RELATION BETWEEN SOIL MECHANICS AND FOUNDATION ENGINEERING
Presidential Address

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The opening of this Conference is an event of unusual significance. It represents the first international council in the perpetual war of the civil engineer against the treacherous forces of nature concealed in the earth. Due to scattered and world-wide efforts extending over a period of 25 years, new and efficient weapons have been forged and the prime purpose of our meeting consists in discussing the means of exploiting the advantages thus secured. For the sake of brevity these recent developments have been given the name of soil mechanics. The transition from the classical theories of the pre-war generation to soil mechanics is synonomous with a transition from a purely abstract treatment of the problems of soil behavior to one based on an intimate knowledge of the manifold and complex properties of the different types of earth. The validity of the older theories of earth pressure and earth resistance was limited to ideal materials whose properties can be described in five lines. However, in order to describe the practically important properties of earth such as nature has produced, one needs a good-sized book. As a consequence, the older theories failed in a great number of cases of outstanding practical importance. This, in a nutshell, was the reason for the necessity of a radical departure from past practice.

Our meeting coincides in time and space with the Tercentenary Celebration of the oldest and most eminent institution of higher learning in the United States. Owing to the hospitality of Harvard University, represented by its president, Dr. Conant, the retrospect over the glorious and scholarly past of this University combines with the official inauguration of a new and important field of applied science.

Origin of Soil Mechanics. Ten years ago the investigations which led to this Conference still had the character of a professional adventure with rather uncertain prospects for success. This adventure began a short time before the war, simultaneously in the U.S.A., in Sweden, and in Germany. It was forced upon us by the rapid widening of the gap between the requirements of canal and foundation design and our inadequate mental grasp of the essentials involved.

In the United States, the catastrophic descent of the slopes of the deepest cut on the Panama Canal issued a warning that we were overstepping the limits of our ability to predict the consequences of our actions. The columns on dam-failures in the engineering magazines never ceased to maintain a feeling of uneasiness among those engaged in harnessing the rivers of the country, and the visible effects of the settlement of heavy public buildings founded on materials other than bed rock demonstrated also to the layman the existence of alarming gaps in our knowledge of so-called terra firma. To close these gaps, the American Society of Civil Engineers in 1913 appointed a Committee to investigate the situation. The outstanding achievement of this Committee, with Mr. R. A. Cummings as chairman, consisted in a realization of the importance of expressing the properties of soils by numerical values. We cannot possibly utilize our practical experience to full advantage, unless the soils to which our experience refers can be recognized unmistakeably in other localities. However, the final answer to this problem of identification still remains to be found, although the progress in this direction is very encouraging.

In Sweden intensified activities in soil research were induced by a series of unexpected and catastrophic slides in the cuts of the Swedish State Railways, which took a heavy toll of lives and of capital. In order to eliminate the danger of the recurrence of similar events, the Swedish State Railways appointed in 1913 a Geotechnical Commission to investigate the degree of safety of the slopes along the existing lines. During the ten years of its existence the Commission, headed by Prof. Fellenius in Stockholm, developed some of the most important fundamental principles for our present methods of stability computations.

In Germany the construction of the Kiel Canal between the North and the Baltic Seas brought more than one surprise to the engineers who built it. Prominent among the accidents was the energetic outward movement of a heavy quay wall, solidly supported by a forest of wooden piles. The piles were strong enough to support the wall, but the clay was not strong enough to support the piles. Therefore the wall and the piles moved out as a unit. The rapid growth of German harbors brought additional variety into the stately collection of unsolved problems. Hence it was more than a mere accident that the research was started in the hydraulic laboratories of that country. The director of the Prussian hydraulic laboratories in Berlin, Mr. Krey, improved the existing methods for the computation of the pressure and the resistance of the earth in connection with retaining walls and bulkheads. He succeeded in developing a rational procedure for computing the forces which act on bulkheads, and furnished important contributions to our knowledge of the shearing resistance of soils.

