May 6th, 12:00 AM

Case Histories for Today's Geotechnique

R.B. Peck

Follow this and additional works at: http://scholarsmine.mst.edu/icchge

Recommended Citation
http://scholarsmine.mst.edu/icchge/1icchge/1icchge-theme9/5

This Article - Conference proceedings is brought to you for free and open access by the Geosciences and Geological and Petroleum Engineering at Scholars' Mine. It has been accepted for inclusion in International Conference on Case Histories in Geotechnical Engineering by an authorized administrator of Scholars' Mine. For more information, please contact weaverjr@mst.edu.
INTRODUCTION

Following the Ninth International Conference on Soil Mechanics and Foundation Engineering in Tokyo, the Japanese Organizing Committee published a Case History Volume for which I was asked to write an introduction. The paper that I submitted (Peck 1981) expressed in a rather formal way my views on the function and value of case histories in geotechnical engineering. You would not want me to repeat those comments now. I shall, therefore, briefly recapitulate my main points for the Tokyo Volume and pass on to the somewhat narrower subject of the kinds of case histories that may be most useful to us today.

In the early days of soil mechanics, there were three principal uses for case histories. The first, applicable only if soil conditions were unusually uniform and simple, was to verify or establish fundamental principles. For example, as recently as 40 years ago it was by no means certain that the formulas for bearing capacity of footings on clay were reliable. The classical paper by Skempton (1942), in which he provided the essential facts concerning the failure of a single footing for an industrial building and showed that the failure load agreed with that predicted on the basis of theory and undrained strength, is in this category. Without such investigations, the engineering profession would not have been justified in accepting the principles and procedures of soil mechanics, and undoubtedly would not have done so.

The second category of case histories was those that contrasted theory and reality. Theory, for example, had demonstrated the conditions under which piping by heave would occur near the downstream toe of a dam on pervious cohesionless deposits. The grain size of the material was shown, not only by theory but by laboratory tests, to be irrelevant. Yet, the extensive case histories, such as those collected by E. W. Lane (1935), showed without a doubt that many piping failures were related to the grain size. Moreover, in contrast to the predictions of the theory, the failures usually occurred some time after first filling of the reservoir. Thus, the case histories required re-examination of the basis for the theory, and led to recognition of the importance of another form of piping, failure by subsurface erosion.

Third, case histories demonstrated how reasonable solutions to engineering problems could be achieved by means of the observational method even when subsurface conditions were complex and, to a degree, unpredictable. The adoption of the observational method in geotechnical engineering (Peck 1969) would certainly not have come to pass without many excellent case histories that demonstrated not only successful techniques, but the accomplishments and shortcomings of the method.

The case histories that I wish to emphasize in this presentation fall in still another category. They might be described as examples of the unexpected or, at least, unforeseen.

In the successful application of the observational procedure, it is essential to have a perception of all the things that might go wrong. That is, the engineer needs a reasonable knowledge of subsurface conditions and a realistic conception of possible departures from these conditions. Part of the observational procedure is to make plans in advance to cope with even the most unfavorable of these departures if they should indeed prevail. If the engineer fails to foresee a possible mode of failure, or if perhaps the unexpected condition is not even in the realm of geotechnics, the observational procedure may not be adequate to prevent a failure or a disappointing performance.

Case histories embodying experiences of this sort can hardly be classified in such a way as to provide a checklist to help the engineer realize the possibilities of such surprises. Nevertheless, by recounting a few, perhaps I can demonstrate the kinds of unexpected developments that we might try to anticipate. In the effort to avoid such surprises, we may discover that we must go beyond the limits of our own specialty.

Some of these case histories have been presented before. I repeat them because different lessons appear when they are viewed in a different context.

FROZEN FIRE LINES

The Grant Park North underground garage in Chicago is a two-story parking structure floating in the soft clay underlying Grant Park and Michigan Avenue near Lake Michigan. The structure is a reinforced-concrete flat-slab box
BEDROCK DISSOLUTION

I have recounted elsewhere (Peck 1969) the story of an industrial plant with many buildings on shallow foundations underlain by about 50 feet of soft clay. The buildings showed signs of progressive settlement. When I was asked to make an investigation, I learned that sodium chloride brine had for many years been produced by pumping from wells extending through the clay and underlying bedrock to depths ranging from 800 to 1100 feet. I also learned that it had not been necessary for many years to furnish water to the injection wells into the brine supply because the well casings had corroded near the boundary between the clay and the bedrock, where a thin deposit of water-bearing gravel furnished enough water through the defective casings to dissolve the salt and produce brine.

