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Recollections from 50-plus years of geotechnical practice or "You can't do it alone"

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SYNOPSIS Beginning with my nine months at Harvard in 1946-1947, my career has been a fortunate series of solutions of challenging geotechnical engineering problems carried out in an atmosphere of personal interaction leading to long-lasting friendships and relationships. This paper ties particular problem solutions to some of the individuals I respect, admire, and credit for my professional growth and personal satisfaction for 50-plus years.

INTRODUCTION

The technical papers that have appeared in the ASCE Geotechnical Journal, Geotechnique, Canadian Geotechnical Journal, Geotechnical Conference Proceedings, and other publications have been the key elements in dissemination of technical knowledge to our subdiscipline of geotechnical engineering within Civil Engineering. However, these papers are for the most part impersonal and essentially devoid of the human element. This paper was prepared to recognize a number of personalities who contributed significantly to my knowledge, growth, and opportunities, and to interject a little human interest into some of the technical problems and solutions described in my publications. A second theme is to emphasize that the team efforts are critical to the optimization of problem solutions. The team effort often involves the geotechnical participants as a subteam, always within the total project team of owner, designers, builders, and operators.

TERZAGHI AND CASAGRANDE

My introduction to geotechnical engineering (then soil mechanics) actually dates back nearly 60 years to my freshman year at the University of Texas at Austin in the spring of 1941. Dr. Karl Terzaghi gave a series of lectures there at the invitation of Prof. Raymond Dawson. Once a week I sat in on his lectures following the strong recommendation of my father, Prof. John A. Focht, Sr. I did not fully comprehend what I heard but I distinctly remember that his sketches on the blackboard were duplicates of the illustrations that would soon appear in "Theoretical Soil Mechanics." This initial introduction to Dr. Terzaghi was followed by more reference to him and his work in my senior class on Soil Mechanics and Foundations late in 1943, which was taught by Prof. Raymond Dawson. I also heard Dr. Terzaghi lecture at the Texas Conferences on Soil Mechanics and Foundation Engineering in 1942 and 1943.

Further exposure to Dr. Terzaghi came in 1946-1947 while I attended Harvard. I can still recognize my crude notes from his lectures as representing illustrations that later appeared in "Soil Mechanics in Engineering Practice." My nine months at Harvard included being crammed with technical guidance and instruction from Dr. Arthur Casagrande and Dr. Terzaghi as well as the development of lifelong friendships with many budding geotechnical engineers. The now well known names I met at Harvard included Stan Wilson, George Sowers, Marty Kapp, and Hugh Sutherland. Others included Dick Loughney, Allan Osberg, John Holman, Bruce Woolpert, Reinard Brandley, and Lionel Peckover. George was the only married one of my study group and I fondly recall that he and Frances had some of us over to supper once a month for a home cooked meal.

I was fortunate enough to be able to take a "reading" course under Arthur Casagrande. My assignment was to read all of the Harvard and MIT reports on the Shear Strength of Clay Research Program reports for the US Army Corps of Engineers and related papers. Then once a week I reported to Arthur what I had read and learned; and he would respond with his opinions – not only on the technical content of the reports and papers but also on the authors themselves. In those sessions Dr. Casagrande revealed a side of him much different from that shown in the classroom. In class he was very objective and unbiased; in his office, his strong and frequently critical feelings were openly expressed. That period in Cambridge set the pattern for my geotechnical career – major technical involvement (and learning), but equally important was the personal involvement with those I was working and learning with and from. Both aspects yielded tremendous personal satisfaction – the solution of difficult challenging problems carried out in an atmosphere of personal interaction leading to long-lasting friendships and relationships.

WATERWAYS EXPERIMENT STATION

Good advice to a new graduate can have a major effect on his career. Arthur Casagrande suggested early in 1947 that I apply for a position at the US Waterways Experiment Station (WES). I took his advice, accepted the offer that came from Vicksburg, and have been thankful ever since. The engineers there were all outstanding technically, individually helpful to the young engineers, and excellent role models both professionally and personally. They were Bill Turnbull, Stan Johnson, Woodie Shockley, and perhaps most important of all at that time, Charles Mansur, my immediate boss. Each of these men was an extremely proficient writer and editor, and taught me how to write a good technical report.

Levee stability considerations occupied much of my effort. The days spent making hand calculations with a mechanical calculator gave others and me an insight into slope stability that cannot be derived from today's use of the computer. That experience created an ability to recognize when computer results are wrong because of an "uncaught" entry error. It is certainly not unique with me; there are many other contemporaries who developed the same feel for slope stability or other numerical analyses that young engineers tend to miss today because learning from computer printouts is much more difficult.

Quality exploration and laboratory testing were trademarks of WES. Dr. Juul Hvorslev was the ultimate authority on exploration. Tommy Goode, head of the Exploration Section, put quality into practice and indoctrinated all of us who came through the Station with a commitment to and an understanding of what constitutes quality and appropriate soil exploration.