I myself, prior to 1912, worked as a superintendent of construction. Year after year, in the Austrian Alps, in Transsylvania, and in Russia, I had ample opportunity to witness the striking contrast between what we expected when digging into the earth or loading it, and what really happened. Deeply impressed by the fundamental futility of pertinent theoretical knowledge, I came to the United States and hoped to discover the philosopher's stone by accumulating and coordinating geological information in the construction camps of the U.S. Reclamation Service. It took me two years of strenuous work to discover that geological information must be supplemented by numerical data which can only be obtained by physical tests carried out in a laboratory. The observations which I made during these years crystallized into a program for physical soil investigations which looked as if it could easily be carried out in one year. In reality the research activities extended over a period of eight years.

Period of Transition. All these early efforts which were started before the war and carried on by isolated groups or by individuals had one important feature in common. They were still guided by the intention to establish a science of soil behavior comparable to the science of bridge design. The major part of the college training of civil engineers consists in the absorption of the laws and rules which apply to relatively simple and well-defined materials, such as steel or concrete. This type of education breeds the illusion that everything connected with engineering should and can be computed on the basis of a priori assumptions. As a consequence, engineers imagined that the future science of foundations would consist in carrying out the following program: Drill a hole into the ground. Send the soil samples obtained from the hole through a laboratory with standardized apparatus served by conscientious human automatons. Collect the figures, introduce them into the equations, and compute the result. Since the thinking was already done by the man who derived the equation, the brains are merely required to secure the contract and to invest the money. The last remnants of this period of unwarranted optimism are still found in attempts to prescribe simple formulas for computing the settlement of buildings or of the safety factor of dams against piping. No such formulas can possibly be obtained except by ignoring a considerable number of vital factors.

Unfortunately, soils are made by nature and not by man, and the products of nature are always complex. After a decade of mental and physical experimentation in the newly developed field, it became obvious that the method of approach must be radically changed. The design of bridges and of other purely artificial structures requires only a knowledge of mechanics. Theory governs the field and experience is a matter of secondary importance except for that acquired over the drafting board. The theoretical results can be depended upon, because the equations contain no important element of uncertainty. However, as soon as we pass from steel and concrete to earth, the omnipotence of theory ceases to exist. In the first place, the earth in its natural stage is never uniform. Second, its properties are too complicated for rigorous theoretical treatment. Finally, even an approximate mathematical solution of some of the most common problems is extremely difficult. Owing to these three factors, the possibilities for successful mathematical treatment of problems involving soils are very limited. In bridge design, the theory provides us with certainties and eliminates the necessity for observations on full-sized structures. In soil mechanics the accuracy of computed results never exceeds that of a crude estimate, and the principal function of theory consists in teaching us what and how to observe in the field. Whenever we explore the natural soil by drilling a hole or by extracting a sample, we alter its state even before the direct contact between the soil and the tool is established, and the effect of this change on the results of our tests can only be learned by experience. The theories which we apply in order to make the step from the test results to a numerical estimate of the effect of our engineering operations are bound to be based on radically simplified assumptions. The importance of the difference between theory and reality can again be learned only by experience. It depends to a large extent on the type of soil. The Proceedings of this Conference contain a great number of instructive examples. Finally, a natural soil is never homogeneous. Its properties change from point to point, while our knowledge of these properties is limited to those few spots at which the samples have been collected. To get information on the importance of the error produced by our inadequate knowledge of the deposits, we are compelled to compare the results of our forecast to those of direct measurement in a great number of cases. Owing to these facts, successful work in soil mechanics and foundation engineering requires not only a thorough grounding in theory combined with an open eye for the possible sources of error, but also an amount of observation and of measurement in the field far in excess of anything attempted by the preceding generations of engineers. Hence the center of gravity of research has shifted from the study and the laboratory into the construction camp where it will remain. The first fruits of this revised and essentially empirical attitude towards the problems of earthwork engineering are assembled in the Proceedings of our Conference.

