Reflections

Reading and reviewing classics is a rewarding experience. The autobiography of Timoshenko is a moving account of academic and scientific life during the most difficult years in modern history: 1900-1950. Revolutions, wars and political strife drove students, professors and scientists in Russia and Europe to great despair, and only a few like Timoshenko survived the ordeal to share their ideas and ideals for future generations. This autobiography brings to life the clash of peoples, cultures and ideals that shaped the course of modern engineering science education in England, France, Germany, and, America. The ideas expressed forcefully highlight absolute ideals that Timoshenko admires in scientific society. This book is full of tips for students, teachers, reviewers, educationists and the general public alike to evaluate themselves, their colleagues and their institutions. It may be that Timoshenko rebukes human vanity, pride and prejudice harshly in his autobiography, but there is no denying that Timoshenko always gets it right when it comes to extracting the truth !

K R Y Simha

As I Remember

Excerpts from the autobiography of

S P Timoshenko

Early Childhood

I still retain some disconnected memories of my earliest childhood, when I was probably no more than three. From the age of five onward I recall my life quite well. In our time the house was already very old, dating probably from the end of the eighteenth century. The garden had been neglected. In spring and summer I spent most of my time there, and now, recalling my childhood, first and foremost I picture our garden. It occupied apparently about forty-nine acres, but to us at the time it seemed infinite. There was another thing that I enjoyed: playing in a pile of sand left in a yard from some building job. From the sand I built fortresses, castles, and, especially, railroads. I became acquainted with the railroad early. In autumn, when the work in the fields was nearly over, Father and Mother would usually go for a week or two to Kiev, about 130 miles away. Those trips were a very important event in my life, and certain details of them I remember to this day.

Primary Education (1889-1896)

Generally I studied hard and usually was at the head of the class. I do not say that I did not like being at the head of the class, but I made no effort to push myself forward or to display my knowledge to the teacher. I was never a show-off. The fellows at school liked me.

Mathematics was my main subject in the upper grades of the realschule, but not everyone had the necessary ability for it. Some needed outside help. Not only did I enjoy studying mathematics, but I also enjoyed explaining mathematical proofs to others. I would go to school early in order to have time for explanations to my comrades for whom mathematics was more difficult. For me, giving explanations, going over proofs of theorems, was pure joy. I think that it was destined for me to become a teacher. If I have succeeded in doing this in life, it was because I was working at what I liked.

College at Petersburg (1896-1901)

I was happy to be admitted to the Institute of Engineers of Ways of Communication. My dream of being an engineer and building rail-roads was beginning to come true. Here at the exams I saw clearly the advantage enjoyed by the examinees who had gone through middle school in the capital or some other large city. They knew better the examination requirements. Many of them had spent the summer in special preparatory schools whose teachers knew the exam requirements down to the last detail. They knew the examiners, their peculiarities, their favorite questions. To us, coming from the country, everything was new and unexpected. Nevertheless, I have always considered that admission on a competitive-exam basis is the fairest way. I would never agree to replacing it with admission based on certificates or any kind of lottery system, as has been proposed. There is no doubt that, despite its various defects, the competitive-exam system generally did, with sufficient accuracy, select from the mass of young examinees those best grounded in mathematics. That grounding, of course, would be extremely important in their subsequent studies as future engineers.

Later at universities in America, as a professor, I encountered students who lacked proper mathematical training. I saw how this affected the level of teaching, which had to be lowered, adjusted downward to the students' level of preparation. Insufficient mathematical training has undoubtedly exerted great influence on the attitude of students toward the science of engineering. The American student, in most cases, is not interested in deducting any kind of formula, or in the basic assumptions underlying such deduction. All he wants is the final result – a formula which he can apply mechanically, without thought, to solve practical problems. It is my belief that the defectiveness of the mathematical education offered in American secondary schools during the early part of this century was one of the main reasons for the low level of development of the engineering sciences in the United States.

The actual introductory lecture was given in the chemistry lecture hall. The lecturer, Konovalov, was a well-known chemist, the pupil of Mendeleyev. He spoke to us about the scientific method in general, telling what it was that distinguished chemistry from alchemy. He said a little about the history of chemistry. We were all interested, felt that here was a real scientist, who wanted to introduce us to his science. Chemistry, I thought at the time, had little bearing on my own future interests, and actually I never again did have to study it, but I sat through Konovalov's course of lectures with great interest.

A great influence upon us all was exerted by the statics-of-structures professor, F S Yasinskiy. Of Polish origin, he spoke Russian not always correctly, and with a strong Polish accent. But his lectures were superb. He was the only engineer and scientist at the Institute whom I could stand to listen to. He devoted his first lectures to the theory of buckling. At the time this was a question not thoroughly understood. With his writings Yasinskiy greatly furthered scientific elucidation of this problem so important to engineers.

He had written a whole book on the subject, which at the time was the most exhaustive study of the stability of elastic systems. Even then we clearly felt how important it is for a lecturer to do original research in the subject on which he lectures.

