# Vignettes of Four Presidents, 1936~1969

## Vignettes des Quatre Présidents, 1936~1969

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THE SETTING

Between 1936 and 1969 our Society had four remarkable presidents. They are well known for their technical contributions, for each of them has left a legacy of outstanding papers and engineering works. Not so well known, especially as time goes by, are the personalities behind the contributions.

It has been my good fortune to know each of them well, some better than others, but all to the degree that we have had warm personal relations. In this talk, I shall try to give you glimpses of the men themselves. The glimpses are by no means biographies. They are no more than impressions that perhaps will give life to the technical contributions.

I have no illusions that these comments will give a well-rounded picture of any of these remarkable men. Nor do I make any apology that the impressions are my own, that they have arisen out of my personal contacts, and that they are necessarily limited by the circumstances of our relationships. Yet, I hope that these brief sketches will help you feel better acquainted with the men who presided over our Society for its first 33 years and thus with the beginnings of our profession.

KARL TERZAGHI

Our first President and the founder of our profession is no stranger to any of us, because he wrote extensively of the development of his own understanding of soil mechanics and, especially in his Presidential Addresses, of his evaluation of the state of the art. Others, too, have not only written of his technical accomplishments but have recounted many anecdotes that illuminate his personality. Hence, the length of these remarks about Terzaghi is by no means proportional to his overwhelming importance, for I do not wish to repeat what you already know.

From 1942 to 1948, Terzaghi and I worked intensively on "Soil Mechanics in Engineering Practice." Somewhat to our surprise, the writing did not go well. It is enough to say that even to Terzaghi the book presented for the first time the necessity for a critical evaluation of the relevance of each facet of soil mechanics to practice; moreover, gaps appeared where soil mechanics was inadequate to cope with some of the most commonplace problems and, if possible, these gaps had to be filled. Yet, this was not his first book. "Erdbaumechanik" and "Theoretical Soil Mechanics" had already appeared and had been remarkably influential. How did Terzaghi regard them?

As to "Erdbaumechanik", I vividly recall one incident while Terzaghi was visiting the University of Illinois. He wished to refer to some information he had included in the book, but after thumbing through the pages and fruitlessly consulting the index, he exploded in frustration. "When I wrote that book," he exclaimed, "I thought I was doing my duty if I merely laid out the bill of fare. It was up to the reader, poor devil, to grub through it to find what nourishment he could. Even the index is no good; not even I can find anything!"

"Theoretical Soil Mechanics" was quite a different matter. It cost him several years of intense concentration and is still a model of clarity and organization. Indeed, it was so well done that, much to his dismay, it gave many engineers and academicians the impression that soil mechanics is primarily a theoretical subject - an idea the very opposite of his own convictions. Why, then, did he write such a book?

It was his overwhelming interest and purpose to write a book for the practitioner, a book on applied soil mechanics. He feared, however, that introducing the necessary theory into the discussions would divert the reader's attention from the main thrust of his approach, so he decided to precede the applied book by one containing all the necessary theory to which he could then refer. In spite of his full realization that he was not a theorist - he

admired elegance in theory and regarded his own efforts as clumsy - he nevertheless felt the necessity to take upon himself the responsibility of examining the available stock of theories, of evaluating their assumptions, and of making the judgments concerning their utility and shortcomings for practice.

He did the job remarkably well. Moreover, the companion volume on applied soil mechanics never appeared; the section on applied soil mechanics in "Soil Mechanics in Engineering Practice" in effect took its place. Yet, because "Theoretical Soil Mechanics" is complete within itself, many engineers have been led to the false impression that theory is the essence of soil mechanics. Terzaghi considered the extract of theory in the second part of "Soil Mechanics in Engineering Practice" to be ample for most practitioners, and the continued sale of "Theoretical Soil Mechanics" at a volume comparable to that of "Soil Mechanics in Engineering Practice" was a source of concern rather than satisfaction to him.

Unfortunately, the reader of "Theoretical Soil Mechanics" would never get even the slightest hint that Terzaghi's avocation and hobby were geology, or that he considered soil mechanics to be a quantified aspect of applied or engineering geology. Yet, in his lectures to students and in his consideration of individual jobs, his emphasis was always on the manner in which Nature had created the deposit, on what variations in properties might be expected as a consequence of the natural events during and after deposition, and on what the physical properties of the geologically differentiable parts of the deposit might be. Boring programs and soil tests were always laid out to illuminate the geology; his interpretation and interpolation of the findings always had a geological basis. Until he understood in detail the geology of a site, he had little confidence in predictions based on tests, theories, or statistical analyses. He would have taken a dim view of today's trend to decrease the time a civil engineering student devotes to geology in order to add corresponding studies of computer science.

