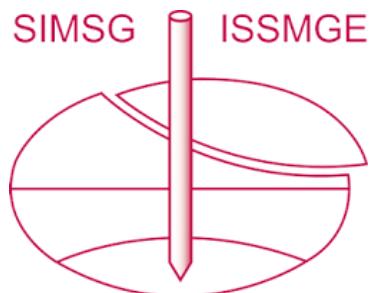


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Piles and Pile Foundations, Settlements of Pile Foundations

Pieux et fondations sur pieux, tassements de ce genre de fondations

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General Reporter / Rapporteur général: Prof. R. B. PECK, U.S.A.

Oral Discussion / Discussion orale:

Prof. A. W. Skempton, Great Britain

Mr. F. L. Cassel, Great Britain

Mr. M. A. Shaarawi, Egypt

Mr. B. Fellenius, Sweden

Mr. R. B. Lundström, Sweden

M. M. Buisson, France

Mr. A. J. da Costa Nunes, Brazil

Mr. M. Peleg, Israel

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General Reporter Session 5 – Rapporteur général Session 5

The General Reporter

During our sessions on Monday and Tuesday, I believe we all came to agree with Prof. Casagrande's remark that the subject of the shearing resistance of soils is probably the most

complicated aspect of soil mechanics. After a review of all the papers submitted in Sessions 4, 5 and 7, one might equally well come to the conclusion that the subject of pile foundations is the most complicated aspect of foundation engineering. It seems safe to assert that, in the interval since the Second International Conference, progress with respect to our knowledge of pile foundations has not kept pace with that of footing or raft foundations or of earth pressures.

There are, I believe, several reasons why this should be true. The first of these is the great cost of full-scale experimentation for research purposes. Experiments to investigate the validity of scientific hypotheses regarding pile foundations ought to be carried out in soils having well defined, uniform properties. The rarity of such soils, and the economic necessity for carrying out our experiments in connection with actual construction projects, have conspired to produce the situation in which we find ourselves; namely, that most of our full-scale experiments have been made in soils too non-uniform to permit the elimination of serious uncertainties in their interpretation.

Because we have been forced to make our tests on soils as we find them, and because most of us work in regions with a limited variety of geological conditions, we have possibly fallen into the error of generalizing too broadly on the basis of our limited experience. Thus, what appear to be general conclusions based upon studies in one region are often at variance with those based on similar studies in regions of somewhat different geological origins.

Finally, we can never lose sight of the fact that the installation of pile foundations in contrast to that of footings, for example, is a highly competitive commercial enterprise. Therefore, many of the most important practical problems in connection with the engineering aspects of pile foundations are not directly related to or compatible with the scientific aspects of the subjects. We will not make real progress if we deliberately ignore the non-scientific elements in the art of engineering.

All these difficulties have at times diverted our attentions from the main problems that we must face to somewhat simpler or more speculative endeavors where progress appears to be more likely. Thus, we have devoted much effort to studies of the capacity of single piles instead of pile foundations; to studies of small-scale models rather than full-sized installations. These comments seem justified by the very small number of Conference papers containing complete case-histories of actual pile foundations. Yet, the factors that I have been discussing force us to the conclusion that real progress in the engineering of pile foundations cannot be made in the realm of theory but must have at least a semi-empirical basis. We must have the patience to collect and digest many, many case histories before we can be permitted the luxury of generalizations.

In all our case histories, as well as in any supplementary experiments, we must record not only the pertinent facts concerning the construction of the foundation and its behaviour for a significant period of time, but we must also determine and describe in quantitative detail the properties of the subsoil. If one were to make a single general criticism of our efforts during the past ten years, I think we would be obliged to say that many otherwise excellent case histories are of greatly diminished value because of the failure to record even the index properties of the soils involved.

Therefore, in my suggestions for our discussions in this session, I have proposed that we should place the emphasis on the behaviour of pile foundations rather than individual piles, that we should discuss our experiences with full recognition of the importance of describing the soils quantitatively, and that we should consider techniques for securing and digesting the great number of reliable case histories of pile foundations that we must accumulate before further substantial progress will be possible.

Le Rapporteur général constate que peu de progrès ont été réalisés au cours des dernières années dans le domaine des fondations sur pieux. Il indique les raisons de cet état de choses et explique pourquoi il envisage de concentrer la discussion sur les comptes rendus d'observations faites sur des fondations existantes en tenant compte des propriétés caractéristiques des sols.

Prof. A. W. Skempton

One of the important problems concerning the design of piled foundations is the relation between the settlement, under a given load, of a single pile and the settlement of a group of piles, each carrying the same load as the single pile. In other words, if we have done a loading test on a single pile, and have the load-settlement curve, how can we deduce what the settlement would be of a pile group foundation. This seems to me to be one of those problems to which the General Reporter has just referred, namely a problem the solution of which is dependent exclusively upon field evidence derived from case records. And this is particularly true if the piles are in sand.

The analogous problem in spread foundations is the ratio between the settlement of a spread foundation on sand and the settlement of a one foot square test plate, under the same

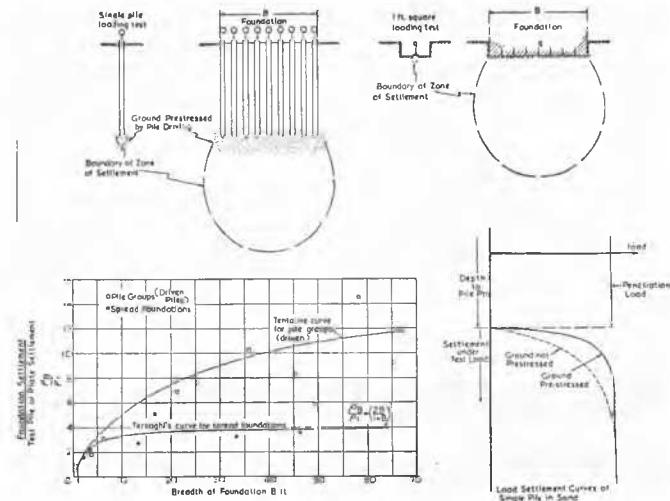


Fig. 1 Settlement of Foundations in Sand
Tassement des fondations dans le sable

pressure. The graph in Fig. 1 shows the settlement ratio curve for this case, which has already been published in the textbook by *Terzaghi and Peck*. The field evidence from which this curve has been derived is not given in the book to which I referred, but I have been through the literature and have plotted the relevant data in the graph.

For the problem with which we are now concerned, namely the settlement ratio for pile groups and single piles, I have so far found eight cases (all for driven piles). Of these, seven were published before 1953 and, on the basis of this information, I ventured to suggest the line shown in the graph, which was first published in a recent Conference in Paris.¹⁾ My excuse for showing this curve again, at the present time, is that the eighth case is that published by Mr. *Lumpert* in the Third Conference Proceedings (1953, vol. II, p. 57). He reports the settlement of a piled foundation about 36 ft. wide as being 8 cm; the piles being 50 ft. long bearing on sandy and gravelly loam underlain by boulder clay and gravel, and carrying loads of about 40 tons. Mr. *Lumpert* gives the settlement ratio of the group to that of a single test pile, as 11. I am inclined to think, from a study of his data, that the ratio is rather greater; possibly as high as 15. However, I have plotted the point using the ratio as given by Mr. *Lumpert* and it will be seen that it is in good agreement with the previous data; thus giving increased confidence in the settlement ratio curve for pile groups in sand.