I was fortunate to work on the comprehensive pile test program for the Combined Morganza Floodway Control Structure on the Mississippi River, which was a major project. I was involved in the planning of the program and later served as the project engineer for analysis of the program results and preparation of a report. The actual load tests were directed by the New Orleans District, Corps of Engineers. The test results provided valuable data for the design of the foundations of this major flood control structure on the lower Mississippi River. These load tests were also a significant contribution to our knowledge on the capacity of large, long piles driven through soft clays into dense sand. The load test results indicated that the skin friction in the backswamp clays would be approximately equal to their undrained shear strength. They also produced valuable data on the end bearing of piles penetrating into the basal dense sand. After I had been called back to active duty for the Korean War in 1950, Charles Mansur prepared a paper on the principal results for the ASCE Soils Journal and submitted it with me as a co-author. That paper, Mansur et al. (1956) won the ASCE Middlebrooks Award for 1957.

In Vicksburg I lived in the same boarding house with Dr. Juul Hvorslev, a most remarkable gentleman and engineer. For several months Juul took Wally Sherman (a classmate at Harvard) and me to work since we were just out of school and in debt. He even offered his car to us to use on a date but neither of us took him up on his generous offer. Juul was an incessant cigarette smoker; I think more than Terzaghi. He carried on an immense correspondence with engineers all around the world and shared the technical content of some of his correspondence with us at mealtime and in the car. Those conversations were an education in themselves. Three other young geotechnical engineers who ate at 2602 Drummond were Bill Emrich, Bob Kaufman, and Bob Cunny.

The best thing that happened to me in Vicksburg was that I met, courted, and married Edith. She was from Brookhaven, about 80 miles from Vicksburg, but working in Vicksburg. We were married just a week after I was called back to active duty in the Army. After a brief stay with me in Colorado before my unit went to Korea, she returned to Vicksburg and her old job. I returned from Korea in 1952 and worked at WES for about 8 months before we moved in April 1953 to Houston to join Greer & McClelland (soon to become McClelland Engineers, Inc.)

HIGH RISE BUILDINGS AND DEEP BASEMENTS

First City National Bank Building. I had the opportunity to work with Phil Rutledge in 1958 on the First City National Bank in downtown Houston. The initial architectural plans called for a full-block deep garage basement, a light above-ground structure on 60 percent of the area, and a 33-story tower on the remainder. With as many as five levels of parking for 800 cars, a 61-ft excavation would be required. Our analyses indicated that there would be more than 12 in. of long term heave of the light structure, which was not tolerable. To provide the owner with a second opinion, we suggested retaining Mueser Rutledge to review our predictions. In our first meeting Phil asked, "If we don't agree, whom shall we call in, Raymond Dawson?" As it turned out, his predictions were slightly larger than mine, so our joint recommendation was to limit the basement to just one level making the excavations 16 ft deep for the light structure and 23 ft for the tower. Slight heave was computed for the light structure and slight settlement for the tower with about 2.5 in. of differential movement expected. His collaboration was very comforting. Sketches of the foundation schemes are presented in McClelland (1961).

In subsequent consulting on the sheeting retention system for the deeper excavation, Phil and I together developed an inclusive lateral earth pressure diagram for the stiff slickensided Beaumont Clay present beneath downtown Houston. We assumed a gross lateral earth pressure coefficient of one-third, then increased that by another third to allow for load concentrations, and finally distributed it in a trapezoidal diagram as recommended by Terzaghi and Peck. I found Phil's comments on sheeting pressures extremely enlightening. Actual measurements on subsequent projects revealed that we were somewhat conservative, which in the absence of any prior experience with the deep-sheeted excavations in stiff clay was appropriate.

The architect made two mistakes on building details by ignoring our predictions of differential movements. While the structural engineer very carefully designed the simple span for the main hallway connection between the two structures at ground floor, the architect detailed a poured terrazzo floor right over the hinges without proper joints. Of course, it cracked and has been unsightly ever since. His second mistake was to put horizontal sun shields about 3 ft wide over each row of windows in the tower. Drainage of each shield was provided only at the building corners despite our prediction of about 2 in. of dishing settlement. This differential settlement caused rainwater to pond on each shield at the middle of the building requiring later placement of a wedge of concrete fill on each shield to create proper drainage out to the corners.