Progress Achieved. After I read these volumes, I could not help remembering an episode which occurred some eighteen years ago. At that time I spent several months in a systematic effort to make an inventory of what we knew or believed we knew about the interaction between structure and earth. For that purpose I went through all the volumes of the leading English, German, and French engineering periodicals which had been published since 1850 and through all the textbooks which I could secure, abstracting all the articles and chapters relating to the subject of my investigations. This occupation was far from being as profitable as I had hoped. The abstract which covered a period of more than half a century contained less positive information than the two volumes of our Proceedings. Nevertheless, my efforts were fully compensated by an illuminating bird's-eye view of the situation which prevailed in the field of foundation engineering prior to the world war. Comparing this situation with that created by the recent developments of soil mechanics, I notice the following changes: a vast improvement in the quality and quantity of observation on full-sized structures, a rapid elimination of the time-honored antagonism between theory and practice, and the replacement of blind faith in rules and prescriptions by a refreshing demand for adequate evidence. I shall now try to present to you the salient features of these recent developments and their practical consequences.

The Conflict between Theory and Reality. One of the outstanding impressions which I got while preparing the abstracts of pre-war publications was that of a steady decline of the capacity for careful observation after the eighteen-eighties. Prior to about 1880 a surprisingly great number of stimulating field observations were published by engineers. A few examples may suffice. The oldest editions of the English textbook by F. W. Simms on practical tunneling are full of valuable data drawn from actual experience in the early days of tunnel construction through English clays. Some of the French papers on the slides which occurred during the construction of the railroad line from Paris to Lyon are masterpieces in the line of keen observation, and the description of the discouraging experiences during the construction of the first German and Austrian railroads across regions of unstable subsoil are still an inexhaustible source of information after half a century. However, after the eighties, the interest in observing and describing the whimsical manifestations of the forces of nature seemed to fade out. I am inclined to explain this decline by a growing confidence, produced by the inertia of the human mind, in the theories concerning the behavior of earth. At the time when the theories originated, their authors were still keenly aware of the bold approximations involved, and nobody thought of accepting them at face value. As the years passed by, these theories were incorporated into the stock of knowledge to be imparted to students during the years of their college training, whereupon they assumed the character of a gospel. Once a theory appears on the question sheet of a college examination, it turns into something to be feared and believed, and many of the engineers who were benefited by a college education applied the theories without even suspecting the narrow limits of their validity. If the structures designed on the basis of these sacred theories stood up, their behavior was considered to be normal and not worth mentioning. If they failed, it was an act of God, which should be concealed from the eyes of mortals, who might believe that the designer was poorly grounded in theory. This uncritical attitude toward the problems of earth behavior induced a growing resentment of those who had eyes to see against the theoretical textbook wisdom. Among the documents of this justified resentment, I wish to mention a paper published in 1908 by the experienced subway expert, J. C. Meem, on the bracing of tunnels and trenches, in the Transactions of the American Society of Civil Engineers. The contents of this paper and of the numerous discussions which followed left no doubt concerning what the authors thought about the college attitude toward earth pressure problems. It was not very complimentary.

However, the feeling of resentment against unwarranted generalization does not suffice to transform an accumulation of haphazard professional experience into a store of knowledge and of general usefulness. In order to accomplish such a transformation, three conditions must be satisfied. First of all, there must be a generally accepted method for describing the soils to which the individual experiences refer. Conventional terms such as "fine, water-bearing sand" may mean almost anything between a loose accumulation of small grains, incapable of sustaining an appreciable load, and a stratum which is almost as hard as rock. The terminology must be based on numerical values of some soil. Otherwise it is worthless. Second, the observation methods must be reliable; otherwise there is too wide a margin for interpretation. If an observer claims that a building did not show any signs of settlement, the structure may have settled through a distance of one-tenth of an inch to four inches, provided the settlement was uniform and the distance to the neighboring structure was appreciable. Finally, the report on the observation must be accompanied by a statement of all the vital factors which were likely to have influenced the object of the observation. Otherwise the observation cannot be used as a basis for a valid conclusion. In order to satisfy this third requirement, the observer must be familiar with the physics and mechanics of the observed phenomenon. Thus, for instance, no valid conclusion can be derived from the results of a settlement observation on a building covering an area of 100 by 100 feet unless we have at least reliable geological information concerning the nature of the subsoil to a depth of at least 150 feet. In one of the cases which I had under observation, a building settled more than one foot owing to the compression of a layer of clay located between a depth of 100 and 130 feet below the surface of the ground.

