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**AFTER DINNER COMMENTS AT 8TH INTERNATIONAL CONFERENCE ON INTERNAL FRICTION**

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I am happy for this opportunity to share with you my early memories of internal friction. I vividly remember one spring day, exactly fifty years ago, in 1935, sitting at a workbench in the Wayman Crow Physics Laboratory of Washington University, St. Louis. Before me was an article in the current issue of *Physics*, a journal which changed the following year into the *Journal of Applied Physics*. In order to explain my reaction to the article before me, I must say a few words about my background. The two men who had the greatest influence on my life were Oppenheimer and Bridgman. After spending an entire day with Oppenheimer, several years previously, I had recognized the futility of competing in research areas dominated by minds of his caliber. Rather than attempting to advance the fundamental laws of physics, I must confine myself to explaining puzzling physical phenomena in terms of known laws of physics. Towards this end I assiduously searched for puzzling physical phenomena in all current issues of British, German and U.S. physics journals. Through his courses at Harvard, Bridgman had transmitted to me his own fascination with thermodynamics. To those of you who are not physicists, I interject that thermodynamics deals with the interaction between the thermal and the dynamical properties of matter.

Here before me was an article by Wegel and Walthers of the Bell Telephone Laboratory. Its title was "Internal Dissipation in Solids for Small Cyclic Strains." They presented measurements on the internal friction of many metals over a wide range of frequency, and over a range of temperature. They concluded that each metal had a characteristic internal friction, and that this was only mildly dependent on frequency. They gave no interpretation of their measurements. Clearly the mechanism of conversion of the energy of vibration into heat was a complete mystery. Suddenly my thermodynamic interest provided some insight, and came to my rescue. As a metal periodically compresses and expands, its temperature must periodically rise and fall. The inhomogeneity of the periodic stresses within a vibrating specimen thereby induce periodic temperature fluctuations, and hence to fluctuating thermal currents. It is this continual flow of heat that causes internal friction.

The exciting feature of this interpretation of one source of internal friction was that it could be precisely calculated. At very low frequencies the vibrations would be essentially isothermal, no thermal gradient. At very high frequencies the vibrations would be adiabatic, no thermal flow. Only at intermediate frequencies, where we have both significant temperature fluctuations and significant thermal flux, does one anticipate high internal friction.

After two years my students confirmed that in a transversely vibrating reed heat currents flowing back and forth across the reed do indeed give rise to the predictable internal friction. The plot of the internal friction gave a maximum just at the predicted frequency, and this maximum had just the predicted height. Just at this time Bennewitz and Rotger published an article in the *Physikalische Zeitschrift*, giving their measurements on the internal friction of wires vibrating transversely. We were of course delighted to see that they were mystified by the peak in their measurements vs. frequency.

Here I inject a few comments to give you some idea of what were the facts of life for a physicist in those depression years. For a higher salary, I transferred from Washington University to City College of New York City, with 15 hours of teaching each week. On arrival I explained my research plans to the Head of the Physics Department. He explained that the city provided no funds for research, but that if I obtained outside funds for equipment, he would find a room in which I could work. Those days were before the National Science Foundation, even before the Office of Naval Research. Fortunately the Engineering Foundation was interested in my plans for finding the various sources for internal friction in metals. They donated \$500 to equip an internal friction laboratory. On hearing their good news my Department Head guided me through a long corridor. At its end he pointed to a door and said, "That room has not been used for many years. It's yours." The door was labelled WOMEN. Next day, I spent my first \$2 to cover up that label with a sign METALS RESEARCH LABORATORY, the only research laboratory in Physics in CCNY at that time.

