Speech of Acceptance of Timoshenko Medal by Zdeněk P. Bažant[1]

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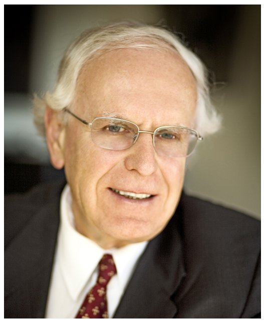

Reminiscences and Reflections of a Mechanician by Luck

Mr. Chairman, Ladies and Gentlemen:

When I wrote from Prague to the great Stephen P. Timoshenko, I would not even have dreamt that a medal bearing his name would once be bestowed upon me. I feel immensely lucky and humbled by joining the august group of previous medalists, and accept this honor with feelings of deep gratitude to the Applied Mechanics Division for selecting me, and to my great solid mechanics colleagues at Northwestern for their friendship and stimulation. I also thank my excellent students and associates for their collaboration; my university for a great academic environment; many agencies for funding; and my wife Iva for her loving support. Missing any of that, I would not be here today.

I feel much sympathy for Timoshenko, who faced in his pre-American career many setbacks. So did I, albeit milder. But overcoming setbacks hardens one's resolve and may provide unexpected opportunities and enrichments.

Timoshenko's formative years as well as mine coincided with the greatest calamity of the last century, the victory of communism in Russia and three decades later its imposition on my native land. His was an amazing life story. His father, a hardworking man, was born in serfdom, the Russian equivalent of slavery. Against severe odds, he became a land surveyor and managed to arrange a good education for his son. After early successes in science and a quick rise in academia to deanship in Kiev, Timoshenko was fired for exceeding the admission quota for Jewish students. The bolshevik revolution in 1917 was a prolonged setback to his academic career and reduced his family to penury. After an adventurous escape through Crimea and Turkey to the West, he taught briefly in Zagreb and joined Westinghouse at the age of 42, set on a path to fame.

I was lucky to have been born into a great intellectual family. For much of my early education I am indebted to my father, Zdeněk J. Bažant, a geotechnical engineering professor in Prague, to my mother Štěpánka, a PhD in sociology, and to my grandfather Zdeněk Bažant, rector and professor of structural mechanics in Prague (who was active in IUTAM since its founding and survived Nazi concentration camp Teresienstadt).

My family background, however, was politically unlucky for those times. The first years of terror after the communist coup on 2/25/48 were, in our family, years of anxiety. The boss and friend of my mother, Milada Horáková, was executed on trumped up political charges, and sociology was banned as bourgeois science. The properties of my maternal grandmother, a widowed very successful entrepreneur, were nationalized. Even though my parents providently donated their large rental apartment building to the state, I was categorized at school as a bourgeois child and slated for coal miner apprenticeship in Kladno. That was the biggest crisis of my career. Nevertheless, thanks to an opportune illness, exaggerated to make me physically unfit for this apprenticeship, and to political intervention from a family friend, I did, luckily, end up in 1952 at high school.

There I became obsessed with math and competed, up to the nationals, in the Mathematical Olympics, which, I must admit, were an excellent educational innovation copied by the communists from Russia. Subsequently, the Czech Technical university in Prague gave me a solid education in traditional civil engineering. Graduating in 1960, I became the fifth-generation civil engineer in my family line.

At my graduation, I was unexpectedly invited to join the party. This presented a stark choice. Acceptance would have ensured my advance, though at great moral cost. Agonizing about it, I eventually found the strength to decline. Subsequently, my application for graduate study was rejected for political reasons. So, I have never been a graduate student, but neither was Timoshenko. This setback eventually turned into an advantage. Were I admitted for graduate study, I would probably not have developed an interest in the practical problems for whose resolution I am honored today.

I was assigned to a state firm, Dopravoprojekt, as a bridge engineer. This led to my first encounter with Timoshenko's work—through a frightening episode of instability in practice. I supervised the construction of a slender arch bridge over the Vltava at Zbraslav near Prague. The erection procedure was innovative. On a light scaffold, the reinforcing bars were welded into a truss arch. Self-supporting after scaffold removal, the arch was to be gradually strengthened by casting layers of concrete. Standing on top of that tall scaffold (and feeling giddy at that height), I directed the decentering. After partial loosening of the supports, I noticed the huge arch developed a slow lateral oscillation. Shocked, I screamed: "Zpět!" (Back!).

Then I found Timoshenko and Geer's book on stability, looked up the energy method, lucidly explained, and estimated the critical load for lateral shear buckling of this truss arch. It appeared that the lateral bracing was insufficient. The arch would have collapsed to the side if fully loosened from the scaffold towers.

At that time I began collecting notes which led three decades later to my book with Luigi Cedolin on Stability of Structures. Also on that occasion, my dad showed me some correspondence that my grandpa conducted with Timoshenko before World War I. This was not surprising, because in those days the Czechs liked to cultivate contacts with countries opposed to the Austrian monarchy.