Humble Building. Planning for the 44-story Humble Building in Houston began late in 1958. Initial schemes included a deep basement under the entire block with only a portion of the site occupied by the tower. My analyses soon led to having the deep basement centered under the tower with only one basement under a plaza in front of the building. With a 49-ft excavation, the predicted dishing of the 6-ft thick mat was over 3 in. when using a uniform soil reaction. Clarence Stacy, who was the structural engineer, was concerned about the resulting bending moment in the center of the mat. I was also concerned. His solution was to compute the moment that the dishing would create elastically and then provide enough steel in the bottom of the mat to carry one-half of that moment. His thought was that creep in the concrete and steel would "take care of the rest". Clarence was the first of several structural engineers who took the time with me to understand the geotechnical aspects of a tall building supported on a deep mat foundation on stiff clay. He responded with guidance to me on some of the intricacies of structural design of a mat. I learned much from him.

With support from Clarence, an instrumentation program was authorized both on the Humble Building and the subsequent 500 Jefferson Building to determine the loads in the struts supporting the excavation sheeting. The results, as presented in Focht (1962), confirmed the general validity of the assumptions that Phil Rutledge and I had made for the First City National Bank Building. Other observations on and off site on these two projects laid the groundwork for subsurface design for the many subsequent major buildings with deep basements in Houston.

Extensive effort was put into design and construction of the deep Humble Building basement to make it watertight. It was completely surrounded by a rubber membrane both beneath the mat and around the outside of the basement walls. The lighting grounding cables under each column were carefully run through tight fitting sleeves in the rubber membrane. But after the temporary dewatering system was shut off and the water level returned to its original level at about 25-ft depth, there was a significant leak at each grounding cable. Someone forgot that the cables were twisted strands rather than a single solid strand. Consequently, the electricians had to fill each cable with solder and add a new sleeve to seal the leaks.

One Shell Plaza. Fazlur Khan was perhaps the most complete engineer I ever worked with. Faz was the chief structural engineer for such projects as the World Trade Center in New York and the John Hancock and Sears Towers in Chicago. But he was also effectively an architect and a pretty good foundation engineer. It was my good fortune to assist him on One and Two Shell Plazas in Houston beginning in 1960. He quickly grasped the concern that I had long felt that a simple settlement analysis by the soil engineer assuming a uniform pressure distribution on a deep mat was not representative of the actual situation. For One Shell we developed a scheme (before finite element programs became so versatile) that permitted iterative analyses by the structural engineer and the soils engineer to arrive at a better estimate of the actual distribution of pressure on the mat resulting from the stiffness effects of the mat and the superstructure, and the tendency of a mat to dish. This procedure described in Focht et al. (1978) gave the soils engineer and the structural engineer much more realistic predictions of mat deflections. Faz was a profound integrator of disciplines and gave me much guidance on how to be more effective in a "team" design. His first assistant on these two projects, Joe Colaco, remained in Houston so we continued to work as a team on a number of subsequent major buildings. Pete Gemeinhardt was my able assistant on these and other projects.

OFFSHORE STRUCTURES

Laterally Loaded Piles. In 1954 we had an opportunity to review and evaluate the results of a full-scale lateral pile load test performed in the Gulf of Mexico. Our results published in McClelland et al (1958) were the beginning of the "p/y" technique for predicting the performance of laterally loaded piles. That paper won the ASCE Laurie Prize for 1959. It also marked the beginning of my still continuing association with Lymon Reese and Hudson Matlock on a wide variety of offshore related problems. Their subsequent tests and analytical studies done in the later 50's and 60's are still the foundation of laterally loaded pile predictions.

Alpha Factor. Tomlinson published his paper showing that f=αc for piles driven into over-consolidated clay in 1957 (Tomlinson (1957). In the year preceding we had had several pile load tests on piles in the Beaumont clay in the Houston area for which the indicated skin friction was much less than the clay strength, only in the order of 1000 psf, despite soil strengths of 1500 to 2500 psf. In the same time period, Dames & Moore reportedly had similar experiences. I was struck by the coincidental development by three different organizations of awareness of the deviation from the widely accepted concept of f=c.

Pullout Tests. Partial or complete overturning of a number of wellhead structures during a hurricane in the Gulf of Mexico led to a series of pullout tests in 1958 on piles installed into sand by a combination of procedures, sponsored by several oil companies. The results were included in McClelland's Terzaghi lecture in McClelland (1974). It was a low cost project; the pullout load was applied by a pair of manually pumped jacks with individual pressure gages. The technical crew was four oil company engineers and me; three of that crew were named "John" (Focht, Bigham, and Lacy). At times two John's were manning the jacks and the other John was reading a deflection dial gage. I remember the confusion when the record keeper would ask, " John, what is your reading?" We did find out that the tensile capacity of a pile jetted into sand was remarkably low even though it may have driven its last few feet.