The next two years were spent in demonstrating the existence of inter-crystalline thermal currents. In order to avoid the internal friction due to transverse thermal currents, we employed longitudinal vibrations. The frequency range which separated isothermal from adiabatic vibration now depended upon grain size. Our experiment therefore required a set of specimens, all of the same material but of widely differing grain size. The Metallurgy Department of Columbia University told me there was only one metallurgist in the country who would take the trouble to prepare such a set of specimens, namely Cyril Stanley Smith of the American Brass Company. My favorite story is how at the next Metallurgical Society meeting I introduced myself to this gentleman, invited him to a bar, and while under the influence of liquor, he promised to prepare the desired set of specimens. Within two years Cyril had prepared the desired specimens, my collaborators Randall and Rose had made the desired measurements, and our perfect correlation of experiments with theoretical predictions had been published. Our Metals Research Laboratory was off to a good start.

This good start was shattered one day by a visit by Paul Anderson, head of Physics Department at Washington State University. He had just been asking Bridgman's advice on whom to ask to fill a post at his department. Bridgman had apparently been fascinated with how I was applying his thermodynamic principles to metals, and had recommended me. I then moved to Washington State.

Cyril Smith continued to supply me with the requested specimens. One specimen, a single crystal bar of alpha brass, deserves special mention. The purpose of this specimen was to demonstrate that in the absence of all thermal currents the internal friction would be extremely low. And it was, less than 1% of the peak value of polycrystalline brass. A nagging question was: What was the origin of this small residual internal friction? Devoid of any inspiration, I wondered what would happen if I raised the temperature. Not surprisingly the internal friction increased as the temperature was raised. Obviously the crystal was manifesting viscous behavior, and the internal friction would continue to increase with rising temperature. I never will forget late one night when I was pushing the temperature higher than ever before. As the temperature passed 400°C, the internal friction began to decline. As the temperature approached 600°C, the internal friction approached the original very low room temperature value. This was no viscous behavior. Here was clearly a new unknown behavior. This behavior remained a mystery until several years later when I learned of Snoek's work on carbon in iron. Snoek had interpreted his internal friction peak as arising from the tetragonal lattice distortion in iron caused by dissolved carbon atoms. Obviously a pair of neighboring zinc atoms produces a similar type of tetragonal distortion in the copper lattice.

During my stay at Washington State University I ran across the 1920 work of Raman on the inelastic behavior of large glass plates when struck by hard steel spheres. Raman dropped steel balls onto large horizontal plates supported at their boundaries, and measured that fraction of their original height to which they would rebound. That fraction he called the coefficient of restitution. Raman found the coefficient of restitution approached unity as the drop height approaches zero, but

rapidly decreased exponentially as the drop height was increased. For small drop heights Raman's observations agreed perfectly with his theory, but rapidly diverged at increasing drop heights. I found that complete agreement of theory with Raman's experiments for all drop heights could be obtained by correctly applying Newton's concept of action and reaction, namely that the force the ball exerts on the plate is equal to the force the plate exerts on the ball.

My article, "The Intrinsic Inelasticity of Large Plates," appeared six months before Pearl Harbor. A month after Pearl Harbor the Watertown Arsenal in Boston asked me to join their staff. They were responsible for the design of armor plate and of armor piercing projectiles.

Surprisingly, the simple principles I had learned from analyzing Raman's experiments of steel balls and glass plates turned out to be directly applicable to some of their problems. Take Newton's principle of action and reaction. It had direct application to penetration of armor by projectiles. Montgomery was fighting Rommel in the African desert. He found his anti-tank guns, so effective against Rommel's tanks at long range, would not penetrate the same tank armor at close range. The answer: at close range Montgomery's armor-piercing shells were moving at such a high velocity that they exerted an especially high force upon the armor, and by Newton's law the armor plate exerted an especially high force upon the shells, so high that their front end plastically deformed and then essentially splashed against the armor. Short term correction: Lower the velocity. Long term correction: Harden the front portion of the projectile.

