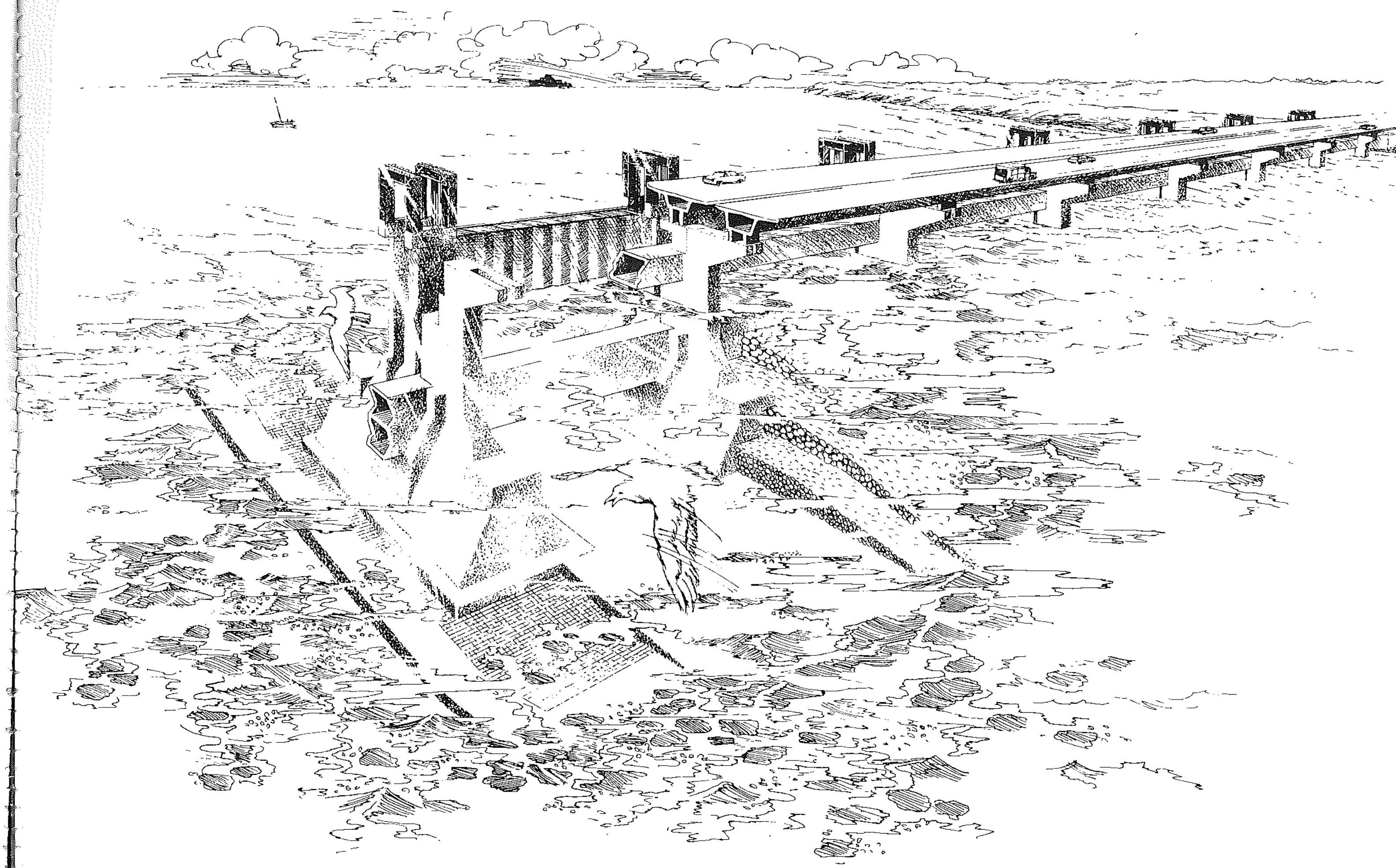
FOUNDATION ASPECTS OF COASTAL STRUCTURES

Proceedings volume 3

International Symposium on Soil Mechanics Research and Foundation Design for the Oosterschelde Storm Surge Barrier.

Delft, The Netherlands, October 9 - 12, 1978.



Oosterschelde Storm Surge Barrier
the Netherlands

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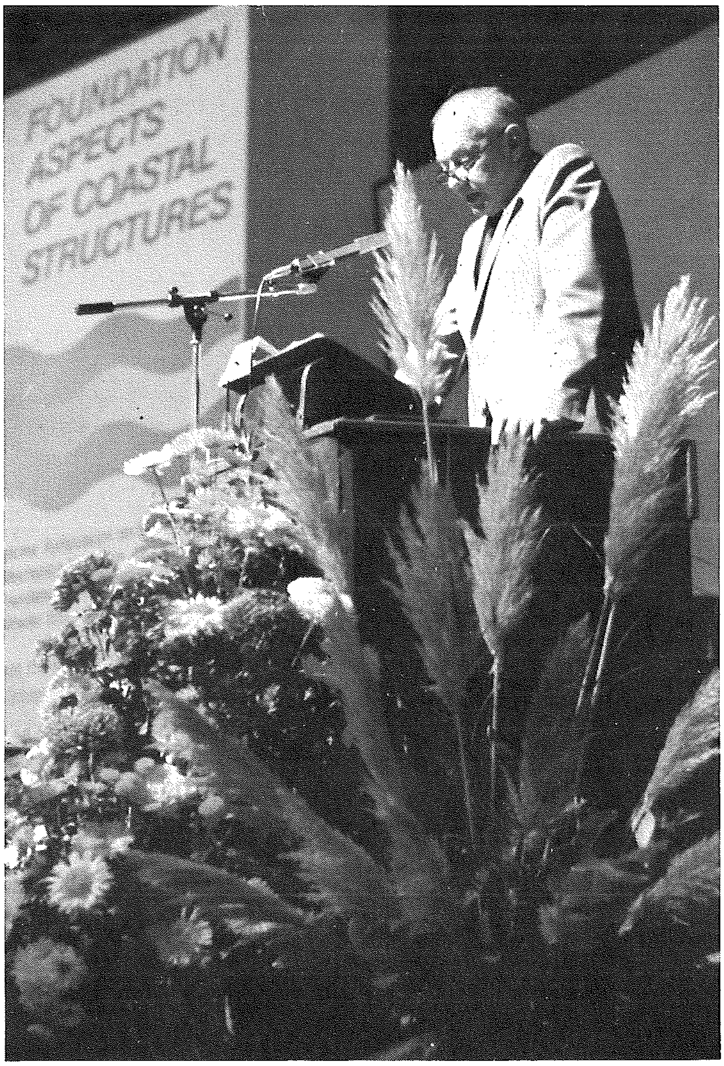
*** Dosbouw v.o.f. is the consortium of building contractors that is building the Oosterschelde Storm Surge Barrier.

CONTENTS VOLUME 1

CONSTENTS VOLUME 2

CONTENTS VOLUME 3

Opening ceremony



Director-General of the Rijkswaterstaat J. W. Tops during the Opening Address.

ADDRESS OF WELCOME

by H. Engel
Director of the Deltadienst of Rijkswaterstaat

Ladies and Gentlemen,
On behalf of the Symposium Committee I bid you welcome to Delft, welcome to our symposium, Foundation Aspects of Coastal Structures.
I would like to extend a special word of welcome to the director-general of our Department of Public Works, Water control and Traffic, generally called Rijkswaterstaat. He will hold the opening address.
Between all our distinguished guests I would like to mention the ladies who had the courage to come with their husbands to a country where the Organizing Committee tells you that the temperature seldom rises above 15 degrees, and that raincoats are advisable. We are very glad you are with us, we hope to see you and we hope you will like Delft, the place we chose not only because it is the cradle of Dutch engineers, but also because it is a lovely old town.
The Symposium has three major objectives, the first is a very egoistic one, to give all the people working on the storm surge barrier a clear view of what the geotechnical people found and what their conclusions are. The second is that we invited you as experts to hear these speeches and hopefully you will criticize them, because from criticism springs truth. The third and the last is the noble reason in the programme, to inform our colleagues from all over the world of our findings.
The organisation of this symposium was started about a year ago under the responsibility of the Netherlands Society for Soil Mechanics and Foundation Engineering as a joint venture between the Delft Soil Mechanics Laboratory and the Rijkswaterstaat.
It was backed up and supported by the Delft University of Technology, the Royal Institution of Engineers in the Netherlands, and the contractors involved in the barrier design. There are about 200 participants, about a third of them come from abroad. It is my sincere wish that all of you should enjoy this symposium both technically and socially. Thank you.
May I invite the director-general of the Rijkswaterstaat to hold his opening address.

OPENING ADDRESS

by J. W. Tops
Director-General of the Rijkswaterstaat

Mr. Chairman, ladies and gentlemen,
Foundations play an important, if not a crucial role in the construction of our coastal defences. As you already know, or will see in the film that will be shown after my introduction, a very important part of the Netherlands lies below sea-level and has to be protected against the North Sea.
Failure in the construction of our coastal defences can cause flooding of 50% of our country. The Delta Project reduces this risk by heightening and/or strengthening the dykes, by closing a number of estuaries and by building a storm surge barrier in the Eastern Scheldt.
Nearly 25 years ago, when the Delta Project was started, the closing of a big estuary like the Eastern Scheldt was considered very difficult, and we decided that we would develop new methods and gain experience while realizing the closing of smaller sea-arms. This is the reason why the Eastern Scheldt, which has a tidal volume about twice as big as the other estuaries in the Delta Project,

was to be the last sea-arm to be protected. In the last twenty years the construction of our civil-engineering works was, however, more and more influenced by environmental considerations. These environmental considerations led to the choice of a storm surge barrier for the Eastern Scheldt instead of a permanently closed dam.
Three conditions are attached to this decision, taken by the government in 1974:

1. the extra costs are limited to a fixed, but very considerably amount;
2. the project has to be ready in 1985;
3. the project has to be technically feasible.

The first condition indicates clearly how much the government is prepared to pay for the preservation of such an important environment as the Eastern Scheldt. Although environmental aspects are very difficult to fit in a cost-benefit analysis, very much importance was attached to the environmental value of this area. We can be sure that environmental and social considerations will, in the future determine to a large extent our infra-structural constructions. This is, of course, only possible when adequate technical solutions can be found.

The second conditions - the time factor - has been introduced not to withhold delta-safety from the population around the Eastern Scheldt any longer than necessary.

The last condition - no doubt the most important one for you - regards the difficulty of the assigned task.

It can be said that the project is of a unique character in many aspects. Practically all elements of the partially opened construction are technically well-known in principle and have been used before. However, the extreme scale of these elements and the combination of them in a rough area such as the mouth of the Eastern Scheldt represents a completely new situation. The problem of the foundation, in which the Delft Soil Mechanics Laboratory is deeply involved, is an aspect of extreme importance. During one and a half years of study, to explore whether the fixed conditions could be met, it was proved several times that we are closely approaching the limits of our technical know-how.

Mr. Chairman, I am of the opinion that a symposium like this can contribute to make everyone understand clearly the way in which we have dealt with the problem of the foundation of the storm surge barrier in the Eastern Scheldt.
We are proud of the fact that we have been able to make this storm surge barrier in the Eastern Scheldt. I leave it up to you to decide whether to agree with the statement of our former minister Westerterp, that the storm surge barrier in the Eastern Scheldt is to be the greatest civil-engineering work of the century.
I hope that the experience gained in the research for the best possible foundation of the storm surge barrier, will provide other engineers with solutions for problems that they will meet in the future.

Mr. Chairman, it is my privilege after this introduction, to wish you a successful symposium, and you, ladies and gentlemen, a number of instructive days.
Thank you.

Session I
Review of the Project and History of the Design

Chairman:
W. Bokhoven, Director of the Delft Soil Mechanics Laboratory,
The Netherlands

Chairman:

I have the pleasure to open the first session of this symposium. Section I concerns a review of the project and the history of the design. It focuses on the theme of the conferences, Soil Mechanics Research and Foundation Design for the Eastern Scheldt Storm Surge Barrier. Focusing implies distance and willingness to select. All three speakers of this morning have played and are still playing an important role in managing and performing the study of the closure of the Eastern Scheldt. They are all Rijkswaterstaat collaborators and you will realise this morning that their approach to the topics to be discussed shares common characteristics, namely awareness of the many problems in respect of designing coastal works in the Netherlands, more emphasis on boundary conditions than on the design details, and finally developed or adapted construction techniques based on new scientific and technical knowledge. These common characteristics in my opinion bear witness to a fine Rijkswaterstaat tradition in designing and building coastal engineering products of high standard.
Ladies and gentlemen, I hope that this introductory session of the geotechnical symposium provides a clear framework for the contributions of the other sessions.

I would like now to introduce Mr. Engel to give an overall picture of the project. Mr. Engel is head of the Deltadienst, that is a special department of Rijkswaterstaat which is in charge of the execution of the Delta Project. It is evident that Mr. Engel is in the best position to give that overall picture. May I ask Mr. Engel to take the floor for his contribution.

OVERALL PICTURE OF THE PROJECT
by H. Engel
Vol. 1, Paper I.1

Chairman:

Thank you Mr. Engel, you really succeeded in your paper and now again on the floor, to illustrate the implications of the project, in view of the environmental aspects. As you pointed out, this confrontation led to a totally different design, much more complicated than the original, and in my opinion never before in the Netherlands has so big a percentage been applied for research for big civil engineering projects.
Thank you very much. Mr. Engel.

May I now invite Mr. Spaargaren for his contribution concerning a review of the various designs.

REVIEW OF THE VARIOUS DESIGNS
by F. Spaargaren
Vol. 1, Paper I.2

Chairman:

Thank you. Mr. Spaargaren. You illustrated very well the many boundary conditions to be respected during the design work. Of these conditions the soil mechanical conditions had great influence on the design. This conference deals only with foundation aspects. It is important that you succeeded so well to relate the soil mechanical conditions to all other criteria to be considered.

This avoids for all those who are not involved in this project the risk of over-estimating the role of the geotechnical engineer, which of course remains of utmost importance. Thank you very much, Mr. Spaargaren.

May I invite the last speaker of this session, Mr. Boehmer. Mr. Boehmer is senior engineer in science and engineering analysis of the Delta Department of Rijkswaterstaat. Mr. Boehmer has been involved in all soil mechanics and foundation problems from the very beginning of the design study of the closure of the Eastern Scheldt. With the contribution of Mr. Boehmer we are inevitably approaching the theme of the conference.

SPECIAL GEOTECHNICAL STUDIES AND THEIR UNEXPECTED EFFECTS
by J. W. Boehmer
Vol. 1, Paper I.3

Chairman:

Thank you very much, Mr. Boehmer. I said before Mr. Boehmer started that we were approaching the theme of the conference, but you have seen yourself that we are already in the middle of the theme. Thank you very much, Mr. Boehmer, for your contribution and for your enthusiastic performance.
Ladies and gentlemen, we have 15 minutes for discussion.
I think that especially the contribution of Mr. Boehmer, on page 16 of his paper you have seen a number of conclusions, can give the possibility for a number of questions.
Who will put the first question?

G. de Josselin de Jong

It has been pointed out by Mr. Engel that the Oosterschelde project is an unusual project, requiring the collaboration of multi-disciplinary teams. Indeed the situation is unusual with respect to the soil mechanics aspects, because the sand has an exceptional low density and shows an unstable behaviour.
Personally, I was not involved in the studies, but I was aware of them, since much activity was going on all around me. I was much impressed by the courage of the young people to develop computer programmes for the sand behaviour and their perseverance to tackle all kind of difficult aspects. It was of benefit to soil mechanics that great effort was made to implement more fundamental mechanical principles, than commonly used in soil mechanics.
However, the behaviour of loose sand is still not completely understood, basically. What we need is more insight into the mechanics of deformation. Then, possibly, constitutive relations could be developed in a similar manner, as Rowe did for his stress-strain relations.
My question to Mr. Engel is how much confidence Rijkswaterstaat has in soil mechanics in its present stage in taking the important decisions with respect to the great works they are responsible for? Has soil mechanics enough reliability at this moment?

H. Engel:

Well, Mr. Chairman, this is a difficult question. In the first place Prof. De Josselin de Jong states the problem of the soil mechanics, and that is indeed in this case the most serious problem. Well perhaps hydrologists or environmentalists would put the most serious problems somewhere else. You state that the soil consists of a not too good sand, well, to another audience I would say it consists of sand at that is much better than most of the soils that we have in the Netherlands. Well, then about the confidence, it is of course a method followed in the past that you show a lot of confidence at the start and afterwards have to regret it bitterly, because you find things that you would have found earlier if you did not put that confidence so high. So our aim was not to prejudge the results. What engineers used to do, is to try to find the limits of our safety. I mean that if we ask a soil mechanics expert to give us the worst idea he can give about the construction, then we will accept that as a boundary until the time when we know better. And within this limiting way we as engineers are sure that we can make a safe storm surge barrier and only on this assumption could we start the further design after the one and a half years the government gave us to come to this conclusion. Of course there will always be new things found when you do research, and of course we always need new models to give a better idea of what really happens. But I think I speak for the whole team of designers that our findings until now give us the engineering prognosis that we can make a solid and stable storm surge barrier.

C.J. Sammons:

The author mentioned that protection from erosion on the inland side of the barrier was to be provided by 5-10 tons heavy blocks. Have the designers experience with using this size of blocks for protection against erosion by high flows? If not, what criteria or formula have they used in the design, and how much confidence have they in its use for flows of the size envisaged?

F. Spaargaren:

Well, I mentioned these heavy blocks as a top layer of the sill. Your question is: Do we have experience in practice with those types of layers and the dimensions of stones, and I have to say: No, until now we have experience with much smaller stones, up to say about one ton in the actual gaps we have already closed in the Delta plan, so what it means is that we extrapolated by means of model tests the stability of this top layer, and we have the back up of the former model tests to the actual gaps, and we have confidence that based on these new model tests these types of blocks can withstand the heavy current.

J. Blaauwendraad:

It's a pity, Mr. Chairman, that Mr. Boehmer could not do everything he wanted for I think the aspect of the relation between the geotechnical engineer and the designer would have been a nice topic to present to this audience. It must be a real problem to do this in a proper way and it may be connected with his slide in which he showed a relation between the total cost of the total project and the costs which are necessary for the studies, and I think it was quite challenging what he said, that when you reduce the costs for studies and research you may increase your total costs. I can understand you do this, you use less money in the early phase of your total study, but could he please explain if it is possible to have so much study in a phase of the total project in which your design is more or less fixed already.

J.W. Boehmer:

I think there is a basic difference between this project and other projects. I call this a project of flexible bidding in which the bidding on some parts of the design is still open after fixing the bids and starting construction on other parts of the design. My feeling is that after the contractor has learned from the design studies what the potential problems or "spooks" during construction can be he then has the right to come up with these spooks when he makes his bid. If these spooks have not been solved yet, he may say: "Well, here is a bid I have to make, there are about six spooks in there and I add 10% for each spook." Now, if the researchers are present and if they can improve their knowledge on the basis of condition control measurements, they can help eliminate these spooks, so the cost of the bidding goes down again. If they are not present, construction costs may rise above the initial estimates as a result of these spooks. I think this is the essence. I think this will be the most important factor which controls the uncertainty in the bidding costs of this project and which requires extra costs for study. The second factor is that the designer has to keep in close contact with his researchers, as long as not all parts of the design have

been fixed.
For example, if he makes the piers heavier or if he replaces more soil, or if he makes his sill heavier, then not only construction problems could become bigger but also the whole design could become more expensive as well. These are two components which besically influence costs before 1985. Then there is a third component, which is the maintenance costs. Although they only enter after 1985, they are in effect part of the total project costs. Although we aim for a design with a minimum of maintenance costs I can imagine that the extra know-how which will result from condition control of the barrier and from further research might decrease maintenance costs in case unforeseen problems arise.
The question ramsins how you can make a cost benefit analyses in which you include these three factors and which results a reasonable guideline for further study effort. I think there are possibilities to do this with a probabilistic approach, but it will take some time before we work that out.
I'd like to invite you to help us doing that, because I know in concrete research, you are discussing the same sort of questions.

H. Engel:

I fear, Mr. Blaauwendraad, that you tried to get Mr. Boehmer out of his field by this question and I must admit that I agree and disagree with Mr. Boehmer.
I agree on the point that continued research is necessary to complement the execution of this work. I do not agree with figure 21 shown in the paper of Mr. Boehmer, and I think we shall at all times have to weigh out the research and the results we expect of them. And as to Dutch contractors they may see spooks when it comes to bidding, but I am sure that most of them are not seeing spooks at all in reality.
Thank you.

R.S. Wright:

Mr. Chairman, I'd like to ask a question of the last speaker regarding the depth of the scour hole on the down stream side of the barrier.
He showed the 25 metres estimated depth of scour and my question is, is there some limit to the depth of the scour, which would be a self limiting feature rather than an unlimited depth of scour hole.

J.W. Boehmer:

If you consider the energy loss over the barrier it turns out that the sill is so high that we have about the maximum energy loss you can have from the water passing through this barrier. As a result there is a lot of turbulence in there, and the turbulence seems not to damp out very fast. Even behind a river bed protection of 600 metres the turbulence is quite heavy, and does not depend so much on the depth of the water over which the flow is going. Only deep scour holes with steep slopes which have been stabilized to prevent slope failures, will eventually reach equilibrium, provided some sand transportation remains present over the barrier.
Suppose now that the steep slopes do fail which results that the riverbed protection moves down and that the depth of the water increases, there still might be 25 metres extra scour hole under the future depth of the edge of the bottom protection (of course it could not go on until 500 metres). This was a reason to decide not to allow any loss of stability on the edge of the bottom protection. Already in 1974 it was decided therefore to densify the edge of the bottom protection and, if slopes get steep, also to protect these slopes by adding gravel or other materials. In doing this we are more flexible to do more research on the scour holes.

Chairman:

Ladies and gentlemen,
I am now obliged to close the session. I can imagine that you still have other

questions. You can put them in writing on the discussion sheet and of course Mr. Boehmer, Mr. Spaargaren and Mr. Engel will be attending the whole conference and you then have time and the possibility to put your own question to them. I would like to thank once again the three authors and the speakers of this morning for their contributions. I think we have now the frame work of the theme of the conference, and I would like to thank you too for your presence here. Thank you very much, I will now close the session.

Session II

Stress-Strain Behaviour of Oosterschelde Sands

Chairman:
J. T. Christian, Stone and Webster Engineering Corporation,
Boston, Massachussetts, U.S.A.

Chairman:

Good afternoon, ladies and gentlemen.
My name is John Christian. I am a consulting engineer with Stone and Webster Engineering Corporation.
I have been fortunate to be a consultant to the Rijkswaterstaat Deltadienst since 1974 on this project. We have today, this afternoon three papers that will be presented.
The first paper that will be presented has two authors. The first author is W.A. Marr, who is a Research Associate at the Massachusetts Institute of Technology in Cambridge Massachusetts in the United States, and he has been a consultant to the Rijkswaterstaat Deltadienst since 1974.
The second author of the paper, who is not here today, is Dr. Kare Hoeg, who is the director of the Norwegian Geotechnical Institute in Oslo.
The paper will be presented by Dr. Marr.

STRESS-STRAIN BEHAVIOUR FROM STRESS PATH TESTS
by W. A. Marr
Vol. 1, Paper II.1

Chairman:

Thank you very much.
Does anyone have any questions?

M. Hamza:

I have two points.
The first one is: Did the use of $K_o \neq 1$ alter the value of ϕ' of the sand? In similar tests I found that K_o-tests have altered the residual shear strength, which is an important factor for liquefaction potential.
Another point is: Did you use cyclic tests to simulate a storm (e.g. a train of waves)?

W.A. Marr:

Your first question relates to the importance of the initial state of stress on strength. When we describe the strength of soil with a friction angle, the value of K has little importance. What I was attempting to show is that one way to talk about strength of soil is to run a test and see how much stress is required to cause it to fail. That is what I was describing rather than translating the strength to a friction angle. I was asking what the strength is if we start with this state of stress and we take it to failure? What strength do we get?
And your second question dealing with irregular waves or building up a set of wave trains in a stress path test, we have done some of them. It is quite complicated when one gets in the laboratory and has to adjust pressures and loads according to some random number generator. We have done a limited amount of it and we think its effect is fairly important. But that is not a very good answer for us as engineers, because we cannot say how important. I would guess different patterns of waves could effect predicted displacements by 50% to 100%.

M. Hamza:

If you do a $K_o = 1$ test as isotropically consolidation test and compare it with the same mean stress with $K_o \neq 1$, i.e. unisotropical consolidation then shear

ϕ-peak would be the same but ϕ-residual would be different. If you are talking about the effect of cyclic loading and liquefaction then the residual effect will be important. The ϕ-residual appears more when you do $K_o \neq 1$ test than when you do $K_o = 1$ test.