Fortunately, not having been a graduate student caused me no setback. Aware that, under the state bureaucratic rules, the number of work hours allotted to a project rose steeply with the perceived difficulty, I volunteered for such projects, reckoning that I could save much time for studying at my workplace. And, if approved by the party cell of the firm, it was possible to obtain a doctorate as an external student while working full time. This meant passing exams without attending any classes and working on the dissertation alone. I saw my dissertation advisor exactly twice—first, to get his approval for what I proposed to do, and, second, to deliver (in 1963) my dissertation on creep effects in concrete structures (subsequently published as a book).

I think it is a pity that nowadays such external study is impossible, because in industry there exist engineers who might benefit. Studying alone, of course, takes more time, and one gets various false preconceptions. Yet, by eventually realizing why they are false, one will master the subject more thoroughly than by being guided in a formal course along a smooth learning path.

After my doctorate, I took advantage of an excellent innovation of Prof. Brdička at Charles University in Prague. He offered a two-year course in theoretical physics which was intended specifically for engineering researchers and did not duplicate any physics and math they were supposed to already know. Every Saturday, he lectured on statistical mechanics, quantum mechanics, chemical thermodynamics, Maxwell equations, etc. Although I forgot most of it, relearning bits of it when needed has been much quicker than starting fresh. This became useful when I got in America into materials modeling. Regrettably, such courses do not exist today. There are, of course, plenty of short courses, summer institutes, etc., but subjects like those cannot be digested quickly.

Upon joining the Czech Technical University, my research involved testing the compression strength of laminate plates and tubes of various sizes. The walls failed by buckling of delaminating layers, which looked to me like a three-dimensional buckling mode of an orthotropic continuum. I managed to get Biot's book and the papers of Trefftz, Biezeno and Hencky, Neuber and Southwell, which all dealt with the critical state criterion for stability of three-dimensional continuous bodies. It was perplexing that each of them arrived at a different criterion.

Thus it occurred to me in 1965 to write to Timoshenko. To my delight, I received an amiable reply, not from Stanford, but from Germany. He wrote that this had remained a controversial unsolved problem for decades. Thus encouraged, I returned to it periodically, but was making no progress. Years later in Toronto, the solution suddenly flashed in my mind—all these critical state criteria become equivalent if the tangential elastic moduli associated with different finite strain measures are properly transformed as a function of the unknown critical stress, and the same simple transformations also establish the equivalence of the objective stress rates of Jaumann, Cotter and Rivlin, Truesdell and Oldroyd, and the Lie derivative, and of Engesser's and Haringx's shear buckling theories.

This experience confirmed to me Thomas Alva Edison's observation that "discovery is 99% perspiration and 1% inspiration". To solve a tough problem, one must, of course, love it, and get so immersed in it as to dream about it at night. If frustrated, work for a while on something else, but return to it once the details are forgotten. Fresh rethinking may then lead to different ideas. The right one may unexpectedly come to mind while riding a ski lift, giving a lecture, or sitting in a symphony hall, but only if one is preoccupied with the problem. Those who think they can pursue research 9 to 5 come up with nothing, even if extremely bright.

My transition to the West in 1966 was a complex story, but easier than Timoshenko's. Fortunately, almost two years of post-doctoral fellowships in Paris and Toronto allowed me to fill many educational gaps. I invested much of my stipend into conference trips and lab visits. At IABSE in New York, Prof. Boris Bresler invited me to the University of California, Berkeley, to work on his gas-cooled reactor project, which required the analysis of creep and chemo-hygro-thermal effects in concrete. Bresler, like Timoshenko, was another successful refugee from communist Russia. His family escaped east rather than west and, after receiving all his basic and engineering education in China, he ended up as Timoshenko's neighbor across San Francisco Bay.

In the 1960s, the material models and methods of structural analysis for concrete, as well as fiber composites, rocks and other quasibrittle materials, were still quite simplistic. The progressive softening damage due to distributed cracking was either ignored or misrepresented as plasticity. The size effect on the strength and ductility of structures was either disregarded or perceived as solely statistical, and thus supposedly covered by safety factors. But everything was about to change by the advent of computers and the finite element method.

A radical change was already manifest when, after Christmas 1968, I arrived at UC Berkeley. Ray Clough's invention of finite elements captivated everybody's mind. Being already curious about the fracturing of concrete, thanks to Robert L'Hermite, my previous famous mentor in Paris, I became fascinated by Jose Rashid's idea to simulate by finite elements the cracking in nuclear reactor vessels in a smeared manner—through strain softening.

However, all this excitement in Davis Hall was not shared across the street in the mechanics department in Etcheverry Hall. I think I was the only one from Davis hall to regularly attend their seminars. Professor Naghdi, then the chairman and a guru of continuum mechanics, noticed me and asked: "By the way, what's your interest?" "Strain-softening, to model distributed cracking of concrete and rock", I replied. Then, in a mildly sarcastic tone, he advised me: "Young man, taking such a controversial path, you will never achieve tenure. A tangential moduli tensor whose matrix is not positive definite is not a sound concept. Materials with such a property do not exist. They would be unstable and could not propagate waves." Soon I realized that Prager, Drucker, Rivlin, Mandel and other continuum mechanics giants thought likewise, and there were classical works beginning with Hadamard to support their view.