Evolution of Capacity Predictions. Improvement in the prediction of the capacity of long piles in clay has been a major activity for me throughout my career. The evolution of the predictive technique advocated by me, adopted by McClelland Engineers, and generally followed by API for offshore piles is reasonably well documented in Focht et al. (1977), Focht (1983), and Pellieter et al. (1993). McClelland et al. (1969) co-authored with Bram McClelland and Bill Emrich received the ASCE State-of-the-Art Civil Engineering Award for 1971. The topic of pile capacity was also the subject of two other papers, Focht et al. (1981) and Vijayvergia et al. (1972). Each of my co-authors added substantially to the understanding by me and the profession of pile capacity in clay. In my opinion, these papers and others on offshore foundation problems were a major reason that McClelland Engineers received the Distinguished Achievement Award for Organizations from the Offshore Technology Conference in 1986, the first time that award went to a consulting firm. Working closely over the years with other leaders of McClelland Engineers – Bram McClelland, Bob Perkins, Bill Emrich, Charles Mansur, and W.T. Reynolds – added much to my personal knowledge, status, and satisfaction.

Laterally Loaded Groups. Planning by British Petroleum of its four Forties structures in the North Sea began in 1971. These structures were to differ from Gulf of Mexico jacket structures in that they were to be supported by circular groups of piles under the four large diameter legs. Initial lateral load analyses by another consultant utilized the elastic procedure developed by Poulus (1971) for the circular groups of eleven 54-in. piles. Their estimate of the modulus of elasticity of the soil was incorrectly obtained from a p/y analysis of the deflection of a single pile. As a result their predicted deflection of the group was very large and unacceptable. Experience and judgment convinced me that such a modulus would be much too small to correctly model the low stress level created in the soil mass by one pile at a point one pile spacing away. Ken Koch and I devised a technique using the Poulos elastic model to predict the elastic deflection due to the group effect and a p/y analysis to predict the deflection due to the high local stresses and inelastic behavior of a single pile. The Young's modulus for the "group" portion of the analysis was picked to represent a low stress level in the soil - say, the initial tangent modulus from most laboratory tests. This approach produced more reasonable looking predictions that were tolerable for the structure. Harry Poulos had spent part of a sabbatical in our office several years before and I was convinced from that interaction that he would concur with our approximate combination of procedures as presented in Focht el al (1973).

Gravity Structures. The Ekofisk tank structure installed in 1973 in the North Sea was the first offshore petroleum gravity structure. The foundation at the site consisted of dense clean sand in about 270 ft of water. The exterior wall of the Doris-type structure was a perforated, energy-dissipating baffle 300 ft in diameter rigidly connected to the interior tanks. The oil storage tanks were in the center with a common mat supporting both components. The Norwegian government wanted a review of its expected performance, particularly under the repeated cyclic loads of a severe North Sea storm and turned to NGI as their consultant. Until that time NGI had had very little involvement with offshore geotechnical engineering. Phillips Oil Co. authorized me to retain Ken Lee of UCLA to assist in evaluation of the storm effects and to attend a hurry-up meeting in Oslo at NGI. I met Ken in New York and we flew together to London on a 747. We had seats in the upstairs lounge and worked the whole flight over to develop a response to the Norwegian questions and concerns. Blow count data from our percussion sampler and extremely hard driving of piles into the sand for a nearby structure convinced us that the sand was quite dense. Ken's earthquake experience led him to believe that the sand was so dense that there would be no liquefaction or other adverse reaction to the cyclic storm loads. That was the judgment story we presented based on our all-night effort on the 747. It was generally accepted by NGI, but Laurits Bjerrum wanted more field demonstration of the sand density rather than just judgement and the opportunity for detailed cyclic load analyses. Ken also wished to run cyclic tests on the sand. Phillips agreed that all were desirable but continued right ahead with fabrication of the structure. Mike Duncan was at NGI at that tine and contributed to the meetings. We spent 6 or 8 months and a considerable amount of Phillips' money to perform a couple of limited penetration cone tests for the first time in water that deep. In fact, the two cone tests indicated a tip resistance of 100 tsf at 3 ft and 200 tsf at 6 ft. These results were so high that we had John Schmertmann review them. He concluded that not only was the sand dense but also that the lateral stresses in it were very high, perhaps a K_o of as much as 5. Ken's test confirmed his quick evaluation on liquefaction potential. After about a year of analyses, NGI concurred with our overnight simple analyses. Sometime later Bjerrum in a conversation with Ralph Peck on the Ekofisk project said casually more-or-less, "The sand turned out to be very dense, just like we and McClelland Engineers thought it was. And we convinced ourselves that the performance in a storm would be okay". The year of considerable effort confirmed the earlier judgments. Ken Lee's contribution is clearly evident in Lee et al. (1975), which won the ASCE Middlebrooks Award for 1976 for us. Considerable credit for the success of Ekofisk must go to William R. Bowles, the Phillips engineer working along with the rest of the team.