Rationalized Observation. Practically none of the above requirements were satisfied by the observations of the pre-war engineers, because the knowledge of the physical properties of the soils and of the forces exerted or transmitted by the water in the soil was by far too inadequate. Therefore the ignorance of the practical engineers differed from that of the faithful textbook believers merely in kind but not in profundity. A single example may suffice to explain what I mean. During my professional career, I met a great number of practical engineers and of experienced contractors who honestly believed that the settlement of a pile foundation involving a load of 20 tons per pile should approximately be equal to the settlement of an individual pile during a loading test under 20 tons. Important decisions were based on this simple assumption. Yet, if we really measure the settlements - and I have done it very often - we find that the settlement of the pile foundation may range anywhere between five and five hundred times that of the individual pile. The failure of experienced engineers to know this commonplace fact can only be due to an idiosyncrasy against measurements, combined with a habit of mistaking the absence of any visible signs of settlement with the absence of settlement.

Owing to the failure of the practical engineers to produce a reliable code for the design of foundations out of their own resources, the antagonism between dogmatic theory and inadequate experience merely led to a state of stagnation which reached its climax in the first decade of our century. However, the subsequent development of soil mechanics eliminated this state of stagnation in a radical fashion. The act of elimination started with an attack on time-honored and sacred institutions such as the classical earth pressure theories, the pile formulas, and the tables of safe bearing values of soils. The attack left a heap of ruins with very little to replace them. Intensified experimentation with soils led to the discovery of a whole series of physical factors of vital importance which escaped the attention of the investigators of the previous generations. Foremost among these factors are those which determine the gradual increase of settlements at a constant load. The knowledge of the existence of these new factors made it necessary to rebuild the theories in accordance with our increased knowledge of the properties of the material. While building a theory one is painfully conscious of the approximations involved and of the gaps which it leaves after it is finished. In order to make these theories applicable to actual cases, it became necessary to observe the performance of full-sized engineering structures far more carefully than it was ever done before. Thus the spirit of conscientious observation characteristic of the middle of the nineteenth century experienced a revival on a very much higher plane. The visual inspection was supplemented by systematic and precise measurements, and the danger of fatal omissions was reduced by a superior knowledge of the physical nature of the processes involved. This inauguration of a new era of direct and intimate contact between the engineer and his structures alone would suffice to justify the time and labor invested in soil mechanics during the brief period of its existence. Our theories will be superseded by better ones, but the results of conscientious observations in the field will remain as a permanent asset of inestimable value to our profession. Whoever peruses the Proceedings of this Conference cannot fail to be impressed by the new spirit disclosed by the text and the diagrams of these volumes. The days of abstract foundation philosophy are gone forever. And so are the days of unwarranted generalizations based on inadequate evidence.

Truth and Fiction in Textbook Engineering. The second outstanding impression which I received while abstracting the engineering periodicals, eighteen years ago, was produced by my discovery of the complete absence of what is commonly called adequate evidence. As the years passed, one formula after another appeared, and one rule after the other was advertised, but when I attempted to locate the empirical evidence on which the claims were based, I found there was none or almost none. This paradoxical fact leads us to one of the most important tasks to be performed if our professional standard is to be elevated. It consists in revising our attitude toward evidence.

In pure science a very sharp distinction is made between hypotheses, theories, and laws. The difference between these three categories resides exclusively in the weight of sustaining evidence. On the other hand, in foundation and earthwork engineering, everything is called a theory after it appears in print, and if the theory finds its way into a textbook, many readers are inclined to consider it a law. In order to find out to what extent a theory deserves its name, it suffices to dissolve it into its principal components and to examine each one individually.

