THE PTI METHOD FOR GRADE SLAB DESIGN – A DISSENTING VIEW AND AN ALTERNATIVE

By

RICHARD P. MARTTER

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INTRODUCTION

Most of you know something about the PTI Grade Slab Design Procedure. Many of you have used it for years in your everyday design work. It occupies approximately 25 pages of the Building Code. It has become the accepted and near-universal method for designing the huge volume of soil-supported grade slab foundations presently constructed on active clay soil.

It was originally created by Professor Kent Wray (Wray 1978) while a graduate student at Texas A&M University and was submitted as his doctoral thesis over 30 years ago. It has been maintained and further developed by Professor Robert Lytton, an eminent Professor of geotechnical science at Texas A&M University, whose chosen field of study is the properties of clay soils.

With such a pedigree, then, why are some engineers in both the structural and geotechnical sectors unhappy with it?

In my opinion, the answer has to do with the philosophical difference between science and engineering and our failure to recognize and allow for that difference. In brief, we took a project, the development of this design procedure, which is primarily of an engineering nature, and solved it with a scientific approach.

It is not so much that it is unsatisfactory work but rather that we made the wrong choice to begin with. We should have chosen an engineering approach to do the development of an engineering procedure.

I began doing grade slab design work in Texas 34 years ago. When traveling back and forth between field and office, I was struck by how the field and office are two different worlds; but our design process did not accommodate that difference. That impression is still with me today.

The paper aims to define and explain that difference and its consequences and then suggest a way to bridge the gap.

We will begin by putting forth a set of principles on which our arguments will be based. In a sense, these principles are quite obvious, but I think it will be helpful in understanding my direction if you keep them in mind.

PRINCIPLES

1. We must distinguish between science and engineering:
   - Science is the discovery and measurement of the properties of natural materials.
   - Engineering takes those discovered properties and puts them to use in practical applications.
   - Engineering is science plus the art of application.
   - Science strives to avoid uncertainty. Engineering must accept uncertainty and grapple with it.
   - We must be careful not to assign an engineering task to a scientific team and vice versa.

2. Geotechnical engineering involves working with materials that are usually something of a mixture in their natural state, making their properties difficult to measure accurately. These properties vary widely, and often randomly, within the scope of a single project.

In the introduction to his text on foundation design, Professor Bowles cautions that

> *"….the reader should realize that foundation loads and soil properties are not likely to be known to a precision closer than ±10 to 20 percent."* (Bowles 1977)

From there on, it is simple mathematical logic to recognize that if the variable inputs to a procedure or program have a significant margin of error, we must recognize the consequences of that error in the output of the procedure.

Or—as has been said once or twice before—"garbage in, garbage out."

3. A computer program to replicate a physical system is based on a model of that system. The output of the program is dependent on the model and is no more comprehensive than the model. The computer program

operates only the system represented by the model—no other system. The model must represent a condition that is dependably consistent and typical. Random activity cannot be modeled.

> *"The analysis stage of the GE (geotech engineering) process is the one on which much of our profession's educational efforts are spent, and because of this it is the area in which most young engineers excel. It is also the area in which IT tools are of greatest use, and as a result some practitioners can become lost in the details and joys of such analyses, avoiding the hard work of thinking and exercising judgment about what to analyze and what the results mean. The result can be an engineer who runs multiple analyses varying every parameter possible and produces lots of impressive graphs but never questions whether the model is appropriate for the actual site conditions."* (Murv 2006)

4. Every engineering procedure that is based on theory must have experimental verification before it is accorded validity. The importance of this principle is paramount when the engineering is being done in a field where there are few precedents for comparisons. These following are examples:

- Nuclear fission in the 1930s and 1940s:

  "It doesn't make any difference how smart you are. It doesn't make any difference how beautiful your theory is. If it isn't confirmed by experimental results, it is wrong." (Feynman 1964)
- Geotechnical engineering in the 1930s, 1940s, and 1950s.

The following quotation is from a biography of Professor Karl Terzaghi (Goodman 1998), who worked during that period. Professor Terzaghi, as many of you know, has been designated "The Father of Geotechnical Engineering." He often found himself at odds with academics who never left their desks to investigate and learn from field conditions. He had this to say about them:

> *"Their highest ambition is to faithfully use the rules they have learned, without considering further whether their results are right or wrong."* (Goodman 1998)

5. We have just talked about the importance of experimental verification of every scientific and engineering theory. An extension of that concept is the study of *failed structures*. A great deal can be learned from the study of failed structures that can be learned in no other way. Professor Henry Petroski of Duke University wrote several books on the subject, in which he said:

> *"There are two approaches to any engineering or design problem: success-based and failure-based. Paradoxically the latter is always far more likely to succeed."* (Petroski 1985)

Our engineering community generally recognizes this truth. The failure of a major building or bridge attracts hordes of technical experts to search exhaustively for the cause of the collapse to learn from it.

PTI DESIGN PROCEDURE

My approach to this discussion will be primarily that of a structural engineer. I will try not to intrude into geotechnical or purely scientific areas because I have no expertise there.

Also, my experience, and thus my commentary, relates to work done in Texas and, to some extent, adjacent states. I am informed that grade slab foundations in areas such as California and Arizona perform in a significantly different manner.

But, lest you think I am taking just a small piece of the pie in restricting this commentary to "little old Texas," let me mention that in the Dallas Yellow Pages, there are 14 pages devoted to listings of companies that specialize in the repair of residential foundations.

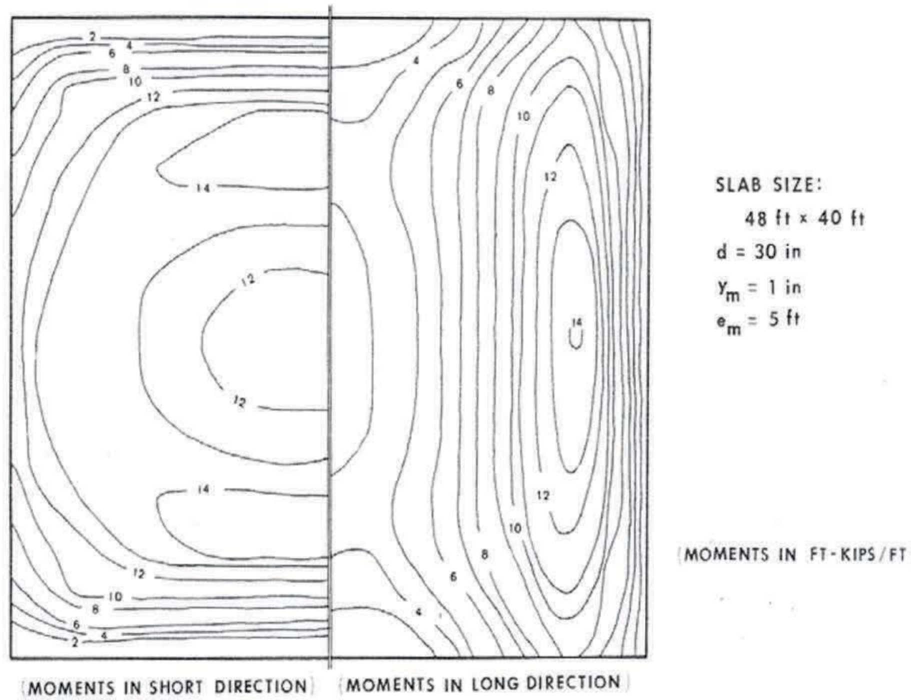
Next we are going to consider some of the ways in which the design method operates and the structural model on which it is based. Virtually all of the work is based on models of simple square or rectangular residential grade slab foundations. The computer program loads them along their perimeters—all four sides—with the pressure of soil heaving upward or with the edge left unsupported when the soil dries out and subsides (Fig. 1). The loads are always fully applied on all four sides. There are no cases of loads applied on portions of the perimeter or of combined loading—that is, some edge or center lift or some neutral. It is a very neat and exclusive model that is quite suitable for basic and limited scientific work.