Disagreement with Industry. On two occasions recommendations from McClelland Engineers and me did not sit well with representatives of the offshore oil industry. The first time was when the first structures were to be built in Alaskan waters where ice loads would be the major environmental load. In the Gulf of Mexico all personnel are planned to be evacuated before a hurricane strikes. The hurricane forces are the maximum environmental load in the Gulf. Early on the industry had accepted our suggestion that a factor of safety of 1.5 would be appropriate for that condition. Consequently, if men were to remain on an Alaskan platform subject to ice loads, we suggested that the factor of safety for the piles should probably be increased to 2.0. This suggestion was met with immediate disapproval and a request was made to remove that recommendation from our report. Their comment was that the factor of safety was their area of decision and that our opinions were not needed. The second time was when Vijay Vijayvergiya and I presented our "Lambda" paper (Vijayvergiva et al. (1972)) indicating that a length effect on the capacity of very long piles in clay was probably making the API criteria of 1971 somewhat unconservative. Rather than seriously consider the "lambda" method or some approximation thereof, API chose to add a flawed procedure, now known as "API Method 2". The deficiency of this method was demonstrated in Focht et al. (1981) co-authored with Lee Kraft and presented at an offshore symposium at AIT in Bangkok. Nevertheless, it remained in API RP2a until 1987 before it was replaced.

San Francisco Paper. In 1982 Harry Seed asked me to prepare the theme lecture on piles for the 1985 ISSMFE Conference to be in San Francisco. I had just been elected a Vice President of ASCE and was very busy with my new duties; I enlisted Mike O'Neill of the University of Houston to be co-author and assist in gathering data on the international state of the practice for design and installation of axially loaded piles. Nearly 200 detailed questionnaires were sent out world wide with a 30 percent return. Working closely with Mike for about a year gave me the benefit of his substantial experience with drilled shafts and driven piles. I still like the last paragraph of that paper ((Focht el al. (1985)).

> "The good technical engineer is one who knows the limits of his experience with problems and soil conditions comparable to his current assignment and makes appropriate extrapolations. He knows what he knows and uses it confidently. More importantly he knows what he does not know, seeks available knowledge, and then proceeds fully acknowledging his limitations and uncertainties."

This description fits Mike O'Neill very well.

DAMS

Livingston Dam. Geotechnical studies for Livingston Dam on the Trinity River near Houston began in 1961. This earth structure was to be 14,000 ft long with a 90-ft maximum height. A 584-ft long gated concrete spillway would be located on a terrace adjacent to the existing river channel. The geotechnical consultant was Associated Soil Engineers, a joint venture of McClelland Engineers and National Soil Services (also of Houston). Ralph Reuss and I were the principals. The critical foundation stratum, which was overlain by the usual alluvial deposits of clay over sand, was found to be stiff to very stiff, slickensided, highly plastic, heavily overconsolidated Miocene clay. After the failure of Waco Dam, we became concerned about the implications of residual strength of the Miocene clay and its impact on embankment stability, particularly at its juncture with the spillway structure. In response to these concerns and at our recommendation, the designers (Brown & Root, Inc. and Forrest & Cotton, Inc.) retained Dr. Arthur Casagrande as a special consultant. We spent a number of hours together looking at undisturbed samples, particularly breaking them apart to examine slickensided surfaces. We had already run a large number of consolidated-drained direct shear tests at very low rates of strain. A few of these were of the Skempton-type in which the upper frame was pushed back several times after a load cycle to produce a total composite movement approaching 1.5 in. to evaluate "residual" strength. At Arthur's suggestion, we also ran three conventional tests on specimens carefully trimmed to have a slickensided surface in the center of each. Because the stratum was deep, all of the tests were run under a vertical load of 6.0 ksf and the strength envelope was drawn through the origin (c=0). As a result of these tests and the multitude of personal examinations, we jointly agreed that a strength of Φ=17 degrees, c=0 could be assigned to the Miocene clay. These parameters were intended to conservatively represent its strength along slickensides based on laboratory test results of 16 to 20 degrees. The residual strengths had been indicated to be in the range of 10 to 14 degrees. Based on triaxial tests, the pore pressure response in the Miocene clay to the embankment load was predicted to be 50%, which was confirmed during construction by piezometers.

Because he believed that the combination of assumptions was conservative, Arthur suggested that a factor of safety of 1.4 would be adequate. However, he added another criteria for acceptability. While we would not put the results in our report, an analysis with residual strength would have to have a factor of safety of at least 1.0. His indication that this was a routine procedure for him on embankment stability made a major impression on Ralph and me. Both of us adopted it as applicable to much of our practice. This recollection of Arthur's approach to selection of a "design" strength convinces me now that he clearly understood then the concepts in the currently advocated "Reliability Based Design". I further believe that he would have remained a proponent of relying heavily on judgement rather than just sophisticated mathematical analyses. His stability criteria for the embankment led to a 1-v on 2.5-h upper slope about 45 ft high, a 100-ft wide berm, and then a 1-v on 2.5-h slope to ground. The overall slope has been quite satisfactory, but the 1-v on 2.5-h slope of highly plastic clay has been subject to minor sloughs. There is no way to describe the value of that interaction with Arthur Casagrande. Ralph and I learned so much from him about his philosophy on embankments, his approach to laboratory testing, and "Calculated Risk".