Every theory consists of three parts, a set of assumptions, a process of reasoning, and a final result. Since the validity of the reasoning can easily be verified, it suffices to concentrate our attention on the first and last parts. Each of these may be dissolved into words expressed by symbols and figures. The first requirement for an acceptable theory should be that the words have a definite meaning. Many of the terms which are used in textbooks on foundation engineering have a very vague one, if any. In this connection, the term "safe bearing value of piles" may be mentioned. Some eight years ago a very expensive factory was established on a whole forest of piles, between 60 and 80 feet long. The machinery erected in this factory was extremely sensitive to unequal settlement. The bearing capacity of the individual piles was most satisfactory. According to all the textbooks and manuals relating to this subject, the load on the piles was equal to or smaller than one-half of the safe bearing value. Yet the owner of the factory refused to share this opinion, because some parts of his factory settled through a distance of one foot. In western Austria stands a post-office building with continuous footings on a very compact bed of sand and gravel, 23 feet thick. The building exerts a pressure of 2.5 tons per square foot on the ground. I do not know of any building code or of any textbook which does not contain a very much higher figure for the safe bearing value of such a stratum. Nevertheless, the settlement of the building ranged between two and three feet. The same books which inform the patient reader on the safe bearing values also contain instructive tables with the values of the coefficient of internal friction of fat and of lean clays and loams. Yet with some skill and experience in laboratory procedure, one can get almost any specified friction value for a given clay. A score of other examples could easily be added.

Considering these unpleasant facts, one of the first requirements for a clean-up in the field of foundation engineering is insistence on a satisfactory explanation of the meaning of the terms. If a theory claims to furnish a safe bearing value, or if it operates with the coefficient of internal friction of clay, one may as well stop reading, unless the author explains in detail what he means by these terms.

The second requirement for an acceptable theory consists in the presence of adequate evidence for the assumptions. If these assumptions were obtained by a radical simplification of reality, which is the rule in connection with theories pertaining to soils, the evidence for the results must be presented. Whatever evidence is available can be classed into one of the following five categories:

(a) No evidence whatsoever;
(b) Evidence obtained by distorting the facts;
(c) Unbalanced evidence; that is, evidence obtained by eliminating all those facts which do not sustain the claim;
(d) Inadequate evidence, covering the entire range of present knowledge, yet insufficient to exclude the possibility of a subsequent discovery of contradictory facts; and
(e) Adequate evidence.

No honest business man and no self-respecting scientist can be expected to put forth a new scheme or a new theory as a "working proposition" unless it is sustained by at least fairly adequate evidence. In any case, we expect him to inform us on the uncertainties involved. Therefore it is surprising to find upon closer scrutiny that many of the accepted rules of foundation engineering are based either on no evidence whatsoever, or on unbalanced evidence, and that the textbooks do not mention this serious failing. These rules seem to pass from one generation of textbooks into the next one by a process of diffusion, whereby the scruples regarding the inadequacy of the evidence disappear.

One of the popular assumptions for which there is no evidence whatsoever is the claim that the coefficient of internal friction of fine moist sand or of clay soils is identical with the tangent of the angle of repose, which again is supposed to be identical with the slope obtained by dumping the material from a low trestle or out of a box. In spite of repeated and convincing proofs of the invalidity of this assumption, it continues to appear in textbooks and to mislead unsuspecting engineers. Another case of a dogma sustained by no evidence whatsoever is the assumption that the hydrostatic uplift exerted by the water in a concrete or in a clay acts over not more than one-third or one-half of the area subject to uplift. This assumption is based exclusively on personal opinion and maintained by majority vote. Yet it has a decisive influence on the design of important and very extensive structures, including the highest storage dams. As soon as we attempt to verify this opinion by physical experiments, such as those described in a paper in the second volume of the Proceedings, we find that it is grossly erroneous. A third dogma supported by no evidence is the assumption of a definite relation between the angle formed by the planes of shear in a cohesive soil and the angle of internal friction in Coulomb's equation for the shearing resistance of such soils. The fallacy involved in this dogma is analyzed in an article of the first volume of the Proceedings. It invalidates the so-called accurate theories of the stability of slopes which are based on this fallacy.