EXAMPLES OF INACCURATE ENGINEERING IN PTI GRADE SLAB DESIGN PROCEDURE

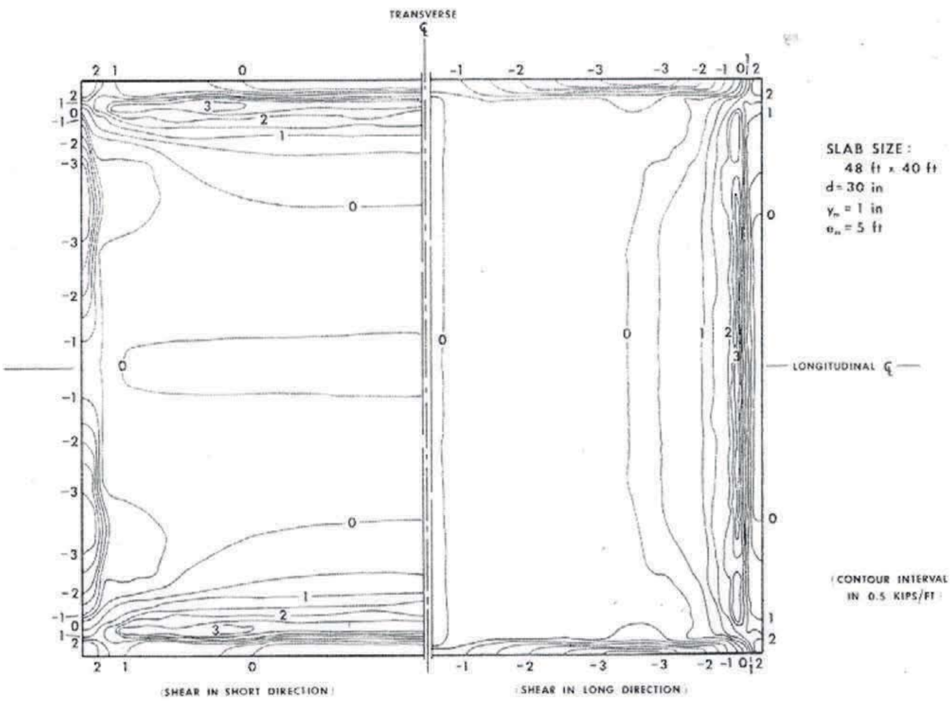
1. Appendix A-10 (second edition) (Fig. 2)

This Appendix was originally put in to show how the computer formulas were generated from the printout of the plots of shear and moment. It was put in the first and second editions but was completely left out of later editions.

What this Appendix says is that the values derived from the center-lift shear plots were checked by conventional methods and found to be approximately double what they



Typical distribution of center lift bending moment over surface of slab.



Typical distribution of center lift shear force over surface of slab.

Fig. 1—Figures 5.2 and 5.6 from PTI Design of Post-Tensioned Slabs-on-Ground, third edition, 2008.

were expected to be. That is a 100% variation in a primary output. However, instead of going back through the procedure to find and repair the discrepancy, the answers were simply multiplied by 0.5 (that is, all of the answers); and the design equations are derived from the plotted curves.

This appears to be questionable from a number of standpoints:

(a) The discrepancy, whatever it is, remains buried in the procedure, detracting from its credibility. If a numerical procedure is inaccurate, it should be investigated and revised.

(b) It further strains credibility to believe that a single constant correction would be equally applicable to the full range of the curved functions.

(c) What about moment values derived from these curves? Moment and shear have a dependent relationship. Can moment values be properly generated over a broad range of shear values that were revised with the use of a single constant?

Observe the equations for center-lift moment. They are ponderous and complex and would appear to be appropriate products of a well-researched derivation. However, sometimes things are not what they appear to be (Fig. 3).

Graphical Results of Computer Studies of Moment, Shear and Deflection

Graphical results of computer studies of the moment, shear and deflection of slabs with various beam depth and soil movements are presented for both center lift and edge left conditions in the following sections. The graphs presented in this Appendix are the primary basis of the design equations presented in Chapter 6.

A.10.1 Maximum Negative Moment for Center Lift Condition.

Plotting negative moment as a function of edge moisture variation distance (edge penetration), the moment data for the center lift condition is shown in Figs. A.10.1, A.10.2, and A.10.3. It can be noted that the following trends appear:

1. Increasing moment with increasing edge penetration
2. Increasing moment with increasing beam depth
3. Increasing moment with increasing differential soil movement
4. Increasing moment with increasing perimeter load

One contradiction to the trend of increasing moment with increasing edge penetration is observed when a heavy perimeter load, edge penetration greater than 5 feet, and a differential soil movement of 1-inch are included as variables. Close inspection of the computer results for the 5-ft edge penetration problems revealed the slab edge to either be in contact or very close to being in contact with the supporting soil. Thus, when the 8-ft edge distance problem was analyzed, there was little opportunity for an increase in the induced moment since the soil would support the slab as it was being compressed into the subgrade.

A.10.2 Maximum Positive Moment for Edge Lift Condition

As with the negative bending moments, the magnitude of the positive bending moment for the edge lift condition first

Analysis of the distribution of the shear force as well as the location of the maximum shear force showed it to be similar to that of the center lift shear forces.

A.10.4 Maximum Shear Forces Resulting From Perimeter Loading and Center Lift Conditions.

Shear forces resulting from the center lift analysis were found to be approximately two times larger than values obtained from check calculations using statics. The shears were also found to be approximately twice as large as the shear forces calculated by computer program PRESS2 for similar conditions of loading and soil support. The reason for the large values obtained from the SLAB2 analysis are attributed to the numerical technique used to accomplish the calculations and the relatively large elements used in the analysis. Consequently, all of the center lift shear results were multiplied by a factor of 0.5 in order to better comply with the principles of statics. The modified values of shear force are plotted as a function of edge penetration distance in Figs. A.10.14 through A.10.16. From these figures, five observations are noted:

1. In general, shear force increases as edge penetration distance increases.
2. Shear force increases as beam depth increases.
3. Shear force increases slightly as perimeter load increases.
4. Shear force increases only slightly as the edge penetration distance increases from 0 to 2 feet.
5. Shear force increases slightly as the differential soil movement increases.

