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2. A summary of approximately 50 words must accompany the paper, and a set of conclusions must end it.

3. The manuscript (an original ribbon copy and two duplicate copies) should be double-spaced on one side of 8½-inch by 11-inch paper. Three copies of all illustrations, tables, etc., must be included.

4. The author's full name, Society membership grade, and footnote reference stating present employment must appear on the first page of the paper. (Authors need not be Society members).

5. Mathematics are recomposed from the copy that is submitted. Because of this, it is necessary that letters be drawn carefully, and that special symbols be properly identified. The letter symbols used should be defined where they first appear, in the illustrations or in the text, and arranged alphabetically in an Appendix.

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8. The desirable average length of a paper is about 10,000 word-equivalents and the absolute maximum is 15,000 word-equivalents. As an approximation, each full manuscript page of text, table, or illustration is the equivalent of 300 words.

9. Technical papers must be written in the third person.

10. A list of key words and an informative abstract should be provided for information retrieval purposes. (Information on preparation available on request.).

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Proceedings of the American Society of Civil Engineers

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(over)

Note.—Part 2 of this Journal is the 1965-33 Newsletter of the Soil Mechanics Division.

The three preceding issues of this Journal are dated January, 1965, March, 1965, and May, 1965.

DISCUSSION

INFORMATION RETRIEVAL

The key words, abstract, and reference "cards" for each article in this Journal represent part of the ASCE participation in the EJC information retrieval plan. The retrieval data are placed herein so that each can be cut out, placed on a 3 x 5 card and given an accession number for the user's file. The accession number is then entered on key word cards so that the user can subsequently match key words to choose the articles he wishes. Details of this program were given in an August 1962 article in CIVIL ENGINEERING, reprints of which are available on request to ASCE headquarters.

KEY WORDS: construction; consultants; earthwork; foundations; professional practice; risk; safety; soil mechanics

ABSTRACT: The meaning of the term "calculated risk" is first explored and the terms "unknown risk" and "human risk" are introduced. Several case histories are then reviewed for the purpose of demonstrating the importance of risks in earthwork and foundation engineering. The final section deals with the question of how to cope with risks, with emphasis on the use and abuse of Boards of Consultants for projects involving great hazards to life and property.

REFERENCE: Casagrande, Arthur, "Role of the 'Calculated Risk' in Earthwork and Foundation Engineering," Journal of the Soil Mechanics and Foundations Division, ASCE, Vol. 91, No. SM4, Proc. Paper 4390, July, 1965, pp. 1-40.

KEY WORDS: clay (material); permeability; pore pressure; seepage; shear strength; soil compaction; soil mechanics; soil structure; testing; thixotropy

ABSTRACT: The effects of molding water content, density, degree of saturation, method of compaction, and thixotropic hardening on the permeability of compacted silty clay have been determined. The formation of a dispersed structure in samples compacted wet of optimum may result in a coefficient of permeability two or three orders of magnitude less than for the same soil compacted dry of optimum. The actual decrease in permeability wet of optimum appears to correlate well with the degree of shear strain applied to the soil during compaction. In line with this, it was found that for samples compacted wet of optimum kneading compaction gave significantly lower values of permeability than did static compaction. Thixotropic hardening was accompanied by an increase in permeability, a result compatible with the concept that thixotropic hardening involves a change to a more flocculent structure. As much as a five-fold increase in permeability may accompany an increase in saturation from the as-compacted state to the fully saturated condition. Because of the great variability in permeability with compaction conditions, selection of an appropriate value for use in problems involving seepage or pore pressure dissipation will be difficult.

REFERENCE: Mitchell, James K., Hooper, Don R., and Campenella, Richard G., "Permeability of Compacted Clay," Journal of the Soil Mechanics and Foundations Division, ASCE, Vol. 91, No. SM4, Proc. Paper 4392, July, 1965, pp. 41-65.

KEY WORDS: borings; density; evaluation; penetration; samplers; soil mechanics; soils (types); testing

ABSTRACT: In foundation test borings, the recovery of soil samples with a 2-in. OD split sample spoon driven with a 140-lb weight falling 30 in. has been in use for more than 30 yr. Recording the number of blows required to drive the spoon 12 in. has been called the "Standard Penetration Test." It provides an approximation of soil densities in situ. It adds little to the cost of boring operations but adds considerably to the evaluation of the results when it is properly performed and it's are limitations recognized. This paper traces the history of the test, the modifications that have been introduced, the variables inherent in the test, and sets forth the factors that affect the results. Applications in granular and cohesive soils are examined.

REFERENCE: Fletcher, Gordon F. A., "Standard Penetration Test: Its Uses and Abuses," Journal of the Soil Mechanics and Foundations Division, ASCE, Vol. 91, No. SM4, Proc. Paper 4395, July, 1965, pp. 67-75.

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SOIL MECHANICS AND FOUNDATIONS DIVISION
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THE TERZAGHI LECTURE[a]

ROLE OF THE "CALCULATED RISK" IN EARTHWORK AND FOUNDATION ENGINEERING

By Arthur Casagrande,[1] Hon. M. ASCE

FOREWORD

I am deeply grateful to my colleagues who elected me to present the 1964 Terzaghi Lecture. There is, indeed, no better way for me to repay all that I owe to Karl Terzaghi than by lecturing in his honor, by keeping alive in my lectures his memory, and by letting my younger colleagues and my students partake in what he has taught me.

On October 21, 1963 I spoke with Karl Terzaghi for the last time. I called on him after returning from several weeks of travel which included attendance at the ASCE convention in San Francisco. I found him still at work, but very weak and suffering severe physical pains. He was anxious to hear about the San Francisco meeting and the first Terzaghi lecture delivered by Ralph Peck, which was so well received by the large audience. With sincere interest he read every line on the plaque of my Terzaghi Award, which I had brought along for him to see. Then he asked me to report about the construction progress of the South Saskatchewan River dam on which he had been serving as consultant. Finally he gave me, for my review, another part of the last paper on which he was still working. Three days later I returned

Note.—Discussion open until December 1, 1965. To extend the closing date one month, a written request must be filed with the Executive Secretary, ASCE. This paper is part of the copyrighted Journal of the Soil Mechanics and Foundations Division, Proceedings of the American Society of Civil Engineers, Vol. 91, No. SM4, July, 1965.

a The Karl Terzaghi Award and Lecture were established by the Soil Mechanics and Foundations Division of the Society by gifts from the many friends and admirers of Karl Terzaghi, Hon. M. ASCE. A distinguished engineer is invited periodically to deliver a "Terzaghi Lecture" at an appropriate meeting of the Society. This, the Second Terzaghi Lecture, was presented during the annual Meeting of ASCE on October 21, 1964.

1 Prof., Soil Mechanics and Foundation Engrg., Harvard Univ., Cambridge, Mass.

with my completed review. Usually I saw him at work silhouetted against the lake behind his home as soon as I looked through the glass panels of the front door; but this time his chair was empty. I gave the manuscript to Mrs. Terzaghi. The next morning at eight, October 25, 1963, she telephoned and said that Karl had passed away an hour earlier.

ON THE MEANING OF "CALCULATED RISK" AND OTHER TYPES OF RISK

Terzaghi's great accomplishment was to replace in earthwork and foundation engineering the large conglomeration of great "unknown risks" of the past in part by rational analyses which are based on the principles of soil mechanics that he developed, and in part by "calculated risks" that we can estimate with the help of soil mechanics, and judgment.

The purpose of this presentation is to discuss all types of risks which are inherent in earthwork and foundation engineering, the extent to which we are justified or forced to assume such risks, and how to cope with them. The title of my paper does not fully cover my subject because I intend to speak not only about calculated risks, but also about risks due to unknown factors, which I shall term "unknown risks," and about uncertainties introduced by human failings which may be termed "human risks."

For an orderly discussion I should start with a definition of the expression "calculated risk." You have all heard it many times; and probably most of you have used it. Its meaning has puzzled me for many years because I observed that it was used particularly when it was not possible to calculate anything, when instead it was necessary to rely on experience and judgment. Eventually, I found in a dictionary that as an adjective, "calculated" means "estimated" rather than "computed"; and that a calculated risk is an estimated risk. Therefore, the often-heard joking remark that a calculated risk is the type of risk that nobody knows how to calculate, is really a play of words on the ambiguity of the adjective "calculated." (I mention in passing that it is difficult to translate correctly "calculated risk" into other languages. I have seen it translated in professional literature into just the opposite of its real meaning.)

Webster's 1961 International Dictionary contains the following two definitions for calculated risk:

1. "A hazard or chance of failure whose degree of probability has been reckoned or estimated before some undertaking is entered upon.
2. "An undertaking or the actual or possible product of an undertaking whose chance of failure has been previously estimated."

Use of this term in other activities is illustrated by the following examples: The May 7, 1963, issue of Engineering News-Record had an editorial advocating that helicopters be permitted to land on the rooftop of the Pan Am Building in New York. It started with this paragraph: "There's A CALCULATED RISK in crossing a street in the busy centers of our cities. There's a calculated risk in living in a flight path of our major airports. There's a calculated risk in flying helicopters to rooftops in cities."

In a 1963 issue of TIME magazine, in the section on "WORLD BUSINESS," there was an article entitled "Calculated Risks" in which the eagerness with which west-European businessmen are extending long-term credits to the Soviet Union was discussed; how they are taking calculated risks in order to gain a toehold in the potentially enormous market in Russia and its European satellites.

Although the term calculated risk is used extensively by engineers, I have not found a published definition specifically pertaining to its use in engineering. Therefore, I have asked a number of colleagues whether or not they have used it and with what meaning. A minority of the replies stated that it is a misleading term, that it can be easily misunderstood and should be avoided. The majority replied that they have been using it, and their definitions agreed in substance and may be summarized as follows: The taking of a carefully considered risk which is based largely on an analysis of factors that require experience and judgment for their evaluation.

This survey and my own observations show that the term "calculated risk" is indeed widely used in engineering, if not somewhat loosely; and that usage and most suggested definitions have in common a meaning that includes the following two distinct steps:

(a) The use of imperfect knowledge, guided by judgment and experience, to estimate the probable ranges for all pertinent quantities that enter into the solution of a problem.

(b) The decision on an appropriate margin of safety, or degree of risk, taking into consideration economic factors and the magnitude of losses that would result from failure.