W.A. Marr:

Thank you. Certainly someone from Imperial College should be an expert on residual strength.

Chairman:

The next paper has two authors. The first is Ton Biegstraaten, who is with the Deltadienst of te Rijkswaterstaat. He has been most active in the development of numerical techniques for analysing the behaviour of the various design schemes that have been proposed. The second author is Cor Kenter, who is with the LGM. He has been a group leader and has been active in various aspects of the design and analyses of the Oosterschelde project from its inception. I believe the first speaker on the subject will be Ton Biegstraaten.

STRESS-STRAIN BEHAVIOUR FOR FINITE ELEMENT METHODS
by A. W. W. M. Biegstraaten and C. J. Kenter
Vol. 1, Paper II.2

M. Hamza:

With regard to the stress-strain relation, you are violating the energy requirement by using Hooke's law and at the same time while calculating stress, you assume thet normal stress may give rise to shear strain although the soil is assumed to be isotropic? Can you prove uniqueness of solutions?

A.W.W.M. Biegstraaten:

If I understand your question correctly, you want to know if by the addition of dilatancy uniqueness of the solution is violated.
Well, let us assume you formulated the problem in anothet way. You are familiar with the elasto-plastic approach?
I took some time to find what I was actually doing by adding all those kinds of initial strain methods and all those kinds of iteration procedures and I have written a test programme. I compared the elasto-plastic approach which can also be formulated for equations like I just described, and I could not find a difference. That is the only answer I can give to you. I do not know if the uniqueness is violated or not, I can only say I found the same answers by using the elasto-plastic approach and the Consol approach.
I want to say furthermore, I think it is far better to use the elasto-plastic approach, but it is a matter of time and effort which was not available to apply that method in Consol, so we had to use the formulations which were developed in the last 4 years.

M. Hamza:

I am interested because it is very good if it is really proved to be the easiest way out, because your formulated matrix uses a very simple law, Hooke's law, and then you go during the iteration, you obtain the stresses, you modify them and carry on iteration. But the only thing is a leak in the solution. That's all. Because it is a good method. The elasto-plastic approach sometimes gives many problems during iterations.

A.W.W.M. Biegstraaten:

Well, I think the elasto-plastic process is mathematically more complicated, but I think it is very much less expensive to use than the method I just described. The method I have just described is that first you make a computation by Hooke's law and then you see, the stresses are that and the strains are so, and I had a correction here and a correction there, and you apply that correction and put some 5 iterations to that step and find a solution. And the solution is accurate enough. That is also checked but I think you can do better with the elasto-plastic approach, but as I said before, time and effort were unavailable to me, to the people who worked with the programme to apply that procedure, but I think the method I described is just as good as the elasto-plastic approach and you can of course discuss if the elasto-plastic approach is unique. That is another point.

M. Hamza:

Thank you.

G. Gudehus:

I want to add a point to this problem of uniqueness. If you compare the elasto-plastic and this incremental elastic approach, they can practically coincide, but they do not generally. I want to call your attention to work on Dr. Darve in Grenoble some years ago where he first started with a Hooke type of approach, as you did, and later on he extended it a little bit. The difference between elasto-plastic and Hooke-type approach is only that you either use or do not use a symmeric matrix. If I understand you, you have used a symmetrical incremental stiffness matrix and the elasto-plastic approach does not give a symmetric matrix. For a rather wide class of boundary value problems you will get practically the same answer.
For certain boundary problems you cannot get the same answers and just for the problems of interest here you cannot expect generally the same answer. The main point is either you have a constraint by constant volume, then the approaches may fail and you cannot expect the same result with the elastic plastic and the incrementally elastic approach. The other point is if you come to any type of failure of the system, that is sudden large displacements or collapse, that means you come close to the loss of uniqueness of deformations, then you can never expect the same results with the two approaches and you cannot even expect uniqueness.

Chairman:

Thank you. I'd like to make a comment on that too. I guess the chairman is not supposed to do that, but I will. It seems to me that there are some very profound differences between elastic and elasto-plastic approaches and I am not sure that really requires very much commenting on.
For one thing it is fairly clear that you can only get failure in purely elastic approaches if you do some interesting things to the analysis when you start getting near failure and your stresses start looking like what ought to be failure stresses. But I think I'd like to disagree about the limitations on the Hooke's law. You can most certainly get shear stresses with a purely Hookeian material.
All you have to do is rotate the principal axes enough so that you have got off diagonal terms in the stiffness matrix. And you most certainly will get shear stresses. That is not an argument necessarily for using elastic materials, but I do not think your comment on the restrictions is completely accurate. Secondly, on the question of symmetry of the stiffness matrix, you can get symmetrical stiffness matrixes with elasto-plastic materials provided they happened to abide by the various normality principles. That is a peculiar problem when you are dealing with soils and if you do not have normality you can also get non-symmetrical stiffness matrixes. It always seemed to me that one of the things that's happening when you are getting non-symmetrical stiffness matrixes is that you are likely to start to get non-unique solutions. Anyway, you had something further to say though, I think.

M. Hamza:

No, I mean with the isotropical Hooke's law you will never get shear strains. I mean he was speaking about isotropical material. You commented assuming anisotropy of course. If you have anisotropic material and you give it all-round pressure you always get shear strains. The other point Prof. Gudehus was talking about is that some problems will never coincide. I think in any particular case of rocking foundation you will never coincide, because the principle stress increment and total principle stress rotation occurring under the edge under the middle will never coincide.

Chairman:

Well, it seems to me that one of the big differences, and one that we will be coming back to later (at least I hope Dr. Marr will have something further to say about this) is this. If you look at the way finite element problems get themselves formulated, you start off with some law that somebody generated or some model or whatever you want to call it, which is the product of experimental work or somebody's imagination, but in any case you have this relation between stress and strain or incremental stress and incremental strain and time and so forth. And some finite element person who usually sits in a small room in the back of the office sits down and with a large piece of paper reduces this thing to an incremental relation which, as far as the finite element programme sees it, is almost inevitably a relationship between some incremental stress and some incremental strain, and it looks an awful lot like an elastic stress-strain relation. Then this is used for solutions and you do various kinds of iterations to make sure that everything is back on whatever yield criteria that you have or that you have satisfied whatever viscous relations you have.
But almost all these solutions proceed by working incrementally as though you were working elastically. It seems to me that one of the great problems that we have in this area - and it really is not an analytical problem, it is a problem with our knowledge of the soils - is that as you try to handle a cyclic loading problem like this you begin to get into other serious problems such as what will happen when you start unloading these models. You can, as Dr. Gudehus and several other people pointed out, get very good agreement between hyperbolic relations and elasto-plastic relations provided the thing you are testing, the model you are running, the finite element problem you are calculating, looks like the thing you ran in the laboratory. And you can load it and get it to agree and there is a huge pile of literature around, some of it unpublished, in which people have developed various kinds of model which worked beautifully so long as they have kept the same geometry for their analyses as they had for the test from which they got the properties.
Then they take these numbers and they plug them into something like, say a plane strain situation, and all of a sudden something does not look quite right. That and the problem of reversing a load seem to me the real difficulties, more so than the question of precisely which of the numerous kinds of non-linear stress-strain relations you choose to use.
Are there any other questions?

P.A. Vermeer:

I'd like to ask a question, because in the proceedings it is written that programme Consol-Genesis is based on another type of model. Has this anything to do with the fact that you wanted to have a symmetrical matrix, yes or no? Furthermore, I want to remark that I find it a little bit peculiar that you demonstrate here that you have a good model and now you go to the future and you change to a quite different model.
Thank you.

Chairman:

That is a good question.

A.W.W.M. Biegstraaten:

I expected the question. The first point was the symmetrical matrix of Consol Genesis. The model Mr. Vermeer is talking about is the Camclay model of some changed version of it. It is not exactly the Camclay model, because we made an adjustment which makes the relation better, Maybe I can explain it in a few minutes. You know in the Camclay-model the ideal ellipse is going through the origin of the stress path diagram. When you remove that constraint you have an extra degree of freedom how the ellipse will move. We are using it by also specifying the shear strain as a function of the stress path. Next to what is usually the volumetric strain. So we made an improvement of the model. That is intermediate. Well, I do not think the solution chosen for Consol-Genesis has anything to do with the symmetrical or non-symmetrical matrix, because there are solutions known by which the programma can have a symmetrical matric and still simulates a non-symmetrical matrix by means of an initial stress or an initial strain procedure. So I do not think that we have chosen this model because we can only handle a symmetrical matrix.
The second point is comparison of the Consol model and this ellipse, this Camclay model. The Consol-Genesis programma must become a sort of official version used by everyone of the Rijkswaterstaat. I think that is very important , because the programme I've just talked about is, you can nearly say a private programme, because I am the only one who knows what is in it. I do not think that it is a good thing to publicise, to give it to persons working in other departments and so on, so there had to come an official version. At the time that was chosen for the Camclay model it was not yet clear how the Consol model would behave when it was made elasto-plastic. Mr. Vermeer has made his own programme which is elasto-plastic and which resembles the Consol model, but he has some, let us say personal experience, he is not involved in the Consol-Genesys project and we could ask for advice of course, but I think it is better when you want to build a more or less official programme that you have your experience near. So one of the main reasons was we were not quite sure how the Consol model would behave when it was elasto-plastic.
That is why we chose the Camclay model or the analogous type that we developed.

Chairman:

Ton, I'm going to interrupt right now.
We will continue this discussion after the last talk. Thank you very much.

We have one more paper to be presented, this paper has three authors. They are Frans Smits from the Delft Soil Mechanics Laboratory, the LGM, who will be presenting the paper.
The other two authors are fortunately also with us today. The second author is Knut Andersen, from the Norwegian Geotechnical Institute and the third is Prof. Gerd Gudehus, from the University of Karlsruhe; you have already heard him earlier in some of the discussions at the end of Frans Smits' presentation.

PORE PRESSURE GENERATION
by F. P. Smits
Vol. 1, Paper II.3

Chairman:

Thank you very much.
Are there any questions for Mr. Smits?

J. Marti:

I would like to ask a question concerning the shear stress that you apply for the pore pressure computation in the models. Are you referring to a maximum value of the shear stress or are you using a horizontal shear stress and in any case how do you relate that to the liquefaction testing which normally has one-dimensional shear?

F.P. Smits:

Apparently you are referring to the problem of rotation of principal axes of stresses. Its effect on pore pressure generation is not well understood at present. I have selected the cyclic shear stress amplitude τ_c in the model as half the total change of shear stress on the plane where the shear stress variation reaches a maximum. Close to the centre of a symmetrically loaded structure such a value of τ_c approaches the horizontal shear stress amplitude, whereas towards the edges for an element under pure rocking action it tends to approach half the maximum variation of principal stresses.
I would like to ask Knut Andersen to describe the shear stress selection procedure in their Hammen 17 caisson study.

K.H. Andersen:

The shear stress level which was used for the pore pressure computations was the single amplitude of the maximum shear stress ratio τ_c/σ'. The pore pressures in the various soil elements beneath the caisson were evaluated from measured pore-pressures in laboratory tests with the same τ_c/σ' as computed for the individual soil elements beneath the caisson. Two different calculations were performed. In the first calculation it was assumed that all elements behaved like simple shear samples. In evaluating τ_c/σ' for the simple shear tests, a K_o' of 0,3 was assumed. In the second calculation, simple shear test results were used in zones beneath the caisson where simple shear mode of deformations is most relevant, and triaxial test results in zones at and beneath the edges.

J. Marti:

I am sorry, I still have a short question. If you are considering a shear stress, the maximum value of the shear stress is generally used for the computer programme. You give here a case for example in which you have a constant maximum shear stress which is rotating in the plain and so depending on how you look at it. You have more cycles actually.
Every 360 degrees of rotation of the shear stress you have a double reversal of derivation of the shear stress. But if the computer programme is only considering what the value of the maximum shear stress is that will not be noted by the programme and will not produce pore pressure generation. So is that the value of the maximum shear stress that you are using?

K.H. Andersen:

A principal stress rotation may cause some pore pressure generation even if the change in maximum τ/σ' relative to the initial stress condition is zero (i.e. $\tau_c/\sigma' = 0$). For $\tau_c/\sigma' = 0$, our calculation procedure will predict zero pore pressure generation. This is one limitation of our calculation procedure.

A.M. Schofield:

Can I just ask what the relationship is between say figure 11 of paper I.2, where we see piers with sills, box beam, and sluice gates which are essentially 3-dimensional. They have a 50 metre-dimension which may correspond to the 46 metre dimension of the example problem in II-3. But if we look in the other direction there is a very much shorter dimension. Does the example problem which is under discussion and a plain problem, is that considered to have a major relationship with the piers with sill, box beams and sluice gates now to be considered?

F.P. Smits:

I cannot answer this question adequately, I guess. The example problem discussed here which is 17 metres wide and 46 metres in the horizontal load direction, has been treated as a 2-dimensional case although it is certainly not. To estimate the effect it should be kept in mind that only the stress conditions that generate the pore pressures and the response to the pore pressures in terms of strains are calculated by a plane strain programme, whereas the dissipation of pore pressures is obtained from a 3-dimensional analysis.

Chairman:

I could offer a partial answer to that. We did some similar analyses in which we looked at the differences between 3-dimensional states of stress and 2-dimensional plane strain states of stress. These were just comparisons of analytical approaches not model tests, and we concluded that the displacements that you were likely to get in the plane strain case were some significant amount, I think it was 50%, larger than you got in the 3-dimensional case, which suggested that using the plane strain analyses was a conservative thing to do. And I think in most cases of the pore pressure dissipation the same sort of thing applies. Clearly what one is interested in, is finding out how large the pore pressures can be for a situation, and limiting the flow clearly makes those pressures larger than they would be in the field.

M. Hamza:

Could you please explain to me if you have done any tests in which σ'_v was changed, i.e. will all these diagrams change if you change the effective consolidation vertical stress? This means that from the diagram you could say that regardless of the value of σ'_v you get that result for the ratio τ_c/σ'_{vo}.
Is that right and can you qualify it?

Mr. F.P. Smits:

As far as mean stress variations are concerned you may distinguish between the variation of the octahedral component of the cyclic stress path and the variation of the initial consolidation stress.
With respect to mean stress variations within cycles in undrained cyclic tests we have not experienced any difference in pore pressure build-up between a common triaxial test path and a test path with the total mean stress kept constant.
With respect to the consolidation stress our experience is limited to a maximum variation of the consolidation stress of 200 percent, and so far we have found a quite linear relation between pore pressure generation per cycle and consolidation stress.

Chairman:

I am going to interrupt here and ask if the authors will please come up to the stage since we are getting into general discussion.
Are there any questions to be addressed to the speakers as a group, or for that matter any obvious disagreements with what they have said which someone would like to point out or ask for comments on.

A. Verruijt:

In the discussion we had before between Mr. Biegstraaten and Professor Gudehus it was not completely clear to me what the restrictions are that are imposed on the model in the Consol-programme. I have the impression that there is a restriction to a symmetric matrix and a symmetric stress-strain relation. If this is true I do not think that the model could simulate a general type of plastic behaviour very well.

A.W.W.M. Biegstraaten:

Well, this is not true. That is a very short answer, but the symmetric relation with which I started is only, let me say, a numerical tool. The programme uses a symmetrical stiffness relation but that does not say that the total stress-strain relation, the total stress-strain behaviour is what you can call symmetric. By all the additions such as dilatancy, and the additional term due to the isotropic stress and the change of orientation of the strain increments you just cannot say that the result is symmetric. What I do is only use a symmetric relation as a basic tool for solving the equilibrium equations. Finally I think, we can be sure that the stress-strain relation is not symmetric.

C.J. Kenter:

It is also cheaper to do it that way.

Chairman:

Do I understand you to say then that in effect you are by all these additions, really introducing initial stresses and initial strains which make the stress-strain relation non-symmetric?

A.W.W.M. Biegstraaten:

That is right.

G. Gudehus:

I should like to make a few comments on the problem of stress-strain laws, because it has become evident that this is the clue to any computer calculation, and I have seen from the list of participants that most of you will not be interested in the most recent details of soil mechanics research. I only want to point out some things so that you do not overestimate the present capacity of the different models describing the stress-strain behaviour and on this basis produce solutions of boundary value problems. The first point is that it is certainly true that soil has a certain type of memory of its past, so you have to cover the past somehow or another, but this cannot be done by the maximum past stress if you consider granular soils.
This is to a certain extent the case for clays but not for granular materials. It is generally an open problem how the influence of the past can be covered by taking only a few remnants of previous stress or strain components. This is number one. The second point is that if you work with a stress path concept you should know in advance which stress paths occur, but this is in principal impossible, because you do not know the behaviour of the material and you do not know how it will influence the behaviour or the process of the elements in the boundary value problem, and this is even more so if you deal with cyclic problems. In the field there are no cycles in the soil elements, neither strain nor stress cycles, so if you try to get a vague idea of the future of the soil elements and you mean that the future consists of stress cycles this is not the case. You may be lucky to hit the behaviour for some special cases, but this is mere luck. I want to follow the point of Mr. Kenter on this case that in this way you can not come very far. This is point number two.
The next point is how you can formulate the behaviour of the material as far as we know it, and this has already been discussed a bit.
There are elastic-plastic models, incrementally linear models, Hooke-type models, and the present stage of development is that respect seems to be that for some very special monotonous paths and even paths with one loading and one unloading there are a few incremental models which describe the behaviour of the material fairly well. The interesting fact is that seemingly widely differ-

ent models cover the material with the same degree of accuracy and this is encouraging. This is a fact that I have just learned about some details of Mr. Biegstraaten's model, and this is one of the serious models to describe the behaviour for one loading and one unloading. It can not as yet describe the behaviour of contractancy, that means that the granular soil has the capacity to contract after certain reversals of the stress or strain path. This is the other side of dilatancy. Dilatancy is fairly well understood and contractancy not as well. But it is there and it can be decisive for liquefaction, because any partial prevention of contraction automatically means increase of pore pressure and so far the existing models do not as yet cover the behaviour really as accurately as we would like them to. And now we come to cyclic processes. Then we have more than one loading and one unloading, we have repeated loading and unloading. In this case we are just at the beginning of understanding what the material does. We have a feeling for such things as pre-shearing, that means the effect of the past is rather complicated, cannot be covered by just one stress variable or by one K_o-value or something like that. We have ageing and such effects. This means that we are not yet in a position to formulate stress-strain law in increments which are sufficiently accurate for general cyclic processes. We are simply not yet in the position. The conclusion reads that we cannot be too optimistic with respect to calculations, computer calculations based on these models. I mean in the general sense. We cannot just rely on the output of computer calculations for general boundary value problems. We have to restrict these to very special boundary value problems, in which the modes which the soil element assumes are fairly well understood, and a sufficient degree of empirical support is at hand.

Chairman:

Thank you. I was told that Dr. Marr wants to say something about that.

W.A. Marr:

I wanted to make a very short statement in which I agreed with Prof. Gudehus on his first and last sentence, which I think were the same points. That is: Do not overestimate our present capability to use stress-strain models in computer programmes to predict field performance. In fact the point I'd like to make is that much of what we are discussing here this afternoon are concepts and ideas that were thought about or developed during the course of the research on this project. But as far as the design goes, I suspect if we invited Frank Spaargaren to tell us how the project was actually designed there are a whole lot of other approximate procedures which are familiar to many of us, based on some very fundamental concepts of geotechnical design. I just wanted to give credit to some people who did a lot of back-of-the-envelope work, who really made the serious decisions. We are the researchers who are now trying to argue about the extra ten percent, I suppose.

Chairman:

Is there any other comment?

J.D. Nieuwenhuis:

I have the feeling at the moment that Mr. Marr is too pessimistic on the application of more sophisticated computations. I thought that approximately all the parts which were presented this afternoon were actually used to adapt the design. The non-linear calculations were used to forecast deformations of the piers.
You will hear tomorrow that we had simpler means which were more productive for making larger series of calculations, but they were actually always compared to the non-linear calculations and pore pressure generation estimation for both cases, for the caissons in the first place and later on for the buried caissons.

The results of the calculations of pore pressure generation were used, partly at least, to abandon elements of the construction so I think you were too pessimistic on the effects.

Chairman:

Well, it is also I think interesting that some of the analyses that were done of the pore pressure dissipation used a procedure which we have also recently applied for calculation of residual deformations. It essentially involved saying as Dr. Gudehus just said, that you cannot look at each individual cycle and follow it all the way out. For one thing there is not time enough to do it. It is a long calculation. And for another thing you really do not know the material that well. So what you can do, if you have enough data on the soil, is to say the effect of many many cycles is essentially to generate so much pressure and then to observe how it dissipates. I think it was remarkable that 3 or 4 different groups made calculations on those deep caissons and came up with what were essentially the same results for engineering purposes.

A.F. van Weele:

I had an entirely different question on the same subject of pore pressure generation. We have heard a lot this afternoon about pore pressure generation and the dangers involved in pore pressure increases especially as far as stability is concerned. A lot of effort has been put into predicting of pore pressure generation. Has there also been effort put in another direction, that is how to prevent it? I can imagine that during for instance storm conditions, that you switch on a dewatering system and the sand has a quite high permeability so I would think that when the storm is just coming that decreases of the water pressures underneath the structure would increase the factor of safety quite a lot. Has this been considered?

Chairman:

I think Mr. Kenter wants to comment on that?

C.J. Kenter:

The kind of method Mr. Van Weele is talking about requires certain actions of an operator during the storm. As far as I remember, just one method of this kind was proposed, although not seriously. The idea was to measure the pore pressures underneath the piers during the storm and to open the gate for a moment when the pore pressure underneath a certain pier would reach a critical value. This would decrease the impact on the pier, drain the pore pressures and consequently preshear the soil.

Chairman:

I have one brief comment on that suggestion. That is that it depends very much on the existance of a very able, thoughtful and intelligent operator who is going to turn the pumps on and turn them off at the right moment. Our experience with the failures of dams indicates that these operators are always seem to be on vacation when the serious problem arises.

G. Gudehus:

Well, to answer your question there is in principle a third means. The first is compaction of course, the second is drainage, the third is a suitable pre-deformation, because pre-shearing has this strong influence which is understood to a small extent. And in principle it must be possible to partly suppress the development of pore pressures by a suitable pre-deformation. In my opinion this is essentially manufactured if you use a suitable pre-stress. It is a question which is subject to discussion how you can produce suitable pre-stressing and pre-deformation, but this is a possibility which we are studying in Karlsruhe and it has a strong effect, sometimes it is stronger than the effect of computation.

P.A. Vermeer:

Prof. Gudehus stated that a model will not model the soil completely, and I think this is rather obvious. If a model could model the soil completely it would be so complicated that it is impossible to run in a computer programme. So I think we should make models and tell and describe what they actually can. do, and what they cannot do, and when making a computation it should be checked because it cannot always be stated before whether or not the model can be used, but very often it can be checked afterwards.

Chairman:

They agree with you. Anyone else?

J.S. Brindley:

One of the parameters that the authors of the last paper listed but did not talk on any further in the paper, was the frequency of the cyclic loading. I should be very interested to know whether they put it aside, because they had already decided that it would not be important, or whether there is a possibility that the system of the foundations, and waves, and build-up of pore pressure could have some resonant frequency which could be severely affected by wave attack.

K.H. Andersen:

With respect to the effect of frequency on soil properties, most tests were run at the actual frequency of the waves so that we actually used the right frequency in the laboratory tests. If I understood Mr. Brindley correctly his question also concerned the effect of frequency on resonance of the structure. I am not sure whether load amplification due to resonance of the structure is a problem or whether that has been investigated.

Chairman:

The dynamic behaviour of the structure was examined and I believe that it is not a problem from the soil point of view. It strikes me that your question really raises a point which is properly of concern to people who work with earthquakes. Most of the testing that is done on soils and earthquakes is done at the same frequencies or in the order of maybe one cycle per second, and we all know that earthquakes have substantially different frequencies and probably whatever effects there are occur in the high frequency range.
If you asked that question to a group of earthquake engineers they will assure you that it has no effect on the soil properties, and that is what everybody is assuming. I do not know whether it is right.

Chairman:

Are there any other questions?

H. Engel:

One small question to the whole forum. As I understood you, there are different models that give about the same results and will not give results in other cases. Allright. Now we found that in the hydraulic models we had a lot of advantage by choosing a model and we used them in a lot of cases to get experience with the workings of the model so that a large group of people are capable of understanding what the results of the model mean. And I would like to ask the forum if they intend now or in the near future to choose one of these models as a kind of a background standard that is used in a lot of cases so that they know what the standard model does, even when they themselves use more advanced models for effects that the standard model does not account for.