Analysis of the distribution as well as the location of the maximum shear force showed it to be concentrated near the perimeter edge of the slab. In each case, the maximum shear force occurred within one-β distance from the slab edge.

Fig. 2—Appendix A.10: PTI Design of Post-Tensioned Slabs-on-Ground, second edition, 1996.

Center Lift Moment

Long Direction

$$M_L = A_o\left[B(e_m)^{1.238} + C\right] \qquad \textbf{(6-13)}$$

where:

$$A_o = \frac{1}{727}\left[(L)^{0.013}(S)^{0.306}(h)^{0.688}(P)^{0.534}(y_m)^{0.193}\right] \qquad \textbf{(6-14)}$$

and for:

$$0 \le e_m \le 5 \qquad B = 1, C = 0 \qquad \textbf{(6-15a)}$$

$$5 < e_m \qquad B = \left(\frac{y_m - 1}{3}\right) \le 1.0 \qquad \textbf{(6-15b)}$$

$$C = \left[8 - \frac{P - 613}{255}\right]\left[\frac{4 - y_m}{3}\right] \ge 0 \qquad \textbf{(6-15c)}$$

Short Direction

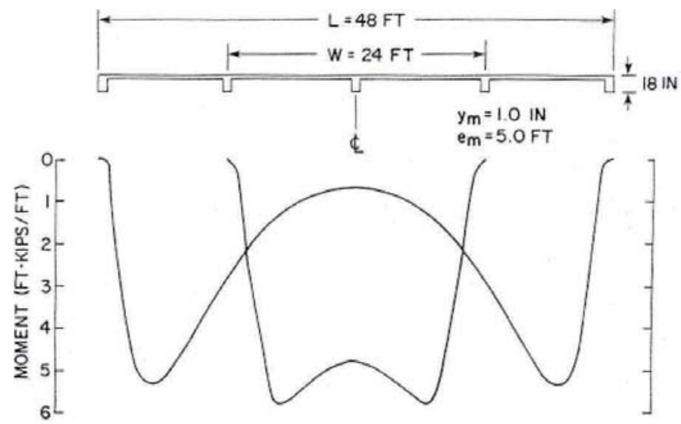
For $L_L/L_S \ge 1.1$:

$$M_s = \left(\frac{58 + e_m}{60}\right)M_L \qquad \textbf{(6-16)}$$

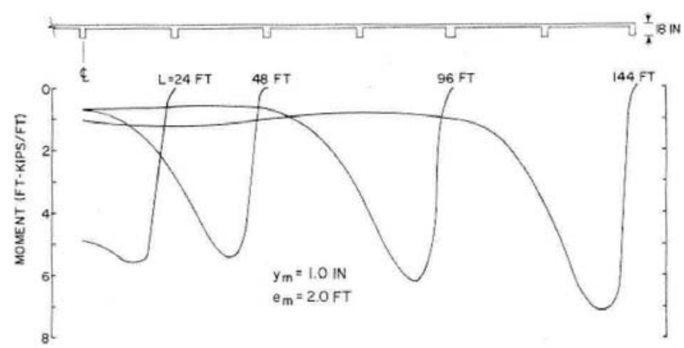
For $L_L/L_S < 1.1$:

$$M_S = M_L \qquad \textbf{(6-17)}$$

Fig. 3—Equations from PTI Design of Post-Tensioned Slabs-on-Ground, third edition, 2008.



Typical variation of moment along the longitudinal and transverse axes of a rectangular slab.



Typical variation of moment along the longitudinal axis as slab length increases.

Fig. 4—Figures 5.1 and 5.3 from PTI Design of Post-Tensioned Slabs-on-Ground, third edition, 2008.

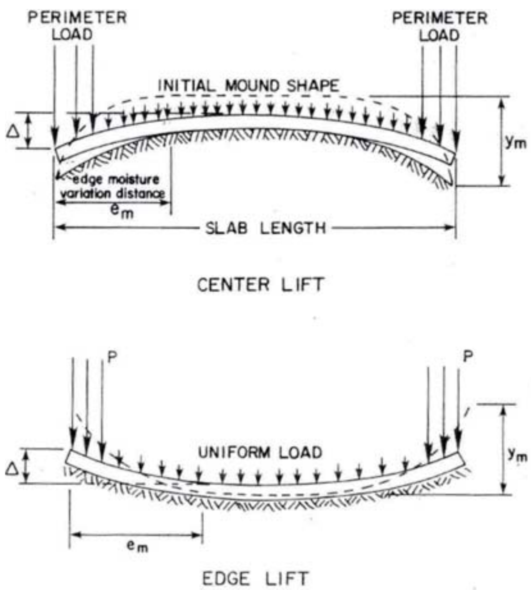
2. Early on in my use of the procedure, my attention was caught by the shape of the various curves showing center-lift moment (Fig. 4). They show narrow negative-moment peaks at the support points. Statics have taught us that such sharp reversals are caused by narrow supports. Just from the shape of the curves, I would guess the supports to be approximately 1 or 2 ft (0.3 or 0.6 m) wide. Such a situation conflicts with my 30-plus years of experience following the behavior of typical residential slabs on active soils. I have studied many center-heave situations and would "bet the farm" on the typical soil support to be at least 6 to 8 ft (1.8 to 2.4 m) and sometimes 10 or 12 ft (3.0 or 3.6 m) wide. For one thing, it would have to be.

The support is composed of a sector of damp clay thrusting upward against a very rigid concrete slab soffit. The (possibly) initially narrow portion of soil thrusting upward would not be able to sustain the elevated bearing pressures and would simply spread out in a much wider support. Considering the plastic nature of damp clay, it would happen quite naturally (refer to Fig. 5).

The width and the stiffness of the supports also have a direct effect on shear stress. This is of interest when using the procedure because shear stress is often a controlling design output.

It is of special empirical interest in the field because of the absence of any evidence of high shear stresses. In years of high-volume grade slab design work, I have never encountered a crack attributable to shear, nor have any of my colleagues.

From an engineering standpoint, this situation is incomprehensible. Why should shear stress be able to control designs, as it often does, by the use of the design procedure, when it is never observed in the field? Something here does not add up.



Soil structure interaction models (108).

Fig. 5—Figure 4.2 from PTI Design of Post-Tensioned Slabs-on-Ground, second edition, 1996, and Section 3.5.3 of third edition, 2008.

Only one conclusion is possible: The computer program is still giving us excessively high values of shear stress, even after the stress was reduced to 50% of the original computed value. But at least now we know why it is occurring.