Terzaghi's almost instinctive use of geology appeared in the first major consulting job we shared after our work on the Chicago Subway - the iron ore storage yard at Cleveland, Ohio. You may recall his Final Report on this project, reprinted in the Terzaghi Anniversary Volume. It happened that I was the first to visit the job. The immediate questions were the length and bearing capacity of the piles beneath the retaining walls that would enclose the iron ore to be stockpiled along the riverfront. As I watched the test boring operations and estimated the undrained strengths of the clay samples being recovered from the subsoil, I concluded that a base failure of the ore stockpile, involving the retaining-wall piles as well, was almost certain. Indeed, on the basis of my estimate of strength, the factor of safety was no more than 0.7. When the officials of the steel company asked me what could be done about the prophesied failure, I had no answer except to suggest they call Terzaghi. They did, without telling him they had already asked my advice, and on his arrival explained, exactly as they had to me, their concern about the retaining-wall piles. He also watched the test boring in progress and, like myself, made a quick mental calculation of the bearing capacity of the storage area. His calculation, like mine, indicated failure. But he noticed one feature I had overlooked. The ore yard was to be located in a valley surrounded by steep banks in sand extending to an upland over 100 feet above the valley, whereas the ore pile was to be only 70 feet high. The valley fill, a fluvio-glacial deposit, must once have extended across the entire valley and subsequently have been eroded by the ancestor of the stream now flowing by the ore storage area. Hence, the glacio-lacustrine clays underlying the valley must have been overconsolidated and quite likely had a greater strength than that suggested by the disturbed drive samples. Furthermore, Terzaghi also noticed that there was no sign of failure beneath or in front of the toes of the steep sand bluffs, even though the geology clearly indicated that the lacustrine clays extended beneath the toes. These favorable indications prompted Terzaghi to judge that the chances were actually quite good that the ore pile would be stable, and that an observational procedure would have a good chance of success. The Final Report, to which I have referred, shows that his optimism was justified.

It would be misleading not to complete this sketch without recalling the many discussions after dinner, when we had spent the day over a manuscript or in the field on one of our jobs, and when Terzaghi inevitably turned to the future of soil mechanics. He feared for it, because he feared it might depart from reality. He feared the consequences of theory for its own sake, of soil tests as an exercise in increasingly sophisticated laboratory expertise, of routine calculation as a substitute for the implications of geology. Above all, he feared that workers in soil mechanics would take for granted that Nature would behave in accordance with their predictions, and that they would fail to take every opportunity to determine how Nature had actually reacted to their designs and construction activities.

A. W. SKEMPTON

Skempton is physically a big man; indeed, everything about him is big -- his smile, his laugh, his voice, his gestures. He even writes with a broad pen. From the first time I met him, I was struck by his tremendous enthusiasm. Whatever he did, whatever he discussed, was pervaded with an air of intense excitement. He never described large objects as merely large, they were enormous. Indeed, he added the word "enormous" to my own working vocabulary; I use it often, and never without thinking of him.

In our field, we regard him primarily as the great clarifier of the principles of soil mechanics. He is the man who gave life to the principle of effective stress, from his first approach with the lambda-theory at Rotterdam to his concept of the semi-empirical pore pressure coefficients. The understanding gained from the introduction of the A and B coefficients was a great step forward, almost as important as Terzaghi's statement of the principle of effective stress itself. He painted in soil mechanics with a broad brush, seeing such simplifying and useful relations as the ratio between compression index and liquid limit, visualizing the significance of the residual angle of internal friction.

Yet, Skempton is equally at home and respected in history, particularly of engineering and architecture, and in geology. Indeed, it was perhaps through his interest in the history of architecture that he and I personally became best acquainted.

In 1954 he came to the University of Illinois to deliver a set of lectures on shear strength. As you would expect, they were masterful lectures and I still keep my notes as treasured possessions. During the same visit, he talked to the members of the local section of the American Society of Civil Engineers and their wives on the structural development of the medieval cathedral. The talk is still remembered by wives who had not before realized that an engineer could be so entertaining, and yet (they realized afterwards) so technical. But Skempton's real interest in that first visit was the old buildings of Chicago. He had long been intrigued with the transition from masonry structures to the modern framed building. Part of the development had occurred in England and in the rest of Europe, but some of the more significant advances were made in Chicago in the late 1800's. Several famous old buildings were still standing, others had recently been destroyed. Skempton was keen to see the survivors before it was too late.