So I decided to play it safe and focus solely on the hygrothermal effects and creep in concrete as a nanoporous material. This was another big issue, to which I was previously introduced in Toronto by visiting professor Treval Powers who, in my view, was the No. 1 cement physicist of the last century (who, incredibly, was never elected to the NAE).

Joining the Northwestern faculty in the fall of 1969 was another lucky move. It gave me my first taste of American academic freedom—a big asset in contrast to the situations in many countries where the senior professor has the power to control the research of all assistant and associate professors in his institute[2]. I was actually hired to teach structural engineering, and was delighted that focusing on mechanics and materials was no problem. My colleagues, students, funding and academic environment have been great, and my career proceeded with no more setbacks.

Inevitably, I became embroiled in lengthy polemics[3] on strain-softening damage, quasibritle fracture, size effect in geomaterials, composites and sea ice, nonlocal models, standardization of fracture tests for concrete and rock, creep and hygrothermal effects in concrete structures, thermodynamics of nano-pore water in cement gel, determination of safety factors, design code updates, etc. But progress was achieved. Also, it was a lot of fun, with one exception—the explanation of the World Trade Center collapse.

I would not have attempted it if my daughter did not work nearby. Right after the first airplane hit, she called me: "Open the TV!" I got worried seeing her building disappear in smoke. Then, like every structural engineer, I was stunned by the collapse. Immediately, I realized this would become a lesson on a par with the Tacoma Narrows Bridge, and called my assistant Yong Zhou. He extracted from the internet the main data on the towers, but not the cross-sectional areas of the columns. Those we quickly calculated using the wind load provisions of the New York building code, and two days later we submitted our paper explaining the collapse. This is how I became the favorite target of the politically motivated misinformation campaign of the so-called 'Truth in 9/11' movement.

At Northwestern, I focused first on concrete creep. My cleanest result, the so-called AAEM method, featured now in all design codes or recommendations, was an easy outcome of many computer solutions of Volterra integral equations. To my surprise, the results agreed up to six digits with a certain combination of the compliance and relaxation functions of aging viscoelasticity. Clearly, a simple algebraic relationship had to exist. It then required no stroke of genius to find it.

It was a similar story with the size effect law for quasibrittle failures. With my assistant B.-H. Oh, we first calibrated a program for the crack band model by the meager test data available. Then we used it to simulate the plots of size effect for many structural geometries. All the plots turned out to be nearly identical in dimensionless coordinates. Knowing this, I needed no divine inspiration to derive that law.

Brute-force computer simulations, of course, cannot provide full understanding. But, if carefully calibrated, they can extend the experimental evidence and reveal the essential trend. Thus one can get a clue for an analytical model—the ultimate prize.

I used this kind of approach over and over. Recently, together with S.-D. Pang and J.-L. Le, I succeeded to deduce the tail distribution of strength on the atomic scale, but could make no headway to determine the probability distribution of the quasibrittle structure strength or the lifetime. So we turned to Monte-Carlo simulations of the multiscale transition. The simulated distributions revealed with high accuracy that the power law tail is indestructible, that its exponent is additive over the scales, and that there is a sharp kink separating the Gaussian and Weibullian portions. Then it was a 'piece of cake' to prove it analytically.

During my studies, I sometimes wondered what a wonderful opportunity it must have been when beautiful facts, such as the critical load of an elastic column, still awaited discovery. But similar opportunities exist today and are actually more numerous. The growing body of human knowledge may be imagined as the growing volume of a sphere. The unknown is the infinite exterior, but what is currently knowable is only what is in contact with the surface of the sphere. As the surface grows, the knowable unknown grows with it, representing the problems ripe to tackle.

The elastic frame analysis is an example of a problem that became ripe around 1920 and became closed 40 years later. But turbulence, which became ripe by 1900, is still far from being a closed subject. Let me venture to predict that the mechanics of damage and quasibrittle fracture, with its scaling and interdisciplinary couplings, is a problem of the same dimension, which will not become closed even a century from now.[4]

To end, let me borrow from Shakespeare[5]:

"My fear is your displeasure;
my court'sy my duty;
and my speech, to beg your pardons."

Notes

[1] Posted on AMD Archive at Harvard University on www.iMechanica.org; published in ASME-AMD Newsletter 2009.

[2] Bažant, Z.P. (1993), "Public funding of university research and graduate programs", *Am. Soc. of Engrg. Education (ASEE) Centennial Annual Conf. Proc.*, held in Urbana, Illinois, 1993, 341–345 (download G2.pdf from Bažant's website).

[3] Bažant, Z.P. (2002). "Reminiscences on four decades of struggle and progress in softening damage and size effect" (in both English and Japanese translation). *Concrete Journal (Tokyo)* 40 (2), 16–28 (Anniversary Issue of Japan Concrete Institute); and updated version republished in *Mechanics (Am. Academy of Mech.)* 32 (5-6), 2003, 1-10 (download S45.pdf from the same).

[4] Bažant, Z.P. (2006). "Vision of the future of solid mechanics" (guest editorial). *J. of Applied Mechanics ASME* 73 (March), 181–182 (download G5.pdf from Bažant's website).

[5] King Henry IV.