This curve is conspicuously higher than that for spread foundations. My tentative explanation of this difference is that the settlement of the pile group is due largely to the compression of sand which has not been pre-stressed, whereas the sand involved in the settlement of a single pile has been pre-stressed by driving; whilst, on the other hand, the sand beneath a test plate and a spread foundation has not been pre-stressed in either case.

A method of using the settlement ratio curve for the design of piled foundations has been given in my Paris paper to which reference has been made.

L'auteur traite de la question de la déduction du tassement d'un groupe de pieux à partir du tassement d'un pieu isolé. Il donne une courbe (Fig. 1), basée sur 8 observations, qui semble indiquer une relation bien définie entre le tassement d'un pieu isolé et le tassement d'un groupe de pieux.

¹⁾ Théorie de la force portante des pieux. Ann. Inst. Techn. Bât. Trav Publ., p. 63-64, 1953.

Mr. F. L. Cassel

I shall not comment on any of the papers before this Conference, as the matter has not been dealt with by any of the contributors. I mean the reduction of bearing capacity of friction piles by vibrations and the consequences both for driving the piles and for settlement under vibratory loads.

It is, of course, well known that vibrations reduce the resistance to penetration of piles driven into loose soil or sand, and lead to settlement. This matter has been dealt with at the Second Conference in a paper by *W. F. Swiger* (Session VII.a.4), enlarged on in a previous article in Engineering News Record (13 May 1948).

Considerable use has been made of this property for pile driving in Russia, where thousands of piles have been driven by electro-mechanical vibrators, instead of by monkeys. In a case described (Civil Engineering London, January 1953, by *S. R. Medvedev*) about 3,700 piles of an average length of 27 ft. were driven in that way. Unfortunately, it was not indicated whether they were friction or end bearing piles, nor what the properties of the soil were.

I wish to refer to two cases where the soil in question was of a plastic nature, namely a soft chalk rock. In one case a completed construction was settling continuously, in the other the construction was altered because of the discovery of the peculiar nature of the soil.

I shall give a short description of the soil in question. The Chalk is part of the cretaceous belt in Southern England. This is a soft rock, rather porous and usually saturated, but some of its strata can assume peculiar forms. In both cases, the Chalk was a putty-like mass, with hard lumps of all sizes from fists size to fine grain size, bedded in a soft plastic matrix. Usually piles come quickly to a set because the plastic portion merges in harder non-plastic beds.

The properties of the Chalk in both cases were very similar. I am giving a few descriptive figures:

Case R.M.

L.L. = 29–34%	W/C of the hard portions 20–36%
P.L. = 27–29%	of the soft matrix 26–33%
P.I. = 2–5	

Where investigated the ratio of hard to soft portions varied from 10/90 to 30/70.

Cohesion = $\frac{1}{2}$ –1 lb./in² (0.035–0.07 kg/cm²)

Angle of shearing resistance = 30°–50°

Wet Density = 122–130 lbs./ft³ (1.95–2.08)

Dry Density = 93–103 lbs./ft³ (1.49–1.65)

Compressibility:

1–4 ton/ft² 0.0075–0.0035 ft²/ton
(10.9–43.6 tons/m²) (0.0069–0.0032 cm²/kg)

Coefficient of Consolidation
for the same range 1535–711 in² × 10⁻⁴
i.e. practically instantaneous.

Voids ratio reduction between 1–2 tons/ft² = 1.2%
Water content reduction between 1–2 tons/ft² = 0.5%

Case N.

L.L. = 29%	Water content in situ = 27–33%
P.L. = 25%	
P.I. = 4	

Shear tests on remoulded matrix:

Cohesion = 0–4 lbs./in² (0–0.28 kg/cm²)

Angle of Shearing resistance = 15°–34°

Triaxial tests had erratic results because of the varying presence of hard lumps in undisturbed cores:

Wet Density = 114–126 lbs./ft³ (1.83–2.02)

Dry Density = 87–97 lbs./ft³ (1.40–1.55)

I am not producing all the figures I possess, because it is difficult to grasp them when only heard. It suffices to say that the cohesion of the soil is practically nil, that the angle of shearing resistance is fairly high, that the density is high and the compressibility very low. Consolidation under increasing loads is inconsiderable and rapid, and does not conform to the picture of consolidating clay but rather to that of a saturated sand.

The water content of the hard lumps was actually higher than that of the matrix and the hard lumps are practically incompressible under the loads applied.

In case N., a paper-making machine had been erected on pile foundations. The piles were continuously settling under the vibrations of the machine, and the settlement menaced to break the machine, until an underpinning on piers was undertaken. The success has not yet been proved.

In case R.M., it was proposed to found Diesel engines for the pumps of a pumping station on piles driven into the Chalk. Test piles driven by monkeys did not get set, before they had reached a depth of 60 ft., but subsequent borings disclosed that the plastic chalk extended deeper than 70 ft.

As it was assumed that the vibrations of the engines would drive the piles constantly deeper, it was recommended to found the engines on a raft instead. The work is not completed yet.

It would be desirable if experience at other locations of reduction of bearing capacity of piles by vibrations in plastic soil was brought to light.

L'auteur donne deux exemples de fondations de machines sur des pieux enfouis dans des craies dont il donne les propriétés caractéristiques. Il insiste sur l'importance des vibrations sur le tassement des pieux.

Mr. M. A. Shaarawi

The Cairo North Power Station is a very heavy and important building. The site is situated beside the Ismailia Canal in the valley of the river Nile and near the Mokattam hills outside Cairo. The construction of this building began in 1949. The structure and machinery were founded on more than 1500 Vibro cast-in-place piles.

Normally the Nile valley is formed by the deposit of silt and clay layers on a siliceous sand stratum. On this site, the borings revealed the presence in the upper layers of calcareous sand and shells bound with mica and magnetite materials. The presence of these foreign materials is due to the action of the wind on the deposited materials of the Mokattam hills which were once submerged by sea-water. From the results of the borings, it was decided to use end-bearing piles in the foundations, and that the piles had to be supported on the medium siliceous sand beyond 13 m depth (Fig. 2).