The first student movement to spread over all of Russia started on February 8, 1899. On that day, the anniversary of Petersburg University, disorders broke out inside the university. The second student strike to spread over all of Russia began in the spring of 1901, when I was finishing at the Institute. What started the affair, I don't remember, but a large role in those disturbances was played by the drafting of striking students into the army. I constantly participated in the rallies, listening with great interest to our speakers, but took no active part and made no speeches. I usually voted for a strike, and considered myself a student of leftist orientation, though I belonged to no political organizations whatever. I was totally absorbed in my engineering work and in the construction projects, and had no intention of spending all my energies on social matters and politics, as some of my left-oriented comrades were proposing to do. They placed me in the category of sympathizer.

First Trip Abroad

In the summer of 1900 the International Exposition opened in Paris. In those days expositions did not have the purely commercial character that they have now. I was interested mainly in the engineering structure. In that section were exhibited many models of different bridges, roofs, canals, harbor installations. One could see the drawings, description, and even design calculations. Very interesting to me was a model of the big arch viaduct over the river Viaur, which had a cantilever arch, with a middle span of over 656 feet. The railroad tracks were at an altitude of more than 328 feet above the water's surface. This was the biggest arch in the world, and they were building it that very summer. I very much wanted to visit the construction site, and Radlov and I agreed to go together.

Military Service (1901-1902)

Military service not only afforded me an opportunity to become better acquainted with lower-class people, but had another advantage as well. I think that the gymnastics and living in a tent in the summer improved my physical health. I grew accustomed to the morning cold and to cold water. Since my military service I have retained the habit of doing calisthenics in the morning and sponging down with cold water. These habits have proved especially important in view of the constantly sedentary nature of my work.

Entry into Teaching (1902)

It became clear to me that, in order to move ahead in my field, I needed a great knowledge of mathematics than that which I possessed as a student at the Ways of Communication Institute. How to obtain this knowledge? My mathematician acquaintances advised me to take exhaustive courses in the different branches of mathematics, but I felt that this was not what I required. An engineer cannot steep himself in the study of mathematics for mathematics' own sake. He needs to be acquainted with the different areas of mathematics only from the standpoint of their possible applications.

The first person to point out to me this situation was Aleksey Nikolayevich Krylov. I had heard several of his reports at the Physical Society, and they had made a very great impression upon me. They concerned engineering problems, and Krylov demonstrated how they could be described in terms of differential equations. Then he found solutions for these equations, and in conclusion gave an engineer's interpretation of the solutions. This was all new for me, and was what I felt I needed. I was not born to be an abstract mathematician. But mathematical applications in the engineering sciences did interest and naturally attract me. A N Krylov's reports showed me the direction in which I was to go. Soon a practical opportunity presented itself for the start of my work in the direction of utilizing mathematics to solve engineering problems. This opportunity was given to me by the Petersburg Polytechnic Institute. The atmosphere of that school exerted an enormous influence upon my entire further development.

The Petersburg Polytechnic Institute (1903-1906)

I decided that I did not wish to become a practicing engineer. At the same time mathematics and mechanics, divorced from applications, did not satisfy me either. I wanted to know those sciences only to the extent that they are necessary for productive work in the applied sciences. In Russia at that time the technical sciences were developing under the influence of the German literature mainly. In the area of mechanics and strength of materials the best texts were the books of A Foppl. So I decided to use the summer of 1904 for a trip to Munich, where Foppl was then professor of mechanics.

I learned a great deal from Foppl during my six-week stay in Munich and I realized that work in laboratory profits students only when directed by an experienced teacher who, during an experiment, continuously explains what is happening, calling their attention to the different aspects of the phenomenon. The laboratory must teach the student how to observe, and the first experiments should be conducted under immediate supervision. A simple tensile test of a specimen of steel should be accompanied by a two-hour lecture, with an explanation of every phenomenon associated with the rupturing. Now if an advocate of 'student independence' leaves the students to themselves, they will run the tensile test, arriving at the all the required results in a half hour but will have learned little. In the autumn of 1906 (I believe it was November) I received from Kiev a telegram of congratulations. Of the seven candidates who took part in the competition. I had been chosen for the chair of Strength of Materials. I was to give courses on strength of materials and graphic statics and conduct laboratory exercises. Being selected for the job was, of course, a great triumph for me. Not yet twenty-eight years old, having never yet given a single lecture, I now all at once had become the representative of engineering mechanics at one of Russia's most illustrious institutions of technical learning.

In selecting me the Council was obviously running a certain risk. While my scientific writings were better than those of the other candidates, the other were older and had more teaching experience. All my subsequent experience showed me that the Council's choice of new professors on the basis of written appraisals, from experts, of the candidates' scientific work is the best method of filling a chair. Promoting assistants and teachers to professor on the basis of length of service and teaching experience is an incomparably worse method. So long as American schools employ that method, they cannot acquire satisfactory teaching staffs. Under that system the talented young are held back, become professors only when they are older – when the energy and enthusiasm of youth, so important in teaching, are gone.