C.J. Kenter:

I agree with mr. Engel that it is very sensible to avoid disperson of experience and to choose a standard model to get a lot of experience with. However, one will always need more advanced research models also, for special effects which are not included in your standard model. I think that the best thing to do in the future, is to choose a certain finite element programme as a standard programme, e.g. Consol-Genesis. This programme is very well described, has a lot of input and output facilities and is very "friendly" for the user. Beside this users-version a research version of the programme should be available, which should be based on the same modules, but which can be entered very easily to change and develop things.

W.A. Marr:

This is just a brief comment to protect my geotechnical brothers a bit. Mr. Engel's comments reminded me that we in geotechnical engineering use a similar approach of a model which we continue to update as we gain more experience. Indeed much of geotechnical engineering is built on such models. I would remind us however that the situation of the Oosterschelde is one of those that puts us at the forefront in many ways. The constraints of low factors of safety required to keep the project within cost, and requirements for small displacements are such that we really have to go beyond what we've done in the past into the area where we have very little evaluated experience. As to the question which model we select to go that far, I think we are really groping for all combinations at this point and it will be some time down the road before it's clear to us which model gives us a better answer.

A.W.W.M. Biegstraaten:

The name Consol-Genesis was mentioned. I've also talked a bit about it, according to me the Consol-Genesis project is especially aimed to make some elaborate model available to a big group of engineers especially within Rijkswaterstaat. As well as that I think it is still necessary to have extra development, of research programmes, as Dr. Marr noted. I would point out that the Genesis-Consol version is also aimed to include further development of these research programmes.

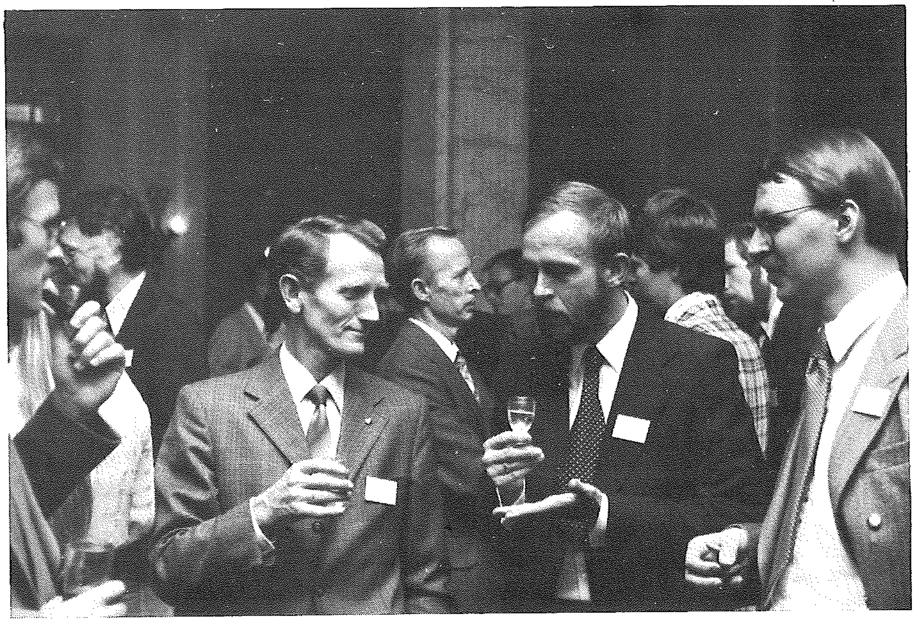
G. Gudehus:

One point is I think that there is no principal difference in the philosophies of hydraulic engineering and foundation engineering as far as development of models is concerned. And in detail it was outlined by Mr. Biegstraaten how we can test the models, that means we work with them and look into the details, and if we look into the details we can really see what they can do and what they cannot yet do. And I think it was made quite clear that to a certain extent they can serve the purpose, but this is not what was needed in this extremely complicated project.
We would like to have a much more developed model. We don't have it as yet and I don't agree with Dr. Marr that it is still a matter of 10% or so. It is much more than 10% and we wouldn't care for 10%. It can easily be hundreds of percents, and if for instance the existing model of Mr. Biegstraaten would be applied to recalculate cyclic triaxial tests, then it would certainly be more than tens of percents, but this is well understood. So there is some caution required with the present models and to go ahead means testing them step by step in small details. A sudden jump to such a big problem as this boundary value problem is hopeless.

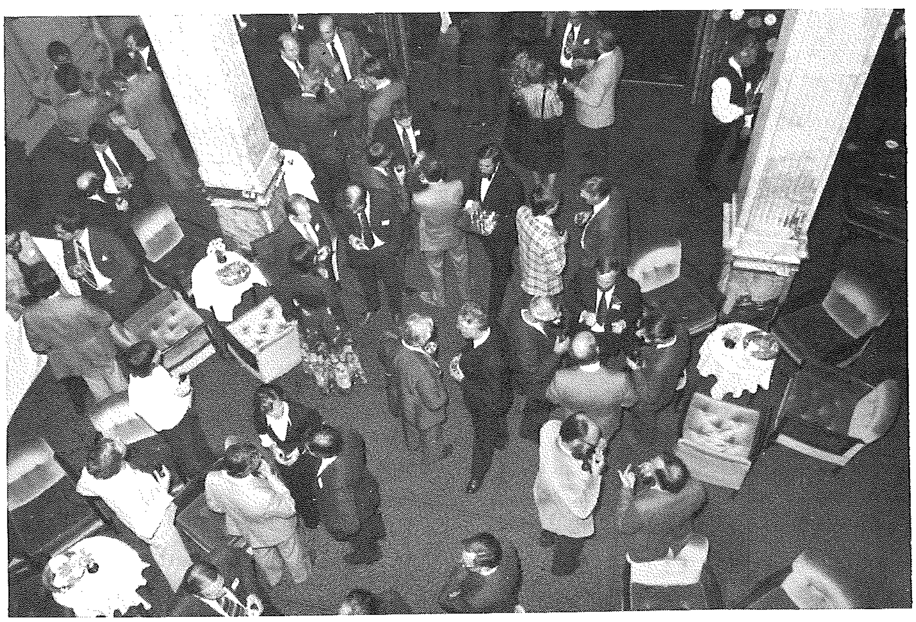
F.B.J. Barends (written comment):

In the final discussion conducted by Dr. Christian, the request of Mr. Engel to the panel to make a standard soil model available to any engineer is not answered from the following points of view:

1. The discussions about the applied models showed anything but a standard.
2. The engineer, being interested in solving his particular problem, is not aware of the applicability of the model. Hence, a standard model at this stage represents a dangerous tool, most likely leading to misuse and wrong interpretation of the numerical results, when not guided by the model creator, or a sufficient comprehension of the material behaviour.
3. A standard method for a not completely understood physical phenomenon is consequently not complete and will most probably soon be old fashioned.



Reception on Monday, October 9th



Session III
Predictions by theoretical Methods

Chairman:
A. Verruijt, Delft University of Technology,
The Netherlands

Chairman:

Good morning ladies and gentlemen. My name is Arnold Verruijt. I'm from the Delft University and I will chair this morning, when we will have four presentations by five speakers. The subject this morning is predictions by theoretical methods, and the first lecture is on one of the boundary conditions of the problem, namely how to determine the loads, but also how to determine the safety factors.
There are four authors, all from Rijkswaterstaat, Mr. Mulder, Mr. Vrijling and Mr. de Quelerij. The presentation will be made by Mr. Kooman.

PROBABILISTIC APPROACH TO DETERMINE LOADS AND SAFETY FACTORS
by D. Kooman
Vol. 1, Paper III-1

Chairman:

Thank you very much Mr. Kooman, I think we have time to allow for one short question now. Yes, Mr. Barends I think?

F.B.J. Barends:

I refer to the graph showing iso-curves for the combination of head loss and wave loads. Integration is performed from a certain linear combination of the probability of head loss and wave loads, defind by the parameters α and β in order to determine the chance of failure.
By what criteria is the position of these lines determined?
If it is given by the designer, then I am interested in the incorporation of the correlation between water depth (related to the head difference) and the actual wave loads.

D. Kooman:

Of course it's not for me to say in what way we can determine these criteria, because it's one of the factors that comes from the designer. He says my failure model acts like this and therefore we take into account the possibility of the deviation of head loss and wave load in the total load. This is a very simple assumption. But when the designer gives us a function for these criteria, maybe a function of head loss and wave load or other parameters, then we incorporate that function into our integration procedure and we will have the distribution of the total loads in a similar way. We did not determine these factors. They are only some examples and therefore it is not for me to say this is the importance of the head loss and that is the importance of the wave load in your problem.

Chairman:

I think we'll have to continue now. As you know there will be a general discussion after all the presentations of this morning, so if you want to come back to any of the points raised by Mr. Kooman you can do so later.
Thank you very much Mr. Kooman. You've shown us a way of going over the statistics of wave loads. We have all realised that the wave loads are a stochastic process and therefore we have the feeling that failure must also be some sort of a statistical process. As a consequence we need a model of how to go from the loads to the deformations. And as I understood you linearised a model that was provided to you by someone else. These other people now have the opportunity to

tell how their model works.
We will have 3 presentations.
The first one will be a joint paper by Mr. Kenter from the Delft Soil Mechanics Laboratory and Mr. Vermeer from the Delft University of Technology. The paper is titled Computation by Finite Elements and the first speaker will be Mr. Kenter.

COMPUTATIONS BY FINITE ELEMENTS
by C. J. Kenter/P. A. Vermeer
Vol. 1, Paper III-2

Chairman:

Thank you very much Mr. Vermeer and thank you very much Mr. Kenter. I think we can have one or two short questions from the floor.

M. Hamza:

I'd like to ask the authors about the prediction.
When we predict we generally would like to see how the actual structure will behave, and also we like to compare test results with our mathematical model. The things we should be looking for are the failure loads and the failure mechanism. And I don't think either of the authors have shown us how the footing will fail, if it will fail. What is the failure mechanism. Also they have not shown us any prediction of the failure, or the actual magnitude of the failure load. Can I have a comment on that please?

C.J. Kenter:

It is very difficult to compute failure behaviour with finite element programmes, because at the moment you come near to failure you get all kinds of numerical instabilities. Therefore an exact computation of the failure line, that is the horizontal line in which the stress-strain curve or force displacement curve ends, is impossible. We can get an idea of the failure line, because we are able to come close to the point at which these curves are bending into horizontal direction with our F.E. methods. We also studied the failure mechanism in that way and compared it to the mechanism observed in model tests. The resemblance was very good.
In both cases we found e.g. a rotation around the Eastern-Scheldt point for an embedded pier during failure; the ratio between rotation and translation was also very similar.

J.W. Boehmer:

You might read the next paper by Smits to see a prediction of failure. I think that's the answer to your question.

Chairman:

That's an additional answer.

A.N. Schofield:

Can I just ask about the analogy which is on page 12 and 13. If we consider for example vertical seepage through a permeameter and if you sketch a pore pressure at the bottom of a permeameter rising through the permeameter with vertical seepage flow lines. Looking on the middle of page 13, we have $u_x = \sigma$, $u_y = 0$, $\sigma_{xx} = \partial\sigma/\partial x$. Now if there is just a variation of σ-vertical, say $\partial\sigma/\partial y$ is a constant and $\partial\sigma/\partial x = 0$, is this predicting the u_x, that there is a shear distorsion, because there is a displacement y and there is a constant stress σ_{yx} through the height of the column. How does this analogy work with the simple example of vertical seepage through a permeameter.
It's the first time I've seen this so I may be quite mistaken. Please explain.

C.J. Kenter:

You may be right, that the analogy of vertical seepage is simple shear. However, it is very difficult to consider this analogy as a physical analogy, because by choosing K = - 1/3 G the material isn't a physical material, or a material one can conceive anymore. The analogy is pure mathematical.

Chairman:

I think the analogy with vertical flow will be simple shear, pure shear, but then that's just a mathematical analogy. Personally I think that it's an amazing sort of analogy if you want to solve a potential problem and then solve it by a bi-potential method. I think that's a sort of peculiar type of solution, I would prefer a direct solution of the potential equation. Gentlemen I think we should continue with two more lectures on methods of prediction. The first lecture will be by Mr. Sellmeijer of the Delft Soil Mechanics Laboratory.

SIMPLE NUMERICAL METHODS TO DETERMINE DISPLACEMENTS AND STABILITY OF PIERS
by J. B. Sellmeijer
Vol. 1, Paper III-3

Chairman:

Thank you very much Mr. Sellmeijer. This was a simple numerical method and yet it was not so simple I think. Are there any questions from the floor to Mr. Sellmeijer?

J.W. Boehmer:

I like Mr. Sellmeijer's conclusion in which he says comparison with the ingenious or complicated or time consuming models is needed, but I would like to know from him how he obtains these criteria for soil parameters from cyclic loading practice, because this has been a problem in the past and it will be a problem in the future.

J.B. Sellmeijer:

I meant especially some of the factors which are introduced, for example the stress distribution factor. In order to know it's value one needs information from the surrounding soil. This can be obtained by more advanced calculation where the soil is treated as a more realistic variable.

J.T. Christian:

I have a comment, which is that I happen to like these simple models. I think they're very handy ways to do things. The distinction between the two methods, simple models and more complicated models, happens to exist in the field of dynamic analyses as well.
This leads me to my question, and that is that in developing these spring constants did you take advantage of the rather large volume of literature that has been developed over the last 10 or 15 years on spring constants for a variety of shapes of foundations, layerings, distributions of soil properties and so forth?
You referred to Barkan whose book really was written in 1948. Since then Veletsos, Wei, Novak, Beredugo, Westmann, Luco, El Sabee, and Roesset have done some work on essentially the same problem. Most of their results of course are frequency dependent because they're worried about dynamic problems, but almost all of them have zero frequency intercepts for their spring constants. I'm wondering if you compared the results which you've got from your case with these more recent analyses.

J.B. Sellmeijer:

I have not made these comparisons so far.

P.W. Rowe:

How does one get the stiffness in the lateral direction compared with the vertical? The stiffness in the lateral direction is going to be decreasing all the way to failure. The actual zone that is subject to high shear gets progressively smaller as the structure goes to failure. Nothing like that occurs in depth. How do you feed in the relative stiffnesses and their varying amounts in lateral and vertical directions?

J.B. Sellmeijer:

What I considered was only a linear stress distribution to compute settlements with. In order to make calculations I'm using cone penetration values. You do a penetration test, you consider the q_c value of it and from that q_c value you calculate the C value, the coefficient of volume compressibility from Terzaghi. I only put that one in my formulas.

P.W. Rowe:

That of course is the situation before you build the structure. But if you start doing that you're altering the stiffness, just local to the base. That has nothing to do with the calculated value.

J.B. Sellmeijer:

The stiffness is considered to depend on the stress level.

P.W. Rowe:

Thank you.

J.T. Christian:

I have, well, it is not a question but a comment. That is that I very much like this sort of simple model. However, these things have a habit of winding up in design manuals and textbooks and things like that and somehow people tend to forget what the range of applicability of them was, and what the range of derivation was. We have had several situations in which as structures get bigger and bigger and the foundations get larger and larger the people still keep using the same formulas and eventually they get so far out of the range for which they were derived that the results become meaningless.

J.B. Sellmeijer:

Yes, but that's why I've made a comment that we always have to compare it with better models.

J.T. Christian:

When you put that in the design manual you should make that comment at the top of the chart and not at the bottom.

Chairman:

I think we should continue with our programme now. Thank you very much Mr. Sellmeijer. Maybe we can come back to the relative merits of the different methods later. The last speaker this morning is Mr. Frans Smits from the Delft Soil Mechanics Laboratory who will give a lecture on the plasticity analysis.

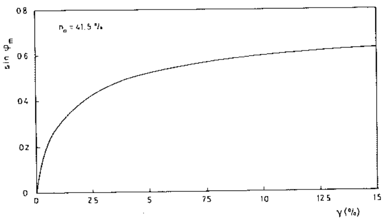
EXCESS PORE PRESSURES AND DISPLACEMENTS DUE TO WAVE INDUCED LOADING OF A CAISSON FOUNDATION AS PREDICTED BY PLASTICITY ANALYSIS
by F. P. Smits
Vol. 1, Paper III-4

Chairman:

Thank you very much, Mr. Smits. Any discussion?
I give the floor to Mr. Vermeer.

P.A. Vermeer:

Mr. Smits has indeed predicted the Neeltje Jans Test-1 reasonably well and I think there is a very simple explanation for it. Therefore I should like Mr. Chairman to see one of the first pictures on which you see the curve which relates the mobilised angle of friction with the shear strain γ.
All finite element methods did not predict large displacement so I wonder why Mr. Smits got large displacements?



Mobilized strength as a function of shear strain

In the model which you used Mr. Smits, you assumed this type of relation and the volume strain is in fact also a function of the shear strain. Shear strain is a function of the mobilised angle of friction. Volume strain is a function of the shear strain. When I look at this picture I think that this is not representative for the Neeltje Jans test, and I think it is quite general for sand at a mean stress of about 50 kN, you will find that the shear strain is not bigger than 1%, this was actually incorporated in the prediction that I gave for Neeltje Jans. I obtained such a curve but then much stiffer from bi-axial tests. I think the other data mentioned by Marr and mentioned by Biegstraaten and Kenter also show that this soil is much too weak, it is more like clay.

F.P. Smits:

The graph has been obtained from simple shear test. It has been chosen for a particular reason and that is the way that I came to a displacement calculation. Independent of that, if we would have used a stress-strain relation as Mr. Vermeer indicates, then this only would have affected the displacement in the early test stages and it would also have predicted failure, starting in parcel 3.

P.A. Vermeer:

I think that also influences the pore pressure generation, because a γ means a certain value of the volume strain and the volume strain has a large impact on the pore pressure. The pore pressure has again an impact on the stress so you go to liquifaction introducing this.

F.P. Smits:

But the fact is that the pore pressure calculations are completely independent of this relation, nothing is used of this relation to come to the height of the actual pore pressures, because it is the β-function and the consolidation process which govern the actual pore pressure build up.

P.A. Vermeer:

I do not understand it, excuse me. You have given a volume strain that was related to γ and you say that volume strain is not influenced by displacements.

F.P. Smits:

Because in the beginning of my presentation I have tried to explain that what I showed as the monotonic loading part has been treated as a fully drained loading, whereas the pore pressure generation and dissipation in this case have been calculated in an uncoupled process by a completely different relation, and only 3 parameters go in there, which are β, k and D. They are not influenced by any volume strain given in the relation that you are referring to.

P.A. Vermeer:

One more quick question. If you divide all these γ-values by 5 will you then obtain deformations which are about 1/5th of the one you predicted?

F.P. Smits:

I will not get exactly 1/5th of the predicted deformations but in the early beginning of the test, in the first 3 parcels, they will be about 50% smaller.

J.W. Boehmer:

I knew this discussion would start, and I am sure this will go on this afternoon. This test was done about 5 years ago and I remember many similar discussions since that time. I don't think that we will find a solution for who is right and who is wrong in the middle of this audience. I think this should still include some more careful work. Yesterday in my talk I gave my view on what happened with this barrier. What I said is that in Neeltje Jans we had a barrier swing and not a barrier slide. Now I am a little bit surprised to hear that today this was a barrier slide so my question to Mr. Smits is how do you define failure. Is it both slide and swing or is it just swing?
My second question is a more serious one. I agree with Mr. Vermeer. If this stuff which is up there on the screen is really representative for sand in the Oosterschelde, then if you make a simple calculation on the back of your notebook you can compute that the design of the barrier as it is now will fail during the very first superwave, so if that is true what should we do?

Chairman:

I think Mr. Boehmer has asked two serious questions.

F.P. Smits:

To your second question, I have not been involved in the present barrier design so much as in the earlier concepts, but from the loading and soil conditions I have seen I don't think that there is danger for failure in cyclic loading due to excessive pore pressure generation.
To your first question, I am amazed to hear that after our discussions you had any doubt that the barrier did not slide. How do I define failure. Although for simplicity of calculations we treat a wave loading problem as a fictitious monotonic loading condition, it is actually not.
Therefore, what we do not fear for is a sudden collapse or failure in the classical sense, unless the soil is very loose, with a density below critical. What may happen, if the mobilized strength becomes large due to an increase of the boundary loading or due to a rise of excess pore pressure, is a kind of progressive failure by successive cycles, accompanied by large cyclic swings in symmetric loading and by large cumulative displacements in asymmetric loading. If this process involves the generation of serious excess pore pressure, the rate of displacement may be speeded up critically, however, it still does not cause a sudden collapse in one or two cycles, due to the dynamic nature of the loading and the pore pressure reducing effect of dilatancy. This progressive failure process is what we really consider and fear, as failure by cyclic wave induced loading.

In selecting the mobilised strength as a function of shear strain, Fig. 2 and the generation of pore pressure in relation to cyclic shear stress ratio Fig. 4, a choice has first to be made of
(a) the test system and its associated stress parth (see Paper II.1 Fig. 2) and
(b) the testing technique.
For example, in the Proc. Conf. Off-Shore Structures I.C.E. London 1974, p. 97, Fig. C32, Rowe showed how the number of cycles to liquefaction could be changed by an order of magnitude, using lubricated end platens, and by a further order of magnitude when changing the wave form and stress path. It is also known that the size of pore pressure developed in saturated sands when measured in the triaxial test is very sensitive to the tendency to small changes in the membrane volume at the boundary. Superimposed on these types of difficulty is the fact that the foundation consists of thousands of "elements" which suffer variable effective stress paths, the nature of which cannot be predetermined even in the drained state let alone that attempted by the author. Can he add to Fig. 7 the results of alternative calculations based on element tests showing the extremes of test data possible not only for the "specified foundation" but also for the case of both loose sand of alternative thickness at the surface and also for densified sand? Such an enquiry, illustrated to a limited extent in Fig. 3 of Paper IV. 3 could greatly enhance understanding of the significance of the chosen theoretical method and the meaning of the first sentence of the discussion.