I intend to use the term "calculated risk" in this paper with this combined meaning. Both steps are inherent in a calculated risk, as illustrated by the following fictitious example.

An embankment is to be built on a clay foundation. From his investigations the designer concludes that the in situ shear strength of this clay may range between 1.0 and 2.0 tons per sq ft. The upper limit is derived from conventional laboratory strength tests on undisturbed samples and on in situ vane tests. The lower limit is based on the designer's experience and judgement of the possible combined effects of (1) lateral transmission of pore pressures due to the stratified character of the clay stratum, which would reduce the average shearing resistance along a potential sliding surface; and (2) the reduction in long-term strength when this clay is subjected to shear deformation at constant water content.

After establishing the controlling range for shearing resistance, the designer selects an allowable (design) value to be used in his stability analyses. If this project is an important dam whose failure would cause catastrophic losses, he might decide to use the very conservative design value of 0.6 tons per sq ft. Thus, he would protect himself against his wide range of uncertainty by an ample margin of safety, i.e. with a factor of safety ranging between the limits of about 1.6 and 3.3. To effect greater economy without compromising safety, he might elect to install numerous piezometers in the clay stratum and to adopt an initial design with a much smaller safety margin. Thus he would utilize the project as a full-scale test, and on the basis of the piezometer observations he would modify the design during construction if required.

If the project were a long highway embankment for which a partial failure would cause only a modest economic loss, the designer would try to achieve greater economy by allowing a greater risk of failure. Therefore, he might use a design value of 1.2 tons per sq ft. Using piezometer observations as a guide, he would add stabilizing berms only if needed. Thus, his initial design would allow a certain probability of failure which the designer hopes to control within tolerable limits with the help of piezometer installations. He may choose to go one step further and deliberately produce failures by constructing full-scale test sections. Thereby he would succeed in reducing to a narrow range the uncertainty concerning the controlling shearing resistance of the clay stratum.

The alternatives in the preceding example not only illustrate the two steps that enter in the evaluation of a calculated risk, but they also demonstrate that the meaning of a statement such as "the designer had to cope with a large calculated risk" is not clear. Does this statement imply (a) a wide range of uncertainty about the strength, or (b) a great risk of failure? With no other information I would assume that it implies a combination of both.

EXAMPLES

By means of several examples I shall illustrate the nature and importance of risks in earthwork and foundation engineering. So many interesting case records in my files clamored for attention that it was difficult for me to settle on only seven examples. I have selected the majority from my own practice, in part because I know them so well, and in part because it is easier for me to be frank about risks for which I have had to accept a major responsibility than for me to discuss risks taken by others.

Unfortunately it is common practice in publications describing projects in applied soil mechanics to present a rationalized picture that has little resemblance to the truth about the actual approach followed in arriving at major design decisions. If judgment based on empirical knowledge played an important role in such decisions, why not admit it? If unknown risks or calculated risks were involved because of the large gaps in our understanding of the mechanics of soils, why not admit it? Authors who, a posteriori, try to rationalize their decisions or make theories fit the facts, merely reveal their limited grasp of the realities of applied soil mechanics. With distorted presentation of important case records they hinder rather than promote progress; and they mislead their younger and less experienced colleagues.

(1) *Piers for Oakland Bay Bridge*.—For my first example, I quote from memory what the late Daniel E. Moran related in a lecture to my students in the spring of 1935:

> "In the course of early investigations for the San Francisco-Oakland Bay bridge, my partner Carlton Proctor was in San Francisco and sent me a cable to Bermuda where I was vacationing. The cable read: 'Can one sink open caissons to a depth of 350 ft?' Open caissons had never before been sunk to such a great depth, and no one knew whether it could be done. There was only one answer I could give. I cabled back one word—YES. I still don't know whether it can be done, because it developed that we could stop the caissons at considerably shallower depths."

His one-word reply was not a wild guess, but the reply of the man who knew most about that subject and who was endowed with brilliant intuition. In his judgment it was feasible, and he had confidence in his own ability to conquer any difficulties. I consider this an example of calculated risk par excellence.

(2) *Panama Canal Slopes in Cucaracha Shale*.—My second example is the great unknown risk which the builders of the Panama Canal faced where the Canal cuts through the bentonitic Cucaracha shale formation. By continual removal of material from these cut slopes over a period of several decades, in an effort to stay ahead of major slides, the slopes have become extremely flat. Stability analyses which were made in connection with the 1946-47 Sea-level Canal Studies[2] showed that the effective shearing resistance of the shale had decreased from an average value of approximately 2 to 3 tons per sq ft in 1912 to as little as 0.4 to 0.5 tons per sq ft in 1947. This final value is approximately equivalent to a friction angle of 10° with zero cohesion, which was also obtained from laboratory direct shear tests on artificial slickensides made by polishing blocks of the shale.[3]

At the time of these investigations the conventional belief was that the shale would have retained a greater long-term strength if the slopes had been cut at the start with an appropriate factor of safety. However, I was inclined to the opinion[4] that this procedure would not have prevented a gradual reduction of the strength to the same very small residual value that we observe today; and that even if the designers had known that eventually the present extremely flat slopes would develop, the empirical procedure of flattening the slopes in stages was basically a satisfactory and economical approach.

(3) *Fort Peck Dam*.—Unknown risks also caused the failure of a portion of the Fort Peck dam in 1938. Using the hydraulic-fill method, the dam was constructed of river sands and finer-grained alluvial soils on a foundation of alluvial sands, gravels and clays with a total thickness up to 130 ft. Beneath this river alluvium is the Bearpaw clay-shale which contains layers of bentonite.

The failure occurred on September 22, 1938, when the dam was almost completed and the reservoir was partially filled. It affected a 1700 ft long section of the upstream shell next to the right (east) abutment where the underlying alluvium consists chiefly of sands. Comparison of the aerial views in Figs. 1 and 2, taken shortly after the slide, with the plan before and after the slide in Fig. 3 and the typical cross-section in Fig. 4, shows the significant and unusual features of the topography of the slide mass. The movement began by a bulging out of the western portion of the affected upstream slope with simultaneous subsidence of the core pool. Then a transverse crack developed at the western end which widened rapidly into a deep gap while the

[2] Binger, W. V., and Thompson, T. F., "Excavation Slopes," Panama Canal Symposium, Proceedings, ASCE, Vol. 74, No. 4, April, 1948, pp. 570-590.

[3] Binger, W. V., "Analytical Studies of Panama Canal Slides," Proceedings, 2nd Internatl. Conf. on Soil Mechanics and Foundation Engrg., Rotterdam, June, 1948, Vol. II, pp. 54-60.

[4] Casagrande, A., discussion of "Excavation Slopes," Panama Canal Symposium, by W. V. Binger and T. F. Thompson, Proceedings, ASCE, Vol. 74, No. 4, April, 1948, pp. 870-874.