The Kiev Polytechnic (1907-1911)

I gave the first introductory lecture on, I think, January 8, 1907. Strength of materials being a required subject for all engineering departments, more than four hundred students showed up for my first lecture. I lectured in the physics lecture hall. Of course I was very nervous but, since I knew exactly what I wanted to say and spoke in the simplest briefest sentences, the students followed the lecture with ease and were satisfied. Also, my voice proved suitable, and I could be heard in the back rows without straining. It was soon determined that I was a pretty good lecturer. In Russian schools of that day this in itself was a great victory, since our students usually did not attend lectures and learned the subjects from books. Fifty-five years have passed since my first lecture hall, though I know in advance what I am to say to the students, since I never give a lecture without careful preparation. Every lecture is written out, but during the lecture I never use my notes.

At the beginning of 1911, right in the middle of the school year, an event occurred that greatly affected my entire future career.

The attitude toward liberal thought in the schools had started to change in the other ministries too. In the Kiev Polytechnic in particular, a source of discord with the Ministry was the restrictive quota for admission of Jews to the Institute. For the Kiev Polytechnic the quota had been set at 15 per cent of the total number of admitted students. After 1906 the governing body of the Institute had ceased adhering to the quota, and by 1910 the number of Jewish students greatly exceeded it. The Minister insisted on expulsion of the number of Jews admitted over the quota, but the governing body did not hurry to meet this demand, and things remained as they were. The upshot was that in early February, 1911, three of the Institute's deans, I among them, were fired – suddenly found overselves with no salary and without the government apartments that until then we had lived in. Out of a feeling of solidarity the left-oriented professors all submitted their resignations. The resignations were accepted, and the Polytechnic had suddenly lost 40 per cent of its faculty.

England Trip (1912)

My works started to be esteemed in the West only after a long delay, by which time I had left Russia. At the Mathematical Congress (1912), I learned of the great importance which the English attached to the final examination, known as the 'Tripos', which the students underwent at the end in which Rayleigh had come out first on this examination, but that Kelvin and Maxwell had come out second. I did not consider this very important. Of all the papers read at the Congress, I recall the brilliant one by von Karman, which showed that the kinetic energy of the vortices occurring behind a cylinder moving through a fluid equals the hydrodynamic resistance. That work, done in Göttingin at Prandtl's laboratory, is probably the very best that von Karman ever published. Karman was disputed by Lamb. I did not understand his objections, but I got the impression that the old man was displeased that such brilliant work had been done in Germany and not in England. Already, even in science, the Anglo-German rivalry was evident. In the area of mechanics, Germany, owing to the better contact there between pure and applied science, was appreciably ahead of England. German universities were more democratic and easier for a talented youth to get into than England's expensive, aristocratic Cambridge and Oxford.

By then I knew that for an impartial evaluation of a subject it was better to rely on the German literature, not the English. Englishmen wrote as though everything had been done by Englishmen, citing almost exclusively English authors. With German authors, one could usually find more complete bibliographic references. After the war, unfortunately, this started to change, and the Germans seemed to become as nationalist in their science as the British.

I mentioned earlier that the books of Lord Rayeleigh exerted a large influence on the development of my scientific work. During the Congress at Cambridge I was hoping to catch a glimpse of this famous scholar. At the time Rayleigh was Chancellor of the University and no longer lectured at all. Leatham, whom I asked about Rayleigh, told me that His Lordship was living on his estate, not far from Cambridge, that his chief interest now was his dairy farm, from which carts bearing the Rayleigh name delivered milk all over London. It seemed to me at the time that interests of this sort did not accord with the title and rank of professor and scholar. No Russian or German scholar would so occupy himself, but the English looked at things more simply. A professor's rank in England did not stand so high as in Germany or Russia.

On one of the evenings of the Congress, Rayeligh gave a reception in the university museum. His Lordship and Ladyship greeted the Congress members on the stairway at the entrance, shaking hands with each. My wish to have a look at Rayleigh was fulfilled. Naturally, though, there was no opportunity to talk with him. Later the same evening I caught sight of him in a remote room of the museum. The room was empty, and Rayleigh was alone, pacing back and forth. Apparently the official ceremony gave him no pleasure. I later told this to Prince Golitsyn, a Petersburg professor known for his work on seismology. He told me that on one of his visits to England he had been invited by Rayleigh to spend several days on his estate. Rayleigh had done very important work in seismology, and Golitsyn was expecting that, living for several days in Rayleigh's house, he would have a chance to talk about science and get Rayleigh's advice. But he did not succeed. Rayleigh kept silent the whole time.

Yugloslavia (1920-1922)

My work at Zagreb Polytechnic unexpectedly ended in the middle of the 1922 summer semester. I received a letter from America, from a former pupil of mine at the Petersburg Polytechnic, one Zelov. He was now employed by a firm known as the Vibration Speciality Company, which balanced various machines and worked to eliminate vibration. The company president, a Russian engineer named Akimov, who was familiar with some of my writings on vibration, thought that I might be useful. Several days later I received an official letter from Akimov himself offering me a job with his company. If I accepted, he would pay my way to America. He offered me a salary of seventy-five dollars a week.

Thus ended the five years of my wanderings through Russia and Western Europe after my departure from Petesburg. Ahead lay America.

Into America (1922)

My daughter, Marina, had been interested in drawing since childhood. Having finished middle school ("high school") in Zagreb, she decided to study art in America. It turned out that one of the country's best schools of painting was in Philadelphia. Marina was admitted without difficulty. In the mornings, after breakfast, we would walk out together, she to her school. I to the office. They were almost next door to each other.