The Miocene clay created a challenge in the design of the spillway as well. With the clay in place, the factor of safety for the spillway against sliding was too low. A pile foundation would have solved that, but the differential settlements between the embankment and the spillway would not be tolerable. Arthur concurred with our suggestion based on good prior experience with process structures of replacing the Miocene clay with carefully compacted clean sand. The thickness of sand beneath the spillway was about 65 ft. The excavation and the resulting thickness of clay remaining under the embankment approaching the spillway were tapered so that the differential settlements were spread out over a significant distance. Details on the spillway and a summary of the strength test results are given in Focht et al. (1975).

Terzaghi and Cavernous Limestone. Dr. Terzaghi was seldom at a loss for words. I remember, however, a technical session at an ASCE convention (probably in the 60's) regarding dams on cavernous limestone. The session was probably sponsored by the TVA. During the discussion period, Terzaghi got up and said something like, "This has been interesting but I make it a practice to not take assignments in cavernous limestone regions". Another equally distinguished looking gentleman rose to reply generally as, "With all due respect to Dr. Terzaghi, there simply are major societal needs for dams in some cavernous limestone regions, and some of us have the job to design and build them so that they will safely fulfill their intended purpose". Dr. Terzaghi had no reply.

Morris Sheppard Dam. As part of a routine 5-yr inspection of Morris Sheppard Dam on the Brazos River west of Fort Worth in December 1986, the inspection team of Freese and Nichols, Inc. and McClelland Engineers, Inc. almost simultaneously discovered problems with the dam. Portions of the concrete-buttress spillway that retained about 130 ft of water had moved downstream as much as 4.5 in., and very high piezometric pressures were observed in the shale beneath the dam. The team quickly recommended to the owner, the Brazos River Authority, that the reservoir be lowered 8 ft to increase the factor of safety by 10 percent. The reservoir was actually lowered 13 ft at the request of the Federal Energy Regulatory Commission. A special consulting board was retained consisting of A.J. Hendron of the University of Illinois and James Libby, consulting engineer. Thus, the team that worked together on the investigation and restoration over the next 7 years consisted of R. A. Thompson and Ron H. Waters of Freese & Nichols, Skip Hendron, Jim Libby, and Bob Ringholz of McClelland Engineers and me. Focht et al. (1990) summarizes the simultaneous investigation and restoration efforts that cost $30 million in comparison to the original 1940 construction cost of $8.8 million. The investigation and monitoring program included 125 borings, 132 piezometers, 10 inclinometers, and 18 extensometers. The restoration work required an 8-ft diameter passageway through the 26 4-ft thick spillway buttresses, a 180,000-cy crushed gravel platform above tail water, 146 6-in. diameter relief wells on 13.5-ft centers with additional wells in one critical area, injection of 2500 cf of sodium silicate-cement grout in 32 holes in three bays, and placement of 86,000 cy of lean concrete ballast between buttresses. The "observational" simultaneous investigation and restoration were successfully completed in late 1994.

My proposal for analysis of sliding stability was to use the performance of the structure as a full scale loading test that indicated a factor of safety of essentially 1.0, our best estimate of the piezometric pressures for a full reservoir before any pressure relief created by the investigative effort, and a simple horizontal sliding surface at a shallow depth. Using a tan Φ of 0.4, a non-frictional resistance of 1.69 ksf was backfigured for an assumed factor of safety of 1.0. As the primary stability criteria for remediation, the minimum factor of safety to be achieved was established at 1.75 using tan Φ= 0.4 and c=1.69 ksf based on observed piezometric levels actually achieved by the remedial measures with modest upward adjustments for potential decrease in efficiency with time of the pressure relief system as installed. This rational procedure was accepted by the consulting board, Ron Corso (then head of the dam safety section of FERC), and Carson Hoge (Engineer-Manager of BRA), and governed the final remediation. The very cooperative interaction within the team and with BRA and FERC was a major contribution to the successful completion of the restoration and all learned from it.

ASCE SERVICE

Professional society service consists of participation in team solution of problems, even if they are not technical or project oriented. It provides interaction with peers and can contribute to both technical knowledge and professional success. Such service over the years has greatly broadened my contacts, added to my knowledge, and enhanced my professional reputation in ways different from that resulting from my technical papers. Consequently, three components of my ASCE service are described in the following subsections as pertinent to the overall theme of this paper.