FIG. 1.—OBLIQUE AERIAL VIEW OF SLIDE IN FORT PECK DAM

FIG. 2.—AERIAL VIEW OF SLIDE IN FORT PECK DAM

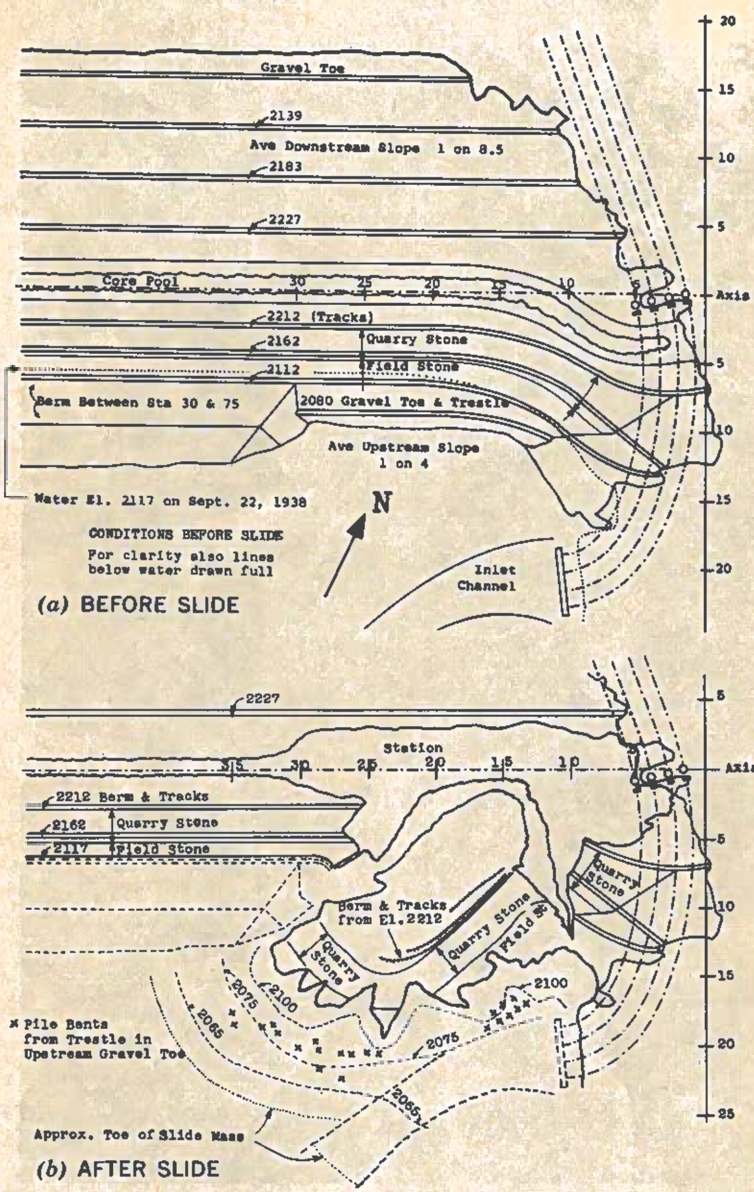

FIG. 3.—PLAN OF FORT PECK DAM SLIDE AREA

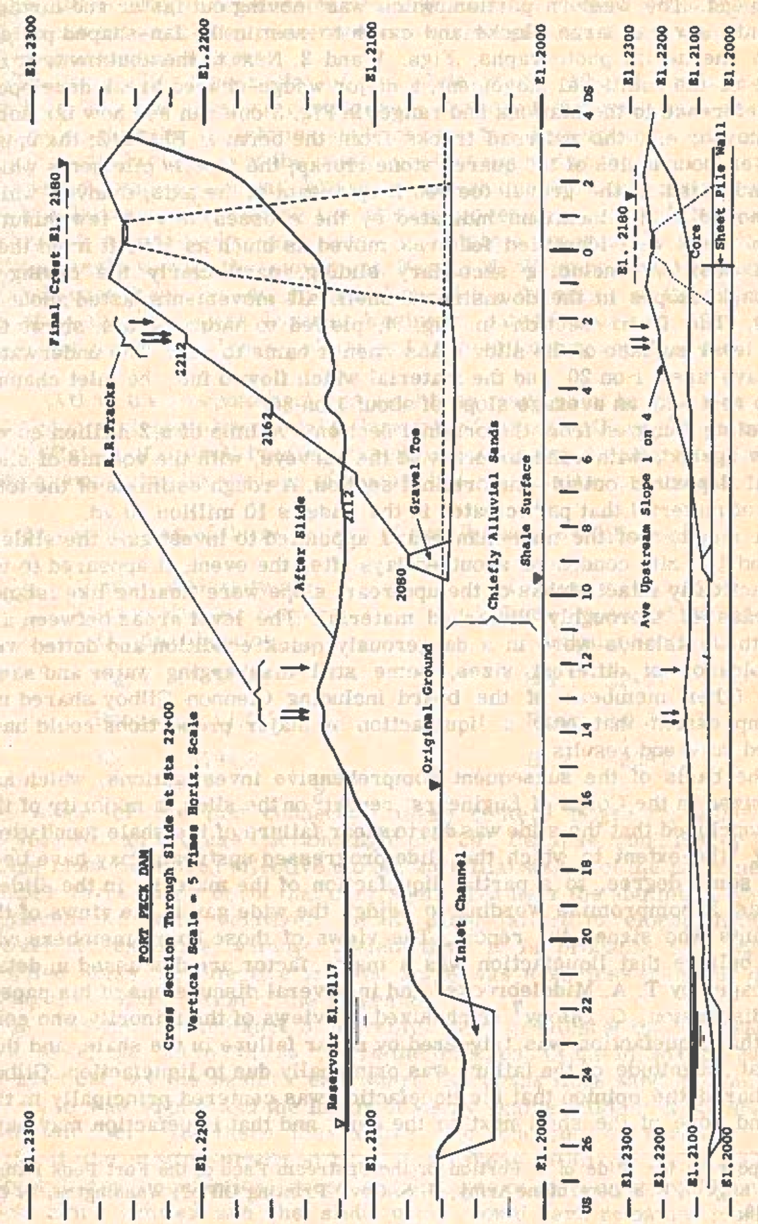

FIG. 4.—CROSS-SECTION OF SLIDE AT STATION 22+00

moving portion of the slope started to swing in a rotational movement as if hinged at the abutment. Through this gap the core pool drained with enormous speed. The western portion which was moving out faster and further, broke into several large blocks and came to rest in the fan-shaped pattern seen in the aerial photographs, Figs. 1 and 2. Next to the abutment, at the fulcrum of the rotational movement, a major wedge-shaped break developed.

By reference to the stations and ranges in Fig. 3 one can see how far some parts moved; e.g. the railroad tracks from the berm at El. 2212; the upper and lower boundaries of the quarry stone riprap; the trestle pile bents which were embedded in the gravel toe 900 ft upstream of the axis, many of which were moved to the locations indicated by the crosses. In very few minutes some of these well identified features moved as much as 1500 ft from their original position. Including secondary sliding, particularly the caving of steep back slopes in the downstream shell, all movements lasted about 10 minutes. The lower section in Fig. 4, plotted to natural scale, shows the almost level surface of the slide mass when it came to rest. The underwater slopes averaged 1 on 20, and the material which flowed into the inlet channel came to rest with an average slope of about 1 on 30.

The slide removed from the original section a volume of 5.2 million cu yd, and this agreed, within the accuracy of the surveys, with the volume of slide material deposited outside the original section. A rough estimate of the total volume of material that participated in the slide is 10 million cu yd.

As a member of the nine-man board appointed to investigate the slide, I inspected the site conditions about ten days after the event. It appeared to me that practically intact blocks of the upstream slope were floating like islands in a mass of thoroughly disturbed material. The level areas between and inside these islands were in a dangerously quick condition and dotted with sand volcanos of different sizes, some still discharging water and sand. Several other members of the board including Glennon Gilboy shared my first impression that only a liquefaction of major proportions could have produced such end results.

On the basis of the subsequent comprehensive investigations, which are summarized in the Corps of Engineers' report[5] on the slide, a majority of the Board concluded that the slide was due to shear failure of the shale foundation, and that "the extent to which the slide progressed upstream may have been due, in some degree, to a partial liquefaction of the material in the slide." This was a compromise wording to bridge the wide gap in the views of the consultants who signed the report. The views of those board members who did not believe that liquefaction was a major factor are discussed in detail in the paper by T. A. Middlebrooks[6] and in several discussions of his paper. In his discussion, G. Gilboy[7] emphasized the views of the minority who concluded that liquefaction was triggered by shear failure in the shale, and that the great magnitude of the failure was principally due to liquefaction. Gilboy and I shared the opinion that the liquefaction was centered principally in the fine sand zone of the shell next to the core, and that liquefaction may have

5 "Report on the Slide of a Portion of the Upstream Face of the Fort Peck Dam," Corps of Engrs., U. S. Dept. of the Army, U. S. Govt. Printing Office, Washington, D. C., July, 1939.

6 Middlebrooks, T. A., "Fort Peck Slide," Transactions, Vol. 107, 1942, pp. 723-764.

7 Ibid., pp. 725-755.

spread into the underlying heavily loaded foundation sands. Except for my verbal statements before the board meetings in 1938/39, and the written comments in Gilboy's discussion, the arguments in support of the liquefaction hypothesis have not been presented to the profession. As I see them, they may be summarized as follows:

(1) From the topography of the slide mass after the failure one can conclude that the shearing resistance in the foundation must have dropped to a very small value (a friction angle of very few degrees and zero cohesion), i.e., to a small fraction of the resistance that existed at the start of the slide. In contrast, experience with slides in clay-shales show that their resistance drops relatively little during the sliding movement, and that the movement occurs at a relatively slow rate. To obtain a quantitative estimate of the friction angle during the movement, Harold M. Westergaard was engaged as special consultant for an investigation of the dynamics of the slide. In his report to the Fort Peck District Engineer, dated January 28, 1939, he presented theoretical analyses and arrived at the following conclusions:

> "All of the computations that have been made point to the same conclusion applying to the partial failure of the Fort Peck Dam: The average coefficient of overall friction was approximately equal to the slope of the line connecting the centers of gravity before and after the motion."

The coordinates of these centers of gravity were as follows:

Center of Gravity of Volume Missing from Original Section		Center of Gravity of Volume Deposited Outside Original Section	
Sta.	17+34.4	Sta.	22+27.1
Range	1+11.5 U	Range	14+34.5 U
El.	2194.9	El.	2087.7

The slope of the line connecting these centers of gravity is 0.076, i.e., equivalent to an average friction angle of 4°20'. Because much larger friction angles must have been effective during the initial stages of the movement, and because there is no doubt that over some area near the abutment, where the displacements were relatively small, shear failure developed through the foundation materials without liquefaction, it is probable that within the liquefied mass the effective friction angle was even less than four degrees during the major portion of the movement.

(2) The rate and distance of movement were greatest at the west end if the slide area, Sta. 20 to Sta. 25, where the thickness of the foundation sand was the greatest. At the eastern end where there was little sand, the surface of the shale was higher and the factor of safety against sliding in the shale was much smaller; yet there the failure was induced as a secondary effect.

(3) If the strains produced by a local shear failure in the shale had not triggered any liquefaction, speed and distance of movement would have been much more limited and the slide mass would have remained much more coherent. Furthermore, arching within the overlying sands might have relieved the stresses in the weakened zone and arrested the movement.

(4) To start liquefaction of alluvial sand, the mass must be subjected to considerable strains. For a number of hours prior to the slide, some movements in the surface of the fill were detected, indicating that shear failure in the underlying shale, and therefore strains in the sand mass were already in progress. Then almost suddenly the rapid movements started.

(5) The slide mass in the outer reaches was stretched horizontally in the direction of movement as well as laterally. This is evidence that the movement was due to an underlying liquefied layer which was getting thinner as the movement progressed.

(6) The fact that many of the undisturbed samples obtained after the slide showed shear planes was interpreted as the strongest evidence against the hypothesis that liquefaction had occurred. However, it is likely that the liquefied layer had flowed out almost completely from under the large blocks of the upstream shell that had been moved bodily on that layer; probably most of the liquified material ended up in the submerged outer zones of the slide mass. With only a few undisturbed sample borings, it would have been very difficult to find any thin remnants of the liquefied layer. Furthermore, the existence of shear planes in a sample does not necessarily prove that this material had not liquified. As the liquefied layer decreased in thickness during the movement, drainage must have caused a decrease in pore pressures. During the last moment of movement the increasing effective stresses could have caused the material to "freeze in its tracks," so to speak, while it still had sufficient momentum to produce shear planes with small displacements.

(7) It was concluded from triaxial compression tests performed after the slide that the shell material could not have liquefied. However, based on present-day knowledge I consider it impossible to determine the susceptibility to liquefaction from the type of triaxial tests which were made at that time. Unfortunately even today we have no laboratory tests that can measure reliably the susceptibility of a sand to liquefaction.

Although the controversy between the board members concerning the mechanism of the failure was never resolved, the majority of the members were able to agree on the redesign of the dam which included a large upstream berm along the entire length of the dam. In addition, all material used for the repair of the slide area and in the new berm, including the hydraulically placed sand, was compacted.

The experience at Fort Peck has influenced the design of other large dams on the Missouri River which are also underlain by alluvial sands and clay-shales. For these later projects the unknown risks of the Fort Peck dam had become calculated risks of which we were well aware, but for which we were not yet able to develop quantitative design procedures. We resorted to two main types of defense against these risks: (1) the use of very flat average slopes, to keep the induced shear stresses in the foundation sands and in the clay-shale very small; and (2) the use of such materials and methods of compaction so that all possibility of liquefaction within the dam itself was eliminated.