The history of old Chicago buildings, and especially their foundations, had been one of my own interests as well. Accordingly, we arranged for an inspection of as many buildings as time would permit. In the company of Sidney Berman, who was then in charge of soil mechanics for the Chicago Subway and who had a genius for enlisting the cooperation, perhaps unwitting, of custodians and security guards, we dashed in and out of the basements, stairwells, and elevator shafts of many an old structure, some almost ruins. There were too many buildings to see and too much to absorb, and Skempton began to despair of accomplishing more than scratching the surface of information he greatly wanted. The solution to that dilemma was, of course, for him to return on several occasions to Illinois.

Architects and engineers in Chicago also had appreciated the significance of the old buildings and had set up a project to microfilm whatever plans were still on file in the offices of long-standing architectural firms. One set of these microfilms, containing thousands of drawings, was filed at the University. On Skempton's next trip he was to review them. I borrowed a microfilm reader and installed it in his bedroom at our home so that he could make the most of his time. My recollection of that trip is of almost none of our accustomed conversations but, instead, of the view of the back of Skempton's head under the microfilm reader while he peered hour by hour at the detailed drawings of the old buildings. He simply had to see them all. His total immersion in the subject, his disappearance into the microfilm reader, were completely characteristic of his enthusiasm for whatever subject he attacked.

I first visited England in 1953 on my return from the Swiss Conference. Skempton took me in complete charge. I had only about three days, and he was determined that I should make the most of them. Well do I remember the first evening in Skempton's living room with him and Silas Glossop, another tremendously enthusiastic man and another of the early group of soil mechanics personalities in Great Britain. I was virtually ignored in the conversation while these two debated hotly and with great relish the places to which I must go and the things I must see. It was long after midnight when we went to bed. We would go to Land's End in Cornwall where I would see the birthplace of the steam locomotive and Trefethen's mine railroads. Moreover, we would have to be on our way no later than 6:00 the next morning. Along about 2:00 A.M., Skempton knocked on my door and said, rather apologetically, that he had been thinking over the situation and realized that he and Silas had proposed a much more strenuous agenda than could be carried out and, furthermore, that on a first visit to England there was much to be seen close to London. Perhaps it would be better to take things a little easier, to see Stonehenge, the Salisbury Plain and Salisbury Cathedral -- one of Skempton's favorites of which his knowledge was intimate.

The picture often returns to my mind of Skempton standing before the fireplace, Scotch and soda in hand, fiercely debating with Glossop the priorities of my introduction into England, ranging in sparkling debate ofer the technical history of England's cathedrals and industry, while I, although present, was only the passive object of their attention. Lest you gain the wrong impression, I was utterly fascinated.

ARTHUR CASAGRANDE

I first met our third president under what might have been quite unfavorable circumstances. I had determined, for personal reasons, that I wished to study soil mechanics under Arthur Casagrande and wrote him accordingly. Unfortunately, my decision was made shortly after the beginning of the second semester of his regular courses. He wrote me politely that it would be a great handicap to enter his group when it had already completed more than half of its curriculum, and he recommended that I wait until the following fall. Since I was not in a position to follow his advice, I wrote that I was coming nonetheless and would take my chances. When I arrived and called at his office, he greeted me most graciously, made every effort to see that Mrs. Peck and I were comfortably settled in Cambridge, and never alluded to the inconvenience that my unseasonal arrival must have caused him. This was my introduction to the graciousness, personal interest, and genuine concern that all his students would agree are hallmarks of his personality.

Arthur Casagrande has said that his interests, in order of their importance, are research, teaching, and practice. Yet, although he certainly has excelled in all three, in my own view he really placed teaching first, followed by practice, and finally by research.

As his students, we came to regard him as the great teacher. Always thoroughly prepared, not dramatic but completely at home in every detail, he developed among us a sense of being in the main stream of soil mechanics. We felt as if we occupied ringside seats in every round of the development and growth of soil mechanics from its beginnings.

Casagrande's relations with Terzaghi were close but often complex and even delicate. During the early days of World War II, while I was a student of Casagrande, he and A. E. Cummings of Chicago together made the necessary guarantees to permit Terzaghi to immigrate to the United States. Terzaghi was installed in an office at Harvard, first as a Lecturer and subsequently as Professor of the Practice of Civil Engineering. Although Casagrande had developed the entire program at Harvard during Terzaghi's nine-year absence from the country, Terzaghi considered it perfectly natural to regard himself as the master in a master-disciple relationship. Moreover, Terzaghi disdained most academic duties other than his lectures. Casagrande, often at considerable expenditure of time, fulfilled Terzaghi's obligations as well as his own. Although Casagrande felt the greatest respect and devotion for Terzaghi, surely Terzaghi was something of a trial and tribulation to him. I can speak from personal experience because Terzaghi, although he rarely spent more than a week at a time with me either on the Chicago Subway or at the University of Illinois, left me exhausted and often more than a little irritated over his requirements. His contacts with Casagrande were not limited to a week at a time.