In a crude but graphic way, the small figure on page 13 of the third edition (Fig. 5) shows how this condition really exists in the field. In center-lift mode, proceeding inward from the edge, the slab above and the soil below gradually come together, the pressure in the soil gradually increases, the vertical load is transferred to the soil over a broad width of the "hump," and shear stresses are moderate while bending stresses become critical.

With plastic soil, it could not be any other way.

3. An extension of this thinking may explain something else. The "beta" distance is defined as the approximate distance inward from the edge of the slab to the point of maximum moment. With this concept in mind, as we inspect a slab that is "malfunctioning" in the center-lift mode, why don't we see a pattern of cracks at or near the beta distance?

Again, I have never seen such a pattern. Invariably, the cracks appear to be randomly located or usually located in response to some anomalous water source or water deficiency. However, they seem to have nothing to do with "beta"—that is to say, I question the existence of beta and thus the assumption of the basic model.

4. At the top of page 37 of the third edition, there is a statement with which I thoroughly agree:

> *"Edge lift moments are difficult to estimate as the soil loading is unknown."*

But if we check Eq. (6-18) (Fig. 6)—the basic formula for the calculation of long direction edge lift moment—we find an impressive instrument making use of six variable inputs, each with its own highly specific exponential function. Where did this come from if rational loading patterns are not available for reference? It appears we have a dichotomy between conceptual uncertainties and numerical specifics.

Edge Lift Moment

Long Direction

$$M_L = \frac{S^{0.1}\,(he_m)^{0.78}\,(y_m)^{0.66}}{7.2L^{0.0065}P^{0.04}} \qquad \textbf{(6-18)}$$

Fig. 6—Equation (6-18) from PTI Design of Post-Tensioned Slabs-on-Ground, third edition, 2008.

Further, for the calculation of short-direction edge-lift moments, we have Eq. (6-19) (Fig. 7) (refer to Example 5 below), which is a function of Eq. (6-18), thereby further compounding the basic uncertainties.

5. Equation (6-19) is a method for figuring the increase in edge-lift design moment in the short-dimension direction of a rectangular (not square) slab. There is no way to analyze it because it arises magically from the depths of the computer program. There does not seem to be any reason for the increases it generates on more typical wider slabs, and experienced engineers are accustomed to looking for reasons because they are the keys to understanding.

Short Direction

For $L_L/L_S \geq 1.1$:

$$M_S = h^{0.35}\left[\frac{19 + e_m}{57.75}\right]M_L \qquad \textbf{(6-19)}$$

For $L_L/L_S < 1.1$:

$$M_S = M_L \qquad \textbf{(6-20)}$$

Fig. 7—Equations (6-19) and (6-20) from PTI Design of Post-Tensioned Slabs-on-Ground, third edition, 2008.

It is important to know that this is not some incidental, sideline formula. When given inputs that are characteristic of highly active but routinely encountered Texas soils, it outputs a factor that increases the edge-lift moments by anywhere from 35% to sometimes over 50%—just because of a (usually) moderate departure from the square shape, regardless of overall size or any other obvious factor.

The questionable nature of this concept is further illustrated in its use in Appendix A.4 in the third edition, where we are introduced to the concept of "overlapping rectangles." Rectangles B and C are designed as if they are long, narrow rectangles with a consequent increase in a short-side moment of 33%. But the "long sides" of the two rectangles exist largely as dashed lines inside the perimeter of Rectangle A or as short sides of that rectangle. The "long side" in the calculation is really the "short side" of the "exposed" rectangle. The assumptions of Eq. (6-19) have been completely submerged.

We seem to have forgotten that computers just run calculations. We should never depend on them to do our conceptual thinking for us.

6. In the third edition, e_m has been given an entirely different derivation from that in previous editions. The edge-lift and center-lift figures have changed—one getting larger while the other usually gets smaller than in previous editions (Fig. 8). One wonders how such a conceptual change would be digested by the formulas in which e_m is a variable, and which formulas have not changed.

The original idea was that the increase in the size of one would offset the decrease in the other, and the final result would have a negligible effect on the design values. Of course, this did not happen; and there was very little reason to believe it should have happened. In the first case, e_m is based on the weather cycle, whereas in the second case, it is based on a soil property. This is nothing less than a change in design philosophy without an appropriate change in the mathematical core of the procedure. The e_m values are variable inputs in several of the design equations; and, in each case, they carry different exponentials.

7. Equation (6-13) (Fig. 9) for the calculation of center-lift design moments, along with the formulas that support it, has two zones of operation separated at the point where e_m = 5.0 ft (1.5 m). There is a significant discontinuity in the curves for moment values at that point (Fig. 10). At y_m values of 4.0 and up, there is virtually no discontinuity. But the discontinuity is quite apparent at $y_m = 3.0$; and at $y_m = 1.5$, it approaches 35%.

Center Lift Moment

Long Direction

$$M_L = A_o\left[B(e_m)^{1.238} + C\right] \qquad \textbf{(6-13)}$$

where:

$$A_o = \frac{1}{727}\left[(L)^{0.013}(S)^{0.306}(h)^{0.688}(P)^{0.534}(y_m)^{0.193}\right] \qquad \textbf{(6-14)}$$

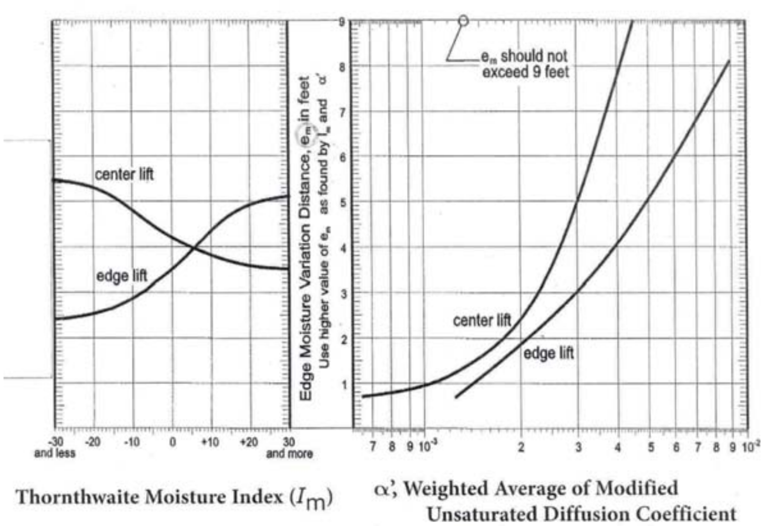
and for:

$$0 \leq e_m \leq 5 \qquad B = 1, C = 0 \qquad \textbf{(6-15a)}$$

$$5 < e_m \qquad B = \left(\frac{y_m - 1}{3}\right) \leq 1.0 \qquad \textbf{(6-15b)}$$

$$C = \left[8 - \frac{P - 613}{255}\right]\left[\frac{4 - y_m}{3}\right] \geq 0 \qquad \textbf{(6-15c)}$$

Fig. 9—Equations (6-13) through (6-15c) from PTI Design of Post-Tensioned Slabs-on-Ground, third edition, 2008.