One such morning we witnessed a strange savage spectacle. Outside one building, a student dormitory, a pitched battle was in progress. People there were covered with blood, their clothing all torn. It seems that upper-class students were beating up the ones newly admitted to the university, called 'freshmen'. It was explained to us that this takes place at the start of every school year. These fracases did not always turn out happily. The next day we read in the newspaper that some of the participants had been taken to the hospital with serious injuries. The newspapers, condemning this type of student tradition, did mention that the thrashing of "freshmen" used to be done in England too, at those most aristocratic universities Cambridge and Oxford. To us it was incomprehensible. At Russian universities nothing of the kind occurred.

At Westinghouse (1923-1927)

By the start of 1923 I had grown aware that our 'company' was having monetary difficulties. I had to look for a new job. Within the next several days I went out to Pittsburgh, where Westinghouse had its factories.

Only the first few months of my work at Westinghouse were confined to the Research Institute. Soon requests for my services were received from the company's various departments. I came into contact with engineers working on practical problems, and I began to act more as a consultant. The matter would usually start with an incident involving some kind, of failure of machine that the company made, and the cause would have to be determined. It might be due to an overstressing of the material allowed by the design, or to insufficient strength of the material. Or a machine's broken part may have been subjected to forces greater than those anticipated in the calculations. Finding the true reason for a failure was extremely important. The machines being usually mass-produced, a failure in any one of them cast doubt upon the reliability of all the others of that type.

I encountered one such problem at the very start of my consulting work. A young engineer, a Dane named L S Jacobsen, came to me from the Motor Division, and told me that in the engines of a certain type designed by his department the main shaft frequently broke. His calculations with the usual formulae for flexure and torsion indicated that the stresses were below the allowable limit. He also showed me several specimens of broken shafts. In every case the fracture had resulted from 'fatigue' of the metal and had occurred in a section where the shaft's diameter abruptly changed. There was no fillet of any kind between the two portions of different diameters. It was obvious that, in the sections were the diameter changed, stresses were concentrated. As the shaft rotated, the stresses not only changed in intensity but changed sign too, causing a 'fatigue' fracture. I explained all this to Jacobsen, but for a designer a general explanation is not enough. He has to have a formula enabling him in each instance to calculate the necessary radius of the interjoining fillet. But no such formula existed. Someone had merely written a differential equation for the case. An attempt had been to solve this complicated equation graphically. I suggested to Jacobsen that he develop the graphic method, and something was done in that direction. But Jacobsen found a more comfortable way to solve the problem. He noticed that the necessary elasticity equation coincided in a plate being traversed by an electric current. If the contour of the plate coincided with the contour of a diametric section of the shaft, and its thickness were proportional to the cube of the distance from the plate's longitudinal axis, then the potential drop across the fillet would be proportional to the sought-for stress in the shaft. Using this relation, Jacobsen was able to compile a table of maximum stresses for different values of the fillet-radius/shaft-diameter ratio. In this way an important engineering problem was solved. Jacobsen's table now appears in many textbooks.

I received quite a few problems also from the division designing the big machines for electric-power stations. Here I had a chance to work with the distinguished Swedish engineer, C R Soderberg. I recall the case of a large-diameter rotor whose lateral vibrations were causing a lot of trouble in the usual factory tests. The reason was that the rotor's normal speed coincided with its critical speed. For the critical speed the usual calculations had yielded values higher than the test speeds, but the calculations assumed a rotor made from a single piece of material, whereas the large diameter rotors consisted of several disks bolted together.

I did a great deal of research for the Railway Communications Division. The Pennsylvania Railroad was being electrified, and this presented many new problems. The building of steam locomotives had a century of history behind it. The dimensions of their various parts had been arrived at gradually, by purely empirical means. But electrification was changing the wheel pressures on the tracks. There were no experimental data. In the case of electric locomotives the strength-versus-dimension question had to be answered by calculation. The factories building steam locomotives were unable to solve the problems inherent in the building of the electric ones. Westinghouse was finally forced not only to design the electric locomotives but to do the necessary research on track strength.

America's railroads had been built by people with virtually no formal engineering education. In the top management of the largest railroads were no graduates of any advanced school of engineering. The track-strength problem had to be solved by the company's Railroad Division working in conjunction with the Mechanics Section of Research Institute. Using my Russian writings on the strength of tracks, I planned a research program. Some preliminary experiments were conducted in the Research Institute laboratory, after which a whole series of experiments was carried out on various American railroads. Accounts of these experiments subsequently appeared in American engineering literature. I myself reported some of the results at the 1926 International Congress of Applied Mechanics in Zurich.

I recall a convention of mechanical engineers in Milwaukee in the fall of 1925. There I presented my paper on stress concentration. It gave very simple solutions for the case of a circular hole and described experimental studies, made with polarized light, of stresses in fillets joining plates of differing width. After I had finished, an evaluation of my paper, prepared by the well-known Harvard Professor G.F. Swain, was read. The reviewer obviously had extremely limited knowledge of strength of materials and never even heard of the high stresses at the edges of round holes. This did not prevent him, however, from vigorously attacking my paper, from branding my theoretical research on stress concentration as a useless fantasy of theoreticians, divorced from any practical application. I decided to “evaluate” this evaluator and delivered a reply in which no words were minced, clearly revealing his total ignorance. The audience, agreeing with me, rewarded me with a thunder of applause. That biting performance was the only one of its kind that I have given in America. No further ones were ever necessary. It being known that I knew how to defend myself, critics grew more careful.

