Geotechnical centrifuge development corrects Terzaghi’s errors.

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(a lecture to the Tokyo Conference of TC2, on 23 September 1998).

(1) Introduction.

In this paper I will quote Terzaghi (1936), and make three points from my own experience.
(i) Geotechnical centrifuge model testing complements the observational method.
(ii) Beam and drum centrifuges complement each other.
(iii) Centrifuge tests, on models made of soil paste, correct “Mohr Coulomb” errors.

(2) Small scale tests and the observational method.

In his Presidential Address, Terzaghi (1936) told the Harvard Conference of the International Society of Soil Mechanics and Foundation Engineering (ISSMFE) that in “the perpetual war of the civil engineer against the treacherous forces of nature concealed in the earth … scattered and world-wide efforts extending over a period of 25 years (have forged) new and efficient weapons and the prime purpose of our meeting consists in discussing the means of exploiting the advantages thus secured”, and “the possibilities for successful mathematical treatment of problems involving soils are very limited. … 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. … 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 measurement in the field far in excess of anything attempted by the preceding generations of engineers. Hence the centre of gravity of research has shifted from the study and the laboratory into the construction camp where it will remain.” He was considering problems that were being encountered by engineers in the construction camps of the US Bureau of Reclamation and in the construction of the Panama Canal.

There is a class of catastrophic events, such as nuclear weapons cratering, that he was not considering, where geotechnical engineers need to learn from models, not to experience a succession of events in the field. Pokrovsky’s (1936a) centrifuge paper is appended to this paper. In discussion, Terzaghi (1936) reacted strongly to all papers on small scale physical modelling as “papers whose authors do not hesitate to generalise the conclusions derived from pure theory or from small scale tests on materials with very little if any resemblance to real soils.” He stated that “One of the principal goals of instruction in soil mechanics should be to discourage this prevailing tendency to unwarranted generalisation.” Six lines later he speaks of “the utter futility of the attempts to discover any single-valued relation between the results of small-scale loading tests and of the settlement of large foundations on stratified soils”. However, in the light of subsequent Soviet developments, it was Terzaghi’s dismissal of Povrovsky’s centrifuge technique that proved to be utterly mistaken.

Pokrovsky’s centrifuge paper opens with a statement that in 1936 the laboratory for Physics of the Military-Engineering Academy of the USSR used a centrifuge. 37 years later several of us who were engaged in centrifuge modelling were at the 1973 Moscow Conference. Before then we had thought that there must have been some difficulty by which Pokrovsky’s techniques had proved to be less useful than he hoped in 1936. At the 1973 Conference our hosts invited all participants who were interested in centrifuge techniques to a meeting for open discussion with Pokrovsky and other Soviet engineers, at the Hydro project, after the Conference. During our visit to the Hydro project we saw the powerful Hydro project centrifuge and were told that Soviet engineers wanted the West to become more fully aware of Pokrovsky’s work. I was surprised to see in a book by Pokrovsky that the Soviets had successfully modelled nuclear weapons cratering.

I mentioned this in subsequent lectures in the USA. The US Defence Nuclear Agency later sponsored crater tests in the Boeing Company centrifuge in Seattle, which led to an order of magnitude reduction of crater size prediction at nuclear explosive levels. In his paper for the San Fransisco TC2 published volume, Schmidt (1988) wrote;

"Results of recent geotechnical centrifuge experiments have dramatically reduced the size estimates for craters formed by near-surface large yield nuclear explosions and by planetary impact of large bodies. Since neither phenomenon can be tested at full scale, centrifuge simulation is the only alternative for obtaining an experimental data base. Estimates of crater size were reduced due to the identification of a strength-gravity transition size, above which cratering efficiency decreases with size. Existing field data were too sparse and were conducted in far too diverse media to observe this pattern. The geotechnical centrifuge has been a valuable experimental technique for investigating explosive and impact cratering behaviour. (The tests) establish the practicality of performing dynamic experiments on the centrifuge, as well as providing a theoretical basis for their interpretation."

Details of the Boeing tests were confidential. Their value in the Cold War was incalculable. After early centrifuge model experience Pokrovsky had become a Soviet expert on cratering, in the small Soviet scientific elite who worked in close secrecy on weapons effects during and after World War II. Russians now tell us that we can not imagine the powerful role he played or the life he lived. His "efforts" gave the Soviets a "new and efficient" research asset. He had the rank of Red Army General. Stalin came to parties where Pokrovsky played the piano at home. In contrast, Terzaghi's comment on the "utter futility" of small scale tests reduced the chance that the US and NATO Allies could find or correct errors, after full scale tests were banned. Their weapons would have been "dramatically" less effective than they estimated.