The magnitude and rate of settlement seemed to agree quite satisfactorily with what would be expected if the clay deposit were underdrained, as indeed it must have been by the leaky injection wells. Since the settlement calculated on this basis seemed to agree reasonably well with the results of the observations, I proposed remedial measures appropriate to this cause.

When Terzaghi read my report, as he did for most of my early jobs, his immediate reaction was that I had overlooked the main cause; subsidence of the bedrock overlying the zones from which the salt had been extracted. Inasmuch as the rock above the salt formations was competent limestone or sandstone, I was initially incredulous. Terzaghi was aware, however, of similar problems in Europe in which not only subsidence but sinkholes had developed. After he convinced me, I had the unpleasant task of explaining my conversion to my client, who was equally incredulous. I was demoted to being a member of a three-man consulting board, of which Terzaghi was the chairman, during the ensuing investigation.

I fell into a trap on this job because I was looking far too narrowly at the evidence. Because I thought I knew the answer, I did not even consider the possibility of other causes. Terzaghi had the advantage of a precedent about which I did not know. However, I should have examined all the evidence before coming to a conclusion.

CAVERNOUS LIMESTONE

The remedial work carried out by the Corps of Engineers between 1975 and 1979 to rehabilitate Wolf Creek Dam in Kentucky is well known, through its extensive coverage in both the engineering literature (Fetzer 1979) and the news media. The dam, which retains Lake Cumberland, one of the largest bodies of water in the eastern United States, was completed in 1951. It included a rolled-fill embankment section founded on alluvium which rested, in turn, on a highly solutioned limestone. After 15 years of service in which no signs of distress were noticed, sinkholes, subsidences, and muddy outflows appeared with increasing frequency and severity. After an extensive investigation it was decided to construct a continuous diaphragm wall, through the embankment and alluvium, deep
enough into the limestone to cut off virtually all zones in which solution cavities existed. The work, unprecedented in its depth and difficulty, was accomplished successfully without draining the lake.

Were the dam to be designed today, both engineering geologists and geotechnical engineers would undoubtedly assess significance of the karstic foundation and, if evaluation of the conditions warranted construction of the project, would take appropriate defensive measures to minimize the adverse effects of the solution channels. There would, under those circumstances, be no case history to fit into the theme of this paper. However, the dam was designed in the late 1930’s and constructed, with an intermission during World War II, between 1941 and 1951. The Corps of Engineers, among the pioneers in application of soil mechanics to earth-dam design and construction, applied the new discipline extensively in designing Wolf Creek Dam. It was not the state of the art of soil mechanics that failed them; it was the emphasis placed on the part of the design into which soil mechanics fit neatly, and the lack of corresponding attention to the implications of engineering geology, which had not yet emerged as a discipline. Thus, the detrimental developments were, at the time, totally unexpected by the designers.

During the design phase, Corps geologists had developed the stratigraphy and called attention to the solution cavities. A consulting geologist, familiar with the area, had prepared an excellent report in which he fully described the nature and occurrence of the cavities, but in which he did not presume to discuss the engineering aspects. The designers, quite reasonably, decided to carry a cutoff wall through the upper, most solutioned, part of the foundation rock and to supplement the cutoff with a grout curtain.

The design of the embankment included stability analyses for conditions during construction, steady seepage, and drawdown. In recognition that the alluvium might be a source of weakness, vertical drains were installed through the alluvium to discharge into an overlying drainage blanket that would also drain the downstream shell of the embankment. Calculations by the theory of consolidation were made to establish the spacing of the wells such that the consolidation would keep pace with construction. All in all, the design, from the surface of the bedrock upward, made use of the best knowledge of the 1930’s. Implicit in the design was the assumption that the alluvium-bedrock contact was a stable boundary.

When the area of the cutoff was exposed, it was discovered that several deep solution channels existed, one of which could be utilized as the trench in which the cutoff could be constructed. The sides of the trench were locally cavernous, but the bottom appeared to be fairly intact. It was decided to establish the grout curtain from the bottom of the channel and to place above it a compacted impervious cutoff at least 10 feet wide. Large side caverns were plugged with concrete; otherwise no special compaction of the fill between the sidewalls and cutoff was required. This was, of course, one of the seats of the future trouble.