As a classical example of a prescription which is in part based on unbalanced evidence, and in part on none at all, the Engineering News Formula may be mentioned. This formula is supposed to represent the relation between the weight and the drop of the hammer, the penetration produced by the blow, and the safe bearing value for the pile. The real meaning of the term "safe bearing value" is nowhere defined. The numerical results furnished by the formula can only be defended by wilfully suppressing at least one-half of the existing evidence. In the form which is intended to apply to piles which are driven by a steam-hammer, the denominator contains a constant, 0.1, which originated in pure imagination. If we discover that a commercial advertisement is based on such evidence, we call it bluff and reject it. However, in the field of foundation engineering the critics are far more lenient. The formula has been published over and over in texts and manuals without any warning to the reader, and it continues to represent an integral part of the majority of building codes and of government regulations. Another example of a conception artificially maintained by means of unbalanced evidence is the theory that the lateral pressure of the earth on the back of a supporting structure should increase, like a hydrostatic pressure, in direct proportion to the depth below the surface. This theory originated some 150 years ago. Under certain conditions, specified in one of the papers of the first volume of the Proceedings, the hydrostatic pressure distribution really exists. However, under other conditions of great practical importance, such as those which exist on both sides of a timbered cut, the distribution of the lateral pressure may be very different from that required by theory. Nevertheless, year after year, the dogma of the hydrostatic pressure distribution is handed out as gospel, and contradictory evidence is consistently ignored.

Grossly unbalanced is also the evidence offered in support of the claim that the settlement of a building can be predicted from the results of one or of several small-scale loading tests performed at the level of the base of the future foundation. For each case of evidence for this claim which has thus far come to my attention, I can quote at least two cases out of my own experience which contradict it. Considering these facts, the academic merits of the underlying theory are utterly irrelevant, because the empirical arguments suffice to invalidate the claim.

In most cases the unbalanced character of the evidence is due merely to our inadequate knowledge. Into this class belongs the assumption that the results of properly conducted shearing tests on so-called undisturbed samples of clay are always identical with the shearing resistance of the untouched clay deposit. For many years I accepted this assumption until I came across several cases which contradict it. This experience makes it necessary to find out, by future observations, the limits of the validity of the original assumption.

I do not doubt that the majority of engineers adopt the suggested attitude toward evidence in all their business transactions. In case they should decide to introduce it also into their professional relations to mother earth, radical changes in their attitude toward accepted rules could not fail to ensue.

Outlook. The skeptical attitude towards our conceptions, and the readiness to modify them in accordance with increasing knowledge of the material, must be considered the second outstanding achievement of soil mechanics. By patient observation we have learned to discriminate between what we really know and what we merely believed. The amount of knowledge sustained by adequate evidence is appallingly modest, and the number of factors with a decisive influence on soil behavior is very much greater than was expected twenty-five years ago. The successful analysis of the reaction of the earth to changes produced by loading or by excavation was paid for by a heavy sacrifice of simplicity. Moreover, the severe restrictions on further progress along purely theoretical lines have become obvious. One of the most instructive examples of these limitations is to be found in the theory of arching in soils behind the timbering of cuts. The theory demonstrates that arching develops. It discloses the mechanics of arching, and reveals the limits between which the distribution of the lateral pressure of the earth may range. At the same time it leaves no doubt that the real distribution of the pressure depends on the method of constructing the timbering. Since we are not in a position to evaluate this influence on the basis of abstract reasoning, we are obliged to secure the required information by direct measurement of the pressures in full-sized cuts. We face a similar situation in almost every other field of soil mechanics. Our advanced knowledge of the mechanics and physics of soils makes it possible to grasp most of the essential factors which govern the stress and strain and the equilibrium of real earth. It has brought to us a realization of the extremely narrow limits of the validity of the older theories, and informs us of the existence of sources of danger which previously were hardly suspected. Nevertheless, in order to make the step from the qualitative appreciation of what is going to occur to a quantitative forecast requires accurate and systematic observations on full-sized structures.