It was eventually decided to employ 17 ins. diameter Vibro cast-in-place piles. Each pile had to carry a working load of 45 tons with a static test load of 90 tons, and the spacing of the piles in a group was fixed at 1.10 m from centre to centre. Several trial piles were driven in different parts of the site and confirmed the presence between 8 and 11 m of a hard stratum all over the site. Moreover, the driving of a few piles in a group proved that this stratum was becoming more and more compact, as the number of piles driven through it was increasing,

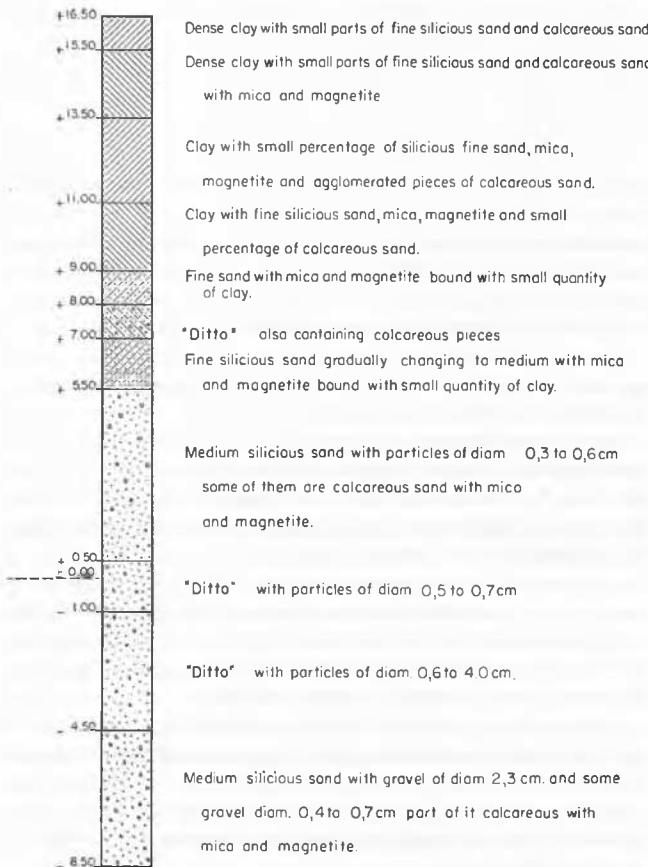


Fig. 2 Soil Profile, Cairo North Power Station
Profil du sol, Cairo North Power Station

and finally it was impracticable to pierce this stratum in order to reach the specified depth (Fig. 3). As the work was extremely urgent, there was not time to proceed immediately with

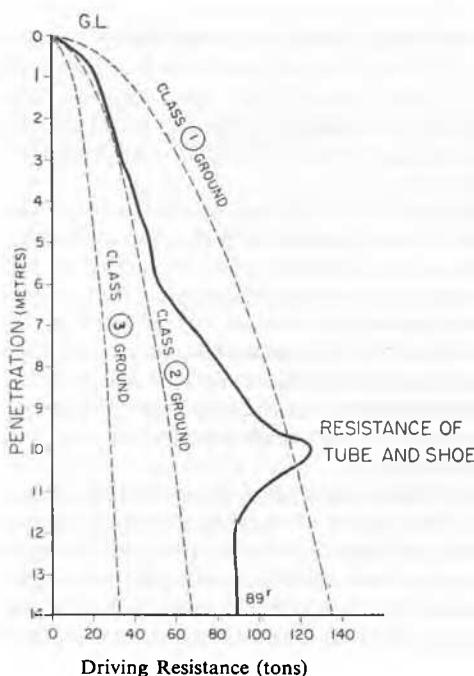


Fig. 3 Diagram of Pile Penetration
Diagramme de battage des pieux

a static loading test to prove that it was quite safe to stop the point of the piles in this stratum as soon as the required set was obtained. Therefore, it was decided to drive first the piles at the centre of each group down to 13 m and over, and then to drive the surrounding piles, trying to drive through the hard stratum as far as possible. In an attempt to have all the piles of one group bearing on the same stratum, considerably smaller sets than necessary were specified for stopping above 13 m. Unfortunately, this resulted in a large number of piles having to stop on the hard stratum, but after having been driven for a long time without appreciable penetration, for some piles absolute refusal was reached.

The piling work was proceeding on these lines and was approaching its end, when the static loading tests started. The tests on the 13 m piles proved to be entirely satisfactory, while the test on the piles founded on the hard stratum began to give appreciable settlements under a small load (Figs. 3, 4). Some of these tests were continued up to a load of 110 tons, proving beyond any doubt the soundness and good quality of the pile itself as well as the good bearing capacity of the stratum under the pile-shoe. It was therefore obvious that an uplift of the short piles had occurred. After a lot of experiments and discussion it was decided that the only sound solution was to rebed these piles. The piles were rebedded by driving, and the uplift seemed to vary between 2 and 3 cm. The static loading tests carried out on the rebedded piles, as can be seen in the figure, proved that the bearing capacity of those piles was satisfactory (Fig. 5).

In our opinion, the uplift of the piles is due to the overdriving of the tube into the compact stratum. This stratum becoming gradually more dense, due to material displaced by the piles going through it, will reach a certain point when it is impossible to increase the density any further. At this point, it will be forced to move around the tube and in the direction of the least resistance which is upwards. This movement lifts the completed piles from the point upwards and appears to cause a certain loosening of the stratum itself.

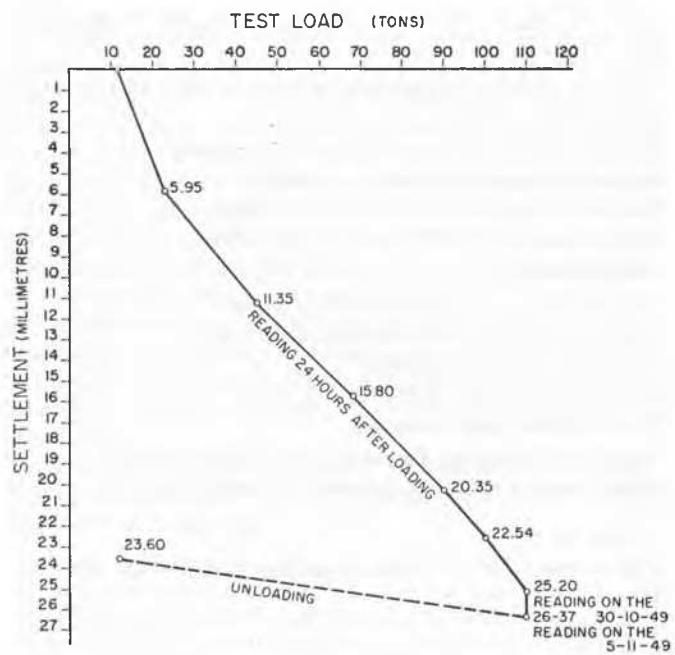


Fig. 4 Load-Settlement Diagram for an Uplifted Pile
Diagramme de l'essai statique d'un pieu soulevé

The followings are the reasons supporting our opinion:

(1) The tests on uplifted piles clearly show that it is not only the clay strata and the piles that have been lifted. If the uplift was due to the clay strata there should have been a clear sign of when the friction finishes and the pile falls back in its original position at which point the support under the shoe begins. Whereas, it seems that the support under the shoe is always present but the stratum has been subjected to a small loosening.

(2) Though a small heaving of the soil was noticed around the tube during the driving through the clay strata, it was ascertained that no uplift of the surrounding piles took place at this stage of the operations.

(3) The uplift of the piles is very small and is always the same whether the pile is in a large or in a small group and also does not vary with the thickness of the clay strata.

(4) Some groups of piles were excavated to a depth of 4 m for examination. No inclination or abnormal displacement of piles were found and the concrete of the piles did not appear to have any defect or cracks.

A photograph of the completed Power Station in Service is given in Fig. 6.