Moisture variation distance e_m *selection chart.*

Fig. 8—Figure 3.6 from PTI Design of Post-Tensioned Slabs-on-Ground, third edition, 2008.

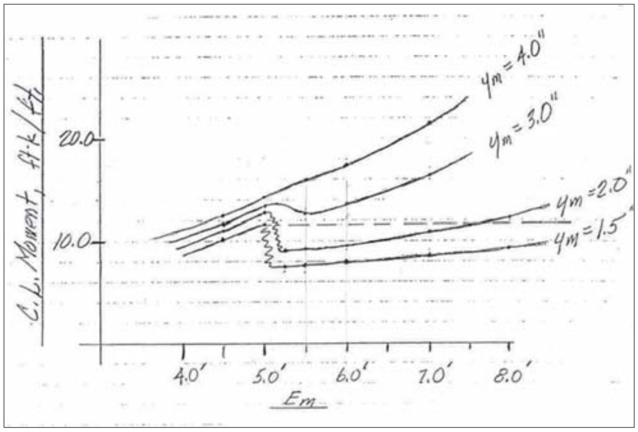


Fig. 10—Discontinuity in M_L *curves at* e_m *= 5.0 ft (1.5 m).*

In the early 1980s when I was using the procedure manually and had very little experience with the method, I was surprised and disappointed. With no advance warning and no explanation, such a discontinuity was very unsettling.

Some years later, an explanation for the discontinuity was presented. It was said that at low values of y_m and higher values of e_m, the edge of the slab was never far off the soil; and as the higher e_m's were attained, the end of the slab would sag down and make contact with the soil, which interfered with its free flexure and thus caused the discontinuity. But there were two problems I did not figure out until later.

One was that the scenario simply could not take place. Let's start from the beginning when the edge of the slab would be resting on the soil. It would slowly separate as the soil subsided, beginning at the edge and progressing further in, in proportion to the subsidence (Fig. 5).

The reverse would be simply that the soil would close the gap starting at the innermost point and progressing outward. There would be no discontinuity—simply a reverse of the original process.

Any other scenario for this activity would involve some unusual circumstances that are not part of a typical discussion.

The other circumstance that conflicts with this scenario is that the curves on the far side of the discontinuity do not reflect such a support at the end. They seem to have their normal curved shape but are displaced by the amount of the discontinuity.

There is no evidence of a "maverick" support out at the end of the cantilever. So if there were a support there, the computer did not know it.

Again, here we seem to have structural procedures that are not at the same level as the geotechnical procedures. Thirty-four years have passed and nothing has been done to address the discontinuity.

8. Geotechnical work, such as the design of foundations, involves working with different types of soils blended in endlessly variable combinations at random locations subject to the whims of natural forces that have had millions of years and endless supplies of water, wind, and seismic activity to mix things up. The geotechnical sector of work is exceeded in the uncertainty of predicted results by perhaps only meteorology or economics.

Geotechnical engineers do recognize this but they do not often give it much emphasis. The following is a quote from a typical Texas geotech report:

> *"Actual soil movement is difficult to predict due to the many unpredictable variables involved."*

Also pertinent (again) is a caution given by Professor Bowles in the Introduction to his basic text *Foundation Analysis & Design*:

> *"The reader should realize that foundation loads and soil properties are not likely to be known to a precision closer than ± 10 or 20 percent."* (Bowles 1977)

Bowles, who was at the dawn of the computer age, went on to say

> *"...electronic calculators...tend to give a fictitiously high precision to computed quantities."*

Another prominent geotechnical engineer said,

> *"Geotechnical engineers must become as proficient in statistics and probability as they are in stability analysis and settlement analysis."* (Murv 2006)

The message from the aforementioned authorities is that all thinking with respect to geotechnical problems must accommodate the existence of uncertainty in the range put forth by Bowles; and to be consistent, to follow through the design procedure with some valid and representative methodology. However, our design procedure does nothing of the kind.

With a single basic formula or relationship, the problem would not be complex; the effect could even be estimated. However, with the PTI procedures, a multitude of formulas and array of inputs, themselves derived from different aspects of geotech work, make the problem complex and extensive. I would not be surprised if a specialized analysis of the PTI procedure from the standpoint of statistical accuracy would produce a range of accuracy in the outputs of ±25%.

A viewpoint with some truth lies, again, in considering the difference between science and engineering. Science is the basis of the procedure with all the uncertainty scrubbed off. But foundation design is an engineering problem, and the removal of the uncertainties has distorted the problem and prevents us from assessing it properly and working toward a realistic solution.

My view is that the use of a specific performance model is a misleading approach to the problem because field performance studies (mine, anyway) indicate that the model used does not relate to actual field applications. It is a laboratory creature.

More on that later.

9. Many valuable lessons about structural performance can be learned only from the observance and analysis of failed structures. In the *PTI Manual*, there are several references to lessons learned from structural failures, but they are usually offhand and superficial. The emphasis is on lab tests and computer modeling.

> *"This again is the paradox of design. Things that succeed teach us little beyond the fact that they have been successful; things that fail provide us with incontrovertible evidence that the limits of design have been exceeded. Emulating success risks failure; studying failure increases our chances of success. The simple principle that is seldom explicitly stated is that the most successful designs are based on the best and most complete assumptions about failure."* (Petroski 1985)

With this in mind, it would seem that we should spend more time studying distressed foundations and learn from it. The authors of the procedure might make the point that it was set up to respond only to climatic changes, not to maintenance lapses or other environmental or construction anomalies.

For perspective, however, we might look at the automobile industry. Basically, automobiles are designed to run smoothly and efficiently for many thousands of miles. But some vehicles are taken off the production lines and subject to road-speed crash tests. The results of these standardized tests are recorded and published. Any manufacturer whose autos do not score well in these tests can expect an immediate drop in sales.

If it is important to buy an automobile that is designed to survive a possible crash, should not a home be designed with at least some consideration of the anomalies that appear to be related to foundation distress?

10. As mentioned previously, the computer program used in the study applies uniform loads—either upward or downward—in various magnitudes to all four sides of the chosen rectangle simultaneously. No cases of partial loadings or combined loadings are considered (Fig. 1).

Such an approach may not describe accurate service environments.

I have looked at many distressed foundations and have never seen one loaded uniformly on all four sides. Conditions vary often significantly within 10 ft (3 m) along the perimeter and can be influenced by exposure to the sun, drainage, planter areas, and irrigation. It is easy to visualize load combinations that would put critical stresses on the foundation structure, and the number of such combinations is endless. But the uniform load assumption used in the procedure is perhaps the least critical of them all.