In 1936 it was clear in Pokrovsky (1936a) that centrifuge techniques worked. No "conclusions derived from pure theory or from small scale tests on materials with very little if any resemblance to real soils" and no "tendency to unwarranted generalisation" are evident in it, or in Volume I of "Centrifuge 98", which has 147 such papers to consider. No comment that "their authors do not hesitate to generalise" is warranted now, 62 years after the Harvard Conference, and it was not warranted then. It is hard for colleagues in Tokyo now to appreciate the circumstances at the 1977 ISSMFE Tokyo Conference, when Soviet colleagues said that the centrifuge was only of military significance. I replied that centrifuge techniques had a basis in applied mechanics. Critical state soil mechanics convinced me that effectively stressed reconstituted soil paste is an elasto-plastic inviscid material in which time effects are due to consolidation. I rejected Soviet analysis in terms of total stress and viscosity. It was difficult to work with Soviet engineers. I turned to the ISSMFE for a technical committee TC2 where we could have an international exchange of information.

The problems that are faced by geotechnical engineers are so complex that we should all discuss how to exploit the advantages of any technique that can help to solve problems. Terzaghi was right to emphasise the importance of field observations, however centrifuge tests help solve problems where "the observational method" cannot be used. Conditions which cannot be replicated in full-scale tests include: tidal flood or river flood; earthquake; prolonged contaminant migration; storm loading on offshore structures. Could Terzaghi's "measurement in the field" ever be applied to a jack up rig deployed offshore in the extreme case of typhoon storm loading? Aspects of such events can be observed at small scale, in reduced time, at low cost, by centrifuge techniques. A violent model test has no publicly unacceptable environmental impact or risk.

Early experience with well compacted selected fill gave me ideas set out in Schofield and Wroth (1968). My ideas continued to evolve as I saw ground behaviour mechanisms in models. For much construction, I think that soil tests could find the critical state properties of soil selected for a fill, or soil at a selected site. Designs could aim for ductility. Construction budgets could include the cost of tests to failure of centrifuge models, and "observational methods" could be based on such tests. There is a choice of alternative centrifuges.

(3) Beam and drum centrifuges.

Pokrovsky (1936a) Fig. 1 shows a 30g centrifuge made from parts of a Ford truck. One half-shaft stands vertically upright above the differential. The centrifuge rotor replaces a back wheel. The rotor arms slope at 1/30. They act as tension members. In that sense this is not a "beam" centrifuge. The model containers swing up about hinges. They are shown end on. In his test a load bears on a plate, and pressures are measured below the ground surface. In Fig. 2 vertical pressure is plotted against depth with five lines showing pressure as follows;

in ground with self weight, and
(I) in an elastic half space under vertical load, and
(II) the sum of these two previous pressures, and
(III) pressures measured in Pokrovsky's centrifuge model test, which agree with
(IV) pressures observed in a full scale test.

Pokrovsky draws a pressure gauge in his Fig. 3. A short length of broken capillary tube was pressed into a small tin full of pink petroleum jelly. The air filled space inside the tube was closed by pink jelly at each end. A rubber membrane covered the jelly. The tin was buried in the model. The model was subjected to high acceleration. The burette was opened. Fluid flowed down along the axis and out to a vessel which applied the required bearing load. After a test the tin was removed and the capillary tube was examined. The pressure increment had compressed the air. Jelly had moved into the ends of the tube leaving a pink stain. Pokrovsky determined the maximum pressure, at that depth, from the minimum length of the air bubble. Both in the full scale test and in the model test he measured pressures up to 50 percent higher than he had calculated theoretically. He had proved that his technique worked, and it was applied to a series of problems where there were no reliable theoretical calculations.

Malushitsky (1975) described the application of Pokrovsky's technique to problems of mine waste embankments. It gave him a capability for analogue modelling of a problem which was not soluble numerically with the facilities available to him. His centrifuge could achieve 320g but typical tests were at below 200g. The inside dimensions of his model were length 1400mm, width 500mm, height 750mm, corresponding at 320g to a prototype volume of 17.2 million cubic metres. He built up models in successive layers of reconstituted waste material which he consolidated in flight for long periods. He tested his models by rapid increase of acceleration until there was a slope failure. Academics in the USSR at that time analysed soil as a viscous material under total stress. The scale of time was expected to be the model scale to some power between 0 and 2. Malushitsky found a value of this factor that was appropriate to his class of problem by the technique of modelling the model. His simple instrumentation and the variability between successive models meant that his work took many years. He tested 255 models in total, and writes that they resulted in elimination of landslides at the waste heaps of an open-cast sulphur mine, reduction in re-excavation in internal dumps in an open cast coal mine, and safe tipping of new dry waste on old hydraulic lagoon disposal areas, with savings to industry of about three quarters of a million roubles per year.