L'auteur décrit des travaux de fondation sur pieux «Vibro» moulés sur place, exécutés pour la «Cairo North Power Station» dans la vallée du Nil aux environs du Caire. A une profondeur de 8-10 m se trouvait une couche compacte de sable de silice. On avait prévu des pieux de 13 m de long, mais, une fois les pieux centraux exécutés, il a été impossible de foncer les autres pieux du groupe jusqu'à cette profondeur. Sur la base d'essais de charge on a constaté que les pieux courts s'enfonçaient sous des charges déjà faibles, phénomène que l'auteur attribue à un soulèvement des pieux déjà provoqué par l'enfoncement du tube dans la couche compacte lors du battage. On a remédié à cet état de choses en rebattant les pieux soulevés.

Mr. B. Fellenius

Concerning the settlement of piles, it may be of interest to describe some results of pile tests made at Gothenburg in Sweden by the Geotechnical Division of the Swedish State Railways. I hope they will be published in the near future.

We test the piles by means of a hydraulic jack connected to four tension piles, each at a distance of about 1 m from the test pile. The clay in Gothenburg is very soft, with a shearing strength of 0.1 to 0.2 kg/cm². The water content is round 67% of the dry weight, sensitivity about 10. The stratum of clay is very thick, over 100 m thick in places. It is of postglacial age, inorganic and nearly clean.

The settlements of buildings on this clay are generally considerable. If we found buildings on piles, we cannot drive the piles to firmer ground in such a clay. We say then that we use floating piles and not the usual friction piles. The settlements will be smaller but still quite considerable.

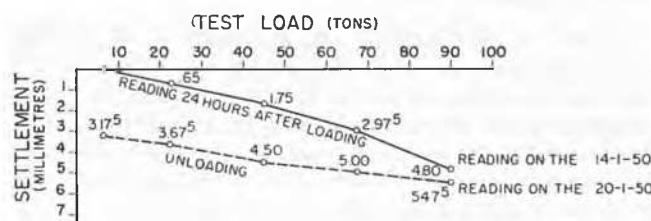


Fig. 5 Load-Settlement Diagram for a Rebedded Pile
Diagramme de l'essai statique d'un pieu rebattu



Fig. 6 Cairo North Power Station now Completed and in Service
Centrale électrique de Caire-Nord actuellement achevée et en service

By testing the piles in this special case, we increase the load by 1 ton every 5 minutes, until the pile sinks down at a velocity of about 0.6 mm/min. (the ultimate load). Then we unload and measure the elastic rebound of the pile. We then get a curve as in Fig. 7.

With wooden piles, it takes about 3 weeks after driving, for the ultimate load of the pile to reach its highest value. I have forced wooden piles nearly 1 m slowly down into the ground, at a speed of $\frac{1}{4}$ mm/min-1 mm/min, and I obtained only about three-quarters of the ultimate load, as found by testing in the way I have just described. Just before beginning to load the pile, it was tested in the usual way and I got a curve as shown in Fig. 7 (diagram 1). After forcing down the pile about 1 m,

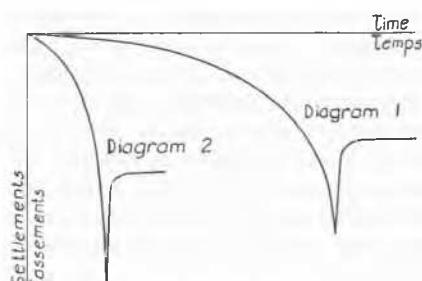


Fig. 7 Settlement vs. Time
Tassement en fonction du temps

I tested the pile in the same way again and I got the same diagram. If I load the pile rapidly, I must wait some days or weeks before I get the same ultimate load. In another case I got a curve like the diagram 2 in Fig. 7.

During this slow loading test, the clay round the pile has not been disturbed in spite of the fact that the pile has settled nearly a whole metre. This is also a flowing or creeping phenomenon. The settlements of buildings founded on piles are not only a consolidation process but also, I believe, particularly in this clay, a creep process.

L'auteur décrit des essais effectués à Gothenburg sur le tassement de pieux dans des couches très épaisses d'argiles tendres. Il arrive à la conclusion que dans les cas étudiés le tassement des pieux est pour une bonne part un phénomène de fluge.

Mr. R. B. Lundström

I want to say a few words about pile testing as carried out in Sweden in clays with a shear strength of about 1-3 tons/m². The piles I am going to speak about are cohesion piles.

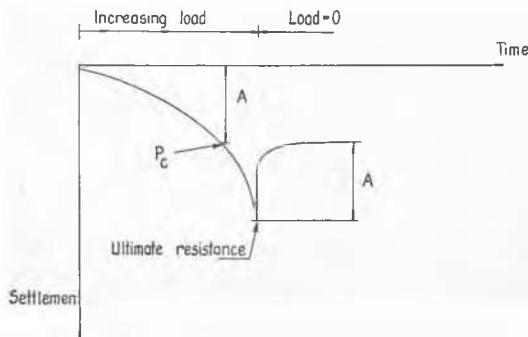


Fig. 8 Settlement—Time Diagram
Graphique illustrant le tassement en fonction du temps

A method of testing piles sometimes used in Sweden consists in increasing the load on a pile by 1 ton every 5 minutes in order to reach the ultimate resistance, then suddenly removing the load. Mr. *Fellenius* has already gone into this method of testing in the previous discussion. We get a settlement curve as shown in Fig. 8. The value *A* is considered to be the greatest possible elastic settlement of the pile, and we can mark off this value on the settlement curve to get the creep load P_c of the pile.

In order to obtain confirmation of this theory I have made different tests on a concrete pile. I have tested the pile according to Fig. 8. I have loaded the pile for a longer period so as to get the true creep load. Further, I have tested the pile as in Fig. 9 in order to find the maximum load for non-increasing settlements after each loading. I have not found any correspondence between the different tests and, in conclusion, it is my opinion that the only value we can rely on, is the limiting load obtained in the last test, as shown in Fig. 9. The factor of safety I have introduced, based on this value, is about 1.5.

When testing this pile and others I have also found that wooden piles reach their highest bearing capacity after about one month, and concrete piles after about 4 to 5 months up to one year after driving.

L'auteur a procédé à des essais comparatifs sur la portance des pieux dans des argiles suédoises (résistance au cisaillement $1\text{--}3 \text{ t/m}^2$), appliquant d'une part la méthode décrite par M. *Fellenius*, augmentation constante de la charge, et, de l'autre, une méthode à charges alternées sans tassement résiduel. Il n'a pas constaté de concordance et est d'avis que, avec le facteur de sécurité de 1,5, la seconde méthode est en meilleure concordance avec les observations.

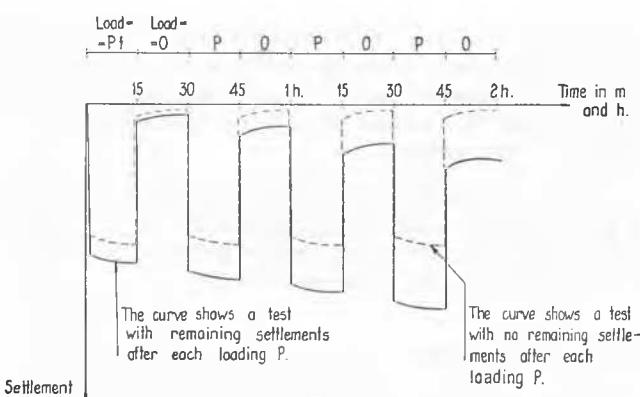


Fig. 9 Time—Settlement Diagram
Graphique illustrant le tassement en fonction du temps

The General Reporter

I wish to thank Mr. *Lundström* for his discussion. This concludes the discussions dealing with field testing of piles. Before we present the next subject, I should like to call your attention to the paper by Mr. *Veder* (Proceedings 1953, vol. II, p. 91), which was not mentioned in the general review. This was an oversight and I trust that you are all acquainted with that paper.