11. In the structural design portion of the first issue of the procedure, there are approximately 15 significant formulas, some of them with up to eight input variables and with exponential modifiers with two, three, and even four significant figures. This work was published over 30 years ago (Fig. 11).

12. In that time period, the procedure has been put to widespread use, having become the most recommended method for the design of the foundations for light frame buildings on active soils; and it has become recognized and accepted by the International Building Code.

Also during that time, several of the deflection formulas have been eliminated; and there have been several other minor changes. But 11 of the most important formulas have not been changed to the slightest degree.

In the normal state of affairs, a complex new design procedure, put into use by numerous skilled professionals and subject to healthy examination and discussion, is going to receive and adopt many useful suggestions for fundamental change in the first few years of its life.

In the case of the PTI Grade Slab Design Procedure, that has not happened. Some of the reasons are as follows:

(a) We have not made a systematic study of the performance of distressed foundations in different geographical areas.

(b) The core of the present work is complex and interrelated—no part of it really stands alone. The complexity actually serves to conceal its inner workings. Therefore, it appears to be impractical to attempt to revise it one piece at a time.

(c) No one appears to have the time or the resources to suggest or prepare an entirely new procedure.

Actually, change does occur rather regularly in the form of new "refinements" in segments of the technology. However, as with most sections of the Building Codes, the "refinements" are more in the form of "addendums" and the procedure becomes larger and more impenetrable year by year.

13. Look back over this whole list or, better yet, read the *PTI Manual*. Did you hear or read anything about experimental verification of any part of the core of the procedure? Remember what Professor Feynman said previously (Principle No. 4) about unverified theories?

What all of this seems to mean is that the design process has become disconnected from the foundations and has taken on a life of its own. The process itself has become the product instead of the foundations. The means have become the end.

However, the following portion of the presentation may stand alone; and its importance is such that it challenges the base of the procedure.

THE PTI GRADE SLAB MODEL

The PTI model for loading and stress distribution has been shown and described previously. It is an orderly presentation of the stress patterns that would result from uniform perimeter loading.

The problem is that observations of real foundations under actual service-load conditions show that the critical reaction loads applied by the soil are variable in location and magnitude to the point that they may reasonably be described as "random." They are certainly not systematic or uniform.

It is true that these loads are influenced by the weather cycle, but they are not "functions" of it. The relationship is not constant or measureable. Other influences, such as drainage and irrigation, appear to have a much larger and more direct and traceable effect on foundation performance; and these things do not occur in an orderly or predictable pattern or schedule, so they cannot be modeled. They are "anomalies."

"Anomaly" is defined in *Webster's Dictionary* as a "departure from the regular arrangement, general rule, or usual method."

The author believes the concept of an anomaly is central to understanding, working with, and designing residential foundations on active clay soils. It is the author's position, based on years of observing and attempting to explain the performance of these foundations, that no two malfunctioning foundations are alike in measureable terms.

The engineering designer, in advance, cannot describe the specific combination of soil properties; the timing, location, severity, depth, and duration of moisture imbalances; and the effect of defensive measures. If the engineering designer assumes he/she can be quantitatively specific about any of these things, much less all of them in concert, he/she departs from reality. Therefore, the assumptions and thus the designs of the engineering designer are invalid.

In defense of the use of the procedure, it may be said that it is solidly based on a lot of good fundamental science, that its variability has been muffled with various empirical corrections, and that its widespread use over a lengthy period of time is a testament to its validity.

In current practice, as witnessed from our design office, experienced geotech engineers who have the most flexibility in using their engineering judgment, regularly revise their calculated e_m's and y_m's in response to feedback from their structural counterparts. In many cases, the early phone call to the structural engineer (or vice versa) has become standard practice. It gives the structural engineer a chance to input his judgment and give feedback on how various designs are performing in areas that are nearby or have similar soil profiles.

At first glance, such communication would appear to be unnecessary and improper. But it has proven its worth in preventing designs that are too heavy or too light, and the dialogue is beneficial in expanding the experience and empirical base of both parties.

Older practitioners in this sector, such as myself, clearly remember the days when foundation engineers felt quite comfortable relying on three factors:

1. Potential vertical rise (PVR), preferably based on swell tests;
2. Experience of others in similar areas; and
3. Soil moisture content at time of construction.

Actually, in consideration of the aforementioned dialogue, we are slowly veering back toward the use of the old system.

When a homeowner waters his lawn with some consistency, which is assumed to be the usual case, the "weather cycle" no longer exists for that foundation. However, the engineer's responsibility continues, as it should.

So when moisture maintenance is obviously irregular, insufficient, absent, or excessive, that condition should be treated as an anomaly; and the design procedure should give the engineer the tools to recognize it and to demonstrate its effect on foundation performance.

There are other anomalies of almost equal effect that are commonly encountered and deserve individual attention. Their investigation and analysis would provide the most valid and useful information on foundation performance.

This is a key, all-important position; and it must be supported with pertinent evidence.

In that regard, I submit the contour maps of the differential elevations on the slab surfaces of 20 typical malfunctioning residential foundations in Texas.* Most of them (probably 80 or 90%) were designed by our firm, but they were not selected or culled in any technical way. They

*The contour maps mentioned in this paper can be downloaded from the PTI website at **www.post-tensioning.org/pti_journal.php.**

where:

$$A_o = \frac{1}{727}\left[(L)^{0.013}(S)^{0.306}(h)^{0.688}(P)^{0.534}(y_m)^{0.193}\right] \quad \textbf{(6-14)}$$

and for:

$$0 \leq e_m \leq 5 \qquad B = 1,\ C = 0 \quad \textbf{(6-15a)}$$

$$5 < e_m \qquad B = \left(\frac{y_m - 1}{3}\right) \leq 1.0 \quad \textbf{(6-15b)}$$

$$C = \left[8 - \frac{P - 613}{255}\right]\left[\frac{4 - y_m}{3}\right] \geq 0 \quad \textbf{(6-15c)}$$

Short Direction

For $L_L/L_S \geq 1.1$:

$$M_s = \left(\frac{58 + e_m}{60}\right) M_L \quad \textbf{(6-16)}$$

For $L_L/L_S < 1.1$:

$$M_S = M_L \quad \textbf{(6-17)}$$

Edge Lift Moment

Long Direction

$$M_L = \frac{S^{0.1}(he_m)^{0.78}(y_m)^{0.66}}{7.2L^{0.0065}P^{0.04}} \quad \textbf{(6-18)}$$

Short Direction

For $L_L/L_S \geq 1.1$:

$$M_S = h^{0.35}\left[\frac{19 + e_m}{57.75}\right] M_L \quad \textbf{(6-19)}$$

For $L_L/L_S < 1.1$:

$$M_S = M_L \quad \textbf{(6-20)}$$

Concrete flexural stresses produced by the applied service moments can be calculated with the following equation:

$$f = \frac{P_r}{A} \pm \frac{M_{L,S}}{S_{t,b}} \pm \frac{P_r e_p}{S_{t,b}} \quad \textbf{(6-21)}$$