F.P. Smits: (answer extended in writing):

I agree with you, there are a lot of factors affecting cyclic behaviour which are insufficiently understood to allow a confidence a priori in predictions of prototype behaviour by calculation. Seed (ref. II.3, page 12) has recently tried to quantify the effect of some factors of uncertainty in a set of assumptions. On the other hand, it is just one reason for carrying out expensive large scale tests to find out how well we are able to predict prototype behaviour by simple analytical methods based upon a best guess of parameter values. Also, although we know that it is very difficult to investigate and to understand cyclic behaviour, we try to establish test conditions and to interprete them in a way that the outcome is as much as possible independent of any stress path conditions. As far as pore pressures are concerned this seems an easier job than regarding the response to the pore pressures in terms of strain.
On the last question you asked me, I have not studied the sensitivity of the outcome of such calculations for the range of uncertainty of input parameters. However, the effect of sand densification may be judged from a comparison of the predicted displacements for Neeltje Jans Test I and II in Fig. A1 and A2. Calculations for Neeltje Jans Test II were based upon an estimated porosity of 39%, they predicted no significant pore pressure generation and they over-estimated the actually measured displacements to some extend. Also, a more elaborate discussion of Test I may support my conclusion about measured and predicted performance.
According to Fig. 7 and 8 both the measured and predicted performance show a marked increase of the rate of displacement early in parcel 3. Now the actual loading schedule differed somewhat from the planned schedule on which predictions have been based. In general the cyclic load amplitude closely followed the planned schedule, except in parcel 3, where it was about 20% higher; however, the static horizontal load component was about 20% higher in all parcels; in addition there was a power failure after 50 cycles in parcel 3, which forced to the decision to slow down the cyclic frequency after parcel 3. This and the measured performance invite to look a little bit closer to what happened early in parcel 3.
The dashed curve (P) in Fig. B shows calculated average net excess pore pressure at surface level below the caisson, based on the planned loading schedule. Note that pore pressures in parcels 3 and 4 decrease after having reached a maximum. This is due to the preshearing effect, discussed in Paper II.3, a

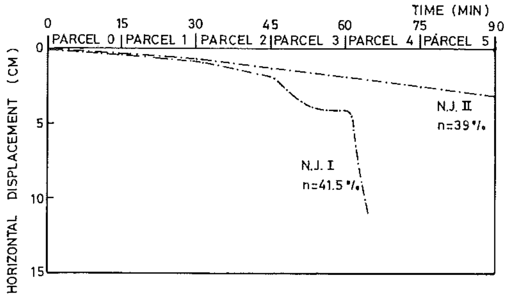


Fig. A1

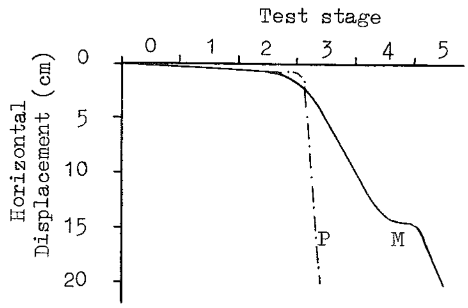


Fig. C1

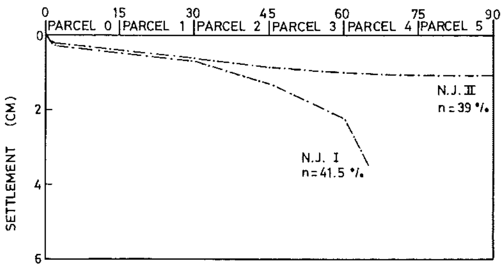


Fig. A2

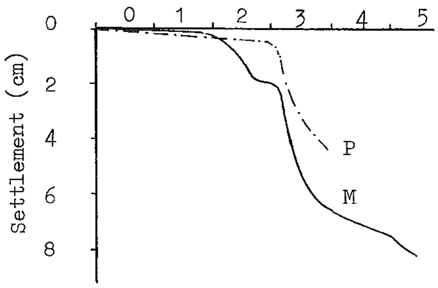


Fig. C2

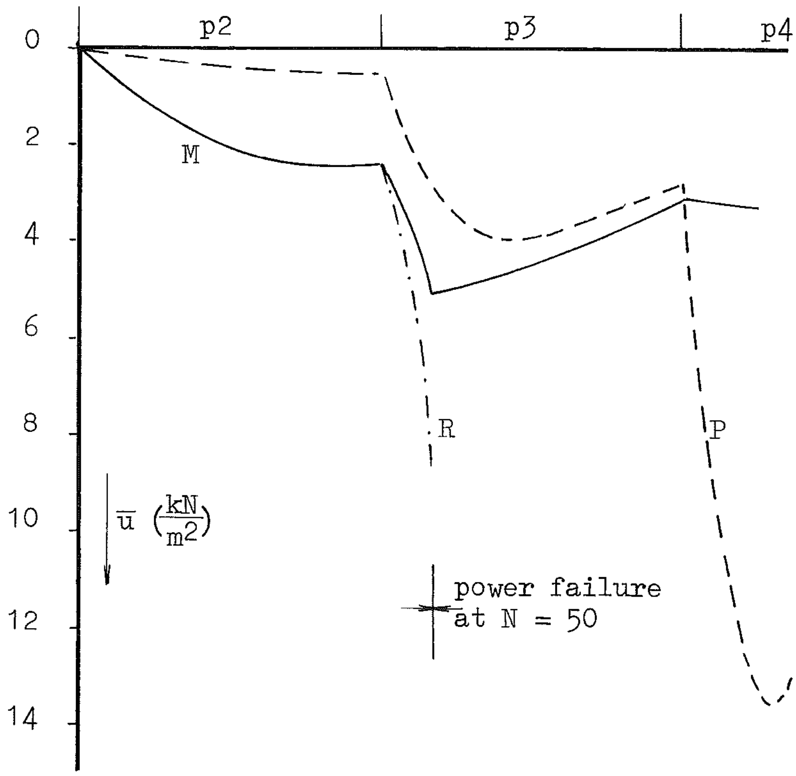


Fig. B

behaviour that also has been observed in model tests and in the present Neeltje Jans Test. Considering the vertical stress level of 32 kN/m^2 due to the caisson weight, the applied ratio of horizontal to vertical load and the maximum strenght to be mobilized, it is easily verified that pore pressures of 13,5 kN/m^2 in parcel 4 lead the caisson foundation very close to a failure condition as predicted by a common stability analysis. In view of the non-rigid behaviour of the soil, therefore, it seems plausible that calculated displacements start to increase in parcel 3 due to the rise of pore pressure.
Unfortunately pore pressures were not measured at surface level, but there are some records of pressures at 0,75 m and 2 m depth. The measured average pore pressures are shown by the solid curve (M) in Fig. B. They are larger than the predicted ones, whereas you expected them smaller in view of the increasing effective stress level with depth. I have tried to explain this by the difference between the actual and scheduled loading. The actual loading in parcel 3 started off with a cyclic load amplitude 20% below the scheduled level, then it increased and overshooted the scheduled level with about 30-35% at cycle number 50, and after the power failure it remained about 10-20% higher. Recalculation of this actual loading condition yields an excess pore pressure at surface level in the first 50 cycles of parcel 3 as shown by the dashed curve (R) in Fig. B, which now comes in the right order with the measured ones at 0,75 and 2 m. In view of the previous discussion it seems quite obvious that this has increased the rate of displacement in parcel 3.
Therefore, I conclude:
1. that the procedure described in the present paper appears quite capable to predict excess pore pressures,
2. that the level of actual excess pore pressure very likely has contributed to the increased rate of displacements at the beginning of parcel 3.

To back up the above philosophy we have another argument. Among the methods, that were mobilized to predict performance of the Neeltje Jans test, were 1 g model tests in the laboratory in which the time factor for pore pressure generation and dissipation was carefully scaled by exchanging the pore water by a viscous fluid. Performance predicted by these model tests, shown in Fig. C1 and C2, also indicated an increased rate of displacements at the start of parcel 3, even more steep than in the field test. This corresponds to the somewhat higher pore pressures observed in parcels 2 and 3 than the measured ones shown in Fig. B.

Therefore, I conclude:
1. that comparison of field evidence with calculated predictions according to the method described here shows, that the Neeltje Jans caisson has failed by cyclic loading due to an increased rate of pore pressure generation,
2. that this probably would have been demonstrated more clearly in the Neeltje Jans test if the scheduled loading program, in particular the wave period, could have been followed.

Chairman:

I would like to start a general discussion.
I think one of the subjects we could talk about is the relative merit of the different methods that have been presented. I don't know if anyone in the floor has something to say on that?

G. Gudehus:

I have one question and one comment concerning non-linearity. We all know that the reaction of the ground to displacement is non-linear, but this is also the case for the increments. The incremental force as a reaction against an incremental displacement is non-linear. This is typical of soils as they are plastic. They are necessary incrementally non-linear and this has certain consequences.
One consequence is that you cannot describe the resistance by a Taylor expansion. The Taylor expansion would be valid if the soil would react like a chain but it

is not a chain, it is incrementally non-linear and this makes the system so complicated.
The second consequence is that it is very difficult to say anything about the mathematical character of the different computer models which have been presented here concerning convergence and uniqueness and even existence of the solution. Simply speaking it is very difficult if not impossible to say whether the produced computer solution contains just the input data or anything more, and this is of course an essential problem if every result really is a result or if it contains something else produced by the computer, and in mathematical terms this should be secured by existance proof and convergence proof. I accept that this is not yet feasable so my question is how did you care for sensitivity for mathematical stability of the different models. Did you carry out comparative calculations to get at least an emperical idea of the mathematical stability of the model?

Chairman:

I think there are about four people here who could give an answer to that. Shall we start with Mr. Kenter?

C.J. Kenter:

We made indeed a lot of comparative calculations in order to study the quality of the final solution. We varied for instance the element-size and distribution, the mesh-size, the member of increments and the stress-strain relation. Each time we checked the physical probability of the results and the differences with other results. We found, that it was rather easy to discover non-convergence and non-uniqueness in the Consol results, because when Consol does not converge it diverges very strongly.

P.A. Vermeer:

Both the Consol model and the Elplast model are based on an in essence non-symmetric matrix and this means that the uniqueness of the solution is not always guaranteed. When stress ratios exceed a specific value it can not be proved from the material properties that there is uniqueness of solution. Now when we come to the analysis the elastoplastic stress strain law is integrated numerically to obtain a relation for finite increments of stress and strain. I think in this integration you can do something. You can increase the non-normality and you can reduce it by your way of integration. What I have done is I used such a rule of integration that this non-normality was not exaggerated. I think that you will attend the coming numerical congress in Aachen and there I will present a paper on it.

Chairman:

Well, maybe Mr. Smits has something to add on the accuracy of his method, an analytical method?

F.P. Smits:

Nothing in relation to Prof. Gudehus' question. In general you may say that any analytical method suffers from not being able to model anything other than uniform soil conditions.

Chairman:

Neither does it to Mr. Sellmeijer's approach which basically involves the influence of the parameters, I think.

J.B. Sellmeijer:

Whatever value you put in the programme you always calculate something, so the only thing you can do is compare it with better models, and then you have to compare it with Consol and that's what you already doubted.

Chairman:

Mr. Sellmeijer is very modest. I think there might be some other questions or remarks or comments from the floor?

W.A. Marr:

I would like to ask a question to Mr. Kooman, who delivered the paper on probabilistic methods. In his paper he indicates for an analysis of sliding off of the barrier structures a probability of failure of about 10^{-7}, (if I recall these numbers correctly), for a condition which, if we do a calculation of a factor of safety for simple sliding, we get with your numbers about 1.3.
Doesn't it seem a little strange that we have such a low probability of failure for such a low factor of safety? This is my first question. The second part is: Is it appropriate to assume a normal probability distribution for a case which has such low probabilities of failure?

Chairman:

Mr. Kooman or Mr. de Quelerij.

L. de Quelerij:

I will try to answer the first question. You are talking about a relatively low probability of failure (p.f.) of about 10^{-7} in comparison with a corresponding safety factor of about 1.35. I think this p.f. agrees very well with our estimation of the safety factor, also when we compare this with other construction components. The estimation of the safety factor is based on the semi-probabilistic approach, taking into account 1) the chance of exceedance of load conditions, 2) the uncertainty of the geotechnical calculation model and 3) the uncertainties of the soil properties. When these uncertainties are taken into account by the use of partial safety factor, we come to a total safety factor of 1.5, as shown in section III.1 par. 4.1. This safety factor corresponds with a p.f. of about 10^{-8}. Maybe, you think it is a very small chance but the p.f. of p.e. the steel gates is about the same. Secondly talking about acceptable risk levels you should think of a p.f. for the barrier as a whole of about 10^{-7} (interpreted from the Delta-law). So the acceptable safety factor of 1.5 and the corresponding p.f. of about 10^{-8} agree well with this level.
It is indeed a small p.f. but not a relatively too small p.f. I hope I've answered your question.

Chairman:

Personally I must say I always have a little difficulty in interpreting probabilities of failure. When we design a certain structure with a factor of safety of 1.4 we expect it to stand and not to fail.

L. de Quelerij:

If I understand the second question of Dr. Marr correctly, he is doubting whether the normal distribution of some of the soil parameters is a correct distribution for low probability of failure. I think you should realize that, when you say "for low probability of failure", the probability of failure is a combination of 1) probability of exceedence of a certain load level, 2) the probability of a lower soil property than assumed. When you only look at the soil properties it can be seen that in the total failure distribution load properties with a chance of about 10^{-5} - 10^{-6} are the most important area of the soil properties. So you are not looking at soil properties with an exceedance probability of 10^{-8} or so but of about 10^{-2}. And so I think for this area we can make a reasonable estimation. Does that answer your question?

L. de Quelerij (extension in writing):

Probabilistic calculations with a normal probability distribution, cut of by a physical minimum at 1% (so a non-symmetrical distribution) turned out to have probability of failure which differs only slightly (less than a factor of 10) from the original (non-cut off) results.

Chairman:

I would like to steer further discussion towards a certain point, and that is that from my impression of the presentations that we had here, that if you want to predict failure of a structure, the only good prediction was by Mr. Smits using plasticity, so to predict failure I have an impression that you can better use the plasticity method, and to predict deformations under normal conditions, under the conditions that are expected under a real structure I am impressed by Mr. Sellmeijer's approach. Unfortunately that leaves no place for the finite element method. That is the conclusion that I am tempted to draw but which actually I do not like. And I am afraid Dr. Christian would not like it either, so can I tempt him to give some comments?

J.T. Christian:

I agree with you. I don't like it particulary either. I think it depends very much on what you are trying to do, and maybe part of the difficulty arises from trying to do things with finite element methods with a little confusion over what the aims are. I think you can really do two things with finite element methods. You can look at problems for which we understand the physics of the problem fairly well and you can extend these to other loading conditions and other geometries. This is really what the structural engineers do. We have after all known how concrete beams behave, or at least we think we know how concrete beams behave, for some time, and so, when we make different shapes of concrete or when we make aeroplane wings that are in complex configurations, we use these methods of analysis to tell us what is likely to happen. The other kind of thing that you can do is to look at cases for which the physics are not very well known, such as the behaviour of soils under cyclic loading with very very soft materials such as you have in the Oosterschelde. You can look at cases like that and you can do parametric studies. You can ask yourself: " What sorts of things cause an effect? If I change this number or that number or if I introduce one kind of physical model or one kind of simplification of the behaviour, what does this do to the kind of response that I get?". You can learn a great deal about the physics of the problem. I think where you get into trouble is when you start trying to use such models to give you absolute numbers which you are then going to use for design. If you are so dependent on the accuracy of the model for the safety of your design, I think that you are in trouble. I think that is one of the great uses of finite element methods, or for that matter any other kind of method, including Mr. Sellmeijer's method for which you can do parametric studies.
As long as I am up on my feet I would also like to ask a question or make a comment on the probabilistic analysis. Do I understand it correctly that in fact this 10^{-7} really is composed of something on the order of 10^{-5} which is the loading and 10^{-2} which is the soil.

L. de Quelerij:

That is correct.

J.T. Christian:

I thought that was perhaps where we were. You know 10^{-7} is getting to be about the probability of being hit by a meteorite. It is a very small number and I have to say that for the last several years I have been involved in the design of nuclear power plants and we like to tell the public that we have these very small probabilities. We also generate some of these very small probabilities and I've become somewhat sceptical, I must admit, about much of this sort of work. I'm particularly sceptical of the sort of extrapolations that we all like to make at the tail ends of probability distributions.

Chairman:

Thank you very much. I'm calculating, you say that 10^{-7} is such a small probability and that it is the chance of being hit by a meteorite. I don't think it is that small a probability. I think there are about 3 billion people on this world so that is about $3 \times 10^{+9}$ people. So that would mean that about 300 people a year would be hit by a meteorite. So I think the probability of being hit by a meteorite is much smaller, because you seldom hear about such accidents. I wonder if anybody of the theoreticians here on the table would comment on the merits of the finite element method and try to defend it a little bit.

C.J. Kenter:

I agree with what Mr. Christian said, and also with what Mr. Sellmeijer said before, that the spring constant methods should be checked by finite element methods. Besides I think we overlook one thing. You talked about displacements which could be computed best by the spring constant method, - after being checked by F.E. methods - and failure that could be computed best by plasticity. There is something else, and I think that is the stresses of the construction. How do you want to compute those accurately if you do not use the finite element method? Further we have to calculate groundwater flow, consolidation, difficult geometries like a pier-floor with skirts. For all those calculations the finite element method is most appropriate.

Chairman:

Well, can Mr. Smits not compute them?

F.P. Smits:

Certainly not by a simple method where you don't do the numerical integration of the stress field, but even then a plasticity analysis by the method of characteristics has serious limitations, as stated in the conclusions of my paper.

J.W. Boehmer:

I would like to make a comment from the designers point of view. I have seen this spring constant calculation be applied to all our barrier alternatives in the past 4 years. The first calculations showed large rotations as compared to the horizontal displacements. Rotations which would not be accepted today. This was 4 years ago. After comparing these spring constant calculations with finite element calculations and with model tests these rotations went down to reasonable sizes today. This is our experience for densified soil. But suppose that in the future we are going to make a foundation on non-densified soil like we did in the Brouwers dam about 8 years ago, then we might be faced with a caisson leaning against the wave and headloss what we saw in the Neeltje Jans test, on non densified soil, and what we saw in the condition control measurement of the Brouwers dam itself.
Can the spring constant method result this mode of deformation.

J.B. Sellmeijer:

I only can say not yet. We can investigate the possibility.

Chairman:

Is it necessary to put in a negative spring constant?

J.B. Sellmeijer:

There are some cases in which we use negative spring constants. For example when you compute the bending of piles. If you calculate piles as if they are straight you get a negative horizontal displacement at the base. As a result of bending it can even be positive. In order to simulate such positive displacement you have to use negative spring constants.

P.W. Rowe:

On the seaward side of the Brouwers dam, there must have been a higher head of water and therefore there is going to be seepage forces. The ground is going to go down under those forces more at the back, and for loose sand, the settlements would be quite big and include a tilt backwards, quite apart from cyclic action and the washing out of sand, which presumably is not going on in the heel, otherwise it would have gone on tilting back.
I have measurements on a gravity dam, which, on filling the reservoir tilted back towards the reservoir because all the rocks go down under the weight of the water. Has this been taken into account?

Chairman:

Has anyone taken into account that it may tilt back because of the seepage forces of the pore water?

P.A. Vermeer:

We have preformed a calculation with on the back side of the caisson a horizontal layer of asphalt, separating the soil from the sea. The water table above the asphalt was raised and hardly any rotation for the caisson was computed while we expected substantial tilt towards the lower lake level. The explanation is that the soil was taken to be not completely saturated and the layer of asphalt on top of it exercised a certain pressure. This pressure was only partly taken by pore pressures and for the other (small) part by the soil.
The soil at the back side of the caisson simply settled, which results in a tendency of a backward tilt. Thus the sand being covered by asphalt behaved like the rocks in Rowe's reservoir, at least in the computation. It seems that such responses can be explained without considering seepage forces.

Chairman:

Any other remarks from the floor?

D. Kooman:

I would like to make some comments on the probability of failure and the value of it. In the Netherlands and in England too, the mortality probability of an individual person due to accidents is about 10^{-4} per year per individual. Mortality due to meteorites is about 10^{-9}. In design practice we use safety factors. Partly safety factors are described in national codes for the designer, to indicate the separation between the characteristic loading and the characteristic strength.
When we compute the probability of failure in the semi-probabilistic method, you find 10^{-7}. That does not include some relation between social acceptancy of risk of human life. The numbers may be small if compared to the 10^{-4} which is practical and the 10^{-7} which has been accepted for many years. I suppose that it remains a feasable value.

J.T. Christian:

I want to clear up the point that the numbers 10^{-7} or 10^{-9} do not refer to the soil properties. They are related to two different types of failure loads as you've discussed already. I do agree that those are indeed the numbers that one comes out with in these analyses. I suppose one of these days we will be able to figure out why it is that what we arrived at by a process of trial and error as a socially acceptable way to do design tends to coincide with what we come out with numerically as a socially acceptable thing. I'm not sure that we completely understand why that is. But I have seen some calculations, not these calculations but other ones on very different projects, in which people try to feed in realistic values for variability of soil properties that they got from tests or from field measurements or whatever, and usually the shoulders on the distributions became much larger than what they were really comfortable with. I've seen several cases in which such calculations got abandoned early in the game because that 10^{-2} part of the calculation, the soil part of it, was becoming rather large and embarassing, particularly when you start integrating over all the probabilities of failure. In other words you have 10^{-5} probability of this particular load and then a 10^{-2} probability of failure. When you start getting really large shoulders on the distribution of the soil properties and when you consider the further tails those can come along and cause a lot of problems. You wind up with some embarassingly high probabilities of failure. Those somehow tend not to get published; they tend to get forgotten somewhere in someone's design calculations.

Chairman:

Well, as a chairman I think I might add some concluding remarks. I think we should be pretty happy with the statistical work that has been presented here. It seems not to have the disadvantages Dr. Christian mentioned of some other statistical methods, the results seem to be quite reasonable and acceptable socially. So I think this is really a worth while result which was stimulated by the large project of the Oosterschelde.
About the theoretical methods of calculation I think I should modify the conclusion I drew a little bit earlier, but still I would like to say that when I look back I think the conclusion should be that for a large project such as the Oosterschelde the lesson we have to learn is that you should not rely on one method only, and certainly not the finite elements only at this stage of knowledge. I think that for failure the plasticity analysis or limit analysis is still much to be favoured. For small deformations in the natural range of forces and displacement, the spring constant method looks very good and the finite element method should be used then to link the small deformations to the final range of failure. When we think that this symposium was set up to round off much of the research that has been done for the Oosterschelde project and particularly the Neeltje Jans tests then it is to be regretted that now we still have some disagreements about why the finite element methods do not tend to the plasticity limit that Mr. Smits finds in his plasticity analysis. So I still think that we should go back and do some more calculations, if we can find someone with a budget of computer time, so this is not the end, it is perhaps a new beginning. Thank you very much.

SIMPLE NUMERICAL METHODS TO DITERMINE DISPLACEMENTS AND STABILITY OF PIERS
QUESTIONS (admitted in writing)

J.W. Boehmer:

What criteria do you suggest to the designer for doing research on complicated analytical models to support the application of:

a) simple linear elastic (spring constant) analysis for pier deformations;
b) simple stability analysis (Brinck Hansen) for pier stability;
c) the combination of the two in the L.G.M. non-linear elastic spring constant analysis for pier behaviour.

J.B. Sellmeijer:

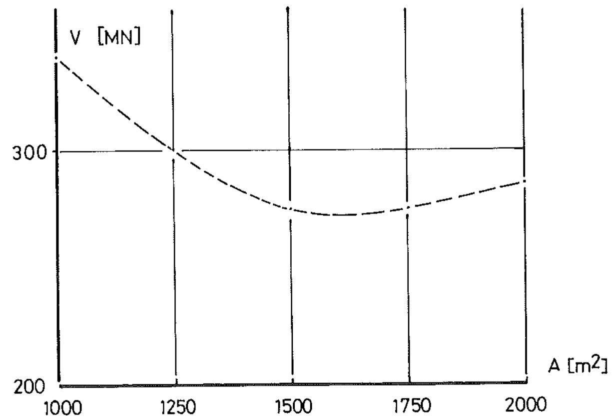
Designers are mainly interested in the feasibility and costs of their design, rather than in the scientific aspects. They need reliable, cheap and fast techniques of calculation, of which the results are easy to understand and interprete. Mostly cheap, fast and simple do not harmonize with reliability in soil mechanics. Therefore the consultant should have at his disposal several techniques, ranging from simple and cheap models to highly advanced computer programmes. The consultant knows which technique meets the designers wishes best.
For parametric studies like the research for the Oosterschelde storm surge barrier, a combination of simple and advanced techniques is most attractive. One operates a simple and cheap device for many computations. The results will be backed-up a few times by more advanced computations. In case of one single problem one must weigh costs against reliability.
It is clear that research in soil mechanics should not be restricted to one type of calculation model only. One must be alert to keep the range of techniques as wide as possible.

P.W. Rowe:

If, as is concluded, comparison is necessary with more advanced analytical models to determine the soil input parameters, does this not mean that the principal value of the method must lie in the opportunity it affords, as indeed with all analytical methods, to derive parametric plots showing the influence of critical dimensions such as depth, length, width and spacing on critical performance factors such as deflexion, tilt and swing under chosen design loads together with safety against particular failure mechanisms? If then the method failed to show at least the form of the curves relating V and A in Fig. 10 of Paper IV.3 for example, it would mean that it did not model the three dimensional interaction of piers, sill and foundation correctly. The same question strictly applies to all analytical procedures. Could the author report the relation between V and A for the design peak, constant spacing S = 40 m, and common sill deflexions?

J.B. Sellmeijer:

Indeed, the principle value of the spring constant method lies in deriving parametric plots, showing the influence of dimension parameters and soil characteristics on the displacements and stability of the piers. The input is simple. Except for data for the geometry and the design loads, the soil characteristics are limited to spring constant values, parameters controling the mobilization of shear stresses and values of friction between structure and soil. All influences on the stresses at the structure have to be represented by those characteristics.
The interaction between piers is considered in the programme. Its influence on the parameters along the foundation plate is discussed in paper III 3 page 8. Its influence on the parameters along the faces is more complicated, since the method used to derive horizontal spring constant values was Menard's empirical three dimensional expression, where interaction is not considered.
The present programme yields the following relation for the weight V of the structure and the area A of the foundation plate.

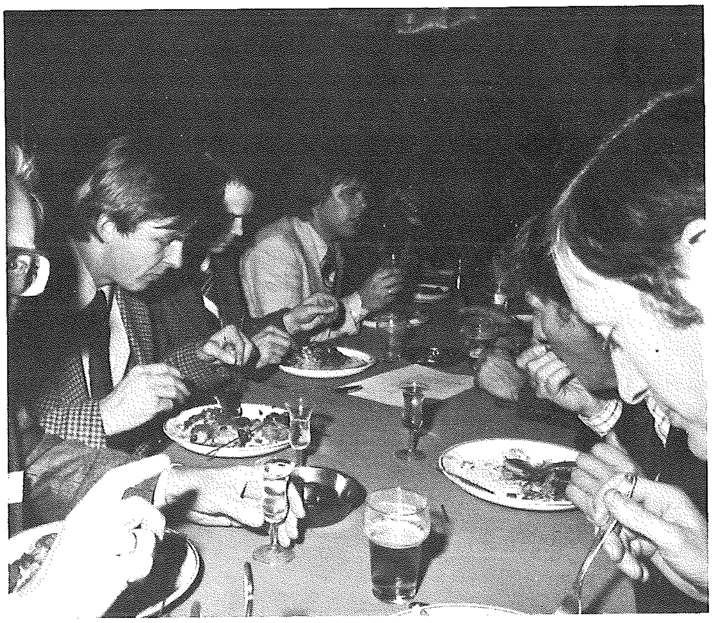


The values of the parameters are:

- height of the structure: 12,5 m
- horizontal load at 10 m above the structure: 160 MN
- spacing: 40 m
- horizontal displacements at the top of the structure 11 cm

Increase of V for increasing A is due to the interaction of piers and sill, when the distance between the piers becomes small. This result is encouraging when compared with the experimental values described in paper IV 3, fig. 10. However, more study into the derivation of the horizontal parameters will be carried out.