We shall now proceed to our discussions concerning the dynamic problems in connection with piles and pile driving. The first will be presented by Mr. *Buisson*.

Le Rapporteur général attire l'attention sur le mémoire de M. *Veder* (Comptes Rendus 1953, vol. II, p. 91), qu'il regrette de n'avoir pas mentionné dans son rapport, puis annonce que la discussion portera sur les problèmes dynamiques relatifs aux pieux.

M. M. Buisson

1° Parmi les résultats donnés dans ma communication (Comptes Rendus 1953, vol. II, p. 16), l'existence d'une hauteur critique de battage me paraît indiscutable. C'est celle qu'il faut atteindre pour développer un certain déplacement du pieu dénommé refus critique. Les valeurs du refus critique données par la courbe dans la communication semblent assez approximatives (Fig. 3).

A partir du moment où ce refus est atteint, le rapport $\lambda = R_s/R_d$ de la résistance statique à la résistance dynamique reste sensiblement constant. Cela a été vérifié des dizaines de fois à l'appareil de pénétration. Il n'y a pas de raisons pour qu'il n'en soit pas de même dans le cas des pieux. Toutefois, ce rapport peut différer beaucoup lorsqu'on passe de l'appareil au pieu pour bien des raisons, et en fait, l'expérience montre qu'il en est ainsi.

a) Avec l'appareil de pénétration λ croît avec la densité sèche. Une règle de proportionnalité me paraît correspondre assez bien à la réalité. On pourrait sensiblement poser $\lambda = 0,75 ds$. Toutefois, le facteur 0,75 est susceptible d'une dispersion importante de l'ordre de 0,25. Dans les argiles et vases il est nécessaire de prendre plutôt $\lambda = 0,5 ds$. b) λ croît avec la profondeur dans une proportion variable avec le sol encaissant et qui ne peut encore être évaluée. Les valeurs données de λ semblent valables au voisinage de 10 m de fiche. c) λ dépend aussi du sol sous-jacent, et diminue si le sol y est moins résistant que juste sous la pointe. Enfin d) λ dépend fortement de la thixotropie, et e) à un moindre degré de la nature du pieu. Il reste donc un champ important de recherches, avant que soit mise au point la méthode pratique d'application.

Il paraît rationnel de chercher à déterminer la hauteur critique h_1 . Cela peut se faire approximativement au moyen d'une formule très simple que j'ai pu vérifier un certain nombre de fois :

$$h_1 = h \sqrt[3]{\frac{e_1}{e}},$$

parce que l'on peut mettre e sous la forme $e = Ah^3$ et d'une façon plus simple $e = Ah^n$ où n est compris entre 2 et 3,5. Cette formule peut être améliorée si l'on dispose de 2 refus enregistrés d'une façon certaine avec les hauteurs h et h' de chute corrigées. On peut alors poser $e = A(h - l)^n$. A et l sont déterminés par les deux mesures. Avec la première formule, la résistance dynamique est alors

$$\frac{\lambda M^2 h_1}{(M + P)e_1},$$

où e_1 est le refus critique. Une condition essentielle d'emploi de ces formules est que les refus mesurés soient supérieurs au millimètre, et qu'une différence d'au moins 1 mm existe entre les deux refus, lorsqu'on veut employer la deuxième formule. Si l'on émet l'hypothèse de proportionnalité des refus observés à la différence de hauteurs de chute, on obtient une formule qui se rapproche de celle donnée depuis longtemps; mais elle en diffère par un terme complémentaire. Les formules employées importent d'ailleurs assez peu, dès l'instant que les valeurs de λ , h_1 et e_1 sont connues. Il semble finalement que la résistance statique dépende surtout des valeurs de λ , h_1 , e_1 et de la thixotropie. J'espère qu'en utilisant statistiquement les résultats des essais, on finira par aboutir à une formule tenant compte suffisamment des circonstances pour éviter les plus gros écueils. Toutefois, je ne pense pas qu'une formule dynamique, aussi évoluée soit-elle, puisse remplacer les reconnaissances préalables, par forages et par essais statiques de pénétration. – Un champ suffisamment vaste sera ainsi laissé aux formules de battage pour justifier leur utilité. Dès maintenant il est en tous cas toujours possible d'étailler en quelque sorte les résultats de battage au moyen de ceux obtenus statistiquement par les appareils de pénétration et par un essai de chargement.

2° Concernant la communication intéressante de M. *van der Veen* (Comptes Rendus 1953, vol. II, p. 84), qu'il me soit permis d'appeler son attention sur les points suivants:

a) En ce qui concerne sa méthode de déduction de la charge de rupture en partant de la courbe de chargement observée, il faut que le chargement dépasse une certaine valeur pour que la méthode puisse être appliquée valablement. De plus, il existe beaucoup de cas où la courbe ne peut se mettre sous une forme exponentielle, parce que la dernière partie de la courbe est pratiquement une droite inclinée en coordonnées normales.

b) En ce qui concerne l'expression logarithmique de l'affaissement en fonction du temps, je pense que, lorsqu'on l'obtient on est encore assez éloigné de la rupture que je définirais comme la charge provoquant un affaissement proportionnel au temps. J'ai obtenu depuis bientôt 20 ans de telles courbes logarithmiques. De plus, il arrive qu'une telle courbe soit observée par répétition de la charge, alors que la courbe de chargement sous la même charge soit stabilisée.

c) En ce qui concerne le facteur de sécurité à prendre par rapport à la rupture, il est pris en France égal à 28/100 ou 30%, et ce chiffre se raccorde très bien à celui qui est apparu dans la discussion entre M. *Geuze* et M. *van der Veen*. Je rappelle que, en dehors de ce critère, la charge admissible retenue est la plus petite des 3 valeurs suivantes: $\frac{1}{2}$ de celle donnant 20 mm d'affaissement, $\frac{2}{3}$ de celle donnant 10 mm et celle donnant 5 mm. Cette façon de procéder conduit à une sécurité normale, sans excès. Il faut conserver une marge suffisante par rapport à la rupture, pour tenir compte à la fois des phénomènes de plasticité, et des inégalités du sol.

1. The author provides additional data to his paper on the relationship between the static and the dynamic resistance of piles. This relation depends on: (a) dry bulk density, (b) depth, (c) material under the point, (d) thixotropy, and (e) type of the pile. Further, the author comments on the critical height of fall.

2. With reference to Mr. *van der Veen's* paper, the author makes some remarks on: (a) the method of deducing the rupture load, (b) the logarithmic representation of the settlement and (c) the computation of the factor of safety.

M. A. J. da Costa Nunes

Le travail de MM. *Buisson* et *Chapon* (Comptes Rendus 1953, vol. II, p. 16) est à notre avis un des plus importants de

la cinquième session. Il a le mérite incontestable de ressusciter l'espoir que l'emploi des formules dynamiques pourrait avoir un autre but que le contrôle des fonçages dans un terrain bien connu du point de vue géotechnique. Cet espoir était bien mort, surtout après le «Report of the Committee on the Bearing Value of Pile Foundations of ASCE».