In retrospect, we may now ask the question, what information is available today to the designer who has to cope with risks involving the strength of clay-shales and the stability of saturated loose sand deposits?

Concerning the strength of clay-shales, extensive empirical knowledge has accumulated during construction of the Oahe Dam in South Dakota, of the South Saskatchewan River Dam in Canada, and of Waco Dam in Texas. In addition, the designer who today faces stability problems in highly overconsolidated clays and clay-shales should refer to the recent proposals by Borowicka[8] and Skempton[9] for determination of a lower limit for the shear strength by repetitive direct shear tests.

Concerning the phenomenon of liquefaction of loose, saturated sand deposits, it would be a rewarding project to reanalyze the great amount of data on the Fort Peck slide in the light of other empirical knowledge which has become available since that event. For example, extensive investigations on the liquefaction of sand along the Mississippi have been carried out and reported on by the Waterways Experiment Station. In such an effort experience gained from older hydraulic fill dams should not be overlooked. In particular I call attention to the remarkable paper by Allen Hazen on "Hydraulic-Fill Dams,"[10] in which he described the failure of a portion of the upstream shell of the Calaveras Dam in California, a failure which strikingly resembles the Fort Peck slide, even to the extent that witnesses used almost identical words to describe the initial swinging movement of the affected portion of the upstream shell. In both cases the reservoir was partially full so that the lower zone of the upstream shell was fully saturated. Allen Hazen also concluded that only in a liquefied condition could the shell material have come to rest with almost a level surface. He proposed an explanation for the mechanism of liquefaction which even in the light of present-day knowledge is remarkably clear and accurate. (To persuade the reader to acquaint himself with Hazen's explanations, which were well ahead of his time, they are appended to this paper.) He was also the first one to recommend compaction of the materials in the shells of hydraulic fill dams as a defense against the possibility of liquefaction.

(4) *Logan Airport.*—My next example is the calculated risk in the design and construction of Logan Airport in Boston. In 1943 Governor Saltonstall of Massachusetts was faced with an important decision—whether to enlarge the existing very small airport in the Boston Harbor, which was close to the center of the city, or to establish the airport about 15 miles inland. He strongly favored the harbor location. Because of my familiarity with the properties of the soft clay underlying the greater Boston area, and because I was then particularly active in military airfield design problems, Governor Saltonstall asked me to review the feasibility of building the airport in the Boston harbor on a hydraulic fill dredged from the same clay. He had been advised that this would not be possible because such a hydraulic clay fill would not be strong enough. He wanted my answer in a hurry because New York had already started construction of Idlewild, and he was concerned that Boston would lose its place in the competition for the future overseas air traffic.

From personal observation of hydraulic dredging of clay for construction of levees in the Atchafalaya Basin, I knew that clay discharges from the pipe in the form of well-rounded clay pebbles in a matrix of slippery mud. How

[8] Borowicka, H., "Vienna Method of Shear Testing, Laboratory Shear Testing of Soils," Technical Publication No. 361, ASTM, Philadelphia, Pa., 1963, pp. 306-314.

[9] Skempton, A. W., "Long-Term Stability of Clay Slopes," Géotechnique, Institution of Civ. Engrs., London, England, June, 1964, pp. 77-101.

[10] Hazen, Allen, "Hydraulic Fill Dams," Transactions, ASCE, Vol. 83, 1920, pp. 1713-1745.

quickly would such a mass consolidate to achieve the required strength? There was no time for large-scale tests, which would probably have been the only reliable method for answering this important question. I estimated that the voids between the clay pebbles would render the entire mass sufficiently pervious so that it would consolidate within a reasonable time to the strength of the clay pebbles themselves, i.e. the strength of a medium-soft clay. On the basis of this judgment the decision was made to go ahead with the Boston harbor site.

The consensus of several experienced dredging contractors, who were consulted during the design stage, was that the hydraulic clay fill would develop side slopes of about 1 on 20. Also, they were of the opinion that it would not

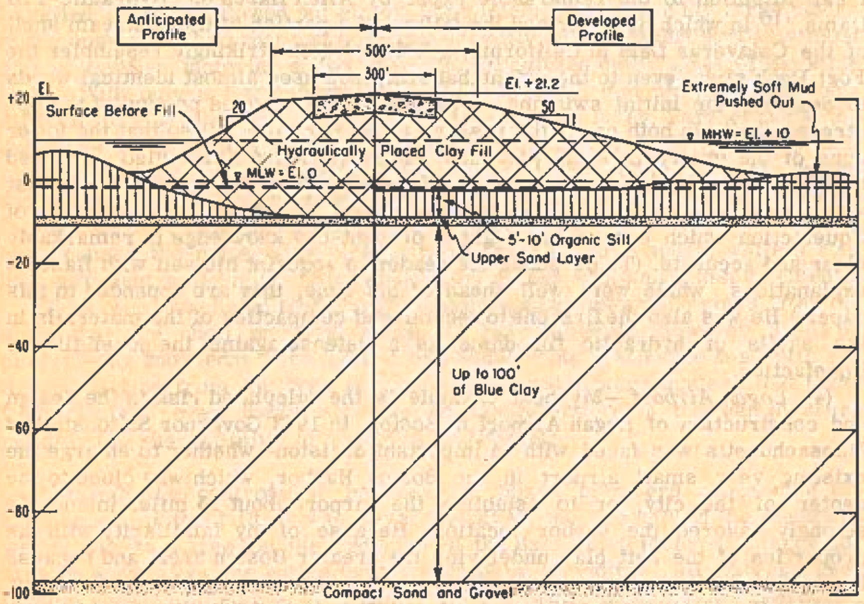

FIG. 5.—COMPARISON OF PROPOSED AND DEVELOPED PROFILE THROUGH HYDRAULIC FILL FOR RUNWAYS AT LOGAN AIRPORT

be necessary to remove the 5 to 10 ft thick layer of very soft organic silt overlying the clay, because it would be squeezed out by the weight of the clay fill. Therefore, the engineers decided not to remove the organic silt. However, the hydraulic clay fill actually assumed average side slopes of 1 on 50, and the organic silt was not displaced by the clay fill. The mixture of clay pebbles and mud had such a small shearing resistance that it simply spread out over the silt almost like a liquid and caused hardly any shear failures within the silt layer. On the left side in Fig. 5 are shown our good intentions and on the right side the actual results of this filling operation.

Much more serious were the implications of the results of the first load tests which we performed as soon as a sufficient fill area became available.

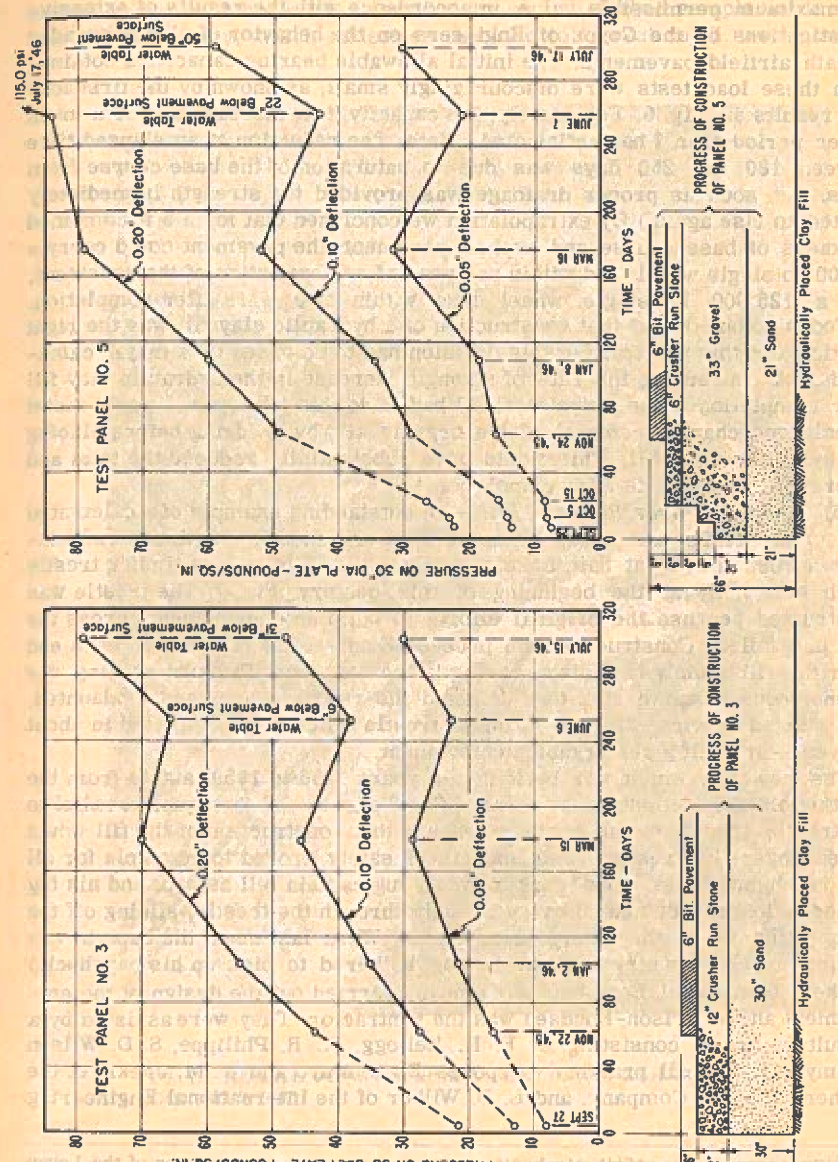

FIG. 6.—INCREASE OF BEARING CAPACITY WITH TIME

A number of test sections were constructed with different combinations of base courses and pavement, and they were subjected to 30 in. diameter plate load tests after several elapsed times. A deflection of 0.2 in. was considered the maximum permissible value, in accordance with the results of extensive investigations by the Corps of Engineers on the behavior of clay subgrades beneath airfield pavements. The initial allowable bearing capacities obtained from these load tests were discouragingly small, as shown by the first load test results in Fig. 6. Fortunately, the capacity kept increasing over a much longer period than I had anticipated. (Note: The reduction at an elapsed time between 180 and 250 days was due to saturation of the base course from rains. As soon as proper drainage was provided the strength immediately started to rise again.) By extrapolation we concluded that for a 5 ft combined thickness of base course and asphalt pavement, the pavement could carry a 65,000 lb single wheel load within one year after completion of the pavement, and a 125,000 lb single wheel load within five years after completion.

