Keynote Lecture: "Creating Case Histories the Hard Way"

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KEYNOTE LECTURE

"Creating Case Histories The Hard Way"

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On previous occasions like this, when I have been privileged to make opening remarks at these Rolla Case History Conferences, I have talked about the lessons that we can learn from studying other people's case histories. The attendance at this conference demonstrates that we all anticipate continuing this process, so it would be redundant for me to give a similar talk this year. Instead, I have chosen to tell you about four jobs, fairly early in my career, in some of which I made serious mistakes and from which I learned much myself. Thanks to forgiving clients the mistakes did not end my career, but they caused considerable embarrassment.

You should realize that most of these jobs took place in what we would now call the early days of soil mechanics. Probably only a few hundred people had been exposed to the new subject at the hands of the early masters and were already practicing as individual consultants. We were captivated by the ability to determine the bearing capacity of a footing on clay from the results of unconfined compression tests instead of having to rely on traditional building codes, and by the ability to forecast the settlement of a foundation on clay by means of heretofore unheard of consolidation tests. We basked in this superior knowledge, and we sometimes used it indiscriminately or without considering its implications. We lacked the wisdom of our mentor, Karl Terzaghi, who was always keenly mindful of the shortcomings and limitations of his contributions to the profession.

One of the first rude shocks came from an apparently straightforward slab foundation for a small building near the town of Ennis, Texas. For several years my colleagues and I had been advising the American Telephone and Telegraph Co. concerning the foundations for the towers and repeater stations that the corporation was constructing at about 30-mile intervals across the country to enhance its long distance service and later to permit transmitting television programs across the United States. We had developed an efficient and comfortable routine. A.T. & T. selected the locations along a route to satisfy their clear-sight requirements and took options on the properties. We reviewed the pertinent geological literature and visited each site in the company of several A.T. & T. engineers together with a representative of the boring contractor. We then specified the boring and sampling procedure, tested the samples, and finally recommended the foundation type and allowable loading for each structure. For the site at Ennis we made unconfined compression tests, as we had done for many sites on clay, and judged the clay to be so stiff that the bearing capacity was not of concern. Moreover, the clay was so stiff that consolidation settlement would be negligible. Consequently, we recommended establishing the small repeater station on a nominal concrete slab on a thin subgrade of compacted gravel.

Most of you have already guessed at the outcome. After only a year the slab began to heave differentially and break up a behavior by no means unknown even then in this part of the arid Southwest. Some of the experiences had been described in the literature, but most of the expertise had been confined to local practitioners. We know about swelling clays but had not personally dealt with them before. In following the routine that had proven to be so efficient, we overlooked the vitally important local experience. Fortunately, our client was philosophical about our shortcomings, perhaps concluding that we would learn from our experience. Obviously, many decades ago, the atmosphere was less litigious than today. The experience was sobering, even shocking. It deservedly made a great impression on me and my colleagues at the time..

The second rude shock also involved stiff clay. The Northern Pacific Railway long maintained extensive facilities to handle iron ore on the shore of Lake Superior in Wisconsin. A timber trestle had served for many years to cross a small valley but had become a source of expensive maintenance as well as a fire danger. To improve the conditions the railroad decided to replace the trestle by a fill. Borings indicated that the valley had been cut into a deep deposit of maroon-colored lacustrine clay associated with the glacial history of the area. The stability of the proposed fill was assessed on the basis of the undrained shear strength of the clay (although this terminology had not yet been adopted). The calculations showed that the factor of safety should be more than adequate to support the fill, which consisted of the same lacustrine clay.

Nevertheless, when the fill had not quite reached the elevation of the track, it began slowly to subside, and the ground in front of the toe began to heave. For a time it seemed as if placing the fill in small increments between waiting periods might make completion possible, but this turned out to be a vain hope. The railroad was understandably unhappy and summoned us to the site.