World War II caused cancellation of the contract when the foundation treatment was complete, and from August 1943 to September 1946 no work was done. By 1946, the value of engineering geology was more fully appreciated and there was recognition that the foundation treatment might have been more extensive had the project been started in 1946 instead of 1941. Because the foundation was completed, however, the new contract called for project testing with the embankment on the original foundation. Only after 15 years did the defects begin to manifest themselves.

In the enthusiasm of the early successes of soil mechanics, the designers and their Board of Consultants devoted their attention largely to what seemed to be the state of the art - the safety of the embankment and the alluvial foundation. These aspects were important. But so were the implications of the solutioned limestone. They may have received less than their deserved attention because the designers and their consultants had new analytical tools with which to solve problems of dam design, were trained more in the use of these tools than by experience in the broad field of dam design, and quite naturally solved the problems that they perceived were the important ones.

Could there be a lesson here for us today? Could our facility with finite-element methods and our interest in constitutive relations absorb so much of our effort that we might quite unintentionally pass over a major source of weakness simply because our backgrounds are too specialized? I need not tell you what I think the answer is.

ORE STORAGE YARD

The foregoing three case histories illustrate events that were not only unexpected, but somewhat beyond the scope of soil mechanics. However, even within soil mechanics, through neglect, oversight, or overconfidence, a potential mode of failure may be overlooked. Such is the case in the following history.

In the years immediately preceding and following World War II, I investigated the stability of a large number of storage yards for iron ore. Most of these were located along the Great Lakes where the ore necessary to keep blast furnaces running through the winter was piled between retaining walls supporting large gantry cranes. The subsoil in most instances included fairly thin deposits of soft to medium clays. Although it was usually possible to make preliminary estimates of the capacity of an ore yard by standard stability analyses, I also made use of a technique for determining the capacity independently by plotting the movements of the retaining walls as a function of the load per unit of area on various areas of the ground surface adjacent to the walls. This technique had proved useful on several walls by the time I was asked to investigate the behavior of a relatively new retaining structure at Warren, Ohio.
The subsoil at Warren, which is not located on a waterway, was somewhat stiffer than that at the ore yards closest to a lake; and in the upper part included a layer of silt. Yet the distortions of the gantry tracks were exceptionally large. I went through what had become my customary routine and concluded that the proposal of the steel company to provide the tracks with a foundation that included vertical and batter piles was reasonable.

Terzaghi and I had worked together on other ore yards for the same company, and I was pleased when the chief engineer suggested that Terzaghi should review my conclusions. When he did so, he immediately pointed out that the silt layer was undoubtedly transmitting large hydrostatic pressure beyond the toes of the loaded ore pile and the uplift exerted on the overlying soil was probably sufficient to eliminate most of the lateral resistance. Had the batter pile solution been adopted without eliminating the pressure in the silt layer, it is unlikely that the movements would have been appreciably reduced. Terzaghi suggested that the silt layer be drained by a system of permanent well points.

As soon as Terzaghi pointed out the significance of the silt layer, I realized that I had overlooked the one most vital point. From then on, little input was needed from Terzaghi on the project, but his quick recognition of my oversight had made the difference between success and failure. Indeed, Terzaghi never quite forgave me for my oversight.

After the successful completion of the job, I prepared a paper under joint authorship with Terzaghi (Terzaghi and Pock 1957) which was to be presented at a meeting of the American Society of Civil Engineers. Terzaghi was ambiguous as to whether he would attend, so I was prepared to present the paper myself. He did come, however, and while we sat together, waiting for the paper to be introduced, he still did not tell me whether he, I, or both of us would make the presentation. When we were introduced, he stood up and said, "I will present the paper." His introductory remarks pointed out that I had prepared a very sober account of the project, that it could be read in due course in the Proceedings of the ASCE, and that he would talk about something else. He did so, in entertaining fashion, somewhat to my disappointment. I did not realize for some time thereafter that he was, in his own way, punishing me for an inexcusable oversight.