Today no one denies that construction of a hydraulic clay fill was the right solution for this project. But this decision had to be based on a major calculated risk concerning the rate of strength increase in the hydraulic clay fill after completion of the pavements. If I had to do this job again, I would insist on only one change—removal of the organic silt by dredging before placing the hydraulic clay fill. This would have substantially reduced the total and differential settlements after completion.[11,12]

(5) *Great Salt Lake Railroad Fill.*—An outstanding example of a calculated risk is the design and construction of Southern Pacific's railroad embankment across the Great Salt Lake to replace the 12 mile long timber trestle which was built at the beginning of this century, Fig. 7. The trestle was constructed because the original efforts to build an embankment across the lake had failed. Construction had proceeded only a few miles from each end when the fill simply kept disappearing in the soft clay. This undertaking was an enormous unknown risk that defeated the railway engineers. Undaunted, they started driving piles for a timber trestle which they completed in about one year—in itself a remarkable achievement.

The new fill, which was built in the years 1956 to 1959, starts from the existing old fill sections in the form of flat S curves and then runs parallel to the trestle 1500 ft to the north, to ensure that construction of the fill would not endanger the trestle. (This margin of safety proved to be ample for all but one human risk. One dark night a tug captain fell asleep, and his tug pushed a loaded 2000 cu yd barge straight through the trestle, slicing off the husky piles as if they were matchsticks. When last seen, the captain was heading across country without having bothered to pick up his paycheck.)

The International Engineering Company carried out the design of the embankment and Morrison-Knudsen was the contractor. They were assisted by a consulting board consisting of F. H. Kellogg, R. R. Philippe, S. D. Wilson and myself. For all practical purposes E. E. Mayo and W. M. Jaekle of the Southern Pacific Company, and L. D. Wilbur of the International Engineering

11 Casagrande, A., "Soil Mechanics in the Design and Construction of the Logan Airport," Journal, Boston Soc. of Civ. Engrs., Boston, Mass., April, 1949, pp. 192-221.

12 Gould, J. P., "Analysis of Pore Pressure and Settlement Observations at Logan International Airport," Harvard Soil Mechanics Series No. 34, Harvard Univ., Cambridge, Mass., December, 1949.

Company, were acting as members of the board by virtue of their close and very effective cooperation with the board.

The board started its investigation in 1955 and quickly realized that a design based on laboratory strength tests on undisturbed samples of the soft and sensitive clay, which underlies the lake to a great depth, would involve

FIG. 7.—AERIAL VIEW OF GREAT SALT LAKE RAILROAD FILL, LOOKING WEST

great uncertainties. Therefore, the board first recommended construction of full-scale test sections. However, we soon learned that they could be built only by mobilizing most of the very expensive equipment needed for construction of the entire embankment. Therefore, for the initial design we had to

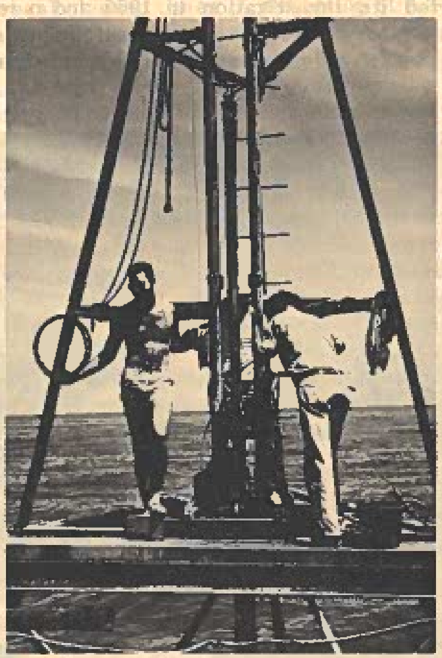

FIG. 8.—SWEDISH FOIL SAMPLER OPERATING IN GREAT SALT LAKE FROM A PORTABLE TRIPOD

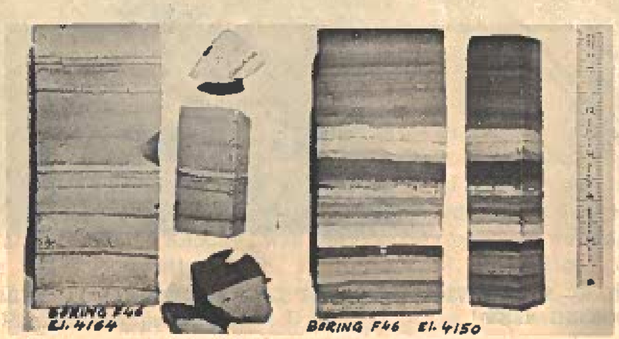

FIG. 9.—UNDISTURBED SPECIMENS OF SOFT GREAT SALT LAKE CLAY

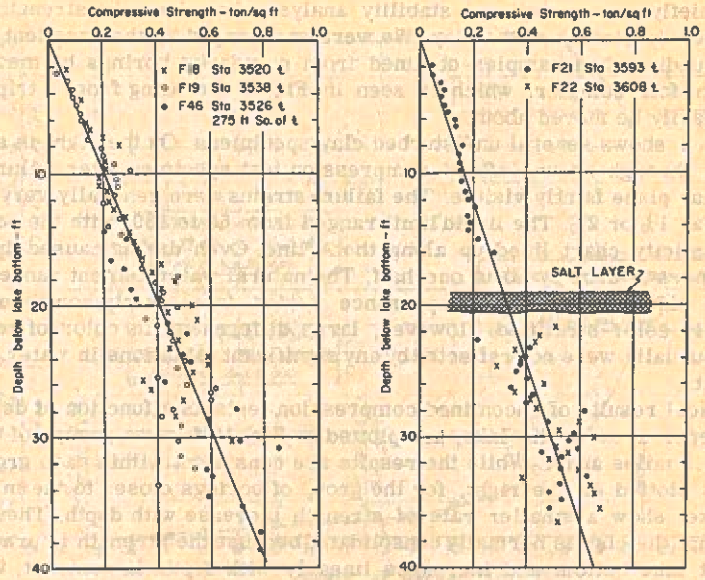

FIG. 10.—TYPICAL STRENGTH VERSUS DEPTH PROFILES FOR NORMALLY CONSOLIDATED GREAT SALT LAKE CLAY

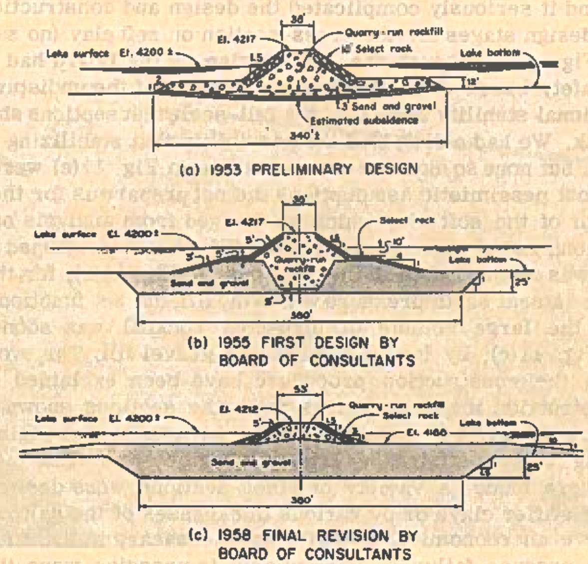

FIG. 11.—EVOLUTION OF GREAT SALT LAKE EMBANKMENT SECTION ON SOFT CLAY

rely chiefly on conventional stability analyses based on the strength of undisturbed specimens of the clay. We were encouraged by the excellent quality of the undisturbed samples obtained from numerous borings by means of a Swedish foil sampler, which is seen in Fig. 8 operating from a tripod that could easily be moved about.

Fig. 9 shows several undisturbed clay specimens. On the right is shown a section through an unconfined compression test specimen after failure, with the shear plane faintly visible. The failure strains were generally very small, as low as 1% or 2%. The liquid limit ranged from 50 to 150, with the points on the plasticity chart lined up along the A-line. Oven-drying caused the plasticity index to drop by about one-half. The natural water content ranged close to the liquid limit. The appearance varied from fairly homogeneous to intensely color-stratified. However, large differences in color of adjoining layers usually were not reflected by any significant variations in water content or limits.

Typical results of unconfined compression tests as a function of depth, for the deeper area of the lake, are plotted in Fig. 10 for two groups of borings about 1.5 miles apart. While the results are consistent within each group, the results plotted on the right, for the group of borings closer to the middle of the lake, show a smaller rate of strength increase with depth. These plots show that the clay is normally consolidated because the strength is practically zero at lake bottom and increases linearly with depth. In contrast, the clay in the shallower areas of the lake and near the shore was found to be overconsolidated as a result of drying at a time when the lake levels were lower.

The salt layer which underlies the fill for many miles, with its upper surface at a depth of 20 to 30 ft below lake bottom, was found to vary greatly in thickness and it seriously complicated the design and construction of the fill.

Several design stages for the cross-section on soft clay (no salt layer) are shown in Fig. 11. Although the 1955 design by the Board had a reasonable factor of safety based on the laboratory strength of the undisturbed samples and conventional stability analyses, the full-scale test sections showed it to be far too weak. We had anticipated the possibility that stabilizing berms might be required, but none so massive as those shown in Fig. 11(c) were envisioned. Even our most pessimistic assumptions did not prepare us for the very low in situ strength of the soft clay which we derived from analysis of the failures of test sections and of other sections of the fill. It also developed that the rock core which was incorporated in the 1955 design, Fig. 11(b), for the purpose of reducing the lateral earth pressure within the fill, did not function as intended. Therefore, the large volume of high-cost rockfill was soon replaced, as shown in Fig. 11(c), by low-cost sand and gravel fill. The evolution of this section and the construction procedure have been explained elsewhere.[13] During construction many variations from the sections shown in Fig. 11(b) and (c) were developed, depending on the progress of dredging and underwater filling that was already accomplished by the time the final design decisions were made. A variety of other sections were designed for areas underlain by stiffer clays or by various thicknesses of the salt layer.