Dinner on Tuesday
October 10th.



Session IV
Predictions by Modeltests

Chairman:
W. J. Heijnen, Delft Soil Mechanics Laboratory.
The Netherlands

Chairman:

Good afternoon ladies and gentlemen. I will chair this session which will deal with the subject of the application of model test results to the prediction of prototype behaviour. We will have three presentations this afternoon given by three speakers. At the end of this session a discussion time of approximately 45 minutes has been planned, and I will ask the authors and the co-authors to join me on this podium. Prof. Lambe will bring up some items for this discussion and I hope many of you will participate.
Model tests were in the past, and still are a common tool for the identification of mechanisms governing behaviour. Many pioneers of the profession developed their calculation models from observations of moving sand particles behind the glass-panel, movements caused by loads on very small footings, piles, shut piles and so on. Nowadays model tests are also widely used for the direct prediction of prototype behaviour from the results of tests with a model, which is as near as possible a replica of the prototype construction. This technique was greatly improved by the centrifuge tests. Large models offered the same possibilities. Another category of model tests considers the model as a small structure to compare its behaviour with that of some methods of analysis. We will hear more about these types of model tests this afternoon. Not all the model tests will be presented in this session. Mr. Smits has already briefly described the 1g model test carried out in the laboratory with a pore fluid with high viscosity. Therefore we left this test out of this session. It is my opinion that all model tests although suffering from shortcomings, with respect to the representation of loads, soil conditions and the interaction faces, have contributed very largely to the confidence in the latest design of the barrier.
I ask your attention now for Prof. T.W. Lambe, professor in soil mechanics at the Massachusetts Institute of Technology in Boston.
He is also consultant of Rijkswaterstaat on this project and he will present this afternoon the large scale model test on a caisson in the working harbour at Neeltje Jans. The co-authors of this paper are Mr. J.W. Boehmer of the Deltadienst of Rijkswaterstaat and Mr. W.F. Rosenbrand of the Delft Soil Mechanics Laboratory. The name of Prof. Lambe will be well known to many of you. His book on soil mechanics is used frequently by students and engineers in this country. Bill, will you please take the floor?

CAISSON TESTS AT NEELTJE JANS
by T. W. Lambe
Vol. 2, Paper IV-1

Chairman:

I thank you very much Prof. Lambe for your clear presentation. I think and hope that some of your statements will be the subject of discussion at the end of this session. We now have a few minutes available for questions from the audience. Are there any questions?

T.W. Lambe:

One of the agreements we have among the co-authors is that I would give the presentation and my co-authors would answer all the questions. The people who made the predictions are here to defend their predictions, so I won't bother to entertain those questions, but fire away.

J. Blaauwendraad:

It has been an objective to measure the failure mechanism. However, only pore pressures have been recorded. So could you not have known at the start of the test that this goal could not be reached?

T.W. Lambe:

I severely wronged my many associates in this venture. There was an enormous amount of instrumentation out there, devices to measure stress, and strain. There were inclinometers, all sorts of devices, and I did not have time to show all of the devices, if you think it appropriate, during the discussion maybe somebody from LGM, like Bert de Leeuw, could describe some of the many instruments that were there. But there were many, many instruments. I just happened to show the piezometers and the reason I showed the piezometers, I must confess, is that they are the only instruments in the foundation that gave readings we had confidence in. We were unsuccessful in getting measurements of total stress, strain, deformation, but we did try.

Chairman:

That answers your question Mr. Blaauwendraad?

J. Blaauwendraad:

That answers it, but not satisfactorily. I am not convinced. When you have not enough tools to measure it, then you could skip it at the start of the test and not at the end I think.

T.W. Lambe:

We had enough tools there. The tools just did not work. We had plenty of devices there.

J. Blaauwendraad:

You said the test was successful, but you have to agree it wasn't.

T.W. Lambe:

There were 3 objectives, and we fully met two of the objectives. The third objective was to obtain information on parameters and mechanisms, and we did not. Clearly you would not expect any field test to be 100% successful. You might expect it, but seldom does it occur.

G. Gudehus:

I have one minor point. Essentially I would underline all the presentations and the conclusions, except the one point, failure. I want to support Mr. Smits' view that there is also a type of failure which is not a collapse.
Under each maximum load you have roughly the same displacement and this is also a type of failure which is called incremental collapse. And in this second sense there was failure in the first Neeltje Jans test.

T.W. Lambe:

I don't think that large deformations occured rapidly and I don't think that you can take out one point and talk about whether there was failure there when we don't even know the stresses or the strains. So I guess this is a matter of disagreement in interpretation of the results. I made the statement that, on a mass sense, the caisson was not close to failure.

Chairman:

Thank you very much Mr. Lambe. We will go on with the session. We now get Mr. De Quelerij of Rijkswaterstaat, who is one of the young engineers in the Deltadienst of Rijkswaterstaat, and together with Mr. Broeze of the Delft Soil Mechanics Laboratory, he prepared the paper on the model tests, on piers at scale 1 : 10, indicated by us as the Kats test. I ask your attention for Mr. De Quelerij, he will explain the Kats test to you.

MODEL TESTS ON PIERS, SCALE 1 : 10
by L. de Quelerij
Vol. 2, Paper IV-2

Chairman:

My compliments for your presentation Mr. De Quelerij. You fully succeeded in confining an enormous amount of interesting results in the short time available. I thank you very much.
Is there anyone who would like to put a question?

H.H. van Raalte:

I would like to ask how great was the influence of the Kats test results on the final design for the foundation?

L. de Quelerij:

I think the influence of the Kats test results, on the final design, especially as far as the deformations are concerned was very large. That means that the maximum deformations we have to use as a boundery condition for the present design were mainly based on the translation of the Kats test results to prototype, taking into account several uncertainties in the test results included, such as uncertainty of the scale factor, uncertainty of the influence of the bottom of the model and some other influences. Together we made a multiplication factor on which we have an upper boundary of the test results. The factor of 75 and 7.5 for respectively the translation and rotation are based upon this.
The result of Kats test M, (base area 25 * 60 m^2), which appears to show less rotations then expected, was one of the main reasons for the final design choice for the smaller base area of the present design namely 25 * 50 m^2. In the second place the greater insight in the behaviour of cyclic pore pressures, especially with respect to the dynamic gradients can be emphasized. The test interpretation on the phenomena is still going on and directly influences the foundation bed filter design.

Chairman:

I hope this answers your question. It is very difficult to give an accurate answer.

Chairman:

The next speaker is Prof. Peter Rowe and he will tell us about the centrifuge tests with models of the piers for the barrier. I hope he will also deal with the matter of scale factors in the model testing technique. As you will experience Prof. Rowe is a good and clear speaker, and therefore I now ask your attention.

CENTRIFUGE TESTS
by P. W. Rowe
Vol. 2, Paper IV-3

Chairman:

Thank you Peter Rowe for your very enthusiastic talk. You ran out of time a little bit, but I'm sure that you have convinced most of us and maybe all the attendants, of the importance of centrifuge model tests. Nevertheless some of your statements will certainly be subject to questions. To my regret we have no time for direct questions to you from the room and I will go on with the general

discussion. I would like to invite all the authors and co-authors to this podium to join me in the discussions. Mr. Broeze, Mr. Rosenbrand, Prof. Lambe, Prof. Rowe, Mr. De Quelerij.
Ladies and gentlemen, as you have seen in the programme this general discussion on the subject of model tests will be preceeded by an introduction by Prof. Lambe and I would like to ask him to start his introduction now.

T.W. Lambe:

I agreed to make a few remarks to introduce this discussion on models and I really hope to put the use of models into some sort of perspective. My main points are that predicting performance is what the game is about, to use Prof. Rowe's word "game", and predicting usually involves both parameters and a method. I then want to suggest to you a classification of analyses, and go from there to suggest roles for model tests and field test. I indicate some limitations and then come to the question which Prof. Rowe has already raised "How does one get the scale factor in models?".
I find it convenient to divide the level of analyses into three groups "approximate", "engineering" and the "very best" that we can do. In the Neeltje Jans and the design we are working primarly then on the "very best" state of knowledge, the finite element model tests and field tests. I have the bravery to suggest that as one increases the effort, the sophistication and the cost of analysis, one gets some improvement in the accuracy of prediction, and then an increase in the sophistication and the cost may not give you much improvement.
When should we use each of these levels? I think level 3 is primarily to lay out an exploration programme and to plan a level 2, the engineering type analysis. Level 2 is to make initial design, to plan model tests, plan field tests, to make a plan for a level 1 analysis. A level 1 analysis is the very best that we can do. It gives us greater insight into what is happening in the field case and helps us plan field measurements. On the Neeltje Jans prediction we were supposed to put in level 3, level 2 and level 1, at various stages. And if you remember I pointed out that we were as close on our level 3 as we were on our level 1.
I have listed all of the types of prediction techniques starting at the top with empirical methods and going all the way across to full scale prototypes. I have picked out sources of error, sources of difficulty in each of the prediction techniques and have indicated where the troubles arise in each one. I think you need this to put analytical and physical model techniques in perspective. So I used that to suggest the primary purposes for model tests and field tests. I think that a very powerful use of model tests, particularly the centrifuge test, is to determine the mechanism i.e. what mechanisms really exist. As Prof. Rowe points out he can get numbers, numbers that he can extrapolate to prototype. Thirdly you can use models to get a kind of base data, base information, to examine the various types of analytical prediction techniques. Field tests, I think, have slightly different purposes. Clearly you can get measurements of gross performance and you can with difficulty obtain data on mechanisms and parameters and you can do something on field tests that you cannot do on model tests and that is you can obtain information on construction costs, construction time and construction procedures. We frequently run field tests primarily for this reason. This brings me to some points which I'm sure will stimulate some discussion. If I choose to compare physical models with any of the other techniques we can point out some pro's and con's. If I look at physical models including the centrifuge, as Prof. Rowe points out, he has to face the difficulty of getting the soil profile, and I certainly worry about the difficulties in reproducing the soil profile in model tests as much as I do in analytical prediction techniques. And we have the scale factor, which is much more of a problem in small model tests, excluding the centrifuge, that it would be in the centrifuge. If we look at some of the limitations of field tests obviously the big limitation is cost, and it is very difficult to study many variables. So this leads me to my main point, that on very important projects, like the barrier one should not try to pick one prediction method or two. I feel that most of the prediction methods are complimentary and that one goes through, as I suggested earlier, a series of methods, using the results of one to

help design for the next. This brings me back to the question I brought up at the start. "How does one get the correct scale factor?". Peter has brought this up, and I know from discussions we've had on this project that this is a burning question, and I would suggest to the chairman that we hear some more discussion on how does one get the correct scale factor.

Chairman:

Thank you for your introduction, I hope the audience will take part in the discussion.

J. Blaauwendraad:

Is it possible to include in the third volume more information and more insight into the different procedures we had. There must be somebody who is in the position to give a birds eye view on all studies. Could he please add a paper to the third volume in which such a survey is shown, stating also why a study has been made, why some parallel studies had to be made etc. etc.

Chairman:

Mr. Blaauwendraad, I fully agree with your suggestion, I promise, to do my best to get included in the third volume a short paper on the matter mentioned by you. I hope I can answer the question in this way.

R.H.J. Kremer:

Why is Prof. Lambe so sure of the improving effect of the densification, as you stated yourself, that the densification increased the variability of properties. Is it possible that the decrease of displacements was among other results caused by better control of the subsoil level and a more successful way of placing of the caisson with less turbulence and so on?

T.W. Lambe:

As I indicated there are no measured data of void ratio or stresses and strains in the foundation, and so one is not sure. It could well be that there are soft spots in the undensified and no soft sports in the densified. I do have the feeling that this is not the case, and I do have a feeling against that loose spot theory. I cannot express it as more than a feeling and that feeling comes from studying the pore pressure contours. I see these pore pressure contours extend into some depth and I see a regular pattern, and so this would give me some feeling that there may have been reasonable distribution of strain in the foundation, but I cannot prove that, since there are no measurements.

Chairman:

There was misunderstanding this afternoon about all the measurements. Prof. Lambe only gave a review of the pore pressure measurements but a lot of instruments were installed in the Neeltje Jans test. Maybe I can ask Mr. De Leeuw to give some explanation about the different measurements we did in the test.

E.H. de Leeuw:

I'm extremely sorry I missed the first 25 minutes or so of Prof. Lambe's presentation, but I have two persons here in the room to testify this was not on purpose. If you permit me I would still like to make a comment on a subject Prof. Lambe must have discussed in his own very clear way. The reason for this comment is that during the tea break I heard several people wondering whether instrumentation of the Neeltje Jans test worked yes or no?
And as one of the persons responsible for the test I would like to tell you that we have had a very rough time setting up the test. There was an extremely severe and short time limit on one hand and on the other hand there were not only 5 fingers but also some 7 predictors each having his own very outspoken ideas about

what to measure where. We have tried to be as liberal as possible in that respect and ended up with a huge amount of instrumentation. To satisfy Dr. Blaauwendraad's curiosity, we had 128 sensors all around the caisson most of them in the subsoil. 128 being one of those magical computer numbers: 2^6. The majority of the sensors were pore water pressure meters, simply because pore water pressure measurements are a known and reliable technique.
Besides that, we measured total pressures in several directions at around 20 points. And special devices, quite complicated ones, were designed and built in a very short time to measure horizontal soil deformation and vertical soil strains. In the vertical direction we measured soil strains down to depths of some 17 meters. Caisson displacements were controlled both by optical methods and a laser system by means of diode-beacons. Those of you who are interested in a fuller description of this instrumentation I would like to refer to a paper presented to the Boss Conference in Trondheim some years ago 1).
Now from the start of the test we and many people around us had our doubts about all this instrumentation. From previous experiences we reckoned that about 50% of all instrumentation would fail to work, and we are proud to say that it turned out that more than 95% of the instrumentation that we put in did work and did give results. Now, whether the results are physically acceptable to us is of course another matter.
When Prof. Lambe said that we couldn't make sense of some of the results, this implies that we got results. The instrumentation did perform, I think beyond anyone's expectation (except our own of course), from a mechanical and electrical point of view, very satisfactorily. The only gap in our know-how to measure soil pressures, soil strains and soil displacements seems to be, how do we install our accurate measuring devices (which are of course elements strange to the environment we are measuring in) into the virgin soil without disturbing the natural response of this soil to any type of Prof. Rowe's torture?

More test results are presented in this volume by W.F. Heins.

Chairman:

Thank you very much Mr. De Leeuw. There were several questions put to me about the densification in Neeltje Jans. Why did the cone resistance not go up after compaction and so on. I think I will skip these questions as tomorrow densification is dealt with extensively and you can put forward a question there.
I will ask the speakers of that session to include a remark on this matter in their performance. May I ask the audience to join us in a discussion about the points stated by Prof. Lambe and especially if there are some remarks concerning the scale factor. This is the main item brought forward. May I give the word to some of you in the audience?

F. Molenkamp:

Prof. Lambe raised the point of scale factors, and as I know the scale factors are dependent on the mode you are looking at. Are you looking at cyclic deformation, are you looking at pore pressures? They all give different scale factors and I think when you want to understand why these are different you have to understand the behaviour of the soil under cyclic loading conditions. I think we can only have a really good idea about the scale factors when we understand exactly what is going on. Would the panel like to comment on whether they agree with this?

L. de Quelerij:

I agree with you that you can only make a good estimation of the scale factors when you understand what is going on. I think there are two main areas which you should use in order to obtain scale factors. In the first place a good

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1) Leeuw, E.H. de (1976): "Large Scale Liquefaction Tests", Proceedings Int. Conf. on Behaviour of Offshore Structures, Trondheim.

understanding of the theory of the phenomena you are studying, e.g. the question of equilibrium, the stress-strain behaviour for different stress paths, and the storage according to the Biot equation. After studying the theory which is necessary to obtain the scale factors, one has to make an estimation of the soil parameters itself and of the dependancy on the stress level. I think the only way to do that is to carry on different types of laboratory tests, especially triaxial tests for different stress levels.
The second area is to determine the scale factors by overall tests. One way is to carry out scale tests (1-g) at smaller scales, as we did for the Kats tests at scale 1 : 100, and to extrapolate these results to scale 1 : 1. The theory is also needed in this case.
A better way however to have a pretty good idea over the whole scale factor, is by running centrifuge tests at different levels. I think it is a pity that Prof. Rowe did not make a very refined study of the scale factor because the centrifuge test is an ideal situation to get an overall scale factor of total movement. I wonder what Prof. Rowe's opinion is about that.

P.W. Rowe:

May I have fig. 16 from my paper, please?
We did scale tests in the centrifuge, three years ago on Neeltje Jans at prototype scale, and we said that the field would be nine times Neeltje Jans.
When it came to the request to test the 1 : 10 scale at Kats we had at that time equipment which had to go to three times the peak prototype load to get failure. At peak we were at 30% of the range of the transducers. If then, one is asked to simulate 1/10 of the scale to peak load we have to run a storm up to 3% of the range of the transducers. You can understand that it took time to change the monitoring equipment to work in this very small stress range equivalent to Kats. We've done that now and the test is scheduled for the end of this month. I'm sorry it was not in the last month. To get back to Dr. Molenkamp's question.
In fig. 16 you see Young's modulus plotted against cell pressure σ_3.
These are drained tests. The relation E proportional $\sqrt{\sigma}$ is the most commonly quoted relation. That is normally found with triaxial tests without any end platen displacement correction and coincides with the middle of fig. 16. But if one makes these end corrections, one gets the sort of curves shown in fig. 16. The curves can't go above y = 1, because that's the plastic limit, and they can't go below y = 0, because that's elastic limit. It is seen that the power y in the expression $E = \sigma_3^y$ increases from the ambient stress state towards failure at high effective stress ratio.
When calculating settlement or tilts one integrates strains at relatively low stress ratio so the power factor is going to be low, but if one calculates lateral deflections at the foundation base which may be a result of high strains just under the footing, the power factors y could be towards the higher limit. If one is concerned with a sill displacement which is a function of translations and rotations, there is a combination of the two ranges of y values due to a special combination of stress paths. When using the expression $E = \sigma_3^y$ a single value of y occurs, and that is why one can have different scale factors. We don't have that situation in the centrifuge because the whole object of the centrifuge was to get the correct stress path.

Chairman:

Thank you. Are there any other questions?

J.D. Nieuwenhuis:

In my opinion the balance of applicability is shifted too strongly to centrifuge tests during the discussion.
Of course 1-g model tests lack the 1 : 1 stress scale, but the models are big (1 : 10) and all details of pier and sill can be modelled. The centrifuge test is outstanding in simulation of stresses but the geometrical scale is small, (1 : 120), and many details such as the thin layers at the top of the sill cannot be

imitated. Moreover the semi-turbulent and turbulent fluid flow through these layers is not correctly reproduced in the centrifuge tests.
Since both types of model tests have their advantages and disadvantages the foundation engineers in the Oosterschelde project were glad to obtain results from both the types of tests.

Chairman:

Thank you. I fully agree with the remark. Is there anybody who wants to say something about this, or add anything?

T.W. Lambe:

I never pass the opportunity to go on record as supporting Prof. Rowe that the centrifuge test is a magnificent stress path test, and I support him now. However, I think one of the most powerful uses of the centrifuge is to indicate mechanism and I would be most interested to hear your comment on where you think the strains occur in the foundation in some of your centrifuge tests of Neeltje Jans when you modelled that.

P.W. Rowe:

We did not measure the distribution of strains in the foundation. That's a difficult enough job. One could have spent two or three years trying to do a very detailed test which might give answers to questions no longer relevant. Instead we studied the critical design depths of the caissons and critical design areas of the piers to assist overall design decisions. We looked also, for example, at the effect of densification and what could happen if there were loose zones. There are indeed a multitude of other objectives, given time.
May I just say to Mr. Nieuwenhuis that we do not attempt to scale the fine grains down. He is talking, I think, about the stones at the surface, which we do scale down, but one should not scale the actual particle size of the sand bed down because that alters the interparticle friction angles, particle shapes and associated properties. When one thinks about it one does not actually use the size of the particle per se in any theory of stability.

G. Gudehus:

I want to make a short comment concerning the failure mechanism. If you are only interested in ordinary failure and you want to get an idea of the failure mechanism, you can get this idea from a small scale test which is not done in the centrifuge. In the fully plastic behaviour this sand is sufficiently self similar to give a realistic failure mechanism for the ordinary collapse, but only for this case. Anything like stress-strain interaction which there is in liquifaction cannot be covered by such primitive model tests, but don't forget the strength of the classical model tests. They can give the first idea of the ordinary failure mechanisms.

F.B.J. Barends:

I have heard much about failure and about the failure mechanism. Is it clear for everybody when failure occurs. How is failure defined, and at what rate of deflections?

Chairman:

Prof. Rowe would like to answer this question. He talked about failure this morning.

P.W. Rowe:

You may remember the load deflection curve, figs. 9 and 15 of my paper. We used the field scale values of about half a meter as arbitrary failure, which was nowhere near ultimate failure in the classical sense. A limiting displacement may well be regarded as failure of a structure for an engineer who wants it to fulfil some function. Models show that it is difficult to cause ultimate failure with limited cyclic loading if the subsoil is drained. Ultimate failure is the load which leads to continual movement, but under cyclic load, the unloading cycle stops the motion. Over the next cycle the structure moves a bit further and stops. So when we apply a cyclic loading programme with a peak load having only one cycle, it is not possible to induce more than a limited displacement. A critical condition may be lifting of the heel which might lead to other types of failure.

T.W. Lambe:

I have given a great deal of thought to what is failure, and I think that is a good philosophical question. The way to resolve it is to start at the outset by listing criteria performance including deformation, stability, flow, force. You state acceptable criteria and define failure as exceeding those acceptable criteria. And I find that it goes over better if you don't use the word failure, but use the word "malfunction" so that if you start off by saying that this caisson must not move more than 5-10 cms, and it moves 20 that is a failure or malfunction. Before I hand over the microphone, I didn't want the audience to think that I was baiting Peter Rowe. I did want him to say that he had run some centrifuge tests in which he had obtained some information on mechanism and I know that he didn't have many measurements of strain in the foundation and I deeply regret that. I do know that there were some tests in which you detected some mechanisms of rocking and moving on the surface and that's what I was hoping you'd bring out. I really wasn't baiting you, I can do it better than that!

A.C. Stapelkamp:

I have an overall question. I think it comes near the question of Mr. Blaauwendraad. What is the value of all the different studies? What is the weakness of one, what is the strength of the other. Is the mentioned "more study" a metter of 10% or means it a 50% smaller storm surge barrier. This I miss in the whole and I think that is the last chance because Thursday it is a little bit different discussion.

Chairman:

I want to answer the question a little bit. When choosing some parts for this symposium we have chosen for topics more or less and what you miss is a red thread through the topics and I already promised to Dr. Blaauwendraad to try to give that in the third volume of the proceeedings. I have to close this session now and I hope to see all of you at Rijswijk tonight.

W.F. Heins (written comment)

This comment on the paper of T.W. Lambe, J.W. Boehmer and W.F. Rosenbrand, "Caisson Tests at Neeltje Jans, The Netherlands" is presented on request of the Scientific Committee of the International Symposium on Soil Mechanics Research and Foundation Design for the Oosterschelde Storm Surge Barrier.

The intention of this comment is to contribute to the discussion whether the foundation of the caisson tended to "fail" or not during Load Parcel 3 of test 1A - caisson on undensified soil -. Failure at this stage was predicted from the results of 1-g model tests and the analytical calculation method based on plasticity theory.
Although the movements of the caisson increased considerably at a certain stage during parcel 3, Lambe et al. did not interprete this as failure. Approximately equal increments of movements were observed during loading stages 4 and 5.
At a certain moment during parcel 3 the stress-controlled loading system could not follow the increasing horizontal movements. For about 10 cycles the load amplitude varied around a much lower value then planned. Although the cause of this pressure drop could not be established it seems not an unrealistic supposition that some failure mechanism, generated in the foundation soil just below the caisson during the preceeding load cycles, may have caused this hitch of the loading system. Due to the drop in the applied loads, pore pressures got the opportunity to dissipate, resulting in a slight improvement of the soil structure. The readings of some of the pressure gauges in the soil below the caisson support this supposition.
Figure 1 to 7 included present the situation and the readings of these pressuremeters during loading parcels 2 and 3. The effect of the hitch of the loading system during parcel 3 can be observed clearly from the readings.
A more detailed view of the readings during the last cycles just before the pressure drop in the loading system, is presented in figures 8, 9 and 10. Some of these readings show irregularities with respect to the frequency of the signal (see e.g. pore pressure meter FV ÷ 4.00).
The same kind of irregularities were also observed in many of the signals taken from the pressure gauges in the Kats model tests just before failure occured.