The cost of such a facility includes both a beam centrifuge and a strong chamber to enclose it safely, with several model containers for successive tests. Each model is made as a different batch of soil. Several weeks are needed for consolidation of a large model. If models are consolidated on the laboratory floor with a downward hydraulic gradient in order not to tie up the centrifuge, more containers are needed. A model has an unwanted load and unload cycle each time a centrifuge starts and stops, for example to adjust some instrument. Some beam centrifuges now have a facility to manipulate tools in the model container while it is in flight, Derkx, Merliot, Garnier, and Cottineau (1998). Such manipulators are costly because they operate in the high acceleration field of the model test.

Drum centrifuges were developed in Cambridge to reduce the cost of centrifuge tests, improve the accuracy, and reduce the labour and the time needed for any one test series. To introduce drum centrifuges safely to other laboratories, I sought a long term commitment from an established manufacturer. Their risk was significant. Financial problems have affected many geotechnical centrifuge developments. They needed protection of their initial investment. Cambridge University has an intellectual property development plan. Some inventive ideas in this field were granted European and US Patents, Schofield (1997). After negotiation, a centrifuge manufacturer in the UK, Thomas Broadbent & Sons Ltd. was granted an exclusive licence to incorporate the new intellectual property in their "Geotech" centrifuges. One of these came into in operation in Australia, Stewart, Boyle, and Randolph (1998); the next three in Japan; and a fifth is to operate in Switzerland. It is safe to stand by these machines. There is no need to build a reinforced concrete chamber. The centrifuge channel that applies the acceleration to the model layer also contains the soil safely.

In a drum centrifuge a channel full of soil is prepared as a single model. Both the volume and the surface area of a model are large. A 2.2m diameter channel, 0.8m high with 0.2m depth, in flight at 320g, corresponds to a test site about 2000m long, 256m wide and 64m deep, with a prototype volume of 32.8 million cubic metres. On one model of a uniformly stratified layer of soil there is room for many tests. If it was decided to plan a series of up to 50 tests, about 20 litres of soil would be closely affected by each test, which is a prototype volume of 655360 cubic metres. A similar beam centrifuge test might be conducted on a model of 80 litres volume at 80g. The prototype volume of 40960 cubic metres is sixteen times less. In a series of tests the walls of the model container are relatively closer to the model test site in the beam centrifuge than in the drum.

The machine is designed for continuous safe operation. Model preparation in the channel, and setting up the test procedure, takes time, but if the test process is automated the model could then be in continuous flight for many weeks. Tools or systems are available to be manipulated and work over the model in flight. With the channel in flight, a safety shield is lowered to allow safe access to the central work support. The channel is brought to rest. Tools or other systems are changed. The work support is bought back to channel speed. Tool systems then work over a chosen test site. Stewart, Boyle, and Randolph (1998) describe both an automated testing system and a data acquisition system. Similar rugged and compact systems provided to the US Army Centrifuge, Waterways Experimental Station, Vicksburg, Miss., and to Toyo Construction Technical Research Institute, Hyogo, Japan, acquire digital data at 5000 samples per second in memory in a logger unit close to the model in the high g field, for uploading to a PC at the control desk. Drum and beam centrifuges operate side by side in centrifuge centres in the UK, Japan, and Australia and complement each other. A manipulator used in a drum centrifuge is controlled from the centre of the drum at low g. When both beam and drum centrifuges are used on a single project, test equipment can be transferred from one machine to the other. In future, parametric studies of the problem on which Terzaghi made comments, “foundations on stratified soils”, will get data with an accuracy unattainable from tests at full scale in the field. Development of centrifuge techniques, plus the observational method applied in the field, then will make soil mechanics a part of experimental applied mechanics.

