of the alkali metals and their alloys might have a low shear modulus in certain directions, "soft modes". Thus they might undergo crystallographic phase changes under appropriate conditions of temperature or imposed stress. Indeed they do: two such studies (out of many reported from the Institute in the late 1940s) describe results on lithium and its alloys with magnesium [13] and alloys of In–Tl [14].

The second, resulting from an idea, was expressed as a "private communication" in a paper written by Cyril Smith on grain boundary migration [15]. Zener had suggested that small precipitates might retard grain boundary migration and provided a short numerical analysis to give the magnitude of the effect. This phenomenon, now known as "Zener Drag", is even now cited in many papers on grain growth, although Zener never published a paper on the effect.

Chicago was an exciting place in the late 1940s, but by 1950 many of the young students, post-docs and visitors left for more permanent jobs elsewhere. Indeed, Zener himself left in 1951 to become Director of the Research Laboratory of the Westinghouse Corporation in Pittsburgh. There he embarked on even new directions of study. Westinghouse had long had an excellent research program in magnetism. His promotion of that field after 1950 is described in a paper by W.J. Carr, Jr. in the symposium honoring Zener's 80th birthday [16]. Carr also describes some of the background in the naming of the Zener diode by Shockley.

A principal effort at Westinghouse was a large scientific and technological development on generation of electrical energy using the thermoelectric effect. Establishing high efficiency depends on obtaining a material with a high value for a parameter defined through the product $\alpha^2/\rho\kappa$ ($\alpha$ is the Seebeck thermoelectric coefficient; $\rho$ is the electrical resistivity; and $\kappa$ is the thermal conductivity). Neither insulators, which have high values of $\rho$, nor metals, with high values of $\kappa$, are appropriate. So, materials in-between, semi-metals, are the materials of choice. The result of this work (and that of other groups) is described in papers by Zener [17, 18] and in a book by two of the Westinghouse scientists, Heikes and Ure [19].

Zener left Westinghouse in the 1960s and held the post of Dean of Science at Texas A&M University for several years. Upon retirement from that institution, he returned to Pittsburgh, where he maintained an office at The Carnegie-Mellon University and continued productive scientific research for the remainder of his life. During the energy crisis of the 1970s he proposed using temperature differences in the ocean as a source of electrical energy. A fluid, such as ammonia, would be pumped in a closed cycle through a turbine which would drive an electrical generator. The liquid refrigerant would be vaporized in warm surface water, pass through the turbine (which would drive an electric generator) and release heat as it condensed in the colder water well below the surface. The temperature differential is not great, less than 20 K, so the Carnot efficiency is low; development of a large amount of power demands a huge system with large flow of water. Clarence presented the idea in a paper in *Physics Today* [20]. Its publication resulted in huge response from readers; Letters to the Editor (pro and con) were published for more than a year. Through it all, Clarence replied with the same calm aplomb which was his life-long style. A more formal paper was published later [21].

Zener continued to publish regularly on many topics. On the occasion of his 80th birthday, he presented a paper titled 'Equation of state for strong electrolytes' [22]; and he published his last paper on still another topic: 'Explosion of drops impacting non-wetting rigid surfaces' [23].

His was a long and distinguished career. He had so many ideas that he could freely give many away to his colleagues and still have plenty for his own use. A genial colleague, he and Ruby, his wife of more than 60 years, became close friends to all of us fortunate enough to know them.

Charles Wert, Professor Emeritus, University of Illinois, Urbana, IL, USA

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