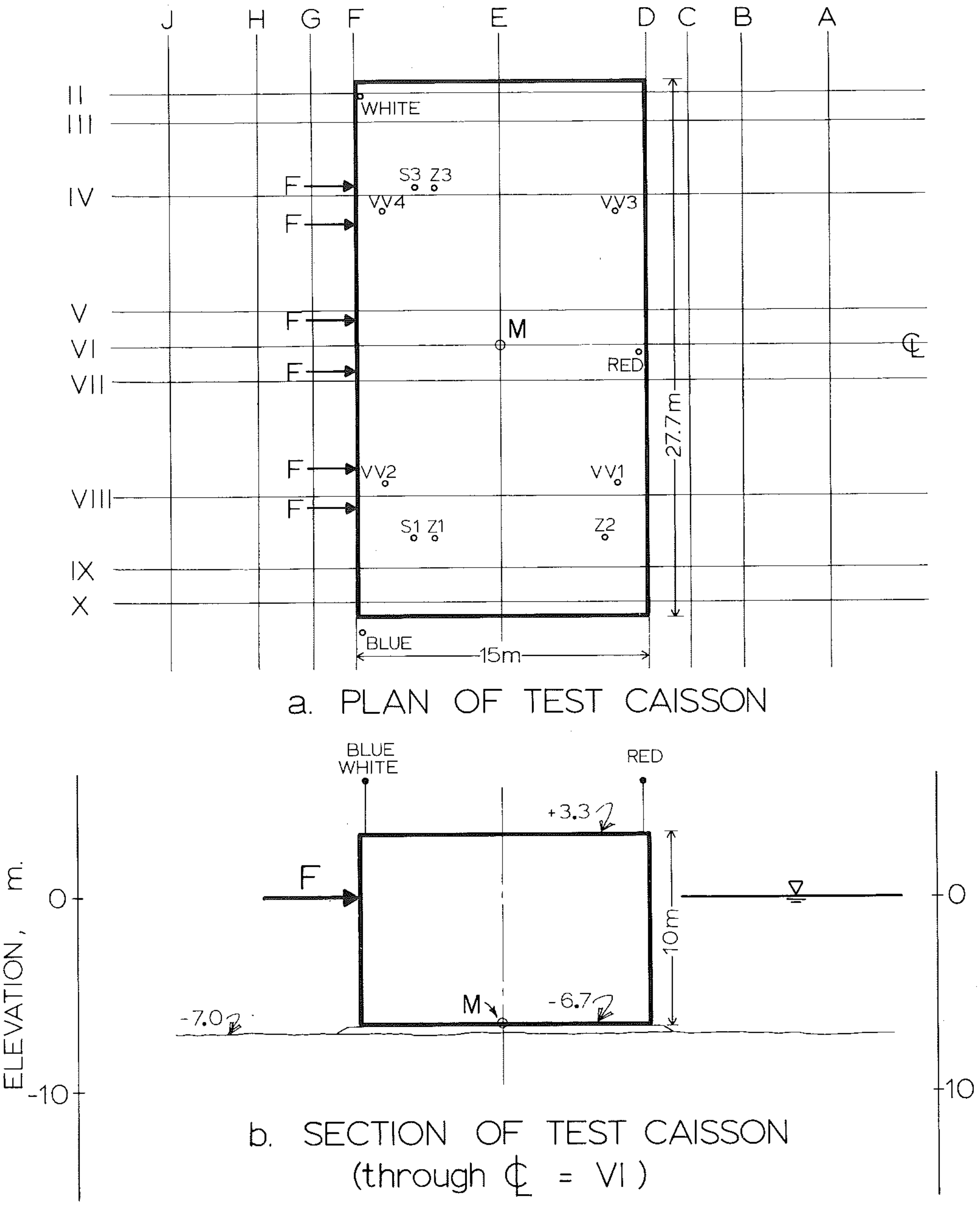


Fig. 1. Instrumentation plan

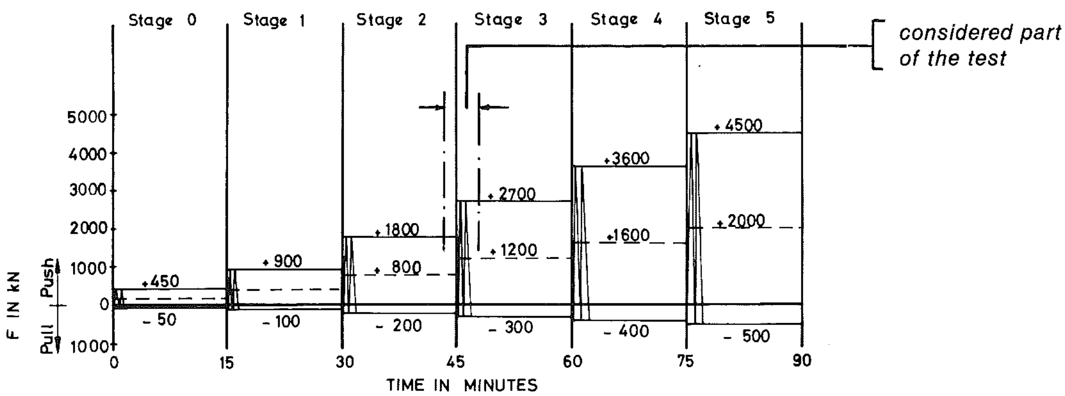


Fig. 2. Planned loading schedule.

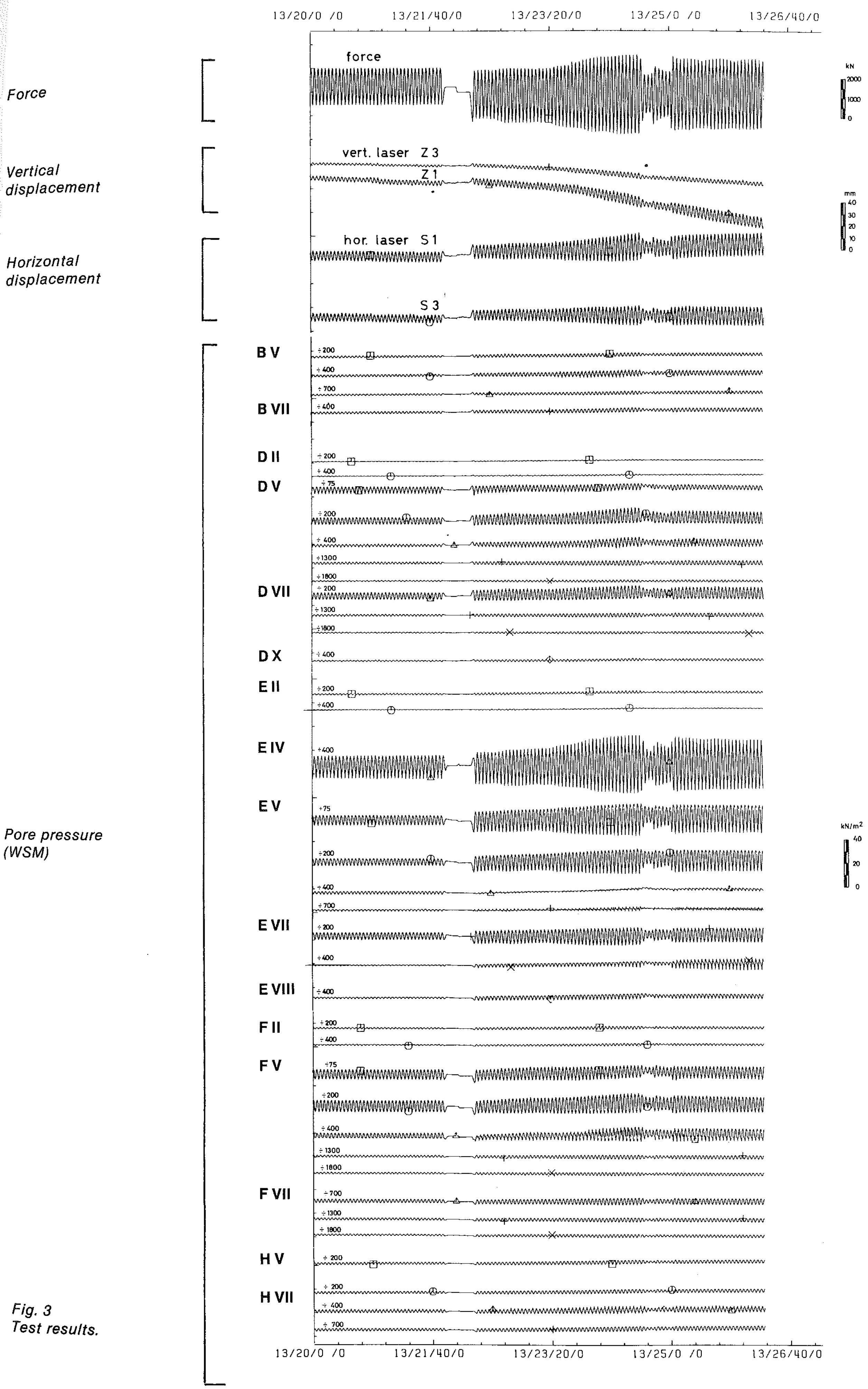


Fig. 3
Test results.

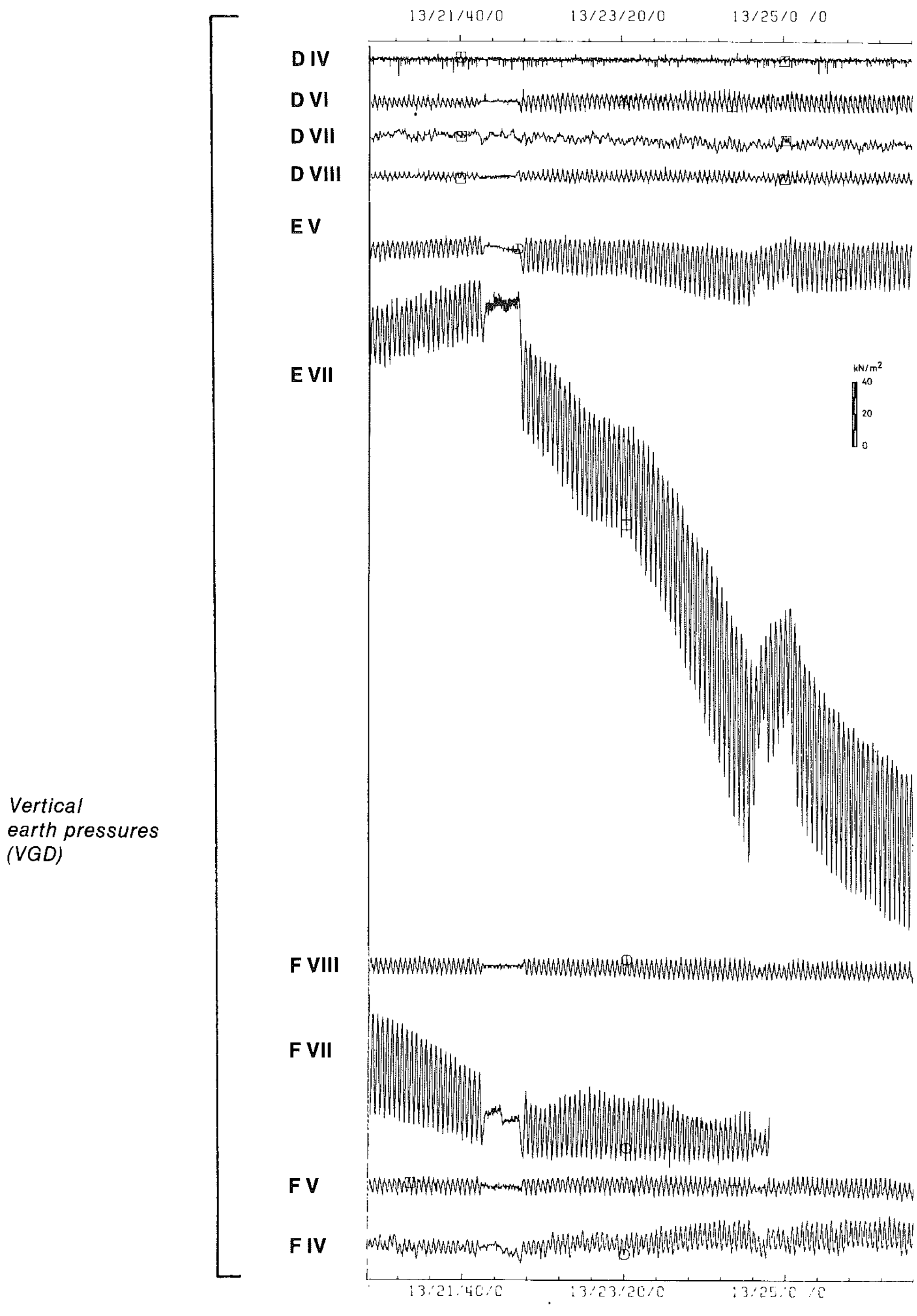


Fig. 4. Test results.

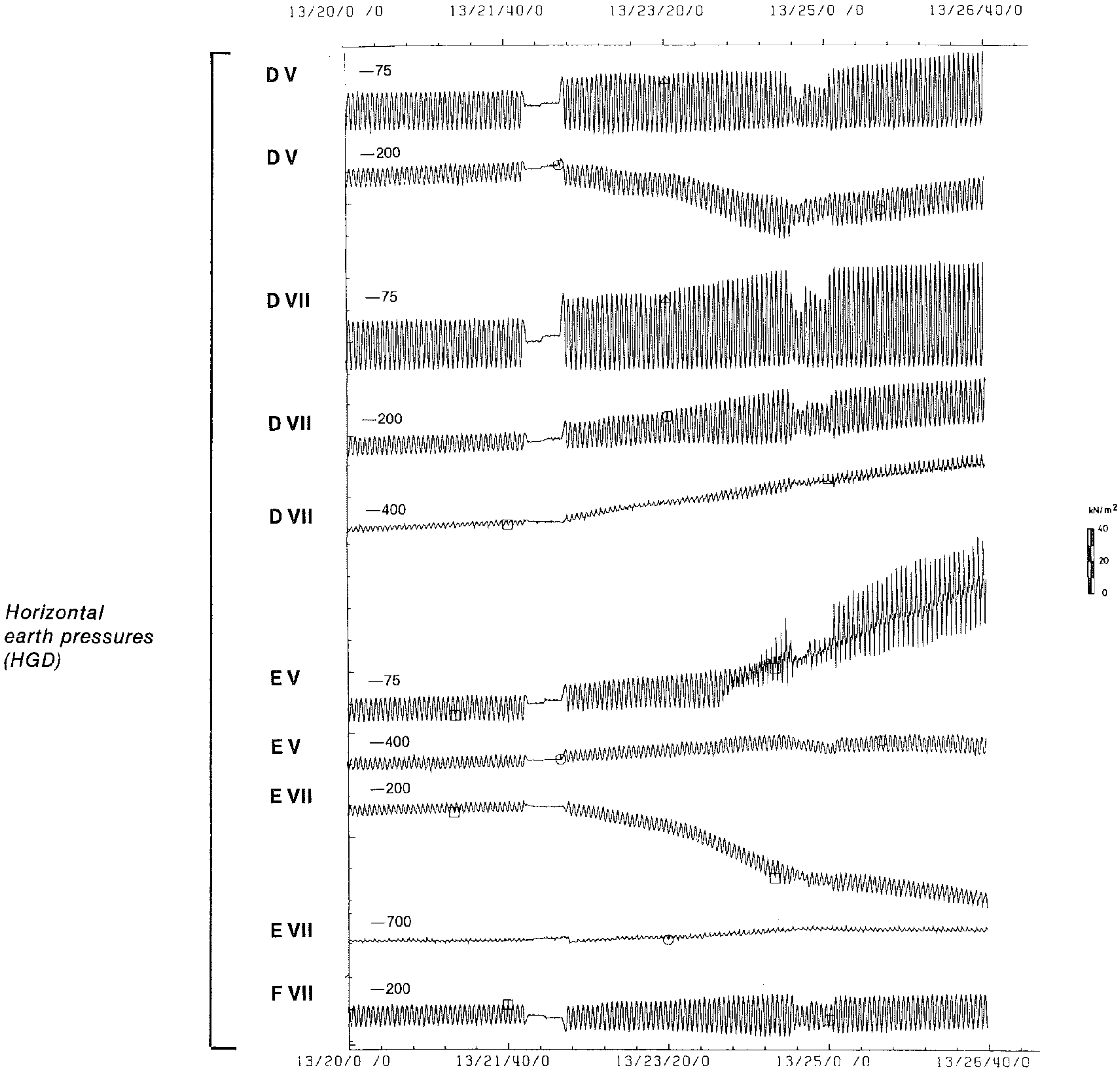


Fig. 5. Test results.

Earth pressure
under 45°
(SGD)

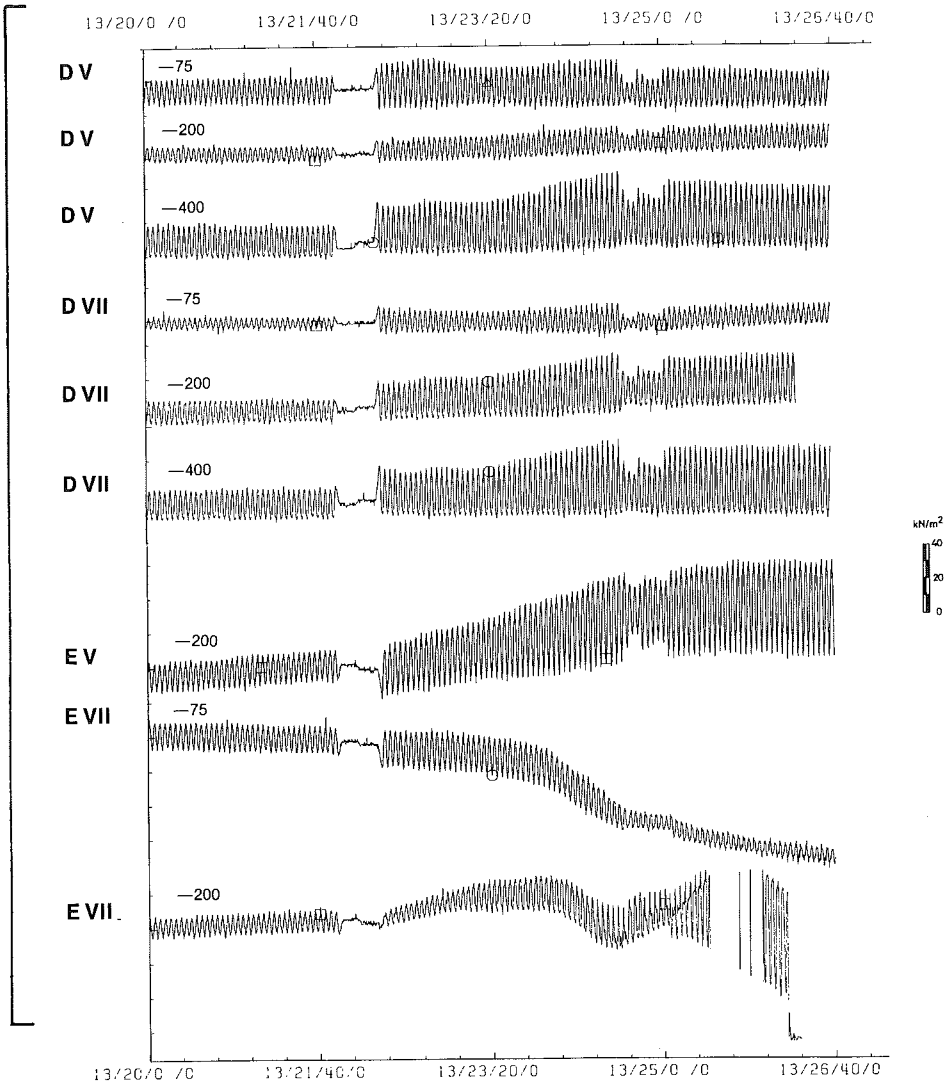


Fig. 6. Test results.

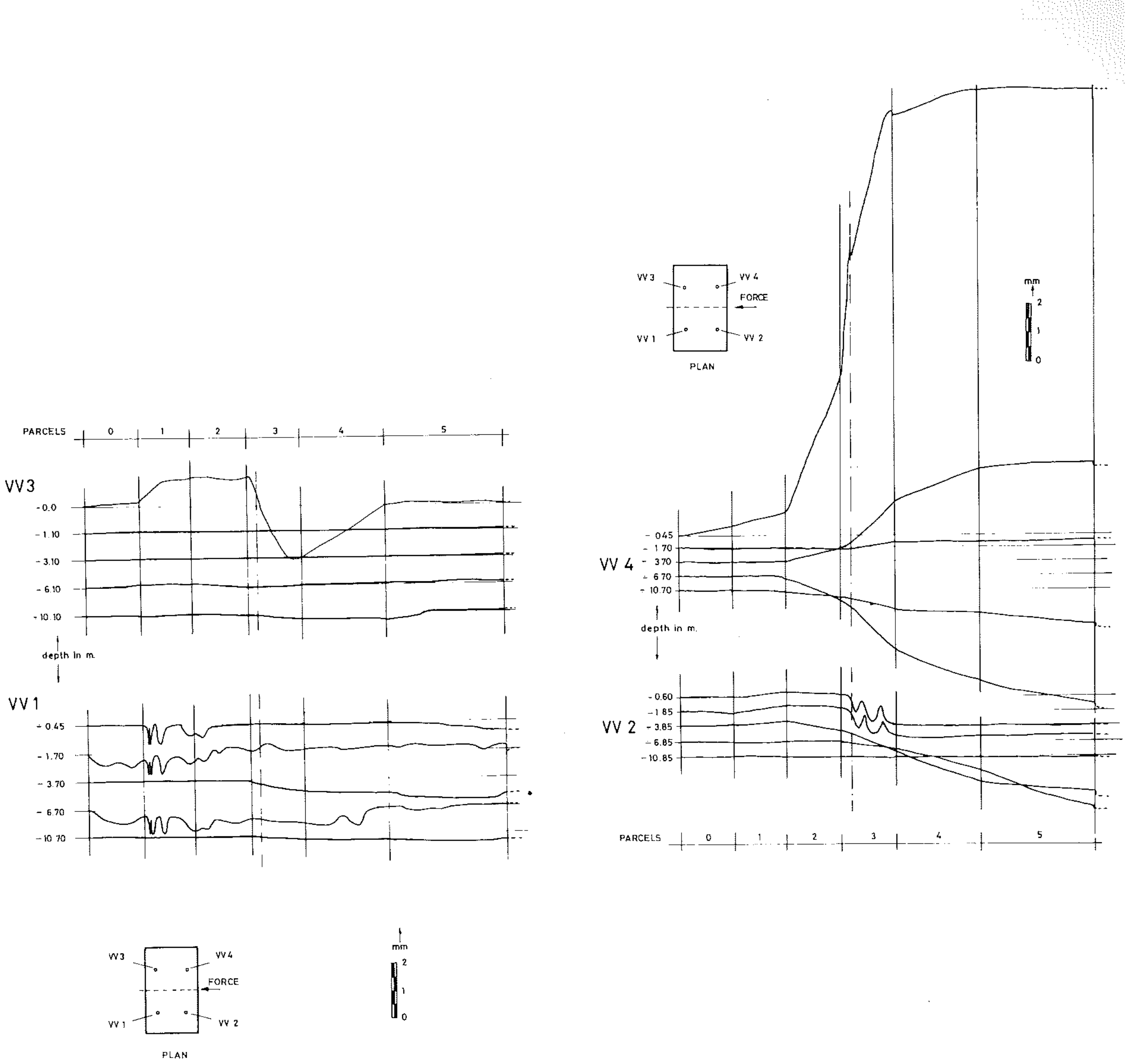


Fig. 7. Test results.
Schematic vertical soil displacements.