To achieve an economical design it was necessary to build full-scale test fills and to produce failures. Particularly impressive were the failures of

13 Casagrande, A., "An Unsolved Problem of Embankment Stability on Soft Ground," Proceedings, 1st Panamerican Conf. on Soil Mechanics and Foundation Engrg., Mexico City, Mexico, September, 1959, Vol. II, pp. 721-746.

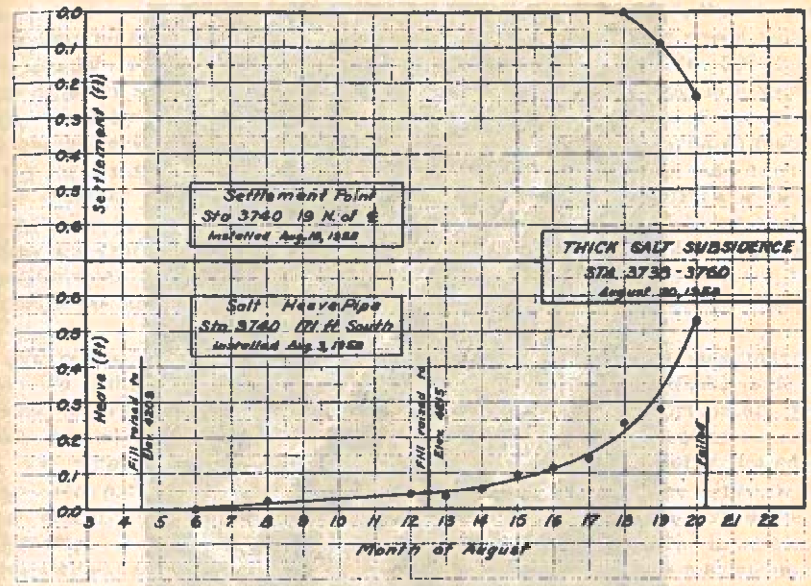

FIG. 12.—HEAVE PIPE OBSERVATIONS PRECEDING THE "MIDLAKE FAILURE"

FIG. 13.—AERIAL VIEW OF "MIDLAKE FAILURE"

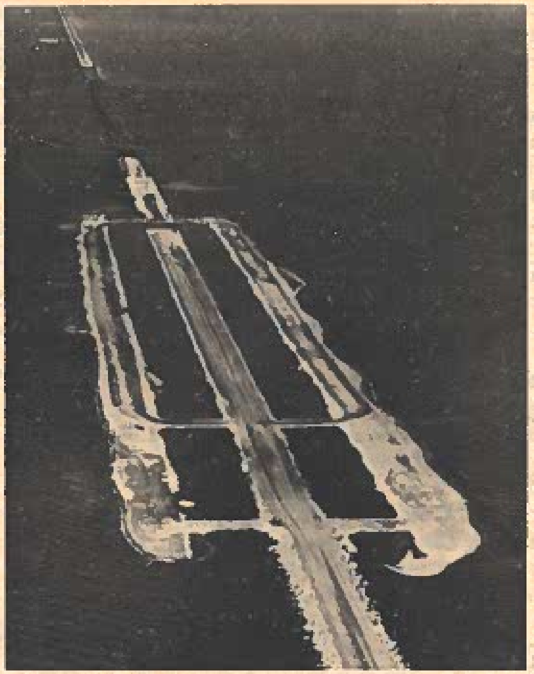

FIG. 14.—AERIAL VIEW OF RECONSTRUCTION OF "MIDLAKE FAILURE"

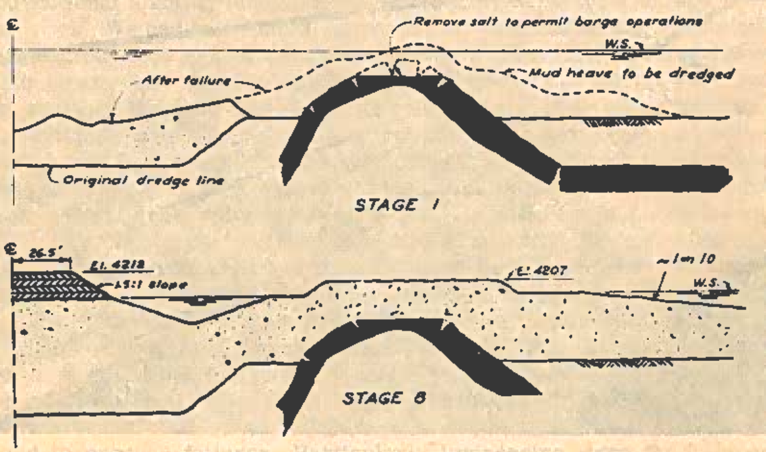

FIG. 15.—CROSS-SECTION THROUGH "MIDLAKE FAILURE" ILLUSTRATING METHOD OF RECONSTRUCTION

fill founded on the salt layer. For practical purposes the salt had to carry the entire lateral thrust of the fill; and when the salt buckled, the fill would sink into the soft clay with extraordinary speed. Here I cannot resist relating an anecdote as it was told to me. Upon completion of one of the 500 ft long test fills on salt, owners' and contractors' engineers standing on it were so impressed by its solid feel that they concluded: This proves that Casagrande is too conservative. The next day this entire test fill disappeared so fast that a tug captain swore he could look down into the hole because the lake could not flow together fast enough. We soon learned that buckling failure of the salt layer was preceded by a slow heaving adjacent to the fill which could be observed easily by means of "heave observation pipes." It became standard procedure to install such pipes where indicated, and to observe them daily. Usually the addition of a small berm would be sufficient to stop any tendency to heave.

There was one section of completed fill in the middle of the lake underlain by a 10 ft thick layer of salt which everyone considered reasonably safe, but which nevertheless was instrumented with heave pipes. Unfortunately, for a short period the results of the heave observations along that section remained in the notebook of an overworked surveyor, instead of being plotted and examined promptly by others. On August 20, 1958, while the surveyor was just completing another heave measurement, he suddenly observed the fill subsiding. A 2000 ft section disappeared in a few minutes in an almost symmetrical failure, referred to as "the midlake failure." The measurements plotted in Fig. 12 speak for themselves. The salt had buckled on both sides at a distance of 180 to 200 ft from the center line, as seen in the aerial view in Fig. 13. Reconstruction followed a procedure developed in connection with the repair of several much smaller subsidences. For example, in the foreground of Fig. 7 one can see on the right of the fill a balancing berm that was built for the purpose of stopping a subsidence in its early stages. Reconstruction of the midlake failure is shown in progress in Fig. 14, and the procedure is explained in Fig. 15. After removal of the salt and clay heaves by dredging to permit barge operations, the two underwater berms were constructed with extremely flat outside slopes. Then truck dumping was used to raise the two berms and the central fill above water, at first maintaining almost exact balance, and later very slowly increasing the differential weight between the central and the berm fills.

Principally on an empirical basis which was developed by extensive full-scale testing and observations, and necessitating many important design changes during construction, the project was completed successfully one year ahead of schedule, and in my judgment with almost optimum economy. Paradoxically, such economy would not have been achieved had we known at the start the strength of the clay that controlled the stability of the embankment during construction. It is even probable that this project would not have been authorized, for such knowledge, combined with the application of a conventional factor of safety, would have forced us to design a fill for which the cost estimate would have been far in excess of our 1955 estimate shown in Table 1. Thus the $50 million limit which Southern Pacific's Board of Directors had established as the maximum expenditure that would be economically justified for this project would have been greatly exceeded; and instead of building the embankment crossing, the deck of the timber trestle would probably have been renewed in accordance with an alternate plan.

I do not wish to leave the impression that the calculated risk in the design of this project also involved risks concerning the long-term safety of this railroad crossing. A fill built on normally consolidated clay has its lowest factor of safety against foundation failure during construction or immediately after its completion. Subsequently, progress of consolidation steadily increases the safety against failure. Therefore, a low initial factor of safety is justified for such a project. (An engineer who had extensive experience in building long highway embankments on soft clay, once remarked that if no failure occurs it proves to him that the embankment was overdesigned.)

Since the summer of 1959, when this new railroad crossing was put in operation, we relied chiefly on settlement observations as a check on the performance of the foundations. The rate of settlements is gradually decreasing in a consistent pattern, reflecting a steadily increasing strength of the clay.

This project is a good example of what Terzaghi liked to call the "observational approach," i.e. the continuous evaluation of observations and new information for redesigning as needed while construction is in progress. It also illustrates another statement that Terzaghi made many times: "In applied soil mechanics a design is not completed until the construction is successfully completed."

TABLE 1.—COST ESTIMATES FOR GREAT SALT LAKE RAILROAD FILL

Date	Job Status	Volume of Fill, in millions of cubic yards	Cost, in millions of dollars
1953	I.E.Co's Preliminary Design	21	$30
1955	Board of Consultants' First Design	32	$49
1956	Start of Construction		
1959	Completion (one year ahead of schedule)	44	$50.5

In common with the Fort Peck Dam, our experience with the Great Salt Lake Fill has greatly increased our ability to cope with certain problems in applied soil mechanics on an empirical basis. However, in spite of the great effort that went into soil testing, field observations and stability analyses before and throughout the construction of the Great Salt Lake Fill, we have not succeeded in making reliable determinations of the following important quantities that govern the stability of this embankment:

(1) the long-term in situ strength of the clay at unchanged water content;
(2) the pore pressures induced by the fill load in the clay outside a zone where the fill load produces significant normal stresses. (Note: Although piezometers commonly provide reliable measurements of pore pressures, most of the piezometers installed on this project became inoperative soon after installation because of salt deposition. Some of those that worked temporarily showed that remarkably high pore pressures were transmitted through the salt layer for many hundreds of feet from the fill.); and
(3) the magnitude of the earth pressure within the fill.

This account of the calculated risks involved in this project would not be complete without mentioning two more calculated risks. The "sand and gravel" used for the main body of the underwater fill was largely a silty sand. The question of its stability under dynamic stresses was of serious concern to me.