In the case of the jackup, the three legs apply cyclic loads of the order of ten thousand tonnes to spud cans bearing on a sea bed. When jackup spud fixity was modelled in the Cambridge 10m diameter beam and 2m diameter drum centrifuges, Dean et al (1993) studied the bearing capacity of conical footings on sand in relation to the behaviour of spudcan footings of jackups, as part of theoretical and experimental studies undertaken over a period of several years. The work is reported in publications in Cambridge M.Phil and Ph.D theses. Tsukamoto (1990) tested foundation fixity of a model jackup with three independent legs, deployed at successive locations on a model “sea bed” round the wall of the 2m drum. Comparing this work on bearing capacity with that of Terzaghi (1943), a significant difference is that foundation fixity now is described by a yield locus rather than by “bearing capacity factors”. The model test data are equivalent to observations in hundreds of storms offshore. The offshore industry has good experience of both beam and drum centrifuge modelling. Each year the offshore industry deploys jackups for ever longer periods in ever deeper water, and offshore engineers need ever better guidance to select jackups that are appropriate for successive projects.

(4) Earth pressure theories and models made of reconstituted soil paste.

Terzaghi (1936) explains that he learned to mistrust theory

“some eighteen years ago ..(i.e. at the end of WW I, when).. 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. .. when the theories originated, their authors were still keenly aware of the bold approximations involved, and nobody thought of accepting them at their 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 behaviour was considered 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 the designer was poorly grounded in theory."

Terzaghi did not comment on bearing failure, but he does comment on slope failure and lateral pressure; a feeling that his paper Terzaghi (1936b) was very significant is evident from the fact that it is reprinted in full in the Proceedings. It is the only paper included in this manner. In 1929 he had made full scale tests on retaining walls, Terzaghi (1934) His papers on those tests draw attention to small movements that can be observed at visible surfaces. He conjectures about the way that force, as measured in trench supports, relates to strain in the ground beside the trench. The title of Terzaghi (1934) is "A fundamental fallacy in earth pressure computations"; his first conclusion is

" The fundamental assumptions of Rankine's earth pressure theory are incompatible with the known relation between stress and strain in soils, including sand. Therefore the use of this theory should be discontinued".

His complaint is that "the factor 'strain' does not enter the theory".

Terzaghi did not act on his dictat that "the use of (Rankine's) theory should be discontinued". His text book Terzaghi (1943) proposed "bearing capacity factors" that he based on Prandtl (1920) and, in spite of his rejection of "classical theories", Rankine's theory continues to find a place in the current edition of his textbook Terzaghi, Peck and Mesri (1996). But there is a simple reason for the problem with "theory"; there is an error in the Mohr Coulomb equation.

Coulomb (1773) solved by statics some problems of plane bodies for which the limiting stress criterion has a form $F(\sigma_x,\tau_{xy},\sigma_y)=0$. Limiting plane equilibrium satisfies the two equations,

$\delta\sigma_x/\delta x+\delta\tau_{xy}/\delta y=0,\ \delta\tau_{xy}/\delta x+\delta\sigma_y/\delta y=0;$

the system of three equations in the three unknowns $(\sigma_x,\tau_{xy},\sigma_y)$ is of the hyperbolic type. For a criterion F=0 of the Mohr Coulomb type, in a plane zone of given stress boundary conditions, there are two characteristic directions along each of which a function of the magnitude and direction of stress maintains a constant value. The stresses are defined at each point on a particular length of boundary. In a triangular "domain of dependence" the stress at each point in the domain is fixed by values of these functions that are propagated to that point from two points on the length of boundary along the two characteristics that reach that place. The mathematics is not affected by Terzaghi's comments. The only good reason for use of the equations to be discontinued is that one or other of the equations does not apply. The equilibrium equations are beyond question. Terzaghi's statement that strain affects earth pressures must mean that there is an error in the Mohr Coulomb equation. Cohesion or friction can not be "true" properties of soil. They can not be independent of test conditions.

Schofield (1993,1998) explained the error in Terzaghi's interpretation of the data of Hvorslev's drained shear box tests. The peak strength of reconstituted clay paste after overconslolidation is not due to molecular attraction of "cohesive" soil particles. A part is critical state friction. The clay also softens and dilates as it is sheared. Taylor's "interlocking" is another part of the peak strength. This phenomenon, also called "arching", involves geometry. Peak strength instability is like strut buckling. It is not defined uniquely in stress space, and "attempts to discover any single-valued relation between the results" in different tests are futile. Mohr's circles have no "true" curved envelope for which the Mohr Coulomb equation is an approximate fit.