GT Executive Committee. In 1975 I was asked to serve on the Executive Committee of the ASCE Soil Mechanics and Foundation Engineering Division. That volunteer assignment gave me the opportunity to work closely over a period of five years with men like Harry Seed, Dick Gray, Ernie Selig, Bob Schuster, Bill Swiger, George Sowers, Woodie Shockley, and John Lysmer. While our principal joint efforts were directed at activities of the Division, there was lots of discussion at meals and in the evenings regarding our interesting projects. The learning experience for me was tremendous. I only hope that I contributed something to each of them in return for what they taught me.

ASCE Officers. Service as President of ASCE in 1989-1990 does not have a direct connection to geotechnical engineering but it was a distinct honor to be recognized by civil engineer peers. There have been a number of other geotechnical engineers who have also had that honor: Wallace Chadwick, Walter Blessey, Lee Walker, Joe Ward, and Russell Stearns. In addition, Frank Newnam had strong geotechnical connections.

The number of geotechnical engineers who have held an ASCE office seems to me has been disproportionately larger than the geotechnical membership. Maybe it is because geotechs tend to work with and for other civil engineers more than other subdisciplines. Thereby, they have proportionately greater exposure within practicing civil engineers. Or maybe, they have a greater commitment to professional service. Other geotechs have served as a Vice President of ASCE were Trent Dames, Bill Moore, Gene McMasters, Bill Zoino, Bob Lawson, Walter LeFevre, and Bill Marcuson. Twenty-three geotechnical engineers have served as an ASCE Director to the best of my knowledge: Charles Britzuis, Jose Capacete, Leroy Crandall, Elio D'Applonia, Raymond Dawson, Ed Fusik, Arthur Greengard, Delon Hampton, Ken Hansen, Richard Hazen, Lloyd Held, Jeff Hilliard, Ron Hirschfeld, Peter Hoadley, James Olson, J.A. Padgett, Ralph Peck, Carlton Proctor, Gardner Reynolds, Ed Rinne, Phil Rutledge, Malcolm Steinberg, and Ed Wilson. Some of these are also very well known for their technical contributions. The rewards of such service are innumerable, but one of the greatest that I have received is the large number of friendships.

Geo-Institute. Perhaps one of my most exciting and rewarding society assignments began in 1994 with a request from Jim Davis, Executive Director of ASCE, to head a small task committee to consider the feasibility of transforming the Geotechnical Engineering Division of ASCE into a new semi-autonomous organization. That and a similar committee looking at the Structural Division recommended to the ASCE Board of Direction in 1995 that two pilot institutes be authorized. I continued as the chair of a Board Task committee consisting of the existing GT Executive Committee and Bill Marcusón to develop a new organization to be called "The Geo-Institute of ASCE". It will reach out and entice participation by associated professionals participating in all aspects of what might be termed the "geo-industry". Mike O'Neill was then Chair of the GT Executive Committee and was followed by Larry Roth, who became in 1996 the first president of the Geo-Institute. The group proposed that membership be open to any professional in the geo-industry - geologists, specialty contractors, geophysicists, material suppliers, etc. It began immediate revision of the technical committee format to improve flexibility and remove bureaucratic restraints. A substantial increase in cooperative efforts is envisioned with other existing organizations – professional, technical, industrial, or commercial – with an interest in geotechnics. A major key to the "Institute" concept is revenue sharing with the parent ASCE organization of funds derived from new activities. Consequently, entrepreneurial efforts to produce revenue to be utilized in expanded committee activities will be one theme of the Geo-Institute. The organizational process has not been smooth; there were many who were opposed to change, others who were apprehensive that start-up funding for the new Institutes would divert funds from their programs, some objected to the inclusion of non-engineers, and others were just skeptics.

The Geo-Institute committee structure will expand from just technical orientation to other topics such as continuing professional development, more emphasis on contacts with local geotechnical groups, and legislative and regulatory concerns. Institutes will be relieved of the former bureaucratic hierarchy under which the technical divisions formerly operated and now will report directly to the ASCE Board. The new organizational format was enthusiastically received by attendees at the GeoLogan Conference (1997) along with considerable favorable comment by representatives of a number of related organizations. My key contribution to the success of the efforts was serving as a communicator and facilitator between the Geo-Institute Board of Governors and ASCE leadership. My corporate memory from lengthy service to both of these constituencies served well in this situation and, I think, facilitated the progress to date. Mike O'Neill, Larry Roth, and Jim Davis deserve credit for their energy and commitment in bringing the Geo-Institute so far so fast.

I enjoyed the involvement and believe that the Geo-Institute is well on its way to achieving its vision of being the premier organization for:

- Advancing the state-of-the-art and the state-of-practice of the worldwide geo-industry.
- Providing effective and timely technology transfer.
- Integrating the technology activities of all individuals engaged in research, education, design, testing, manufacturing, construction, and operation and maintenance in the geo-industry.