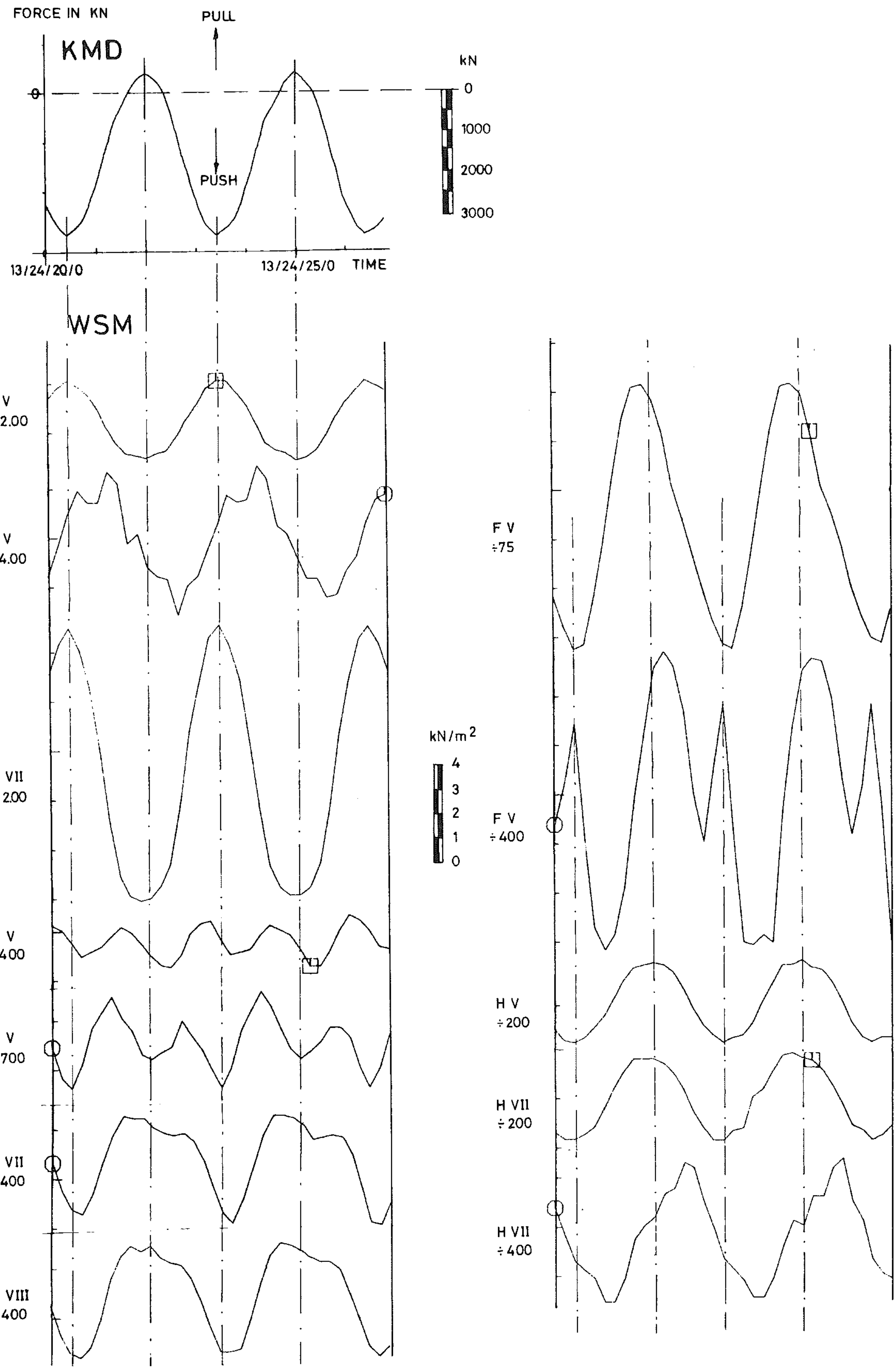
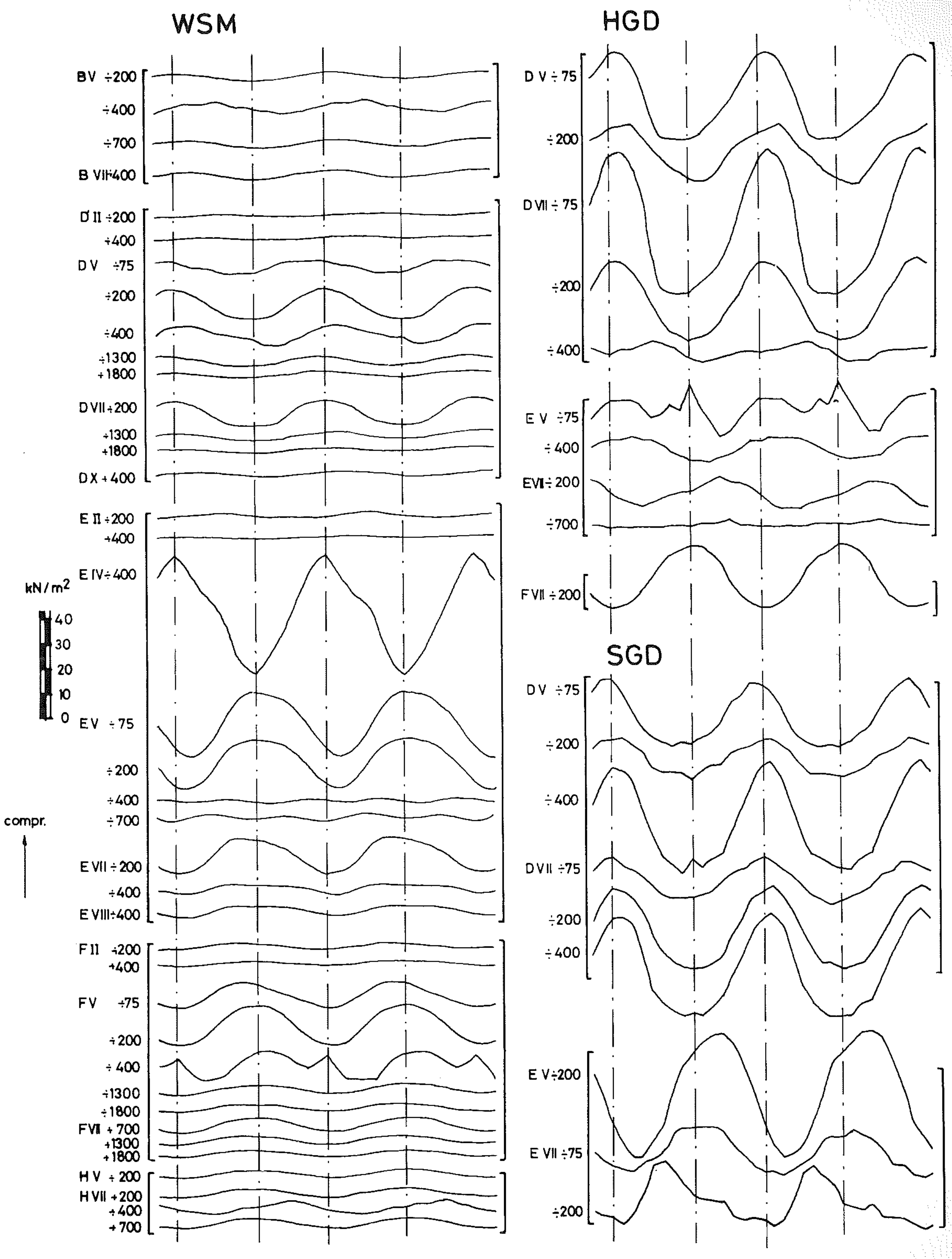


Fig. 8. Frequency doubling and change in sine wave of some of the test results.


WSM
HGD
SGD
BV ÷200
÷400
÷700
B VII÷400
D II ÷200
÷400
D V ÷75
÷200
÷400
÷1300
÷1800
D VII÷200
÷1300
÷1800
D X ÷400
E II ÷200
÷400
E IV÷400
kN/m²
40
30
20
10
0
E V ÷75
÷200
÷400
÷700
compr.
E VII ÷200
÷400
E VIII÷400
F II ÷200
÷400
F V ÷75
÷200
÷400
÷1300
÷1800
F VII ÷700
÷1300
÷1800
H V ÷200
H VII ÷200
÷400
÷700
D V ÷75
÷200
D VII ÷75
÷200
÷400
E V ÷75
÷400
E VII÷200
÷700
F VII ÷200
D V ÷75
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E V÷200
E VII÷75
÷200

Fig. 9.

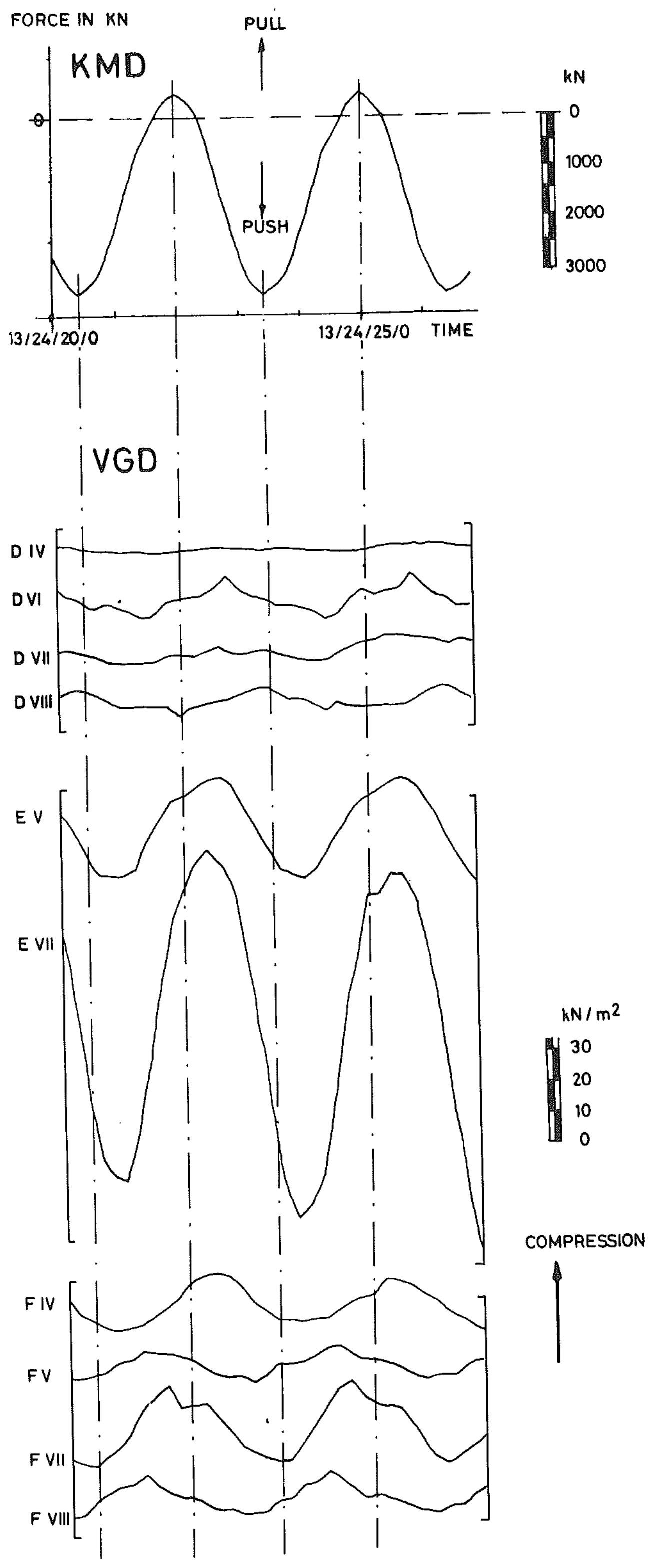


Fig. 10. Test results at all sensors.

Excursion to the construction site on October 11th.

Session V

Site Investigation and Densification Studies

Chairman:
E. P. Hudig, Dosbouw v.o.f.,
The Netherlands

Chairman:

Ladies and gentlemen. Thank you for your attendance. Today the session number 5 will deal with site investigation and densification studies. Site investigation as you all will know is a very crucial part of every geotechnical study. I must say that we are very fortunate that Mr. Vermeiden is able to give us a lecture. He has been working for 40 years at the Delft Soil Mechanics Laboratory and he has in the past worked a lot on cone penetration tests on water. He has done much work, together with Mr. Begemann on the continued soil sampling, and I may mention that the diving bell pontoon which you saw yesterday, which was called Johan V, has actually been named after Johan Vermeiden.
In a moment we will hear from Johan Vermeiden, but first I would like to explain something about the background of the densification studies which we have had in this project. I myself have been involved in the project for almost five years. In the very first design that we made six weeks after the request for a storm surge barrier, no compaction was foreseen either the subsoil or the sill. Later on it appeared that we needed compaction for the subsoil because we were afraid of liquifaction. From further tests during the caisson period it appeared even more strongly that probably the sill material would have to be compacted really very well. As you may have heard from Mr. Spaargaren one of the main reasons to abandon the caisson method and to take a pier foundation was that we were not sure about compacting the sill material due to siltation of the sill. It is a pity that we have not discussed the studies done on compaction of the gravel and stone materials, but maybe in a later paper for another conference we will be able to say something about the compaction of the sill material. So we will be talking about compaction of the sand subsoil. Mr. Davis will present the results of some laboratory tests. Mr. Jaworski of the North-Eastern University, USA, will tell us about densification tests. Mr. Vermeiden will start now on site investigation.

SITE INVESTIGATION OF SOILS
by J. Vermeiden
Vol. 2, Paper V.1a

P. Lubking:

Ladies and gentlemen. In the next film a short impression is given of the geotechnical pontoon Johan V - for insiders beauty Johan - and the diving bell which is lowered from that pontoon. After the positioning of the barge a cone penetration test and a boring according to the Begemann system will be carried out; the cone penetration test under atmospheric conditions, the boring under an over pressure of 2.3 bar.

Chairman:

We now go on to a short presentation of Dr. De Mulder, who is project engineer at the Geological Survey of the Netherlands. I'd like to invite Dr. De Mulder up here.

ENGINEERING GEOLOGY - CONSTRUCTION OF A STRATIGRAFIC MODEL
by E. F. J. de Mulder
Vol. 2, Paper V.1b

Chairman:

Thank you very much Mr. De Mulder. Ladies and gentlemen I'd like to carry on immediately with a presentation by Mr. Davis and then we can see before the coffee break whether there is time to ask a few questions to Mr. Vermeiden or Mr. De Mulder. I may introduce Mr. Davis as working for the Rijkswaterstaat at the Hague since a few months. Before that he was for almost 8 years with the Delft Soil Mechanics Laboratory. He has coordinated all studies on densification at LGM and I'd like to say that it is his hard and intensive work that densification studies have been very useful.

LABORATORY INVESTIGATIONS REGARDING THE ARTIFICIAL DENSIFICATION OF SAND AND GRAVEL MATERIALS IN THE OOSTERSCHELDE
by P. G. J. Davies
Vol. 2, Paper V.2

Chairman:

Peter, I would like to thank you very much for your clear presentation and you can all imagine that this has been an interesting study for the compacting aspects of the Oosterschelde. I'd like to suggest that we have a general discussion at the end of the morning after we have heard Walter Jaworski and Ton Pladet. We now carry on with our morning session on densification.
Our next speaker will be Mr. Jaworski, of the North-Eastern University at Boston in the USA. I'm very glad that he is here to tell us something about the work he did for the Lambe group on densification studies.

METHODS AND CONTROL FOR DEEP DENSIFICATION
by W. E. Jaworski
Vol. 2, Paper V.3

Chairman:

Thank you very much Walt, for your very clear presentation. I am sure many of us have seen quite a few new aspects. I'd like to mention that we have quite a number of questions, and we hope that in the discussion most of them will be answered.
I'd like now to call immediately upon Mr. Pladet to carry on with his presentation. Ton Pladet has been borrowed by Dosbouw from the Stevin Group where ha has been working for quite a long time. He started his work there on the Zeeland brug, with the excavation and sinking of the enormous piles underneath the bridge.
His speciality was to investigate the soil underneath the piles after they had been brought to the required depth and if necessary to compact the soil directly under them with small concrete vibratory needles. That was his start on the compaction road. You will hear now how far that has gone.

DENSIFICATION OF THE SUBSOIL IN FIELD PRACTICE-RESULTS OBTAINED WITH A DEEP COMPACTION METHOD
by A. A. Pladet
Paper V.4

Chairman:

Thank you Mr. Pladet for your presentation. I am glad to say we are ahead of schedule at the moment, and now I'd like to invite the authors of this morning up here to answer questions. The first question we already know. I'd like to invite Mr. De Rouck up here to ask Mr. Vermeiden a question about pore pressure measurements in tidal areas and how the pore pressure follows the high and low tides.

J. de Rouck:

I have a question concerning soil investigation, especially concerning the measurement of pore water pressures. At the location where the storm surge barrier has to be built we have a tidal movement of approximately 3 metres. My question is how does the pore water pressure change with depth as a function of the tidal movement.

J. Vermeiden:

Mr. De Rouck, my answer can be very short, because in the area where the dam has to be built we have not measured that. We have made this kind of measurement in the Western Scheldt, but I can't tell you about the results of the measurements, because it was a long time ago.
Perhaps there is someone else who can say something about that.

Chairman:

Well I understand that some of the measurements have been done, and I think it would be wise if people who know more about this get into contact with Mr. De Rouck and tell him about it. I'd like to carry on with the discussion because we have quite a number of questions.

A.F. van Weele:

The results of the compaction tests show that a maximum of 30-35 Hz frequency was used. Is this correct? Has a frequency of up to 50 Hz also been tried out? Did the results coincide with maximum energy imput which seems more important than amplitude, frequency etc.
Did the tests represent the field conditions? Has the laboratory result been checked by field results?

P.G.J. Davis:

We have experience with 50 Hz. A few years ago we started vibrating investigations and we played with a frequency of 25 Hertz and a frequency of 50 Hertz. Only in that investigation the amplitude variation was from 0 to about 1 mm. It turned out that with a frequency of 50 Hertz you got less compaction than with a frequency of 25 Hertz. I think that Mr. Pladet has perhaps some in situ experience with a frequency of 25 and 50 Hertz. I don't know.

A.A. Pladet:

Our experience was that you can get the same quality with 50 Hertz, but it takes more time, so as contractor you choose the method with the shortest time and we are compacting now on 25 Hertz.

Chairman:

Thank you, I hope that answers your first question, Prof. Van Weele. Your second question was whether the laboratory tests were representative for the field situation and whether these have been checked with each other. Is that correct? I'd like then to ask Mr. Davis to answer this question.

P.G.J. Davis:

Of course the conditions in the laboratory are not the same as the field conditions. In the field you have larger soil masses to compact, and also the horizontal stresses in the ground differ from the laboratory, which influences the amount of compaction. Also the vertical state of stress in the ground differs in the vibrating cylinder. So I think in any case the minimum n-values you get with the vibrating gest you won't get in the in situ soil conditions. But I think the influence of the parameters on the amount of compaction will be the same as in situ. We have not checked all results, but there are a few checks. For example what I have said about the influence of the silt content has been investigated in Durban, South Africa with vibro flotations, and those results are exactly the same. The influence of the silt content is exactly the same as we found in the laboratory. Further on there have been a few other investigations where we influenced some parameters in situ, and at this moment we are testing plate vibrators where we are varying the amplitude, the frequency, and also the blow-power of the plate vibrator. In my opinion you will find the same influence.

A.C.J. Baker:

The angular and the rounded gravels had very different gradings. In general, gravels with wide gradings are easier to compact. Was any account taken of this in the comparisons?

P.G.J. Davis:

We don't have experience with different gradings because for the tests we use the sea gravel which will be used in the Oosterschelde project. So it wasn't a parameter study on grain size.

A.C.J. Baker:

For angular gravels an amplitude of about 5 mm is recommended. Figure 15 stops at 4 mm, is there any evidence to show that 6 mm amplitude is not better still?

P.G.J. Davis:

I've said already a limitation of the vibrating table was the amplitude of approximately 4 mm. The amplitude of 5 mm is a kind of guess, but as you can see from the figure it doesn't matter so much if you have an amplitude of 3 or 6 mm, because in that range you have about 90% of the maximum amount of compaction.

Chairman:

Does that answer your question? I understand that the test was not done, and I don't think it's possible with the apparatus that we have at the moment.

A.C.J. Baker:

Is there any evidence to show that larger amplitudes might not be better?

P.G.J. Davis:

Yes, therefore I have to go back to earlier tests that we did. We got a minimum n-value for a certain amplitude, and after that minimum n-value the compaction at further growing amplitudes will be less and the curve will go to a horizontal line.

A.C.J. Baker:

Was any consideration given to optimising compaction against energy output, the energy being a function of amplitude, frequency and to some extent surcharge pressure.

P.G.J. Davis:

This optimising hasn't been done, because for the laboratory tests and the in situ tests, we had a given compaction method, with a given energy output. That was the method of Van Hattem and Blankevoort.
For the compaction of the gravel we are testing out in situ how much energy and what other parameters we have to choose for example what kind of blow power we have to use. Results aren't available at this moment, however.

G. Gudehus:

The laboratory studies are summarised by recommendations for amplitude, frequency and surcharge. Now material properties of sand cannot be explained in terms of these quentities. Amplitude and frequency are system qualities and not material qualities. Is it justified then to transfer the laboratory results to the field compaction implying a completely different mechanical system?

Chairman:
Actually this is very similar to the second question of Prof. Van Weele and my own opinion, if I may say something about it is, of course they are so different that you cannot say they are completely applicable, but what you get is an impression of trends, which direction it goes, and I must say that it seems that you do learn something from the laboratory tests but you cannot leave it there, you have to do testing in situ and that is actually the same as what Walt Jaworski says about it. I'd like to ask what he thinks about this.

W.E. Jaworski:

Well, the behaviour you're finding in the lab is in a confined state. One of the problems of testing in a lab is that you've got the material in a mould. To give an example of the problems you run into, we were looking at soils vibrating horizontally and vertically. And we found that if we do not insert certain types of acceleration - e.g. certain shear stresses - to the soil they wouldn't densify. If we try to extrapolate that to the field, we have to look at the complex nature of what is hapening in the field. The whole thing becomes three dimensional. We're trying to extrapolate from a very controlled situation in the lab to the field condition which is uncontrolled. I think the parameter we should be looking at is in the first place acceleration. What are the threshold accelerations required in the field to move the particles, and then secondly how does this acceleration vary with depth. This is the type of testing Mr. Davis was trying to do. Furthermore the spatial variation in the densification is to be investigated. Trying to extrapolate data from the lab is useful to give us an indication of what we can expect, but we still have to have the test section to actually measure field accelerations and work from that point.

Chairman:

I hope Prof. Gudehus that answers your question. We all understand our limitations in the soil mechanics, and we will never be able to find an exact answer to all the questions. I'd like to carry on to the questions of Dr. Hamza.

M. Hamza:

You have shown us that regardless of the initial void ratio the sand arrives at a constant void ratio after compaction. Have you compared this void ratio with the critical void ratios of corresponding effective mean stress?
This will have importance in evaluating the structure behaviour. We do compaction to improve the soil, and if we want to see how the soil improves we always have to measure it against a framework which we can understand. So if you tell me the void ratio is 30% I don't expect this soil will be dilative, will be contractive under shear, under the structure. I would like to relate it to a framework say, critical state line, critical void ratio line. Now this value which you said is constant even if you start from a value less than it and you vibrate, you always increase to it. Is this value below the critical line, above it or on it?
And I would like to extend, to ask if there is any method you have checked and used with which you could improve the soil beyond the critical line, or in other words can we put an upper limit to the improvement we can obtain, and could this limit be explained in a proper framework of soil mechanics understanding?

J.W. Boehmer:
Last Monday I showed you a chart which I called the liquifaction chart, but you could call it the densification chart or whatever you want as well. Now I showed you that if you want to avoid liquifaction slides, the way I have been defining them on Monday, you have to bring the relative density back down below the line, which I call critical liquifaction slide line. If on the other hand you are trying to avoid a flow slide, I showed you that you have to come somewhere below the line, which I call the critical flow slide line. We don't have the exact numbers, but this is just to give you an impression. Now, if you don't want any

settlement at all, I thought I showed you, but I might not have been clear enough, that you could go to the critical dilation line. Now it's just a matter of doing some tests to expand these different criteria and apply them to cost benefit analysis to see what we are finally getting at.

Chairman:

Thank you, Mr. Boehmer, actually I'd like to keep it at this and if you have any more questions please contact Mr. Davis or Mr. Boehmer outside, because I think we have to continue with your second question.

M. Hamza:

You have shown us that if the silt percentage is more than 20% no improvement occurs but on the contrary, it decreases. Is it true even if you allow the pore water pressure to dissipate under surcharge loading? If you wait enough you get improvement. Then you should wait for the pore water pressure to dissipate. When you had high silt content it is shown that the vibration method on the contrary, made it even worse. Now I think this is because at the time you measured your porosity you did not take enough time to allow the pore water pressure to dissipate under the surcharge applied in your test. Did you or did you not?