FIG. 16.—AERIAL VIEW OF BALDWIN HILLS RESERVOIR ON THE DAY OF FAILURE (LOOKING NORTH)

Blasting tests in steep underwater slopes of this material proved that the good gradation and angularity of the grains (chiefly quartz) rendered the mass remarkably stable to shock. This conclusion was substantiated later when on August 30, 1962, the fill was exposed to a severe earthquake which caused only slight local movements. The other risk concerns the relationship between crest elevation and lake level. The lake level fluctuated between a maximum of about El. 4216 almost 100 years ago and a recent minimum of about El. 4196. The design was based on an assumed lake El. 4200 which is approximately the average for the preceding 30 years. There is a long-term trend toward lower lake levels. For this reason, and also to keep the stresses in the clay as small as possible, the relatively low crest El. 4212 was elected for the final design.

Should you some day travel on or fly over this Great Salt Lake Fill, remember that what you see of the entire long railroad embankment is only a very small portion of the total volume of this fill, even a smaller ratio than for the proverbial iceberg; and that this ratio is a monument to a calculated risk which almost defeated those responsible for the design. And when you observe in the middle of the lake three parallel embankments almost a half-mile long, remember that this section of the embankment stands as a monument not only to a calculated risk but also to a human risk.

No other project has caused me so many sleepless nights as this one.

(6) Baldwin Hills Reservoir.—My next example is the Baldwin Hills Reservoir in Los Angeles which failed in December 1963. Essential data, including Figs. 16, 17 and 21, were obtained from the published report prepared by the

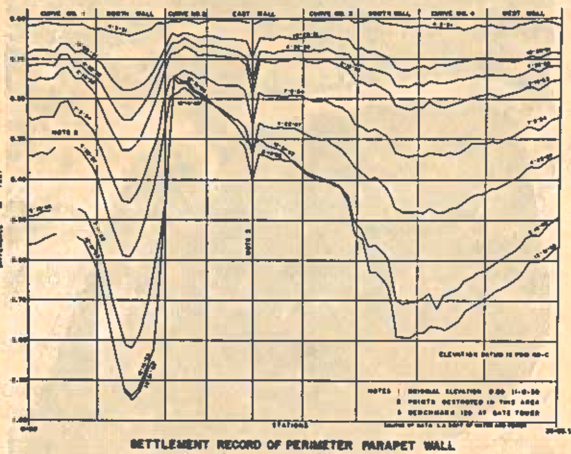

FIG. 17.—SETTLEMENT RECORD OF PERIMETER PARAPET WALL, BALDWIN HILLS RESERVOIR

State of California Engineering Board of Inquiry.[14] Fig. 16 is an aerial view while the water was still pouring through the crevasse that had developed in the natural ground forming the right abutment of the earth dam. The reservoir had been carved from the sides of a valley and closed off by an earth dam with a maximum height of about 130 ft. An open and active geologic fault crosses the reservoir in north-south direction in a direct line with the crevasse.

In Fig. 17 are plotted the settlements along the entire crest of the reservoir. The progress of the settlements with time was quite regular. During the 12.5 year life of the reservoir, the maximum settlement of almost one foot developed under the highest portion of the dam, which in this plot is designated "North Wall." The crevasse developed in the right abutment portion, designated

14 "Investigation of Failure of Baldwin Hills Reservoir," California Dept. of Water Resources, Sacramento, Calif., April, 1964.

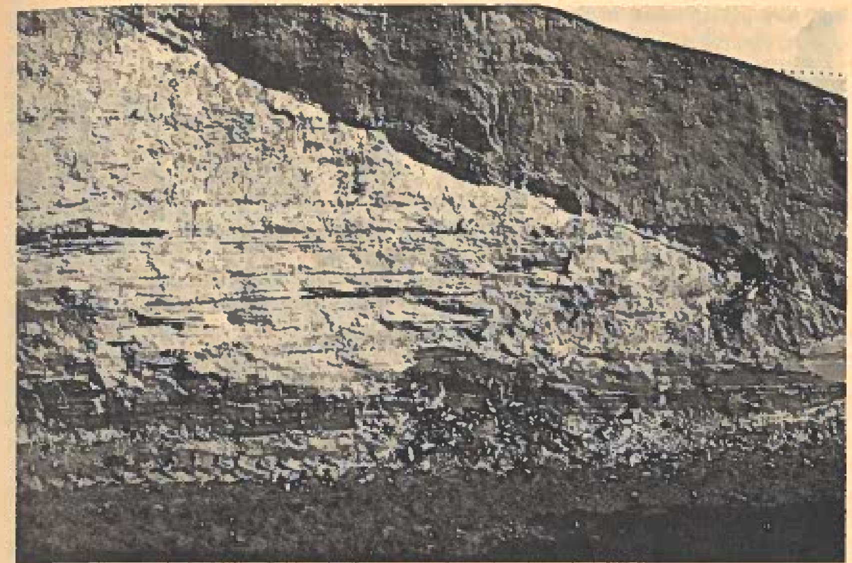

FIG. 18.—RIGHT (EAST) SIDE OF CREVASSE, BALDWIN HILLS RESERVOIR

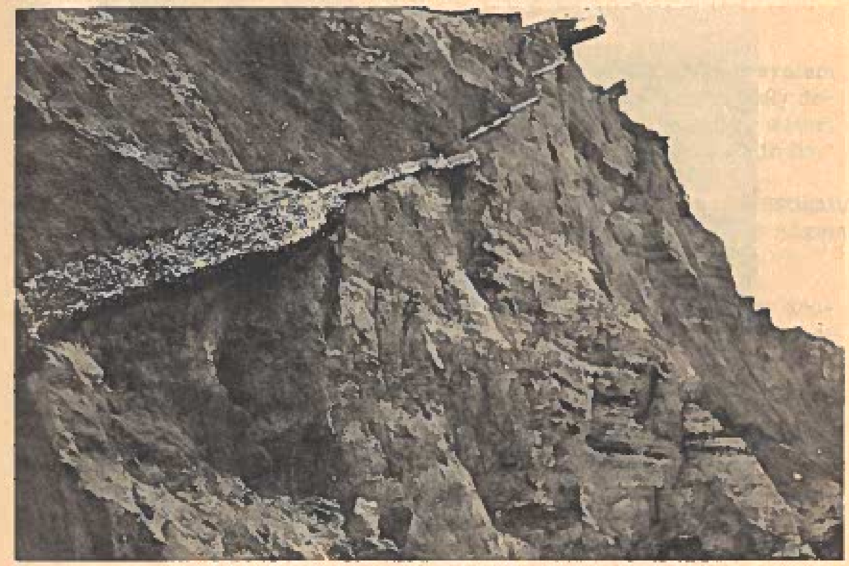

FIG. 19.—LEFT (WEST) SIDE OF CREVASSE, BALDWIN HILLS RESERVOIR

FIG. 20.—LARGEST SINK HOLE ALONG FAULT MERGED WITH CREVASSE

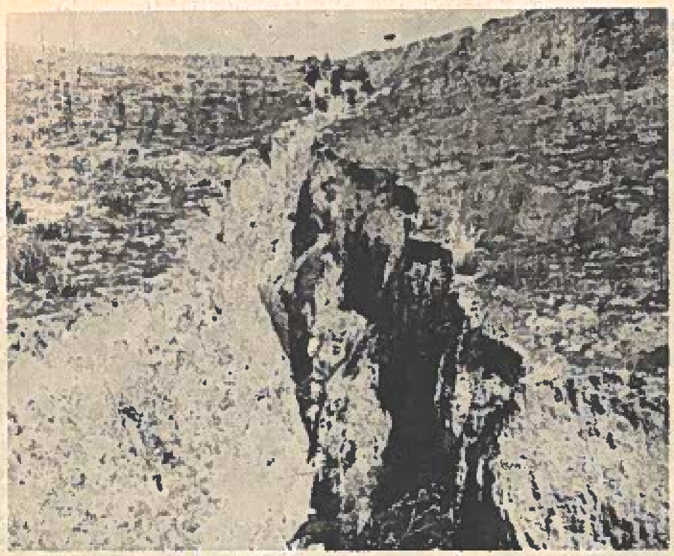

FIG. 21.—ILLUSTRATION OF ERODIBILITY OF INGLEWOOD FORMATION IN WHICH THE BALDWIN RESERVOIR WAS CONSTRUCTED (GULLY IS ABOUT 40 FT DEEP)

"Curve No. 2" in Fig. 17, which had settled a total of about 0.2 ft. The sharp increase in settlement between this abutment and the dam was chiefly due to the steep original ground slope. Thus, the stage was set for tension forces between the abutment and the dam, a tendency that was aggravated by the fact that this junction of dam and abutment was located in a curve which arched outward, such that the water load also tended to produce tension forces.

The entire reservoir is underlain by very weakly cemented silts, sands and gravels. The abutment in which the crevasse developed, Figs. 18 and 19, consisted chiefly of loose, fine sands and silts that are very easily eroded. Some of the fine sand layers had so little cohesion that one could dislodge grains by blowing at them. To prevent seepage from getting into these dangerous foundation and abutment soils, the reservoir was lined with a 10 ft thick clay layer which tapered to 5 ft at the top of the slope. Beneath the clay was a 4 in. drainage layer consisting of cemented pea gravel, i.e. a porous concrete to drain the seepage water. Just below the pea gravel a system of 4 in. tile drains was installed to collect any seepage passing through the clay lining. Beneath the pea gravel was a 1/4 in. thick asphaltic layer intended to prevent seepage from reaching the foundation.

After the failure a number of sink holes were found in the reseroir bottom along the fault line. The largest one, Fig. 20, merged with the crevasse. Along the fault the offset on top of the clay lining averaged 2 to 3 in. In the course of detailed exploration beneath the sink holes, several large cavities were found in the natural foundation materials. Obviously underground erosion had been in progress for a long time.

From the State Report[14] I reproduce Fig. 21, which illustrates how easily the natural materials that underlie the reservoir can be eroded. From the State Report[14] I quote also the last paragraph of the conclusions which summarizes very well the salient aspects:

> "Sitting on the flank of the sensitive Newport-Inglewood fault system with its associated tectonic restlessness, at the rim of a rapidly depressing subsidence basin, on a foundation adversely influenced by water, this reservoir was called upon to do more than it was able to do."