On my way back from Milwaukee I stopped in at the University of Illinois in Urbana. I had already met its Professor H M Westergaard and wished now to become better acquainted with his work in America. He was Danish, a graduate of Copenhagen Polytechnic. He had taken his doctorate in Germany, emigrated to America, and was teaching mechanics. Here, I discovered, they had already heard of my spirited reply to the professor from Harvard, and approved of it. On Sunday mornings at nine o’clock, at a designated corner, our ‘hiking club’ assembled. It was a small group, usually fewer than ten. The only American was J Ormondroyd, later mechanics professor at the University of Michigan. All the others were foreigners, from different countries. The Russians were I and G Karelitz, my pupil at the Petersburg Polytechnic, who was to become professor of applied mechanics at Columbia. A frequent participant in the walks was J P Den Hartog, my very close associate at the Research Institute, who afterwards became mechanics professor at MIT. Another of our hikers was Soderberg, who also became a professor at MIT, and subsequently dean there. Who could have imagined at the time that within some ten years this group of young engineers would be playing leading roles in the development of mechanics in America? We were all really interested in mechanics. I remember that on one walk we came across a swing. On the spot, through experiment and calculation, we solved the problem of how a swing is ‘pumped’ by displacement of the center of gravity of the person sitting or standing on it.

Back in Europe (1925)

In the fall of 1925 I read in some journal that an International Congress on Mechanics was to be held in Zurich the following autumn. In addition to the congress, I was interested in the convention of the British Association for the Advancement of Science to take place at Oxford, and in visiting the big industrial testing laboratories.

We began with England, where I went to Cambridge. A company there had started making instruments for recording vibration, and I wanted to see them. I also had in mind meeting Kapitsa, who by now had been living in Cambridge for six years. To celebrate my arrival, he gave a little tea party to which several young physicists were invited. Later he invited me to dinner at Trinity College, where I met a large group of British scientists. I remember that sitting across from me was Sir Horace Lamb, whose writings on elasticity I knew well. Kapitsa did not behave too nicely at the table. He spoke loudly in Russian, making remarks about people present. I was glad when dinner was over and the assemblage broke up into small groups. I spent most of the time after dinner talking with Sir Horace Lamb. He had been a professor at Manchester but was now retired, living out his years at Cambridge, where he had once studied. The old gentleman was obviously very interested in the scientific work going on at American research institutes. I told him what we were doing at Westinghouse.

During the congress most of the visiting scientists lived in the different colleges. Being not a professor but the representative of an engineering firm, I had to stay in a hotel. I went to hear the reports of the physics section. I remember one paper that dealt with the new theories of atomic structure. After the author had finished reading it, Professor Carl Runge made a speech. Back in the eighties he himself had worked on a mathematical theory of the distribution of spectral lines. And here, forty years later, a new theory of the atom's structure explained the laws that he had encountered then. He concluded his speech with an allusion to the Gospel: "now are thy slaves set free."

I had first met Runge in 1905 in Göttingen while I was attending lectures during the summer semester. His lectures were remarkable for their clarity and elegance of exposition. In lecturing talent I would compare him with our own Kirpichev. At meetings of the Applied-Mechanics Seminar and Mathematical Society, what he said was always very interesting. I had dealings with him later too, in connection with publication of some of my writings in the journal *Zeitschrift für Mathematik und Physik*, of which he was editor. It was said that in the life of Göttingen's mathematicians Runge played a major role. The chairman of the department was Klein, a strict and imperious man who sometimes had disagreements with the younger teachers. For someone to smooth away the trouble, they would go to Runge. He would have a chat with Klein, who then would soften his attitude.

I remember another paper that was read in the physics section at that same congress. I must report that on this occasion the convention of Britain's Association for the Advancement of Science was being held under somewhat unusual circumstances. It was attended by the heir to the British throne, who had once been a student at Oxford. He was visiting all of the sections, one after the other. On the day in question it was the turn of the physics section. Up front some physicist was speaking, writing equations on the blackboard. Suddenly there was a commotion at the entrance. The Prince of Wales came in. I saw the section chairman lead aside the physicist who was speaking (and who had not finished), and up onto the platform stepped Rutherford. He described in his stentorian voice his well-known work on the smashing of atoms with special rays. It was sad to watch this world-famous scientist trying to show off to the prince. The last time I saw Rutherford was at the International Mechanics Congress at Cambridge in 1934. Though he had never had anything to do with mechanics, at the ceremonial dinner he had a place at the head table, next to the chairman, and as usual was doing quite a bit of talking. My neighbor at the table told me quietly: "We call him here the Loudspeaker."