It was Casagrande who proposed the First International Conference of 1936, and Terzaghi who considered it too great a gamble at such an early stage in soil mechanics. Casagrande proceeded nonetheless and created the conference that established soil mechanics as a legitimate and essential part of civil engineering. Terzaghi soon admitted that he had evaluated the situation incorrectly, and relished his position as President of the Conference and of the International Society.

Casagrande's enthusiasm for soil mechanics does not take the form of dramatic performances. Indeed, for his Presidential Address at the Fourth International Conference, he chose to discuss classification and information retrieval systems for soil mechanics literature. I am sure he knew the limitations of this subject as the basis for an inspirational address. Yet, because of its importance to soil mechanics, he felt it was his duty. Without the stimulus of that address, I doubt that we would have our excellent abstract and retrieval system today.

On the job, as a consultant on a major dam for example, Arthur Casagrande can only be described as an artist. He gives the greatest care to the selection, treatment, and arrangement of the materials to be placed in the dam. For him, it is not enough that the calculated factor of safety should have an accepted value. He works toward the best dam that can be constructed within economic limits and insists on including every reasonable detail that can improve performance, even if the results of the details are difficult to quantify. Where possible, for instance, he insists that the dam be curved upstream, and that the inclinations of the abutments be favorable with respect to resisting downstream movements even if the axis has to be adjusted or relocated when the abutments have been stripped. He refuses to debate the influence of these refinements on the value of the factor of safety; he simply points out that they produce a better dam. No one has a keener

sense of the inadequacies of design procedures to take into account all the ways water may act to endanger man's structures.

No one, either, is more aware of the significance of the properties of real soils. When the Corps of Engineers asked him to train large numbers of their officers in the construction of military airfields during World War II, he realized that these men would have to make their decisions and carry out their work largely without the benefit of laboratories. He collected a great variety of soils and, hour after hour, sat in the midst of a group of his trainees, fingering and examining the soils, describing and discussing their characteristics, developing the ability to estimate numerical values of the physical properties by manual and visual observation and the simplest of tests, and pointing out how the soils would perform during and after construction under differing field conditions. Thus he demonstrated how much can be deduced from literally getting one's hands dirty. Yet, at the other end of the spectrum, he decried engineers who accepted the sophisticated test results of others if they, themselves, had not performed many such tests, and if they did not know from their personal experience the influence of disturbance and poor laboratory techniques on the soil properties. For Arthur Casagrande, in soil mechanics, the emphasis is on the soil.

Last year, Arthur Casagrande and I served with seven other engineers on the Independent Panel to Review the Cause of Failure of Teton Dam. At the time of our final meeting, when the deadline for completing our report was upon us, we were still struggling with one issue: how much emphasis to place on the results of calculations showing the theoretical possibility of hydraulic fracturing of the core material at the base of the key trench. There was no disagreement that the results of the calculations, carried out by finite-element analyses, should be presented. Taken at face value, they indicated the likelihood of fracturing. Yet, many assumptions were involved in the calculations, and no clear scientific demonstration of the mechanism of the phenomenon, under conditions corresponding to those prevailing during reservoir filling, was available. The chairman of the Panel appointed a subcommittee, consisting of five members, to develop acceptable wording. The debate, though earnest, was vigorous, at times more realistically described as a shouting match. At the height of the tumultuous session, one of the other panelists paused at the meeting room, listened briefly, and beat a hasty retreat. Next morning, when we presented our agreed-upon wording, he commented, "What other professor could you imagine with whom four former students could express themselves so freely and forcefully, with no holds barred?" We realized that, by coincidence, all four of us had indeed been Casagrande's students at various times many years ago, but in the debate he was one of us. Then, as always, he presumed no master-disciple relationship. It is not his nature to demand respect; instead, he earns affection.

On the job, after dinner, when consultants and the people on the job gather for fellowship, Arthur becomes the center of attention as he draws on his great store of anecdotes. He has a remarkable memory for enlightening or amusing incidents, many recalling the early days of soil mechanics, and he recounts them with great skill and relish. Even meetings of consulting boards are enlivened, and sometimes a tense moment broken, by one of his always pertinent anecdotes.

## LAURITS BJERRUM

Our fourth president, Laurits Bjerrum, had much in common with Skempton, with whom he had a close personal bond. Perhaps he painted with a less broad brush and with greater pursuit of detail, but with no less enthusiasm. Full of life and vigor, he lived intensely, worked intensely, and took the greatest joy in his work, his family, and his friends. There were no halfway measures for him - he lived and worked with zest.