In addition to the detailed investigations by the State Board, an investigation was carried out by the Board of Inquiry appointed by the Mayor of Los Angeles. The report of this board contained the following statements:

> "The Baldwin Hills Reservoir was built across a series of minor geological faults . . . accurately located during construction of the reservoir. These minor faults were reported as active by the Department of Water & Power geologist who prepared the final geological report on the reservoir site.
>
> "A Board of Consultants consisting of a geologist and two engineers which was appointed by the Department of Water & Power in connection with the design and construction of the reservoir was aware of this system of minor faults in the reservoir area, and reported in 1948 that it was 'very unlikely that any appreciable movement will again occur along these auxiliary faults.' That Board of Consultants also in 1948 approved the 'feasibility of the proposed Baldwin Hills Project, the suitability of the site, and the plans of design.'

"Movements took place along the faults below the reservoir, probably beginning shortly after completion. These movements increased at an accelerated rate after December of 1957.

"Movements on the faults fractured the drainage system underneath the reservoir and ruptured the asphalt membrane which was intended to prevent leakage of reservoir water into the underlying soil and rock. These fractures probably began in 1951 at several places along the faults.

"The breaks in the asphalt membrane permitted drainage water from the reservoir in the pea gravel to flow into the underlying natural soil or rock along the local faults with the result that piping (channeling) took place in the soil or rock at various locations."

The designers of this project and their consultants were well aware that the City of Los Angeles must have within its vast limits strategically located reservoirs to ensure a dependable water supply and protection in case of large fires. The Baldwin Hills are the only elevated area in southwestern Los Angeles which appeared suitable for a distribution reservoir. Were these persuasive arguments responsible for taking a grave risk, or did the designers underestimate the risk?

(7) Foundations for Brookhaven Synchrotron.—My last example is the foundation design for the 33 billion electron volt synchrotron at Brookhaven National Laboratory on Long Island, New York. The principal element is an 850 ft diameter concrete conduit, called the tunnel, which is covered with a substantial mound of earth. In this tunnel there is a stainless steel tube in which protons are accelerated by means of hundreds of magnets weighing about 20 tons each. Stone & Webster, who were engaged for the design, and I as consultant, were presented by the physicists with the requirement that relative to each other these magnets must stay aligned with an accuracy of plus or minus 0.005 in.—an unheard of accuracy for foundations.

Fortunately we had good foundation soils, the Long Island sand which extends to great depth. However, the original topography of the area was far from level; there were groundwater fluctuations to consider; there was the weight of the mound of earth over the tunnel; there was the target building with very heavy but movable shielding loads; etc. It seemed almost an impossible task. Bill Swiger of Stone & Webster and I soon realized that even if the magnets are supported on piles, secondary compression in the sand may cause excessive differential movements. Large-area load tests were made to obtain as much information on long-term time-effects in sand as was possible in the available time. With strict temperature control in the tunnel and other refinements, we finally arrived at what we believed was a satisfactory solution. Then came the day when we were invited to answer questions. The auditorium at Brookhaven was filled with physicists, and there I was trying to explain how we foundation engineers extraploate from crude tests and using simple computations the probable movements of their vital magnets. I could see in the eyes of Dr. G.K. Green, Brookhaven's Associate Director who led this inquisition, and also from the expressions on the faces of many in the audience, what they were thinking. I vividly remember the scene. After I had finished my explanations, Dr. Green said: "Tell me frankly, you haven't just pulled your figures out of the air?" He accompanied his question with a sweeping motion of his arm, demonstrating how I might have caught my numbers skillfully in the air as they were flying by. I replied: "No, Dr. Green, we foundation engineers do not pull figures out of the air; we pull them out of the ground." And stooping down I demonstrated with a similar sweeping motion how we do it. After the laughter had died down, I had the nerve to ask him whether he could explain just how he arrived at the five mils limitation. He hesitated a bit and said: "Well, my colleagues and I made our computations, and we arrived at the conclusion that we could stand 20 mils. But we did not trust our computations, so we cut it down to five mils to be on the safe side." I thanked Dr. Green and his colleagues for showing so much confidence in the ability of foundation engineers to control settlements to such accuracy, when they had so little confidence in their own computations.

In several years of operation, except for temporary new construction in the area and for changes in the arrangement of shielding loads in the target area, the relative movements between magnets have not exceeded 10 mils. Furthermore, once when some construction was in progress near the tunnel, a settlement of about 100 mils occurred locally; but the machine continued to operate for one week without causing the operators to notice or object.

Of course we could not be certain about our prediction. But we knew that at worst the magnets would have had to be realigned more frequently than the owners desired; (and the shaky confidence of Brookhaven's physicists in soil mechanics would have suffered a mortal blow).

DISCUSSION

Throughout the history of engineering, progress required bold advances beyond the limits of knowledge, often at great risks. What should the engineer's attitude be toward such risks? In part, I should like to answer this question in Terzaghi's words, by quoting from a letter he wrote to André Coyne immediately after the failure of the Malpasset Dam:

"When I read in the papers about the failure of the Malpasset Dam, my thoughts turned immediately to you and to the terrible shock you must have experienced when the sad news reached you. In situations of this kind it is at the outset impossible to divorce the technical aspects of the event from the human tragedies involved. Yet every fair-minded engineer will remember that failures of this kind are, unfortunately, essential and inevitable links in the chain of progress in the realm of engineering, because there are no other means for detecting the limits to the validity of our concepts and procedures. I have witnessed the shocking manifestations of this painful process during the First World War in the field of aviation, when we tried to proceed in a few years from the primitive types of airplanes to larger and more elaborate ones, and in the field of dam construction the price of our lessons is equally high.

"Having known you well for many years, I feel confident that the failure was not a consequence of an error in your design. Therefore, it will serve the vital purpose of disclosing a factor which in the past has not received the attention which it requires. The fact that its implications became manifest on one of your jobs is not your fault, because the occurrence of failures at the borderline of our knowledge is governed by the laws of statistics, and these laws hit at random. None of us is immune. You as an individual, and the equally innocent victims of the failure have paid one of the many fees which nature has stipulated for the advancement in the realm of dam construction. Therefore, the torments which you experienced should at least be tempered by the knowledge that the sympathies of your colleagues in the engineering profession will be coupled with their gratitude for the benefits which they have derived from your bold poineering."

When referring to the pioneer days of military aviation (he was in charge of an aviation research establishment during the first World War), Terzaghi might well have added that the days of risk in designing airplanes have by no means passed. Even today, after a new design is developed and the airplane is carefully constructed of materials manufactured to precise specifications, it must be subjected to many months of exhaustive testing before it is finally certified for service. And even then one can not be certain that all weaknesses have been discovered.

By comparison, in foundation and earthwork engineering we often deal with very erratic, natural materials whose properties are in part far from clearly understood. Then we build upon such materials and with such materials, huge dams which hold back more potential destruction than man could create by any other peacetime activity. Therefore it seems hardly necessary to stress the great importance of including in the education of those who specialize in soil mechanics, a realistic approach to the risks involved in earthwork and foundation engineering.

Classification of Risks and Potential Losses.—To facilitate discussion, I will use the following rough classification of risks and potential losses in earthwork and foundation engineering:

Classification of Risks

A. Engineering Risks
   1. Unknown risks.
   2. Calculated risks.

B. Human Risks
   Most human risks, both unknown and calculated, fall into the following:

   1. Unsatisfactory organization, including division of responsibility between design and supervision of construction.
   2. Unsatisfactory use of available knowledge and judgment.
   3. Corruption.

Often there is no sharp line of demarkation between these three groups of human risks. In particular, division of responsibility is frequently the cause of insufficient use of available knowledge and judgment, and it can also facilitate corrupt practices.

Classification of Potential Losses

I. Catastrophic loss of lives and property.
II. Heavy loss of lives and property.
III. Serious financial loss; probably no loss of lives.
IV. Tolerable financial loss; no loss of lives.

Engineering Risks.

Unknown Engineering Risks.—By definition such risks cannot be identified until they reveal themselves by a failure or other event that can be observed and investigated. I hope that I am not too optimistic when I state my belief that soil mechanics has advanced far enough to permit at least a qualitative estimate of the response of all soils and rocks on our globe when subjected to our conventional engineering activities; in other words, that we are not likely to encounter any major unknown engineering risks. However, there may well be some unknown risks in store for us in connection with the application of nuclear energy to large-scale excavations, e.g. in materials such as the Cucaracha formation in the Panama Canal Zone.

Calculated Engineering Risks.—I list below some of the more important calculated risks in applied soil mechanics for which we still depend largely on crude empirical knowledge and judgment because quantitative analyses are either non-existent or of very doubtful validity:

1. Liquefaction slides in granular soils.
2. Liquefaction slides in extremely sensitive clays.
3. Stress-deformation and strength characteristics of coarse-granular materials, including rockfills, under very high confining pressures.
4. Long-term stress-deformation and strength properties of clays at constant water content.
5. Stability characteristics of highly plastic, stiff clays and clay-shales.
6. Control of transverse and longitudinal cracks in the core of high rockfill dams.
7. Effects of earthquakes on high earth and rockfill dams.

In passing, I mention that significant progress in our ability to cope with these problems will depend largely on expensive investigations carried out in connection with major projects.

The margin of safety that we incorporate into our structures should bear a direct relationship to the magnitude of potential losses, and it must also take into account the range of uncertainty involved. If this range is very small, then we are approaching problems in structural engineering which are resolved by the use of a conventional factor of safety. When the range is large, we cannot usually express the results in terms of a numerical factor of safety; we then speak of a margin of safety which is based largely on experience and judgment.

Projects in Category I, i.e. those involving potentially catastrophic losses, are almost always undertaken by owners with ample financial resources who are fully aware of their great responsibility. Therefore, the best knowledge and judgment are mobilized to ensure the best possible design and construction. On such projects the designer will resort to a liberal margin of safety and/or to independent lines of defense whenever he deals with aspects that he cannot evaluate with any degree of certainty. For example, in the case of very high dams the designer will resort to particularly conservative foundation and abutment treatments; he may resort to additional freeboard in order to allow for possible slumping during earthquakes; he will pay special attention to the transition zones between the core and rock shells to ensure that any transverse cracks in the core will be self-healing; he may resort to arching of a high dam between steep abutment slopes; he will require the best possible