Depuis cette époque, la façon de voir ces choses était synthétisée d'une façon lapidaire, par la phrase de *Terzaghi*: «Celui qui utilise une formule dynamique est exactement dans la même situation que le monsieur qui tente sa chance dans une machine à jouer, il est à la merci des lois de la probabilité.»

Cependant, la possibilité d'attaquer les problèmes de la mécanique des sols d'un point de vue statistique, n'est pas exclue, et c'est à notre avis une voie qui promet beaucoup. L'esprit du travail de MM. *Buisson* et *Chapon* nous paraît donc bien actuel.

Un point qui nous paraît un peu douteux est celui que les auteurs eux-mêmes ont trouvé paradoxal. Si l'on a deux pieux dans un terrain de même nature, mais de caractéristiques différentes, comme c'est le cas pour la majorité des terrains naturels, les deux pieux peuvent avoir des résistances statiques et dynamiques fort différentes. Comme il s'agit, en somme, du même type de terrain, le refus critique serait le même, et si un pieu donne au battage le refus critique et l'autre la moitié du même refus (ce qui est courant), est-il naturel de conclure que les deux ont la même résistance?

Nous voyons que les auteurs n'ont pas défini ce que l'on doit considérer comme «résistance statique». Il est connu que le type de rupture des sols compacts est, en général, différent de celui des sols peu compacts. Dans ce cas, le coefficient λ doit dépendre, en plus des facteurs énumérés dans le travail, encore du type de rupture considéré et de la mobilisation plus ou moins complète du frottement latéral. Nous pensons qu'il faut éclaircir expérimentalement ce point-là. Nous croyons en plus qu'il n'y a pas de similitude simple, dans le cas général, entre les fonçages de pieux de même longueur et de diamètres différents, dans un type déterminé de terrain. Comme la résistance de frottement et la résistance de pointe dépendent, en plus de la nature du terrain, du diamètre et de la longueur du pieu et de l'épaisseur des couches, on n'aura, en général, pas le même coefficient λ pour un terrain donné. Nous répétons dans ce cas la phrase de M. *Cambefort* (Comptes Rendus 1953, vol. II, p. 29): «La similitude dans les essais de pénétration ne peut pas se faire par une simple règle de trois».

Un deuxième point de vue que je voudrais discuter, est la question du facteur correctif de résistance, en vue des modifications de la résistance statique que subissent les pieux, plus ou moins longtemps après le battage. Les auteurs indiquent que la résistance statique est déduite des essais qui ont lieu au moins un mois environ après le battage, et que ce délai est nécessaire pour que le renseignement soit valable, soit que le sol se détende après le battage (cas des sables) soit qu'il se consolide. Je veux croire que les auteurs se réfèrent peut-être non pas à une détente du sable proprement dite, mais à des surpressions dans l'eau, d'ailleurs de durée relativement réduite, qui augmentent la résistance dynamique apparente (voir le travail de M. *Plantema* aux Comptes Rendus du Congrès de Rotterdam 1948). Nous avons fait au Brésil quelques expériences de laboratoire qui semblent indiquer qu'il n'y a pas dans les sables une diminution mesurable du degré de compacité en fonction du temps, et nous connaissons d'autres expériences, en particulier celle de *Loos* au Kongresshalle de Nuremberg, qui semblent conclure que la compacité est tout à fait permanente.

Nous pensons que MM. *Buisson* et *Chapon* ont devant eux

un grand travail à faire. Nous voulons suggérer la continuation de ces essais, avec séparation des résistances latérales et de pointe; à notre point de vue, c'est là la seule façon de voir clair dans le domaine de la capacité de charge des pieux.

Nous voudrions, pour terminer, dire quelques mots sur le travail de M. Cambefort (*Comptes Rendus* 1953, vol. II, p. 22) sur la force portante des groupes de pieux. La recherche de M. Cambefort est d'une nature vraiment fondamentale, comme il est bien noté dans le *Rapport général* de M. Peck, quand il dit que «la force portante et le tassement des groupes de pieux n'ont pas été traités de façon aussi approfondie». Il nous semble que la confusion due à la terminologie que M. Terzaghi a bien regrettée au cours des discussions au Comité exécutif, a abouti dans le travail en discussion à une conclusion qui, d'après notre expérience, n'est pas tout à fait exacte. En effet, nous pensons que la capacité de charge ou la force portante d'un groupe de N pieux est, en général, plus grande que N fois celle d'un pieu comme le démontrent aussi, d'ailleurs, les expériences très importantes de M. Cambefort. Au point de vue théorique, on arrive aussi à la même conclusion parce que le sol entre les pieux rapprochés est solidaire des pieux, et l'on a donc une plus grande surface de charge du terrain, ce qui veut dire une capacité de charge unitaire également plus grande, surtout dans les terrains pulvérulents. Cependant, encore en général, la charge admissible d'un groupe de pieux est plus petite que N fois celle d'un pieu, car les tassements du groupe sont plus grands.

Références

A.S.C.E. (1941): *Progress Report of the Committee on the Bearing Value of Pile Foundations*. *Proceedings A.S.C.E.* May 1941—Discussions September—October—November 1941, January—February 1942.
Terzaghi, K. (1942): *Proceedings A.S.C.E.*, February.

The author welcomes Messrs. Buisson and Chapon's paper since it renews the hope that pile driving formulae may be applied. Then he draws attention to still unsolved questions, such as static resistance, importance of the critical rate of penetration of two piles with different static and dynamic resistance which are embedded in strata of the same nature. Further, Mr. Costa Nunes expresses his doubts on the subsequent loosening of the sand which sometimes occur after pile driving. He suggests that the authors should continue their work distinguishing between point and lateral resistance.

With reference to Mr. Cambefort's paper the author wishes to stress the difference between (a) the bearing capacity of a pile group which is higher than the sum of the bearing capacity of the individual piles; and (b) the permissible load which is smaller for the pile group than the total load of the individual piles, owing to the fact that settlements are greater.

Mr. M. Peleg

In Israel buildings on heavy soils are often founded on piles which are cast *in situ* into holes bored in the soil. The piles have enlarged bases and their depth is generally between 2 and 6 m.

There is a certain disagreement on the question as to what is the part played in the bearing capacity of these piles by compression at the area of the base on one hand, and by adhesion and friction at the circumferential surface on the other hand. Laboratory loading tests on some 150 model piles 1 : 10 full scale have been made at the Israel Institute of Technology in connection with the investigation of this problem. Although these tests could not give a measure of the actual stresses between normal-sized piles and the surrounding soil, nevertheless some conclusions could be drawn about the part played

by both above mentioned factors in the bearing capacity of the pile. The tests have shown that significant adhesion and friction stresses on the circumference of the piles appear only when the settlement of the piles reaches comparatively large values, hence the compression stresses at the foot of the enlarged base should have reached their ultimate value long before. This means that the action of adhesion and friction in these models was unimportant as compared with the action of compression. In addition it should be remembered that owing to dry weather in Israel during the major part of the year, the heavy soils contract and crack, and often in the upper 1–2 m there is almost no adhesion between the soil and the foundation piles.