When the convention was over, we left England, proceeded via Antwerp and Brussels to Germany, and stopped at Essen, where I wanted to inspect the Krupp factory. They were no longer making guns there but were engaged in peaceful commerce, building many locomotives. At their research institute they were doing interesting research on the shear in mind steel at the onset of plastic strain. From Essen we went to Göttingen. Twenty years had passed since I lived there. I had retained the brightest memory of that period. I had arrived there in 1905, a beginning teacher known to no one, and immediately I was given valuable advice-on what lectures to attend, what books to read, what work most needed doing. Until then no one had ever given me such advice, and I had wasted a great deal of time doing things that later proved unnecessary.

In the evening I met A Nadai, former assistant to Professor Ludwig Prandtl. He now headed the applied-mechanics laboratory, where I once had worked. Prandtl was no longer interested in applied mechanics and was spending all his time on aerodynamics. The next day Nadai showed me his projects in the laboratory. He was conducting experiments on Plasticity, and wanted to test the validity of certain of Prandtl's conclusions. From our talks I learned that he was not content with his position at Göttingen, but would like to emigrate to America. I promised him that I would speak to the management at Westinghouse, which I did when I returned to America. Nadai was subsequently offered a job at the company's research institute, where he worked until he retired. He accomplished many things there, including publishing a well known two-volume book on plasticity.

It (the congress) was held at the Zurich polytechnic institute. The assembly hall, where the opening ceremony occurred, was jammed. Presiding was mechanics professor E Meissner, whose works I knew well. He was doing research on strains in spherical shells. A few words were spoken also by A Stodola, author of a famous book on steam turbines. I had once studied the part of it devoted to strength of turbines. It was the first attempt to bring elasticity theory into the study of stressed in machine parts. The book was a huge success and had a large influence on machine design. Of the papers read at the congress, the most interesting was that of the Englishman, G I Taylor who spoke on plastic strains in the crystals of mild steel.

Professor at Michigan (1927-1935)

In the spring of 1927 I received a telegram from the dean of the school of Engineering at the University of Michigan informing me that a special chair of Research in Mechanics was being established there and he offered it to me. It was the answer to my dream.

My office-mate told me that professors were obliged to sit in their offices from nine in the morning until five at night, with a short break for lunch. I learned that they did not find this requirement burdensome, because those who were engineers usually had private clients on the outside, whose work they would do on university time in their university offices, and in this way increase their earnings. If a professor stayed home, he couldn't get anything done, because his wife would immediately load him down with household chores, or leave the children in his care and go out herself. With American ladies afternoon card parties are very popular. From this I gathered that, by working at home and showing up only for my classes, I was breaking established university rules. I continued the practice anyway through all the rest of my time at American universities.

My office-mate informed me that the professor whose desk I was using, a Dutchman, was the mechanics department's chief theoretician and an expert on calculation of statically indeterminate systems. He had supposedly discovered a special method for calculating such systems and had published an entire book on the subject. From further explanations I concluded that his 'special method' was none other than the well-known Mohr method.

This same office-mate later showed me a little book, used at the university, which explained the Mohr method for calculating beam deflections. The name Mohr was not mentioned in it, only the name of some former professor at the University of Michigan. This cavalier treatment of other people's literary property was a good indication of the abilities of those who taught engineering. To me this was a far cry from Russia and the Petersburg Polytechnic!

My office-mate became chummier and chummier. Sometimes he even gave me a lift in his car. He started showing an interest in my living arrangements. Learning that I was not very satisfied with my apartment, he explained that most professors owned their own homes, that buying a house was made easy by the banks, which gave large loans, so that a buyer needed to put down only a small amount, paying off the rest in monthly installments. To this I rejoined that I had no intention of incurring debts, would abstain from buying a house for the time being. After that his interest in my affairs instantly vanished. He lapsed into silence, stopped giving me lifts in his car. Later it came out that at the very time of our nice conversations he was trying to sell his house. Finding that I was not a buyer, he had no further need of me. I often encountered this shopkeeper mentality in Americans, so I learned to attach no meaning to their kindnesses and attentions.

At the end of the semester the head of the mechanics department called a meeting of the professors to discuss the problems that should be given in the exam. This was the first time that I had met the entire staff of mechanics professors. Some of the professors had brought along prepared problems. All involved numerical data, required numerical answers. When I suggested that the calculations be carried out to a prescribed degree of accuracy. I discovered that the matter of the accuracy of calculations was totally unfamiliar to them. It seems that algebra was too hard for the students, so they preferred arithmetic. Only later did I learn how weak in mathematics US high-school graduates were. The same was true of languages and the humanities. The students had to be taught English composition and history in college. The last year of college was spent teaching subjects that every European high-school graduate knew. The students were not ready to study strength of materials until the third year, yet with the fourth year their engineering schooling was over. Clearly, under these conditions the training received by American engineers was inferior to that given engineers in Europe.

For the exam the students of all the strength of materials sections were brought together in one large hall. All received the same problems and were allowed the same amount of time. 'Policing' of the exam was done by the students themselves. Abuses such as copying or getting help from someone were rare. In America the attitude of students towards this is very different from the attititude, let us say, in Russia. A Russian student would not tell on a student sitting next to him who had copied. The American does. The technique of correcting examination papers is marvelously developed in America. The day after the exam the students got their papers back with all the errors indicated and a numerical grade. My general conclusion was that American professors taught their students no better than those in Russia had, but examined them unquestionably faster.