The borrow pits had been excavated, in the same lacustrine clays as the foundation for the fill, by means of large scrapers. Even a cursory glance at the excavated surfaces showed huge polished, slickensided, curved surfaces several meters in extent, quite randomly oriented throughout the deposit. Today, we would immediately conclude that the shearing strength along these surfaces would be at the residual value, but at the time of the project Skempton had not yet drawn attention to the concept of residual strength. Nevertheless, it was obvious that the polished surfaces were weaknesses in the mass of clay that had reduced its strength. Had their presence been suspected, some reasonable allowance would probably been made, but the slickensiding had gone unnoticed. Probably this experience, more than any other, increased my awareness of the implications of the secondary structure of soils and the necessity to look for the evidence of such so-called minor geologic details. Again, it was field behavior, epitomized as a case history, that impressed this necessity on me.

Somewhat more recently, but still some years ago, a system of evaporating ponds for recovering various dissolved chemicals was being constructed at the southern end of the Dead Sea. The ponds were enclosed by dikes of gravel, dumped from barges, in which central cores were to be constructed by the slurry-trench procedure. Difficulties arose because the unique chemistry of the Dead Sea water altered the behavior of the usual colloidal materials, such as bentonite, customarily utilized to stabilize the walls of the trenches in which the core materials are deposited. A controversy arose between the contractor and the design engineers: the designers maintained that the cutoff was unacceptable because the specifications were not followed, whereas the contractor maintained that the core material and the gravel zone it penetrated alongside the prescribed core were of low enough permeability to satisfy the intent of the design. The contractor appointed a board of consultants, of which the late Laurits Bjerrum and I were two members, to help verify his contention. At the time, Dr. Bjerrum was head of the Norwegian Geotechnical Institute, and the Institute had just developed a piezometer consisting of a porous tip at the end of a pipe that could be pushed into the ground and, after a brief period of equilibration, the pore pressure measured by means of a Bourdon gage at the top of the pipe. Provision also was made to inject water into the system at a measured rate and pressure in order to determine the permeability of the soil surrounding the tip. Laurits suggested that a series of such permeability tests in the core of the dikes could establish the permeability of the dikes as built and could settle the issue.

Several hundred such tests were made, and it became apparent that the coefficient of permeability derived from all the cores was a remarkably uniform 10^{-4} cm/sec. Initially, such a low value seemed encouraging, but the consistency of the values seemed to be abnormal. Consequently, NGI instituted an investigation that demonstrated that 10^{-4} cm/sec represented the maximum rate at which water could be forced through the apparatus and its porous stone even if the tip was submerged only in water at the applied pressures. At this particular time in the development of soil mechanics, hydraulic fracturing was just being recognized as a significant phenomenon, and Laurits suddenly realized that every test in the Dead Sea cores had simply fractured the cores, whereupon the inferred permeability was that of the equipment, not the dikes.

Increased understanding of hydraulic fracturing was, of course, an unintended byproduct of the Dead Sea investigation. It was a matter of no little embarrassment to the members of the Board of Consultants that so many tests were made before the mechanism of the phenomenon was realized. But, in this instance at least, a developing case history led directly to an improvement in fundamental knowledge.

Finally, one memorable case history drove home to me the point that some evident signs of distress may have nothing to do with either deficient structural or geotechnical inadequacies. The example was an upscale apartment house on Chicago's near North Side. It was constructed for a local judge as an investment for his retirement, but the earliest tenants complained about highly visible cracks in their plastered and painted walls. The judge understandably suspected that the foundation engineer had done an inadequate job, and I was asked to investigate. The cracks were indeed evident, but the differential settlements were very small. When I inspected some of the apartments, I noticed that most of the walls had been painted - as was then the vogue - in very dark colors: deep green, dark brown for example. The slightest relative movement, exposing the brittle white plaster, led to a highly visible defect. The judge was a little dubious when I explained the situation and suggested that walls be covered with cloth and then painted, but the cosmetic treatment was successful and the engineer was off the hook. This case history may not hinge on soil mechanics, but it represents the kind of knowledge a foundation engineer needs and is unlikely to acquire except through personal experience or that of others as described in case histories.

When soil mechanics was young and I was only somewhat younger, experiences such as those I have described came, as they did to me, often the hard way. Now, fortunately, well documented accounts help us all - young and not so young alike - to avoid many pitfalls. Those recounted in the Proceedings of this Conference should help us to avoid even more.