Neither Coulomb or Rankine tested soil in a shear box. They distinguished between solid rock with strength in a tensile test, and material whose disturbed parts do not adhere. A drained slope of their soil has an “angle of repose” which is the “angle of friction”. The angle will be constant if shear deformation down slope is at constant volume. Soil which deforms continuously at constant volume in a drained shear test, or below a slope at “repose”, is in critical states with $q=Mp'$. This applies equally to soil with coarse and with fine grains.

Schofield and Wroth (1968) describe liquid limit tests as undrained behaviour of soil in critical states. The apparent cohesion of test samples is equal to suction times critical state friction, increasing as effective stress increases. Soil yields in a stable manner on the wet side of critical states, at stress ratios less than critical, where $q<Mp'$. Plastic yielding of such soil is predicted by the cam clay theory. A model made of reconstituted soil paste will have zones of plastic yielding, in which very many tests of elementary volumes of soil each experience a different “true triaxial test”, with compression and shear strain on the wet side of critical states. The soil can exhibit anisotropy if it has such tendencies. The geotechnical centrifuge is a good apparatus for testing reconstituted soil on the wet side of, or near to, critical states.

Failure with $q>Mp'$ on the dry side of critical states involves unstable behaviour. Faulted soil dilates in shear, causing water to be sucked into “slick” soil paste on the failure plane. Soil cracks at low effective stress. If there are cracks, pipes, or channels in a zone across which there is a high hydraulic gradient, rapid transmission of pore pressure into the soil will transform what was initially a stiff, lightly stressed, continuous soil body into a clastic debris flow. When I see this effect in centrifuge model tests I describe it as “liquefaction”. Any claim that progressive failure at homologous points in a model and a prototype are similar should be validated by modelling of models. Interlocking geometry and effective stress must be correct everywhere in zones through which instability propagates. Some instability may not scale.

In “the study and the laboratory” at MIT work by Taylor, and later work in CUED, disproved Mohr’s “sacred theory”. Today “a thorough grounding in theory” includes critical state ideas. They apply to types of soil that are selected for a compacted fill, to types of ground that suit construction, and to reconstituted soil paste formed into test specimens or centrifuge models. Undisturbed ground or soft rock needs more work on ageing or creep, and on adhesive or cohesive bonds. More work is needed on types of soil that engineers today would not select as suitable material for a fill, and would not select as suitable for the foundation of a building.

(5) The report of TC2 to the Istanbul Conference in 2001.

The work of TC2 differs from the work of other research committees. The factor “strain”, that Terzaghi wanted to see introduced into the theories, is present as significant plastic strain in mechanisms that we observe. Tests in which models experience large strains disclose three regimes of large strain behaviour. fissure, fault, fold, that are also evident at full scale. We can help to plan observational methods, with larger strains and more extensive parametric studies than are achieved in the field. Our test data are used to validate numerical models. Terzaghi said with regret that: “There is no complete theory of the settlement of foundations or of the lateral pressure of earth and there never will be”. For us in TC2, and for those who observe behaviour in the field and in site investigations, “never” is too long a time. One day there may be non-linear elastic constitutive models, numerical analyses, and predictions of soil structure interaction validated by model tests and field data. We aspire to something better than Terzaghi’s “crude estimate”. Our small scale tests have a fundamental basis. A model made of soil paste relies on the first “law” of soil mechanics as stated by Coulomb, “reconstituted soil has no adhesion”. Both he and Rankine dismissed cohesion from design calculations. Critical state soil mechanics originally claimed that the design problem “is not so difficult if we consider the ultimate fully remoulded condition that might occur if the process of uniform distortion were carried on until the soil flowed as a frictional fluid. TC2 is near, not only to the “centre of gravity of research” but also to that of design.

When BGS (British Geotechnical Society) was asked to support our initial TC2 committee it agreed to a modest commitment. The BGS would underwrite the publication of the first TC2 publication. They had a substantial profit from publishing the proceedings of the European Conference in Brighton and made it clear to the TC2 Chairman and Secretary (Schofield and

Craig) that BGS did not expect to make a loss on our TC2 publication. We did not expect TC2 committee members to find funds for travel. We took our message to seminars in the cities of our committee members; Tokyo, Manchester, Davies. We held our first meeting of TC2 after the San Fransisco Conference. BGS lost money on our publication. Subsequent workshops, conferences, and publications, now make TC2 the most successful technical committee. It has the capability, and I think it has the duty, to address a matter that affects all geotechnical engineers. Terzaghi made errors at the Harvard Conference in 1936 which TC2 can report. The errors should be corrected in 2001.

(6) References.

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