That meeting of the Professors' Council made such an impression on me that I never went to another. At first I thought that this pitiful plight of professors was due to the lack of academic freedom, that the professors, having no power, could exert no influence on university life. Later I saw that the status of professors in America is not rated as high as in Europe. Here a good physician or engineer never dreamt of becoming a professor, but preferred private practice. The average quality of the engineers, for example, at the Westinghouse Research Institute was higher than that of the engineering professors at the University of Michigan. Giving any kind of power to people like that would, of course, be pointless. I did not let this state of affairs disturb me. I had gone to the university to gain free time for scientific work, and this I succeeded in doing. On the teaching of classes I spent little time.

European Summer (1928)

At the end of May classes were over, and I could leave for Europe. This was the big advantage that teaching had over a job in industry. I did not have to draw up a detailed plan for the trip or think about writing a report. Since most of my students were from the aeronautics department. I decided to use the trip to familiarize myself with aerodynamic laboratories and airplane factories. En route from Paris to Berlin I stopped at Aachen. Professor of Mechanics at the polytechnic there was von Karman, whom I knew well from my days at Göttingen. In Aachen, where he was teaching the mechanics of aircraft structures, he had established a laboratory for testing the strength of airplane parts. After showing me around the laboratory, he suggested that we go for a drive through the city and look at some of the ancient buildings. I consented with pleasure, not dreaming that there was any risk involved. Von Karman, an inexperienced driver, drove very fast. I came through unscathed but later heard that he had several accidents, once even snapped off the toll bar at the frontier between Germany and Holland. About ten years later I again had to ride in a car with him at the wheel. His driving had not improved. The situation was quite dangerous, for now we were not in Aachen but on a crowded highway in Los Angeles. Fortunately there was a young American with us, who unceremoniously took over.

From Aachen I went on to Göttingen, having been asked by Westinghouse to find among Prandtl's pupils a specialist in hydrodynamics who would be interested in coming to work for the company's Research Institute. At that time young Germans were eager to get to America, and I found the desired specialist in the person of O G Tietjens. He accepted the company's terms, emigrated to Pittsburgh that same year. Prandtl helped me get acquainted with Germany's aircraft industry, told me which were the most interesting factories, and supplied me with some letters of recommendation.

Chicago Congress (1933)

In the summer of 1933 at the Chicago World's Fair the mechanical and civil engineers held joint meetings. Matters of interest to both societies were discussed. One of the sessions was devoted to structural stability. At it I read a report on the stability of plate-girder bridges. My pupil, D H Young, read a paper there too. He propounded a rational method for determining the critical load of relatively short steel bars, which was an important innovation.

At the congress I met my old acquaintance, Professor Theodore Von Karman. He apparently had decided to leave Aachen and settle permanently in California. We talked of the persecution of the Jews by the Nazis. Having known each other a long time, we could discuss the question with complete frankness. The Nazi propaganda was outrageous, of course, yet in broad strata of German population were some underlying factors contributing to its success. Von Karman spoke of the group of Jews who had come to Germany from the East after the Russian revolution. Though completely alien to German culture, by dint of their solidarity they quickly infiltrated Germany's financial and economic world. Jews began to exert a large influence on the country's spiritual life: literature, arts, science. Here Von Karman started talking about the university. We recalled the days of Felix Klein. When an outstanding Jewish mathematician appeared on the scene, Klein would offer him a professorship at Göttingen, and no one said a word. But when, by using their connections, Jews of mediocre talent began flooding the universities, it made the Germans angry, and they neglected to oppose the Nazis with sufficient vigor.

California Invitation

At the start of the fall semester of the 1934-35 school year I received an unexpected invitation from the University of California. The most interesting side trip was my visit to Stanford, where I gave a lecture for the teaching staff of the school of engineering. The dean of the school at that time was Professor Theodore Hoover, brother of the former President of the United States. The lectures at Berkeley over, we went to Southern California, to Pasadena, home of the California Institute of Technology. Von Karman had arranged our visit. He met us at the railroad station and drove us to the faculty club, where there were rooms for visiting professors. After breakfast we toured Los Angeles and Hollywood, visiting the motion-picture studios. That evening I lectured on the work being done at Westinghouse on measurement of stresses in rails. Next day I went out to watch construction of the world's largest telescope and observatory at Mt. Wilson. That night we had to start back to Ann Arbor. There we met with cold weather and snow-nothing like California. I resumed my lectures and my classes with doctoral candidates.

In 1935 the Summer School of Mechanics was unusually well-attended. As visiting lecturer we had succeeded in getting Professor Southwell from England. Young American university teachers were very eager to hear this outstanding man. He was to give a course on elasticity theory, and I lectured on vibration.

But then a change occurred that put an end to the training of PhDs in mechanics at the University of Michigan. Almost simultaneously I received from Stanford University and the University of California offers of positions wherein my job would be to set up the same kind of training program for doctoral candidates that I had initiated in Ann Arbor. Both offers interested me. My wife and I very much liked California with its warm climate and southern vegetation.

We left Ann Arbor without regret. In the nine years that we had been there we made no close friends.