CONTROLLED UNDERSEEPAGE

The last case I shall describe is not yet history. It deals with some of the dikes on the LG-3 Reservoir of the James Bay project. The reservoir is formed by the main LG-3 dam across the Rivière LaGrande and 67 dikes with an aggregate length of 14 miles to close gaps in the reservoir rim. The reservoir itself covers an area of about 950 square miles (Murphy and Le- vay 1981). Following the procedure that had been established for the LG-2 reservoir, the general position of the reservoir rim was established by tracing the contour of maximum reservoir level and locating saddles at positions where they appeared to have minimum volume. Airphoto and field surveys then established the axes of the dikes to take advantage of local topographic features. Refraction seismic surveys were used to define the bedrock surface and numerous borings were made to determine subsurface conditions. Because of the remote and arctic conditions, borings were usually spaced at several hundred feet, but where pervious conditions were encountered, estimates of permeability from grain size determinations were often supplemented by drillhole permeability tests.

It was known that several of the saddles were underlain by deep bedrock valleys containing glacial and alluvial deposits. Several similar channels with a maximum depth of about 250 feet had been encountered beneath Dike D-20 at the LG-2 reservoir. The control measures at different locations consisted of slurry trench cutoffs to glacial till, of excavated and backfilled cutoffs to rock, and of upstream blankets. When the reservoir was raised, the performance of the upstream blankets was excellent. Where defects existed in the slurry trench section, a series of relief wells was installed. The maximum flow from any of the wells was on the order of a few hundred gallons per minute, and the total flow beneath a one-mile length was about 3,000 gallons per minute.

At LG-3 several of the buried valleys extended to depths of nearly 300 feet and there were indications of pervious material in various parts. It was considered reasonable, and justified on the basis of the performance of D-20, to provide upstream blankets with a gradient of about 1:15 beneath most of these dikes. Several of the dikes were essentially freeboard structures with very little head against them. Nevertheless, when the reservoir was being filled for the first time, large concentrations of seepage were noted at a few places downstream. In one instance, before the water level actually reached the elevation of the toe of the upstream blanket which was located at a distance of several hundred feet from the axis of the dike, flows on the order of 2000 gallons per minute appeared downstream in a landslide with a 70-foot scarp occurred in a steep slope about 3000 feet downstream from the dike.

It was apparent that the permeability of the lower portion of some of the buried valleys was far greater than had been anticipated. Accordingly, the reservoir was lowered and a program of more detailed exploration undertaken. The program resulted in the construction of additional cutoff walls at a cost of several million dollars. Fortunately, the decrease in the market for power and the capabilities of the rest of the system resulted in no loss of revenue on account of lowering the reservoir, and indeed the cost of the cutoff walls was favorable in comparison to the probable cost had they been built earlier in the construction period when competition was less keen. In spite of these fortunate conditions, the remarkably high permeability of some of the channels, which came as a surprise, could have been responsible for large economic losses.

The observational procedure and careful surveillance identified the problems quickly and allowed the necessary remedial measures to be taken.
Nevertheless, the permeability and continuity of the lower portions of the valley fills were in some instances far greater than anyone had anticipated. During construction of one cutoff wall, clean boulders and cobbles, with sizes ranging from 6 to 24 inches, were recovered from a limited zone.

It can be said, with considerable justification, that the observational procedure was successful in this instance. Nevertheless, it is appropriate to question whether techniques existed by which the conditions could have been forecast more accurately at a reasonable expenditure of time and money.

The answer to this question seems to be negative, although the presence of an unusually large number of rounded boulders on the surface in the vicinity of one of the saddles, when considered in its geomorphological context, might have been a tipoff. A review of satellite and airphoto imagery suggests the presence of pervious deposits in former streams, but the evidence can hardly be considered indicative of the extremely high and continuous permeability encountered in the lower parts of a few deep channels. Closer spacing of drill holes and more extensive in-situ permeability testing might have disclosed the conditions, but the probability of missing the limited critical zones would have remained, and the time and cost for the investigations would probably have been unreasonable.

Hence, this case represents an example of a totally unexpected magnitude of permeability, although the design was premised on permeabilities that under most circumstances would themselves have been considered high. The observational procedure proved to be capable of coping with the actual conditions and of assuring safety, but the results would have been much less tolerable had the opportunity not existed for lowering the reservoir without appreciable economic loss.

CONCLUSION

Most of the foregoing examples cannot be considered precedents in the sense that similar conditions are often likely to be encountered elsewhere. On the contrary, they serve primarily to indicate that the engineer should make serious efforts to foresee potential unfavorable developments, not only those within his own field of expertise, but also those that lie outside his or possibly any other discipline. The broader the experience of the individual engineer, or the greater his ability to work with people of other backgrounds, the greater the likelihood that he will not be surprised by a totally unexpected development.

REFERENCES