L'auteur cite des essais effectués à l'Israel Institute of Technology avec des pieux moulés dans le sol. On a constaté que l'adhésion et le frottement latéral entraient en jeu seulement lorsque le tassement avait atteint des valeurs assez importantes et l'on estime que la résistance à la base jouait un rôle prépondérant. En Israël, la contraction du sol dû à l'assèchement réduit considérablement l'adhésion du sol aux pieux.

Prof. E. C. W. A. Geuze

In his present paper Mr. van der Veen (*Proceedings* 1953, vol. II, p. 84) has advanced a semi-empirical relation between the load on a pile and the time settlement. According to his results, the settlement of the pile toe would be a linear function of the logarithm of time, even in the case of a point-bearing pile, i.e. if the toe is embedded in a cohesionless sand layer of sufficient thickness, as presented in Fig. 10 of his paper.

Secondary time effects covering time periods of more than 20 hours are in contradiction with the nature of the sand strata. However, they are entirely consistent with the gradual yield of the upper cohesive soil layers under the shearing forces developed by skin friction, resulting from the settlement of the pile with increasing load on the pile head. The gradual yield of the cohesive layers is due to the relaxation of shearing stresses as observed in shearing tests when a deformation of constant magnitude is suddenly applied.

Hence the load on the pile toe increases with time following some exponential function, which might explain its observed semi-logarithmic straight line character.

The settlement of the pile at the highest loads also shows a time dependency, however, for entirely different reasons.

As I have pointed out in my lecture at the Paris Conference on the Bearing Capacity of Piles, model tests carried out at the Delft Soil Mechanics Laboratory have shown that the settlements of the pile toe in a sand layer reaches its final value at each load increment in a very short time, provided the load on the toe is rigorously kept constant. With a model pile of about 20 cm diameter this period amounts to about 10 seconds. If a load of about 50% of the ultimate toe resistance has been reached, the next increases will cause a state of unstable equilibrium, resulting in a series of breakdowns of the grain structure at a constant load. This state has been called the "hesitating" part of the load-settlement curve. Further increments clearly indicate the viscous behaviour of a saturated sand layer at high shearing stresses, giving a series of curves which end in straight lines, i.e. a constant speed of deformation at a constant stress. These results are also contradictory to Mr. van der Veen's formula on the time dependency of the settlement.

The most important fact to be observed in our own test results is the "hesitating" part of the load-time-settlement curve. In the case of sandy bearing strata, the transition from

time-independent to strongly time-dependent settlements covers a very limited part of the range of loads. From the transition load up to the ultimate value the resistance depends on the speed of the penetration of the pile toe. This is the reason why it is impossible to determine the limiting value of pile resistance from a loading test, and that is the point where the speed of penetration comes in.

In the case of a 20 cm diameter model pile, an increase of 7% in this ultimate value has been obtained by increasing the speed in penetration 10 times. This fact explains why the usual load-settlement curves of point bearing piles in sandy strata do not show a definite ultimate value.

Summarizing this part of my observations, I would propose to accept those loads belonging to the time-independent range as allowable loads on the pile-toe.

I intentionally rule out that part of the load on the pile head which can be safely transmitted to the cohesive soil layers by skin friction, as we are still far from understanding the mechanics of this part of the problem.

In our model tests the transition point from time-independent to time-dependent settlements was found at 70% of the ultimate load with a 10 cm² cone point, at 67% with a 30 cm² cone point, at 60% with a 100 cm² cone point and at 52% with a 300 cm² cone point.

Under repeated loading and unloading Mr. van der Veen found a slight permanent set at about 60% of the ultimate load on the pile head, whereas a marked effect was obtained at about 70% of that value.

In my opinion this fact proves more than anything else the occurrence of a yield value of pile point resistance in sand layers, similar to our own experience with model tests, and this value should be taken as the logical limiting value of permissible pile point loads. Similar tests on pile points of increasing diameters will be needed in order to investigate the effect of the dimension of the pile point on the value of the transition load.

A second point of interest is represented by the comparison between a penetration test and 7 loading tests in Mr. van der Veen's paper. By taking the ratio of the penetration value at the depth of the pile point to the ultimate value of pile point resistance, results were obtained as represented in Fig. 11 of Mr. van der Veen's paper. This diagram suggests a scatter of ratios between 75% and 180%, if pile points of different areas are used. No account however has been given of the method of interpreting cone test results, such as used by the Delft Soil Mechanics Laboratory and described by Messrs. van Mierlo and Koppelan in the periodical "Bouw" of January 1952.

Apart from this consideration, which affects the magnitude of the ratios, the scatter is highly dependent on the statistical variations of the soil properties in vertical and lateral directions of the loaded area.

It therefore seems unnecessary to apply such a high factor of safety as Mr. van der Veen has accepted, which tends to increase the number of piles, whereas great economy and safety would be obtained by a suitable increase of the number of soundings.

L'auteur fait quelques remarques sur l'article de M. van der Veen (Comptes Rendus 1953, vol. II, p. 84). Il est d'avis que, pour des charges modérées, les tassements secondaires augmentant avec le temps proviennent de la relaxation des contraintes de cisaillement le long de la surface latérale. Pour des charges plus élevées, l'augmentation du tassement avec le temps s'explique par un équilibre instable avec ruptures locales de la structure (phase d'«hésitation»). Pour des charges encore plus élevées on constate un comportement visqueux du sol avec vitesse de tassement constante. La charge admissible ne doit pas provoquer des tassements augmentant avec le temps.

L'auteur commente également la comparaison entre l'essai de pénétration et les essais de charge et les conclusions de M. van der Veen relatives au choix du coefficient de sécurité.

Mr. C. van der Veen

I should like to make a few remarks on Prof. Geuze's discussion on my paper concerning the bearing capacity of a single end-bearing pile. Prof. Geuze's explanation of the time dependence of the settlement of a loaded pile seems to me to be of extreme interest.

I am not quite sure that the secondary time effect I have measured, when exerting a relatively small pressure on the pile, is caused by a gradual yielding of the upper cohesive soil layers, as Prof. Geuze supposes. So far, however, there are reasons why I prefer to believe that from the very beginning, or nearly the very beginning, there is a time dependence between the settlement which is caused by the properties of the sand layer in which the pile toe is placed.

I am strengthened in that belief by the figures Prof. Geuze has given. He states that in his model tests he found that time dependence of the settlements began at 70% of the ultimate failure load when a cone with a 10 cm² point was used. When a 100 cm² point was used time dependence began at 60% and for a cone of 300 cm² the percentage had already diminished to 52%.

Now the toe of the pile which is given as an example in my paper had a area of 2500 cm², which is far larger than the largest of Prof. Geuze's test-piles. Therefore, it seems to me quite possible that the percentage of 52% found by Prof. Geuze in the case of a 300 cm² point would be much lower if a 2500 cm² point were used.

There remains the fact that the time dependence found by Prof. Geuze indicates a constant speed of deformation at a constant stress, whereas I found a dependence on the logarithm of the time. I cannot make out for the moment what can be the cause of this difference between our measurements. To me seems more important the fact that the time effect has been measured. But, if Prof. Geuze is right and if his proposal (that only the loads in the time independent range can be safely allowed on a pile) is accepted, we have to face the fact that in the case of a 300 cm² point only about 50% of the ultimate load on the point is permissible, and I fear that for pile points in the range of 2000 or 3000 cm² not more than 40 or perhaps 30% is permissible.