For another example, remember the importance for internal friction, whether we are dealing with isothermal or adiabatic deformation, or with a mixture of the two. The ability of armor plate to resist penetration by a projectile is drastically weakened because of the adiabatic nature of its plastic deformation. The attack by a projectile is so rapid that there is no time for the escape of the heat generated by plastic deformation. The stress necessary to maintain continued shear deformation in a metal continually increases if the deformation is isothermal, but reaches a maximum, and thereafter becomes smaller, if the deformation is adiabatic. Once this maximum is reached, all further plastic shear takes place along the surface of maximum temperature. In the case of a plate attacked by a projectile, this surface forms a cylinder, with a molten boundary between it and the rest of the plate.

The basic problem for projectiles and for armor plate was the same. We had to learn to make steel projectiles so strong that they could penetrate any armor they attacked. We had to make steel armor so strong that it would resist penetration by any projectile. At that time metallurgy was an art. To achieve our desired military goal, metallurgy had to become a science. Clearly internal friction furnishes an invaluable tool in this transformation from an art to a science. In 1945 we at the Watertown Arsenal sent a series of papers to the American Institute of Mining and Metallurgical Engineers directed towards this goal. As the war was coming to an end, Cyril Smith invited me to join him at the University of Chicago. There he was forming an Institute for the Study of Metals. This would indeed be an ideal place to carry out my plans.

We were able to attract a group of outstanding young post doc's. Ting Sui Ke, coming from the MIT Radiation Laboratory, perfected the torsion pendulum system of measuring anelastic effects, and used this method to demonstrate many new phenomena. The viscous behavior of grain boundaries deserves special mention. Upon his return to China, Ke built up a strong activity continuing his excellent Chicago work.

Charlie Wert, from Iowa University, was quick to recognize the application to metallurgy science of the carbon peak in iron. He accurately measured the diffusion coefficient of carbon in  $\alpha$  iron, as well as the growth rates of iron-carbide precipitate particles, and related these measured quantities to basic physical concepts, and to the problem of hardening of steel. He transferred to Urbana and

there he trained an outstanding group of research physicists to continue solving metallurgical problems.

Arthur Nowick, from Columbia, was especially stimulating for his insistence on vigorous thinking about metallurgical problems. In particular he demonstrated how to measure changes in vacancy concentration by measuring the internal friction due to induced pair orientation of solute atoms. His review articles, culminating in his book Anelastic Relaxation in Crystalline Solids, has contributed greatly to spreading the use of anelastic measurements throughout the metallurgical world. He has also contributed through his students, first at Yale, then at Columbia.

Dijkstra, coming from Snoek's group at the Eindhoven Phillips Lab, brought us the valuable experience of this group. In particular, his pioneering work studying nitrogen in iron has encouraged many more recent works in this area. David Lazarus and Ron Sladek, then graduate students at Chicago, were unique at profiting from this outstanding staff.

Those of you who knew me at Chicago did not learn to respect my experimental ability. I therefore wish to tell a story of an experiment which I challenge you to repeat more accurately. I have spoken of steel balls dropping on glass plates, of projectiles striking armor plates. My story relates to heads colliding with glass windshields. This spring I was travelling along a six-lane highway just south of Phoenix, 3 lanes going east separated by a 30 ft wide strip from 3 lanes going west. The center of this 30 ft. gap has a strong 3 ft. high barrier. I was in the center lane travelling east at the commuting speed of 50 mph. Suddenly I saw from the left corner of my eyes a car 10 ft. away spinning on gravel and on a collision course with me. I did not see the collision, I did not hear the collision, nor did I feel my head strike the windshield. I did remember saying Christ! The good Lord thought I meant to pray for help, and guided my car straight ahead. Knowing how far away the colliding car was in my unerased memory, and how fast I was going, I calculated that 100 milliseconds of memory was erased by the head-windshield collision. Ting Sui Ke, Charlie Wert, Arthur Nowick, David Lazarus, and Ron Sladek, I now challenge you to devise an experiment measuring how many milliseconds of memory a blackout erases, with the constraint that your head not be injured neither inside or out.