Foremost among the sources of error requiring thorough investigation is the difference between the soil in its original state, and after it is delivered in the laboratory. In some cases the correction for the errors produced by the effect of sampling and handling can be made by computing the deformation of the subsoil for earlier stages of construction, and subsequently comparing the results with those of direct measurement. The first volume of the Proceedings contains a very instructive example for a successful operation of this kind.

Since we have achieved a reasonably clear conception of the possibilities and limitations of future research, the function of this Conference is simple. It consists essentially in establishing personal contacts between those who are interested in the subject from a theoretical or a practical point of view, and in stimulating exchange of experience. Though it originated not more than twenty-five years ago, soil mechanics is already old enough to have acquired the modesty which springs from experience. We know today that nothing worth while can be accomplished in this discipline without the intelligent and patient cooperation of the practicing engineer in the field. Some of the most valuable contributions to the Proceedings are a direct result of such cooperation. For this reason, we are very happy to welcome among the guests of the Conference a great number of outstanding executives and experienced construction engineers. Since these men owe their success and their professional standing to a keen discrimination between reality and fiction, I am sure they will appreciate our feelings against half-baked textbook wisdom, and assist us in getting down to tangible facts.

ADDRESS BY DANIEL E. MORAN

Vice President of the Conference and Chairman of its American Committee

As Chairman of the American Committee I can add but little to the eloquent addresses of welcome you have just listened to. The Committee and the Officers of the Convention cannot but feel gratified by the world-wide response to the invitations issued in the name of the great University whose guests we are. These responses have come from engineers and scientists from all parts of the world, from our good neighbors Canada and Mexico, from Cuba and the Republics of South America, from Great Britain, and ten of the principal countries of Continental Europe, from Africa, Asia and Australia, as well as from the United States of America.

Without specific authority I may say that we as "members" and guests are greatly honored by having been bidden to this conference sponsored by the oldest and greatest of American Universities, now celebrating the 300th Anniversary of her founding. For you, Mr. President, we wish long and happy years of service, for Harvard itself we hope and pray that she may continue in the future as in the past, free, unbidden and unafraid, holding high the torch of enlightment and leading men and women in the paths of wisdom and knowledge.

The wide-spread and remarkable interest in this Conference can be readily understood when the importance and vital necessity of the subject is appreciated. For years, Engineers and Scientists have studied, classified and tested structural materials of all kinds. But all structures depend for stability on contact with some stress resisting solid material, a part of the Earth's Crust. The material may be any one, or a combination of several of an indefinite number or kinds of material. The difficulties in the way of evaluating these different kinds of material, in determining the laws governing their behavior, and in coordinating results, seemed so insurmountable that until recent years no real attempt, no practical start was made. True, some physicists, mathematicians, and engineers evolved theories generally based on arbitrary, sometimes erroneous assumptions, but the results were of questionable value in guiding engineers. Until a few years ago little had been done, and even now little is generally known of the facts which have been developed by your efforts. As a simple example; well-known text books, treating foundation design, now in common use define clay as "A general name for cohesive soils" and purport to give its physical, chemical and geological properties, (Hool & Kinne Pg. 361-2--Foundation Abutments and Footings, 1923) but say no more about its structural properties than Baker in 1889, who stated that "damp clay will squeeze out in every direction when a moderately heavy pressure is brought upon it" (Baker Pg. 190--A Treatise of Masonry Construction, 1889). Furthermore, these books recommend "as essential to the proper design of foundations the accurate determination of local conditions--the character of the underlying strata--and the making of excavations or borings" (Jacoby & Davis--Foundations of Bridges and Buildings,--Page 585, 1925) and then fall back on the recommendations of Baker to determine the bearing capacity "by direct experiment, good judgment and experience" (Page 188); never a word about soil mechanics or what may be done with a boring sample or the dangers of basing designs on inadequate or improper borings.

Until Terzaghi's articles appeared in the Engineering News I know of no published explanation, in the English Language, of the underlying reasons for the consolidation of clay under increased loads.

Twenty years ago the matter of foundation design was largely an art, the designers being guided by uncorrelated experiences, rules of thumb, prejudices, and wild guesses, all made in the name of "good practice". Today order and rational designs are slowly taking the place of ignorance and error.