In contradiction with Prof. Geuze's conclusions that a lower factor of safety than that given in my paper could be used. This would lead to a factor of safety which is nearly twice as high. I exclude for the moment that part of the total factor of safety which is to be introduced in view of the uncertainties involved in the methods to predetermine the ultimate resistance of a pile.

L'auteur répond à la critique du Prof. Geuze. Il est d'avis que, pour des pieux ayant une grande section de base, les tassements dépendent du temps pour des charges représentant une plus faible proportion de la charge limite de rupture que pour les pieux expérimentés par le Prof. Geuze. Si l'on choisit comme charges admissibles celles qui ne provoquent pas une augmentation des tassements avec le temps, l'on arrive à des coefficients de sécurité plus élevés que celui que l'auteur propose.

Mr. C. van der Veen

Some of the papers in this section contain valuable data concerning the dynamics of pile driving. I think it very neces-

sary that attention should paid to the dynamic behaviour of the soil, because very little is known about it and not much progress is being made.

I should like to make a few remarks on Mr. *Bendel*'s paper "Stresses in piles and walls during pile driving" (Proceedings 1953, vol. II, p. 7). The author mentions tests in which he has measured strains in a concrete pile, while it was being driven, and those in a wall which stood alongside. He finds it surprising that while the pile only deformed when the hammer strikes, the wall deformed for more than half a second as is illustrated in Fig. 2 of this paper. Perhaps this can be explained by assuming that the building, under the influence of the pile driving, vibrates with its own natural frequency. I think that the author should make quite sure that the vibrations measured in the wall are caused by the pile driving at all!

What is really surprising is that the deformations in the head of the pile was largest when the toe of the pile reached the rock level. When the hammer strikes the head of a pile of some length, the stress wave which is caused is still independent of what is happening at the toe of the pile. This is clearly demonstrated in Mr. *Nanninga*'s paper. In two cases I have measured stresses in piles during driving through loose strata into a hard sand layer. In both cases the stress wave occurring at the head of the pile was independent of the resistance at the toe of the pile.

The reflected wave is certainly influenced by the resistance of the soil at the toe of the pile. When reaching rock this wave will have about the same shape as the original wave; the stresses which occur in the head of the pile are, in the most unfavourable case, equal. When the pile toe is in loose soil, tensile stresses can occur, which in my opinion are more likely to have been one of the possible causes of the destruction of the pile head.

At any rate tensile stresses were measured, as is demonstrated in Fig. 2 (measurement V 3) of the author's paper. Therefore it is not clear why he disregards these tensile stresses.

Perhaps some of the surprising test results can be explained by the circumstance that a 4-channel dynamic strain recorder was used. This instrument is unreliable, if used for recording stresses during pile driving. A cathode ray oscilloscope should have been used.

A very interesting paper has been submitted by Mr. *Nanninga* who proposes the use of a hammer which is adapted to the cross section of the pile and provided with a jacket in order to transfer the kinetic energy of the hammer completely to the pile.

If this could be realized a more efficient method of pile driving would be obtained. However, will the stress wave in the core of the hammer, as constructed by *Nanninga*, travel down through the jacket as he expects? In my opinion only an experiment will give an answer to that question.

Secondly, it seems to be a disadvantage for the practical use of such a hammer that the cross section of the hammer depends on the cross section and the modulus of elasticity of the pile. This means that only a certain hammer can be used for a certain type of piles.

I agree with Mr. *Nanninga* and with the General Reporter that it will not be easy to get a dynamic pile-driving formula with a general validity. If for instance the conclusions of Messrs. *Buisson* and *Chapon* in their paper on the "Relation between the static and dynamic resistance of piles" (Proceedings 1953, vol. II, p. 161) are accepted, there remains the fact the point resistance and the friction along the pile shaft are not separated.

There is one point in their paper which is not clear to me. How was it possible to determine λ as well as e_0 in the revised Dutch formula? It is stated (page 20) that λ was determined from *Boonstra*'s pile loading tests in cases when e was larger than e_0 ; but it is not clear how this can be done if the value of e_0 is not known.

L'auteur commente d'abord l'article de M. *Bendel*. Les vibrations continues de la maison avoisinant le pieu peuvent provenir de la fréquence propre. Il s'étonne du fait que les contraintes de compression du pieu soient maximales lorsque le pieu touche le sol. L'onde de pression provoquée par un choc est indépendante des conditions à la base du pieu. L'onde réfléchie dépend par contre de ces conditions et peut provoquer dans des matériaux meubles des contraintes de traction suffisantes pour expliquer une rupture.

La proposition de M. *Nanninga* relative à la forme du mouton présente une idée très intéressante.

Finalement l'auteur demande à MM. *Buisson* et *Chapon* des précisions sur la façon de déterminer λ et e .

Prof. D. Haber-Schaim

Permettez-moi quelques remarques sur les recherches et les discussions que nous avons entendues au cours de ce congrès. Le but de toute recherche reste toujours d'apprendre les conditions d'adaptation entre le sol et le bâtiment.

Le sol n'est pas le dernier but, c'est le bâtiment. Tout tassement d'un bâtiment est un processus réciproque entre le sol et le bâtiment. Nous sommes intéressés à ce que ce processus d'adaptation, de déformations réciproques s'accomplisse sans dommage pour le bâtiment.

Il n'est pas dans l'intérêt de la santé du bâtiment que le processus de consolidation se développe aux frais de notre ouvrage. Ce n'est pas la fondation seule, mais tout le bâtiment dans sa totalité qui participe au tassement pendant la consolidation du sol.

Nous devons chercher quels sont les tassements compatibles avec notre but. Maintenant abordons le thème d'aujourd'hui. Pour étudier le problème de l'influence réciproque de deux pieux, j'ai fait des essais avec 15 pieux forés dans un sable argileux. Le diamètre de tous les pieux était de 25 cm.

Dans ce travail de recherches nous avons voulu comparer le pieu simple avec le pieu qui fait partie d'un groupe sous le double effet des longueurs différentes, en même temps que de l'influence réciproque de deux pieux. C'est un moyen d'apprendre comment se produisent les déformations réciproques ainsi que les tassements des groupes de pieux. Nous avons mesuré le tassement du pieu sous charge et en même temps le mouvement du second pieu.

Je ne veux pas donner maintenant le rapport de cette étude mais communiquer quelques résultats préliminaires:

1° La différence entre les tassements des pieux du même type du même groupe est remarquable, il n'y a pas deux pieux qui tassent de la même façon.

2° L'influence des bulbes est négligeable, et souvent nuisible à cause des erreurs pendant l'exécution.

3° Le pieu non chargé s'est éloigné d'une quantité sa et a remonté d'une quantité sh .

Il serait désirable que d'autres collègues puissent faire de pareilles recherches en vue de confirmer ces effets de déformation.

The author gives the results of loading tests on a group of 15 bored piles. No two piles showed the same settlement. The pile bulb does not exert any favourable influence; it may even be detrimental in case of faulty construction. Nevertheless one pile of the group which did not carry any load moved with the others.