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All of the individuals named herein made important contributions to my career. There were many others not named. A major unnamed group consists of the Owners of the projects. They were all committed to proper and safe solutions to the geotechnical problems on their projects.

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References

Focht, J.A., Jr. (1962), "Strut Loads in Deep Braced Cuts in Pleistocene Clay," Presented, ASCE Convention, Houston, Feb. 1962.

Focht, J.A., Jr. (1983), "Axial Capacity of Offshore Piles in Clay," *Shanghai Symposium on Marine Geotechnology and Nearshore Structures*, 1983, Shanghai, China, 476-504.

Focht, J.A., Jr. (1994), "Lessons Learned From Missed Predictions," *J. Geotech. Engrg. Div.*, ASCE, 120 (10), 1651-1683.

Focht, J.A., Jr., Khan, F.R. and Gemeinhardt, J.P. (1978), "Performance of One Shell Plaza Deep Mat Foundation," *J. Geotech. Engrg Div.*, ASCE, 104 (6), 755-780.

Focht, J.A., Jr. and Koch, K.J. (1973), "Rational Analysis of the Lateral Performance of Offshore Pile Groups," *Proc. 5th Offshore Technology Conference*, Houston, Vol 2, 701-708.

Focht, J.A., Jr. and Kraft, L.M., Jr. (1977), "Progress in Marine Geotechnical Engineering," *J. Geotech. Engrg. Div.*, ASCE, 197 (10), 1097-1118.

Focht, J.A., Jr. and Kraft, L.M., Jr. (1981), "Prediction of Capacity of Long Piles in Clay," Presented, Symposium on Geotechnical Aspects of Offshore and Nearshore Structures, Bangkok, Thailand, Dec. 1981.

Focht, J.A., Jr. and O'Neill, M.W. (1985), "International State-of-the-Practice for Design and Installation of Axially Loaded Piles," *Proc., 11th Int. Conf. On Soil Mech. And Found. Engrg.*, San Francisco, Vol. 1, 187-209.

Focht, J.A., Jr. and Reuss, R.F. (1975), "Performance of Large Gated Spillways in Texas on Deep Compacted Sand Fills," Presented, Univ. of Calif., Berkeley, Conference on Recent Developments in Design, Construction and Performance of Embankment Dams, June 1975.

Focht, J.A., Jr. and Ringholz, R.P. (1990), "Morris Sheppard Dam A Rational Approach to Stability Analysis," *Optimizing the Resources for Water Management*, ASCE Conference, April 17-21, 1990, Ft. Worth, TX, 277-281.

Kraft, L.M., Jr., Focht, J.A., Jr. and Amersinghe, S. (1981), "Friction Capacity of Piles Driven Into Clay," *J. Geotech. Engrg. Div.*, ASCE, 107 (11), 1521-11541.

Lee, K.L. and Focht, J.A., Jr. (1975), "Liquefaction at the Ekofisk Tank in the North Sea," *J. Geotech. Engrg Div.*, ASCE, 101 (1), 1-18.

Mansur, C.I. and Focht, J.A., Jr., (1956), "Pile-Loading Tests, Morganza Floodway Control Structure," *Trans.*, ASCE, Vol. 121, 555-587.

McClelland, B. (1961), "Foundation Heave and Multi-Story Buildings," *Progressive Architecture*, June 1961.

McClelland, B. (1974), "Design of Deep Penetration Piles for Ocean Structures," *J. Geotech. Engrg. Div.*, ASCE, 100 (7), 709-747.

McClelland, B. and Focht, J.A., Jr. (1958), "Soil Modulus for Laterally Loaded Piles." *Trans.*, ASCE Vol. 123, 1049-1086.

McClelland, B., Focht, J.A., Jr. and Emrich, W.J. (1969), "Problems in Design and Installation of Offshore Piles," *J. Soil Mech. & Found. Div.*, ASCE, 95 (6), 1491-1514.

Pelletier, J.H., Murff, J.D. and Young, A.C. (1993), "Historical Development and Assessment of the Current API Design Methods for Axially Loaded Piles," *Proc., 25th Annual Offshore Technology Conference*, Houston, Vol. 3, 253-282.

Poulos, H.G. (1971), "Behavior of Laterally Loaded Piles: II-Pile Groups," *J. Soil Mech and Found. Div.*, ASCE 97 (5), 733-751.

Tomlinson, M.J. (1957), "The Adhesion of Piles Driven in Clay Soils," *Proc., 4th Int'l Conf. On Soil Mech. And Found. Engrg.*, London, Vol. 2, 66-71.

Vijayvergiya, V.N. and Focht, J.A., Jr. (1972), "A New Way to Predict the Capacity of Offshore Piles In Clay," *Proc., 4th Annual Offshore Technology Conference*, Vol. 2, 865-874.