Stiffness

Minimum foundation stiffness:

$$E_{cr} I_{L \text{ or } S} \geq 12{,}000 M_{L \text{ or } S} L_{S \text{ or } L} C_\Delta z_{L \text{ or } S} \quad \textbf{(6-22)}$$

Center Lift Moment

Long Direction

$$M_L = A_o\left[B(e_m)^{1.238} + C\right] \quad \textbf{(6-13)}$$

Applied Service Load Shear:

Expected values of service shear forces in kips per foot of width of foundation and stresses in kips per square inch may be calculated from the following formulas:

Center Lift:

Short Direction Shear:

$$V_S = \frac{1}{1350}\left(L^{0.19}S^{0.45}h^{0.20}P^{0.54}y_m^{0.04}e_m^{0.97}\right) \quad \textbf{(6-23)}$$

Long Direction Shear:

$$V_L = \frac{1}{1940}\left(L^{0.09}S^{0.71}h^{0.43}P^{0.44}y_m^{0.16}e_m^{0.93}\right) \quad \textbf{(6-24)}$$

Edge Lift (for both directions):

$$V_L = V_S = \frac{L^{0.07}h^{0.4}P^{0.03}e_m^{0.16}y_m^{0.67}}{3S^{0.015}} \quad \textbf{(6-25)}$$

Applied Service Load Shear Stress, v:

Ribbed Foundations:

$$v = \frac{VW}{nhb} \quad \textbf{(6-26)}$$

Only the rib area is considered in calculating the cross-sectional area resisting shear force in a ribbed foundation.

Uniform Thickness Foundations:

$$v = \frac{V}{12H} \quad \textbf{(6-27)}$$

Compare v to v_c. If v exceeds v_c, provide shear reinforcement in accordance with the following:

$$\frac{A_v}{s} = \frac{(v - v_c)b}{0.4f_y} \quad \textbf{(6-28)}$$

Fig. 11—Equations (6-13) through (6-28) from PTI Design of Post-Tensioned Slabs-on-Ground, first edition, 1980.

had only one thing in common—our telephone rang and a voice said, "…please come to look at my foundation… something is going wrong." These were all the contour maps I could locate in our files. Only two or three were omitted because they were too small or incomplete.

My point is that this is a general representative sampling. It is not culled or sorted to represent any point of view.

It must be noted that the contour maps are true elevation contour maps and are thus not exactly comparable to the maps (or diagrams) such as Fig. 5.2 (Fig. 1) from the third edition, which shows the modeled variation in bending moment across the surface of the slab. However, the difference in specifics is inconsequential because the published version clearly illustrates the assumption of orderly activity around the edge of the slab, whereas the field plans clearly show the random actual distribution of elevations and thus the loads and stresses that they create.

We believe that the 20 slabs shown constitute a sample of sufficient breadth to support the conclusion that the loading of the soils on the underside of the typical residential slab on active clay soils in a Texas-type environment is an effectively random distribution. It follows no regular or predictable pattern and thus cannot be modeled.

The fundamental problem is not the need for a representative model and a more modern and sophisticated finite-element program….the problem is the difficulty of defining the actual upward "loading" condition of the soil on the bottom side of the foundation under a realistic variety of circumstances.

Furthermore, the models used for the distribution of loads and stresses in the PTI procedure (Fig. 1), being based on the confining assumption of continuous and uniform perimeter loading, may not always be accurate. The very purpose of such a model is to represent realistic patterns of soil activity, which then may serve as the basis for further analysis.

AN ALTERNATIVE

In selecting a direction for the development of an alternative design procedure, I believe our circumstances are similar to those of Professor Terzaghi as he contemplated his first definitive work. He proposed to develop a semi-empirical engineering approach based on the observed behavior of soils in the field. He mandated: "As soon as we pass from steel and concrete to earth, the omnipotence of theory ceases to exist."

As his work progressed and he refined the concept, he wrote:

> *"In Soil Mechanics, the accuracy of computed results never exceeds that of a crude estimate, and the principle function of theory consists in teaching us what and how to observe in the field… …hence the center of gravity of research has shifted from the study and the laboratory into the construction camp where it will remain."* (Goodman 1998)

In endeavoring, then, to select a proper new direction for the development of a design procedure, I believe the first step is to resist the urge to construct a model and subject it to analysis. It is difficult and based on assumptions to model a random process, and then attempt to learn specific things from its analysis, and we have made that assumption...twice.

I believe we should then develop a systematic method for studying the performance of existing mal-performing foundations, with a standardized method of observing and reporting, so that the results can be archived and collated. We should strive to pinpoint the factors that are most decisive in determining foundation performance. We should use statistical methods in "weighing" those factors and their effect upon designs and performance.

We should maintain a central data bank, preferably at a major university. A small team of graduate students could maintain the databank and develop statistically correct methods for incorporating newly arrived field data into the "bank."

For the first few years, we should be endeavoring to select the factors most important to foundation performance, focusing on reports of foundations pushed to their limit or beyond it.

After a few years, we should be able to finalize a list of factors to watch; we may almost be able to do that now. Then our focus could shift to the "weighing" of the various factors. Each year, our review and analysis methods should enable us to "hone" the various weights and certainly to develop different weight schedules for different geographic areas.

An advantage inherent in this type of system is that it would eliminate the assumptions that are buried in our present complex procedure. Mistakes of any type would be easily traceable because the line of logic would be simple and short: "This factor has an effect. Is it more or less than we have been estimating? Should we increase or decrease its weight?" Everything would be available for inspection and evaluation. Nothing would be hidden in the brain of the computer.

Also, it would enable us to make at least some use of our engineering experience and judgment, something for which there is not much room in the present procedure.

The most difficult work, as usual, would be for the geotechnical engineers. They would be expected to come up with a number that would represent the basic potential of the site for soil movement—something equivalent to our traditional potential vertical rise (PVR). I might be out of my area of expertise. However, to start the discussion, I would say that the number should certainly be based largely upon swell tests, simply because the swell (or shrinkage) of the soil is what we are going to have to design for. But perhaps this new PVR could be modified by some of the more recent work on soil suction and other factors.

A suggestion is that we are presently giving too much attention to the climate moisture cycle. Homeowners should water their lawns; and when they do, the climate cycle is irrelevant. The procedure should penalize them only when they do not irrigate or when they irrigate improperly.

It is a given that most of our foundation malfunctions in Texas are due to improper or absent drainage accommodations, perhaps 80% in my experience. I cannot imagine a design procedure that would not include, in some quantitative way, the effects of a malfunctioning drainage environment. But it would be difficult to simulate in a laboratory, and there is no way of forecasting its occurrence or its magnitude, which, of course, is one of the big reasons it is not in our PTI procedure.

On the other hand, however, there is the very real benefit that if drainage is included in the design procedure, we can easily and officially show that a deficiency in drainage would be the cause of problems; and it would then be much easier to show that the rest of the design is not the problem. If drainage is not in the procedure, it will be, and is, difficult to show what an important factor it is.