Since I was writing books on structural mechanics and elasticity, I naturally had to remain abreast of the new literature on these subjects, hence could actively participate in the discussions of the papers being presented. On this occasion, though, several were read on large deflections of thin shells the mathematics of which was over my head, and I could not properly evaluate them. This plunged me into melancholy. I could no longer keep up with the rapid developments in my field. I was getting old; I was now 59. A second reminder of my advancing age hit me at that same congress, during the formal dinner at the end. Westergaard was presiding. As is the American custom, he began introducing the guests of honor seated at the head table. He left me until last. Then, after saying my name, he made a speech about my contribution to the teaching of mechanics in America. The speech was loudly applauded by the Americans present, among them quite a few of my pupils. It is pleasing, of course, to know that one's work of many years has been appreciated. Still, hearing it all summed up like that connotes… the end. One feels gloomy.

In the fall of 1938 I received from the American Society of Mechanical Engineers a formal invitation to be present at their annual meeting. From private letters I learned that a group of my pupils and friends were planning a special dinner in my honor on the occasion of my sixtieth birthday. Public appearances always upset me, yet I could not refuse, so went to the meeting. Among those invited was a group of my old Russian acquaintances. Speeches were made, reminiscences bandied. At the end they presented me with a book, a collection of scientific writings on mechanics by my pupils and friends. That book is dear to me.

Reunion with his Brother (1946)

When the war (World War II) ended, my brother, in a prisoner's camp set up by the Americans, was in great danger. The camps were being visited by Soviet representatives, who took special interest in people known for anti-Communist activities. They were shipped off to Russia and exterminated. My brother was lucky. The Bolsheviks overlooked him, and a year after the war he managed to join me in California.

The arrival in 1946 of my brother Sergei and his wife coincided with a dark period in my life. My wife died. My brother and his wife then came to live in my home, and their sympathy and concern helped me survive that ordeal.

Postwar Europe Sojourn (1947)

In the fall of 1946 I was notified that Britain's Institution of Mechanical Engineers had awarded me their international James Watt Medal for my work in Mechanics. The society requested that I appear in person to receive the medal and give a talk on my work. I decided to go to Europe in the early spring of 1947 and describe the work on fatigue in metals being done by my pupils under my supervision.

After three days of running around Karlsruhe I finally had all the documents necessary for my trip through the occupation zone. I was assigned a car and driver and could now make my tour of the German testing laboratories. I decided to start with Göttingen. I knew the laboratory there from before the war and would immediately be able to notice what changes had occurred. I also knew its director, Professor Prandtl, my old teacher. From him I hoped to obtain information about the position of the experimental sciences in Germany during the war, and about what was being done to get the laboratories going again. Göttingen had not been damaged. By unspoken agreement the British had not bombed Göttingen and Heidelberg, and the Germans had spared Oxford and Cambridge.

Life at Stanford (1948-1955)

My duties at the university had greatly diminished. Two new professors had been brought in to work with the doctoral candidates. One was J N Goodier, my former student at the University of Michigan, and the other was W Flugge from Germany. All my free time I now devoted to writing my book on the history of strength of materials. On my way through London and Paris I had picked up many old books on mechanics at secondhand-book shops and now set about studying them.

In the spring of 1948 I again went to Europe. I met my old pupil from the Ways of Communication Institute, S Woinowsky-Krieger. He had managed to get out of Russia after the revolution, and finished his engineering education at the Berlin Polytechnic, where he took his doctorate. After graduating from the Polytechnics he had gone into private practice, mainly designing and building bridges and other engineering structures. Here in Frankfurt he was working on the restoration of the famed Paulskirche, where during the revolution of 1848-49 the German National Assembly had held its meetings. This was one of the first buildings restored after the war, and Woinowsky-Krieger can be proud of this marvelous edifice. I noticed that, while we were inspecting the church and also on our way back in the streetcar, he avoided speaking Russian. Later he reminded me that under the conditions of the peace the Americans were obliged to hand over to the Communists all Russians on German territory when the war ended. And now here, three years after peace had been concluded, it was still dangerous to speak Russian. Communists spies were tracking down Russians in the areas held by the Allies and spiriting them back to Russia to meet their fate. This cooperation of the Allies with Stalin was completely incomprehensible to me.

From Frankfurt I went on to Göttingen and visited Prandtl. The position of professors had somewhat improved during the past year. I attended a meeting there of the German applied mathematics society. Some people from America were present also. The Americans apparently did not intend to boycott German scientists.

At the end of the 1954-55 school year my teaching contract with Stanford expired. The University offered to renew it, but I declined. About a year after my 1958 trip to Russia I received notification that I had been elected to membership in the Soviet Academy of Sciences. That I had once been elected a member of the Russian Academy before the Revolution was not mentioned. Anyway, I was invited by the Soviet embassy in Washington to go there to receive the membership certificate. I declined to make the journey, and the certificate was mailed to me.

Back Home and Beyond

That trip to Russia (1958) I dare say, was the most interesting of my recent travels. My life these last years has flowed on tranquilly, with no special changes. I usually spend the winter in Palo Alto, preparing new editions of my books. Then in the summer I go to Europe and pass the time in Switzerland. It was during one such summer that I wrote down these reminiscences.