He was a superb selector and organizer of talent, with the ability to generate support for and from his colleagues. NGI quickly became not only a great research organization but the world-recognized finishing school of soil mechanics. Yet, for all its technical achievements and its attraction for foreign scholars, its people were always Bjerrum's family.

He was also a great showman with a keen sense of the dramatic. What to many would have been a dull technical subject became a mystery, a compelling detective story in his hands. Long will he be remembered for his part in the first ASCE Specialty Conference in Soil Mechanics, usually referred to as the Boulder Shear Conference, in 1960.

The now familiar format of the Boulder Conference was an experiment - panel sessions on selected topics, with a moderator and with participants consisting of the "big names". The first several sessions were rather formal. Each panelist gave a short prepared speech; later, the panelists debated politely among themselves. The sessions were informative and interesting, but hardly exciting.

I was the moderator of one of the last sessions, and our panel decided to encourage some spontaneity. I proposed that the panel discuss informally a specific problem:

how to estimate the shear strength of an existing slope on which an embankment is suddenly built. Stanley Wilson volunteered to start the discussion and had barely stated what soil tests he would perform when Bjerrum jumped from his seat, commandeered the microphone, and exclaimed, "I disagree!" Not only was the audience startled, so was Stan and so was I. There followed a lively debate, unrehearsed and unanticipated, that delighted the audience and that pinpointed, better than any sober technical discussion, the real issues, the shortcomings of our knowledge. Typically, Bjerrum not only played the game - he made the rules and sparked the competition. The audience truly saw what they had come for: the "big names" in action.

Bjerrum's acquaintance with Terzaghi began later than that of Skempton, Casagrande, or myself. Yet, their relation was remarkably close: they appreciated each other instinctively and fully. Possibly the best of all insights into Terzaghi's professional approach is expressed in Bjerrum's short contribution, "Some Notes on Terzaghi's Method of Working," in the Terzaghi Anniversary Volume, of which he was one of the editors. Perhaps a prime reason for their mutual appreciation and esteem was that both were, first and foremost, engineers, albeit with remarkable scientific aptitude and curiosity. The Terzaghi Library at NGI stands as a monument not only to Bjerrum's appreciation of Terzaghi's place in the history of civil engineering, but also to the close personal relation that prompted Terzaghi to entrust his files and library to his enthusiastic young colleague.

Bjerrum and I were no more than co-workers in our field until the Good Friday earthquake in Alaska in 1964. By good fortune, he and I were appointed by the Corps of Engineers to a three-man consulting board on the landslides associated with the disaster. We found stimulation not only in the project but in each other, and determined in the future to keep, if possible, at least one project in which we would be associated. And so, successively, we worked together on a cofferdam failure on the Ohio River, on the dikes of the Dead Sea project, and on the James Bay hydroelectric development in Canada. We looked forward to and enjoyed many a long evening, when these projects brought us together, discussing not only the projects but many frontiers of our knowledge: secondary consolidation, strain energy, diagenetic bonds, liquefaction. He was the innovator, the proposer; I was the objector, the conservative. Each of us pressed his view, and in defending his view brought the discussion closer to reality.

During our last meeting in Montreal on the James Bay project, Bjerrum had bought tickets to a piano concert. It was held on the same stage in the same hall where he had accepted the Presidency of our Society, and we recalled many of the pleasant incidents of that conference. Little did we realize that it would be our last time together.

Like Terzaghi, he was a fighter, for progress, for freedom in our Society, for his Institute. He was, indeed, irrepressible. Many of you will remember the closing banquet at the Mexico Conference, the unexpectedly huge crowd, the deluge, the roving orchestras each creating more decibels than the other. It was at this banquet that Laurits had planned to introduce me, with appropriate remarks, as the new President. When the time came, he started his speech, but the roving orchestras were not aware of the ceremonies, and the attending guests could hear only the music. Nevertheless, Bjerrum was determined to continue his speech, and continue it he did. Although I sat close by, I heard it not, and never learned what he said until I read the written version in the published Proceedings. Characteristically, he fought against the odds.

## THE GROUP PORTRAIT

Seldom has a new profession been so fortunate as to develop under the leadership of four such men, all devoted to the advancement of their field, all master contributors to its growth, all influential far beyond their own localities, and all the most congenial and respectful of friends.

Even outsiders note and remark on the family relationship among workers in soil mechanics the world over. This Conference, the ninth family reunion, testifies to the enduring influence of our first four presidents.