A second benefit is that if drainage is included as a part of an empirical design procedure, we will be obliged to study its effect on foundation performance and learn more about it. I think that would be an obviously good thing that we have been minimizing for over 20 years.

It does appear that there is another yawning gap in our present approach that I believe needs attention. It is that no one is paying systematic attention to the moisture content of the building pad at the time of construction. Usually, it seems possible for a soil sample to be taken in February or March and for construction to commence in August or the following year. It seems to me we should control this in a better way because there is wide agreement among engineers that the moisture content of the building pad is an important determinant of potential soils activity and thus foundation performance. If we cannot control it, we should at least put allowance for it in our design procedure.

The geotech engineer would further be expected to come up with estimates of the various "risk factors" because they would be primarily related to site conditions; and the engineer would be the only one (usually) to have seen the site.

However, the structural engineer would be responsible for combining and summarizing all of the risk factors and might see a reason for modifying one or several of them. It would be the structural engineer's responsibility to come up with a combined "bottom-line" risk factor with which to modify the basic PVR factor received from the geotech engineer.

After this modification is done, the final modified PVR could be used to select, perhaps by a graphical means (similar to the PCA pavement design procedure), the beam size and spacing of the final stiffened slab design. An advantage of the graphical method is that it would establish the approximate nature of the procedure, as opposed to the surgical precision of the present computer output. A sample configuration of a graphical procedure is enclosed (Fig. 12).

However, in a practical sense, the weighting of the various factors could be put in algebraic form, and one or several basic equations could probably be sufficient. In the early stages of configuration, there will probably be a number of stages of revision and manipulation, and it will be a lot more convenient to revise some algebraic exponents than to draw new curves every time.

Also, it seems intuitive that the procedure should not make use of a simple sum of the "risk factors." Because the first-considered risk factors would have a primary effect on the design, the effect of succeeding risk factors should be progressively muted because all would probably not be fully effective at the same time (a little like the "live load reduction" factors). This could easily be done with the application of fractional exponents to the sum of the risk factors and modified quite easily as we gained experience with the use of the procedure.

It would be required for the structural engineer to list his or her assumptions on the foundation plan so that the builder and owner (and defendant's legal counsel) might know what was expected of the homeowner.

CONCLUSIONS

The basic advantage of a design procedure of this type is that it would seek to find and work directly with the environmental anomalous factors that seem to cause nearly all foundation malfunctions. Most experienced observers report that it is difficult to see the effect of things such as fabric factors, gamma numbers, and even Thornthwaite Indexes. We do not deny the existence of such factors, but their effect

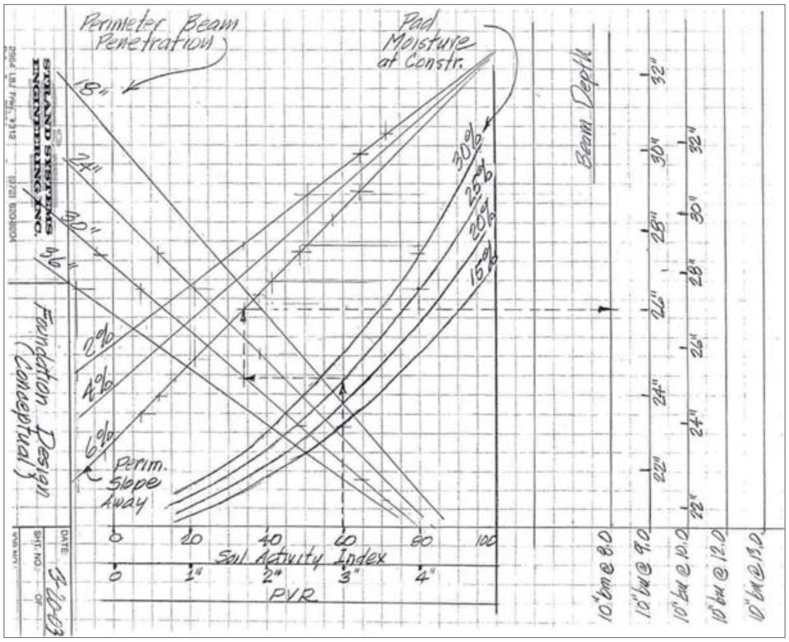


Fig. 12—Sample configuration of a graphical post-tensioned design procedure.

upon performance is difficult to identify and thus to predict. It would seem to be a more effective approach to include factors of that type in the calculation of the PVR number and leave the structural engineers to deal with tangible issues such as perimeter drainage, pad moisture, perimeter moisture barriers, nearby trees, negative site slope, irrigation outlook, roof gutters to protect drainage, perimeter grade beam pipe penetrations, fill soils, building configuration and loading, and sensitivity to foundation flexure.

We should study the foundations as Terzaghi would do and concentrate on things that actually affect performance. We would directly achieve a better focus of our structural design efforts and investigations. This change of emphasis would be no less than the recognition of reality. If functional truth is available to us, it should form the basis of our work.

It is somewhat eerie that Terzaghi, some 60 years ago, was able to describe so well the circumstances that brought about our present situation.

REFERENCES

Bowles, J. E., 1977, *Foundation Analysis and Design*, McGraw Hill, New York.

Feynman, R. M., 1964, "The Character of Physical Law," Messenger Lectures, Cornell University, Ithaca, NY.

Goodman, R. E., 1998, *Karl Terzaghi, The Engineer as Artist*, ASCE Press, Reston, VA.

Murv, W. A., 2006, "Using Our Best Judgment," *Civil Engineering*, Sept.

Petroski, H., 1985, *Success through Failure: The Paradox of Design,* St. Martin's Press, New York.

PTI Committee DC-10, 1980, "Design of Post-Tensioned Slabs-on-Ground," first edition, Post-Tensioning Institute, Farmington Hills, MI.

PTI Committee DC-10, 1996, "Design of Post-Tensioned Slabs-on-Ground," second edition, Post-Tensioning Institute, Farmington Hills, MI.

PTI Committee DC-10, 2008, "Design of Post-Tensioned Slabs-on-Ground," third edition, Post-Tensioning Institute, Farmington Hills, MI.

Wray, W. K., 1978, "Development of a Design Procedure for Residential and Light Commercial Slabs-on-Ground Constructed Over Expansive Soils," PhD thesis, Texas A&M University, College Station, TX.

Richard P. Martter *is a Senior Consulting Engineer at Strand Systems Engineering in Dallas, TX. He has MSCE and MBA degrees from Stanford University. He has been selected as a Fellow and a Legend of the Post-Tensioning Institute. In California, in the 1960s, he designed the first viable ductile iron anchorage for single-strand post-tensioning and the required stressing equipment. Since that time, he has specialized in the design of grade slabs on active soils. His current research interests include the development of alternative empirical procedures for grade slab design.*