P.G.J. Davis:

No, I didn't.

M. Hamza:

You didn't. Maybe if you had, you would have had an improvement. And so you have done injustice to the method because generally the pore water pressure dissipates quite quickly in the field with the silt, maybe in one day or two days. It depends of course on the drainage path but I don't expect very large drainage paths in this case, especially near the high stress level around the foundation. I think you would have had an improvement.

P.G.J. Davis:

That's possible.

Chairman:

I think we can leave it at this, although we're not at the end of the questions, but I should still like one question from the floor. This will be the last one.

J.T. Christian:

I've noticed many of the authors describe field relative density in their charts. Would they please tell us how they define the relative density in the field and how they measured it?

J.D. Nieuwenhuis:

I have the impression that we did great injustice to the audience this morning. I'd like to apologize. You have remarked by now that there were two different groups working, and from time to time they co-operated. There is the group who tried to find the best method to compact or to densify, and a group who stated what density was needed. I must say that for the materials used now, gravels and sand (as Jan Willem Boehmer showed), we did come above critical density in our case. So for all cases with the existing equipment, being unable to do more than 4 mm of amplitude we arrived at sand-gravel mixtures with a density above the critical density level. This is apart from all other questions whether we did not destroy the structure or the fabric. One of the first questions was: "Wouldn't it be better to have equipment which could apply an amplitude of 6 mm or more?". We don't know! We only know that if we do tests at 4 mm of amplitude the line relating settlement and amplitude still decreases, so we still got increasing

density, therefore it may well be true that it's better to have larger amplitudes. We didn't need them because the density we arrived at was sufficient for the prescribed critical densities. There was another thing said by Mr. Davis. He said: "Well we know for sure that we arrive at 90% of relative density". Well, then everybody in the audience could say "What is relative density?" as was said by John Christian, because the relative density could be higher at an amplitude of 6 mm and we don't know it. So what is meant by Davis is the relative density we could attain by the apparatus which was used by him, and which had a maximum amplitude of 4 mm and a frequency of approximately 30 Hz, and it is the maximum density attained with this apparatus after varying almost all parameters. So there is no absolute measure. We only know that if you "torture" too much of course, you will crush the particles and then it's difficult to say whether you are then still treating the same material. I hope at least to have answered some of your questions now.
Thank you.

Chairman:

Thank you ladies and gentlemen.

Visit to the construction dock at Neeltje Jans.

Session VI
Soil-Structure Interaction

Chairman:
W. Stevelink, Rijkswaterstaat,
The Netherlands

Chairman:

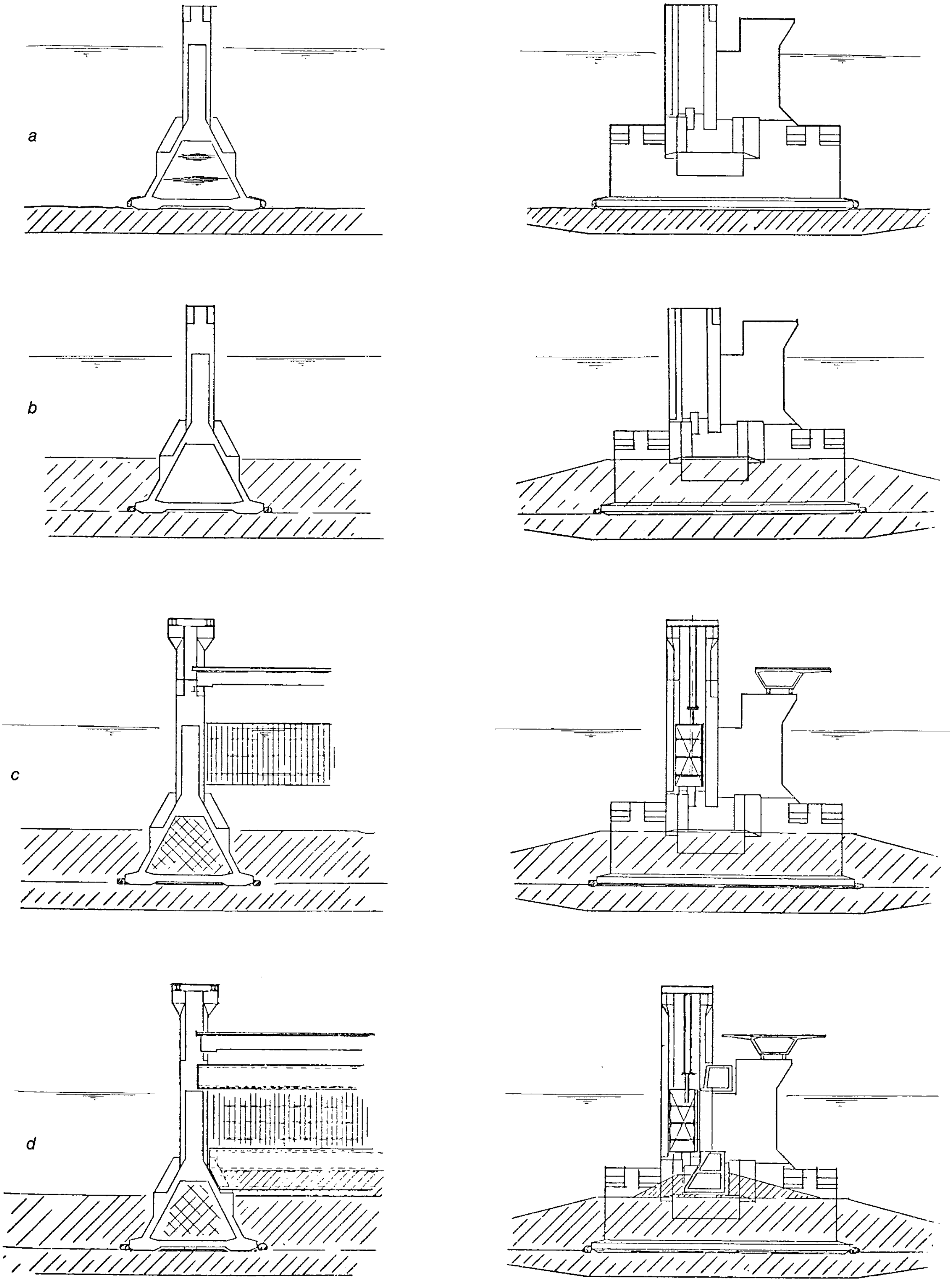
Ladies and gentlemen,

It's a pleasure for me to open this last session of the symposium. In my opinion the topics of this session are very interesting, especially for the designers of the structural elements and for the foundation specialists. As a result of several requests from the attendants the scientific committee has decided to insert a short explanation about the storm surge barrier itself. I hope Mr. Van Geest, project engineer of the design office from the Department Locks and Weirs of Rijkswaterstaat will give you a better idea about the construction. Mr. Van Geest.

THE DESIGN OF THE BARRIER IN RELATION TO THE DEFORMATIONS

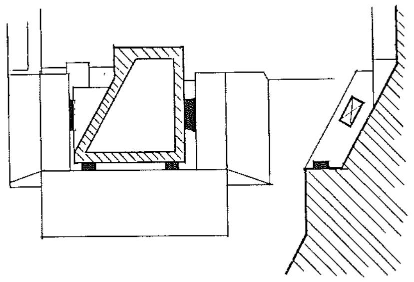
by J. M. van Geest
(inserted lecture)

Mr. Chairman, ladies and gentlemen, my topic is the design of the barrier in relation to the deformations. Last Monday the development of the design of the barrier was shown, moving from surface caissons on a sill to three types of embedded caissons. In this short presentation I will give you some more detailed information about the final design. The barrier is formed by the sill, the underbeam, the gate and the upperbeam. In the Oosterschelde we need a frame to support the movable and the unmovable parts of the pier, i.e. the gates and the concrete beams. In the shallow caisson design this was a monolitic frame. The distance between the frame elements on each side of the opening was fixed in that case.
There are no relative deformations around the gate. In the final design the frame elements do have relative deformations between each other since we have seperated piers. The placing of the piers and storms during the execution phase give deformations. Later on when the barrier is finished and closed and the long expected storm comes, each pier undergoes other additional deformations. We need movable connections between the gate, the beams and the piers. I will try to make this clear by discussing the phases in which the barrier will be built.
In the river the foundation layer is constructed in a dredged trench. In the meantime the piers are built in a drained construction dock. A catamaran with a lifting capacity of 14.000 tons lifts the pier and takes the pier to its location in the Oosterschelde and puts it down on the foundation layer (fig. a). The foundation layer is not quite flat. This causes a maximum rotation of the pier of roughly 0.8 cm per meter in one direction and 0.4 cm per meter in the other direction.
Both the catamaran and the pier, hanging in the ship, are moving. This can cause a maximum translation of 30 cm in two directions. The stability of the piers requires water ballast in the caissons. Initially the pier bears on ribs. Then the sill will be constructed (fig. b).
A part of the weight of the sill is effectively added to the weight of the pier. So after completion of the sill the stability of the pier is very high and the caisson can be emptied. After this the movement of the pier will be so little, even by a storm that the final measurement of the pier can be done. Then the definite lengths of the beams and the gates are determined. They will be different in each opening. Now all further movements are important for the bearing constructions of the beams and the guidance of the gates. The room between the foundation layer and the bottom of the pier is filled up with grout in order to obtain a good soil structure interaction and to increase the moment of inertia. To increase the weight of the pier later on the room in the caisson will be filled up with sand and water (fig. c). In the meantime the gate and the bridge are assembled. In the bridge is a space for the electro mechanical equipment serving the hydraulic lifting jacks of



Construction phases of the Barrier

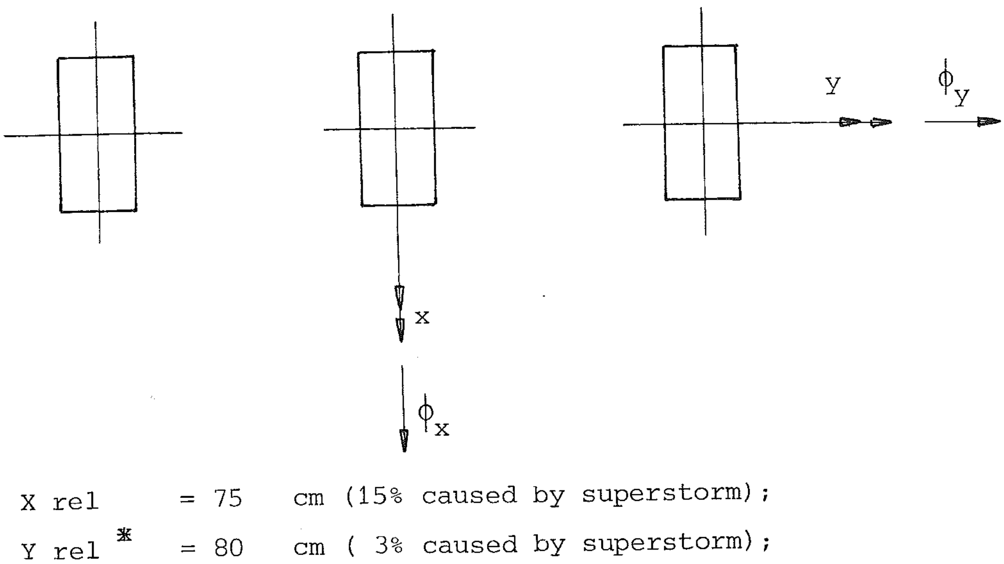
the gates. By assembling the beams the barrier will be finished. (fig. d).
Let me now discuss the deformations for the structural elements. Only the relative deformations between two piers are of interest for the connection details.



The beam bears on rubber blocks. The deviations caused during the execution phase will be corrected by flat jacks. So the rubber blocks and the flat jacks form one element. By pumping up the flat jacks there will be a correction of the deviations. Then the jacks will be pumped up with grout. This also gives a pre-stressing in the vertical blocks. The beam connection is torsion-stiff on one side. The 25 cm thick rubber blocks with a diameter of approximately 2 m allow for deformations in the final phase of the barrier.

The rotation deviations in the execution phase and in the final phase are together important for the gates. The longitudinal rotation along the axes of the gate does not cause problems since the gate is not torsion-stiff in that direction. The transversal rotation is critical for the width of the steel plate in the guidance of the gate. This decides the depth of the rabbet and the width of the upper part of the pier. So the transversal rotations are the most important rotations for the structure. Finally from the most important relative deformations in the table below you see that the predicted deformations are only a small part of the total deformations and I may cautiously conclude that finally the designers succeeded in making a design, rather independent of the accuracy of the geotechnical predictions of the longitudinal deformations. Thank you.

Most important relative deformations.



X rel = 75 cm (15% caused by superstorm);

Y rel * = 80 cm (3% caused by superstorm);

ϕ_x rel = 1,4 cm/m' (4% caused by superstorm);

ϕ_y rel = 0,8 mm/m' (20% caused by superstorm).

* 95% corrected during the execution.

Chairman:

Thank you Mr. Van Geest for your explanation. This next presentation will deal with ground water flow in the sill. This contribution is a co-production of Mr. Barends and Mr. Thabet. Mr. Barends, working in the research division of the Delft Soil Mechanics Laboratory, is a specialist in ground water flow. Mr. Thabet from the Delft Hydraulics Laboratory has been attached to the sill design team of Rijkswaterstaat for the last few years. Mr. Barends.

GROUND WATER FLOW AND DYNAMIC GRADIENTS
by F. B. J. Barends
Vol. 2, Paper VI-1

Chairman:

Thank you very much for your clear lecture, Mr. Barends. You made clear the complexity of all the problems with or for the designers. Thank you very much.
The third contribution by Mr. Nieuwenhuis will deal with the loads acting on the concrete structures. For several years Mr. Nieuwenhuis was the chairman of the study group for soil mechanics for the storm surge barrier, so he will be an eminent man for this part I think. Mr. Nieuwenhuis.

INTERACTION FORCES BETWEEN PIERS AND SILL STRUCTURE
by J. D. Nieuwenhuis
Vol. 2, Paper VI-2

Chairman:

Mr. Nieuwenhuis, in my opinion we got an eminent lecture. Thank you very much and through you also the co-authors, Dr. Molenkamp from the Delft Soil Mechanics Laboratory and Mr. Van Geest from Rijkswaterstaat.
The fourth contribution in this session is in my opinion an excellent example of cooperation between the contractors, the design department and the research laboratories. The authors of this contribution are namely Mr. Hudig and Mr. De Haan, both engineers from the Contracting Consortium Dosbouw, Mr. Van Rossen, the engineer in the field from the Department Locks and Weirs of Rijkswaterstaat and Mr. Stam, project adviser from the Delft Soil Mechanics Laboratory. Mr. Hudig, may I invite you to take the floor.

FULL SCALE DIRECT SHEAR AND PENETRATION TESTS FOR THE DESIGN OF THE INTERMEDIARY LAYER AT THE INTERFACE BETWEEN CONCRETE STRUCTURE AND FOUNDATION LAYER
by E. P. Hudig
Vol. 2, Paper VI-3

Chairman:

Mr. Hudig, we are very glad that you returned to Holland this week from Nigeria because now we have got an excellent contribution from you. You made the connection between the design of the structure, the execution and these full scale tests clear to us. I think with this lecture you have answered many questions on these subjects. Thank you very much, and also all the other authors.
The last subject in this session will be the sill design. Mr. d'Angremond and Mr. Van der Does de Bye will give a lecture on this item. Mr. d'Angremond from Dosbouw was a very important and inventive member of the sill design team. Mr. Van der Does de Bye is an engineer of the hydraulic research division of Rijkswaterstaat. Mr. d'Angremond.

Chairman:

Mr. d'Angremond and Mr. Van der Does de Bye, I would like to thank both of you for your very interesting contribution about this essential part of the structure. Ladies and gentlemen, we have only 15 minutes for the discussion. May I ask you to keep your questions very short. May I ask the speakers and co-authors to come here onto the floor?
Who has the first question?

R.S. Wright:

I'd like to ask Mr. Hudig if it is planned to grout the base of the piers from the interior chamber of the pier, or from the top of the pier, or from some other location.

E.P. Hudig:

I hear from the man on my left that it is intended to do it from inside the chamber. This of course gives a difficulty. It means that you have to be able to empty the internal chamber after the sill has been brought on and this gives extra requirements to the concrete structure. Thank you.

J.B. Sellmeijer:

I want to put a question to Mr. Barends. He showed a storage equation containing terms for change of the permeability. Those terms consist of a quadratic term and a linear term. Most of the time you disregard the quadratic term to make it suitable for an analitical approach. The linear term only has a differentiation with respect to z. My question is: Do I have to conclude that modification of the permeability in x and y direction will not influence the solution or is negligence not allowed?

F.B.J. Barends:

I succeeded in solving this storage equation which is not linear, because there is a dot product of gradients for irrotational cases, by making it linear with certain transformations and I don't know exactly what the effect is of ommitting terms you mentioned, like the linear gradient in z. It has some effect in the depth but mainly it shows how far the influence extends in the horizontal plane.

Chairman:

Ladies and gentlemen, thank you for your discussion. I ask your attention for the closing ceremony.

Closing Ceremony

Chairman:
W. Bokhoven, Director of the Delft Soil Mechanics Laboratory,
The Netherlands

Chairman:

Ladies and gentlemen, the symposium is approaching its end. Before closing we have two speakers. In the first place Mr. Heijnen of LGM who will try to give a general review of the contributions and the discussions we have had. And afterwards Prof. Van Weele will take the floor for his closing address. May I invite Mr. Heijnen to take the floor?

TECHNICAL CLOSING

by W. J. Heijnen,
Chairman of the Scientific Committee

Mr. Chairman, ladies and gentlemen. We arrived at the end of this symposium. I hope you all survived. It is my task now to give you in a very short time an impression of the technical side of this event. I will not try to review all contributions. I think you share my opinion that the writers have reached a good scientific level. During this symposium some general questions regarding the implementation of this scientific work in the design procedure of the storm surge barrier have been heard. I will use this opportunity to elaborate a little bit on this side of the problems.
One year ago the first rough version of the programme of this symposium was made. We had to face the difficulty how to show the function of the applied advanced soil mechanical techniques in the total design procedure of the foundation of the barrier, giving at the same time a clear and sufficiently extensive presentation of the various methods itself. It may be possible that the accent in this symposium was laid too much on this last aim; the presentation of the methods.
I think that we succeeded in presenting you the applied, advanced and simplified methods. We also supplied you with extensive information - you have heard about this in this afternoon session - which was obtained from many unique tests. How results of these methods, tests etc. were implied in the design procedure did probably not become very clear to you. But I can assure you that all results, even the contradictionary ones, were used for the preparation of the decisions to be taken by the designers. Of course it is not possible for me to give you now a clear and complete review of how the various investigations fitted in the leading thread of the design proces during the design period of 4 years. When Rijkswaterstaat was asked by the government to investigate whether a storm surge barrier with gates could be built in the mouth of the Oosterschelde within a given frame work of money and time, it was clear from the beginning that, apart from the orthodox methods, advanced techniques had to be applied for a thorough and reliable analysis of expected foundation behaviour. The estimation of the behaviour of loose sand layers, varying considerally in quality, when exposed to the effect of cyclic loadings on the structure, cannot be tackled by conventional investigation and calculation methods only. This lesson we already learned from offshore experience.
The theme of this symposium, the foundation aspects of coastal structures, was chosen because of the strong relation with this kind of problems. Which were the critical aspects in the analysis of the foundation for the storm surge barrier and how do the various investigation methods fit in this picture.
At this moment I can only touch the surface of this subject. I hope however that my remarks complete your views on this symposium.
The main questions were:

1. Is the construction sufficient safe on the short and long term, sufficient safe with respect to collapse? Important in this respect is pore pressure generation and the coupling of this fenomenon to the stability computations.
2. Will the deformations during the lifetime of the construction stay within the tolerances set by the designers? A governing aspect is the effect of repeated

loading on the deformations. Expressed in a more crude way: Do we have to expect the structure to move slowly in the direction of Zierikzee under the influence of the repeated horizontal wave loads? Differential deformations due to variations in quality of the soil and load differences had also to be taken into account.

3. What are the requirements for the prevention of migration of soil particles from the foundation soil under ambient dynamic pore pressure gradients. This question is related to the design of filters in the foundation of the pier.
4. The interaction between foundation bed and sill at one side and components of the concrete piers and gate beams on the other side. Knowledge about interaction forces was needed for the strength conputation of the concrete structures. Dominant aspects are the torsion and uneven deformations of the foundation and forces from the compacted or uncompacted sill on the edges of the base slab and the sides of the caisson and the lower gate beam.
5. A probabilistic approach was required in order to tune probabilities of collapse and failure of all components of the construction.
6. The stability of the piers during all phases of the execution.

It is obvious that a solution for this type of questions requires much more research than usually is the case for land structures. Moreover computation results can hardly be checked with field experience.
In order to have the necessary flexibility in the design procedure the designers required data in a parametric form. The influence of various parameters had to be studied thoroughly. The variability and the uncertainty of the soil conditions was of dominant importance. In consequence of this I think the choise of an approach on various levels of knowledge is fully justified. The results of simplified methods - indispensible for a parametric approach - were always compared with the results of advanced computations. Large and small scale model tests were performed for checking the results of the theoretical methods.
Despite uncertainties with respect to scale factors and model imperfections I estimate the value of these model tests very high. They were and still are the only links we had with reality during the design procedure.
I am convinced that all methods used for the study of expected foundation behaviour for the Oosterschelde barrier have had their value. The model test however gave us confidence. Experts as well as designers have always been aware of the shortcomings of calculations and analogue models. Even the notion we have of the soil conditions may deviate somewhat from reality. It has all been taken into account in the design of the foundation of the storm surge barrier. Even this most pessimistic guess of foundation behaviour is amply within the tolerances set by the designers.
The discussions in the various sessions of this symposium have been of great value. In spite of differences of opinion regarding the applicability and confidence of the various methods we all had the same goal; to improve our knowledge of soil behaviour and the methods for the prediction of foundation behaviour.
There is still a lot of work left.
I thank all participants and specially those who have taken part in the discussions for bringing this symposium up to such a high level.
On behalf of the Scientific Committee I want to express my gratitude to all authors and co-authors. I know how difficult the task was they had. Their contribution was the base for this successful symposium.
Mr. Chairman, ladies and gentlemen. We could not solve all problems here. However I hope that this symposium has contributed to the mutual understanding between the research workers of our profession in the participating countries.
I thank you very much for your presence.

Chairman:

Thank you Mr. Heijnen. I give the last word to Prof. Van Weele, President of the Netherland Society for Soil Mechanics and Foundation Engineering, which is part of the Royal Institution of Engineers in the Netherlands.

FORMAL CLOSING

by A. F. van Weele,
President of the Netherlands Society for Soil Mechanics and Foundation Engineering

Well, Mr. Chairman, ladies and gentlemen. Our symposium is coming to a close now, and it is my honour and it's also a pleasure for me to act as your last speaker and I will do that very briefly.

First of all I want to make some remarks about my personal experience during the days I was here in this room. I think that the conference reflected very well the problems of soil mechanics and foundation engineering, because it is rather easy to have contributions on the theoretical aspects of foundations in idealized soils. The main point of our symposium here, was that we were actually discussing a real foundation in a real situation with a soil which does not reflect any theoretical back ground, it's much more complicated than it seems. I think this was also reflected very well during our symposium. It is sometimes easy to have ideal situations and to solve problems, but it is very difficult to find the soil which is in accordance with that. I am sure that it would also be of great interest to us all if the Rijkswaterstaat and the Delft Soil Mechanics Laboratory would invite us once again, after 1985, when the structure has been in use and when we'll know something about its behaviour during and after a very big storm. It is very interesting to know now more or less what the predictions are, or what the expectations are, but it is even more interesting to know how the structure will behave, and I would like very much to have with the same people the same discussions and then to see what has been the result.

Our society asked approximately one year ago the Rijkswaterstaat if they would be willing to hold a special meeting for members of our society, because we thought that with so much experience and effort put into this subject it would be very important for members of our society to learn about the results. And I must say that this idea was immediately taken over by Rijkswaterstaat and the Deltadienst. They even came up with a counter proposal, not only to present it to the Dutch audience but also to take the trouble of not speaking in our own language but in English and very much translated Dutch has been presented these days and that extra effort was made in order to have the possibility to convey the findings to you, coming as you do from many parts of the world.

I am very grateful to you that we got this opportunity, and we are very grateful that you have given us such a very clear insight. I have the feeling that nothing was hidden and even when the questions were very nasty you tried to give an honest and a good answer, which may not be satisfactory to all of us, but it is the real answer and we have to accept that. A very important task, especially for the last speaker is to express our sincere thanks to all those who have been very active in the organization of this symposium. They have done that entirely in addition to their normal work, because that was one of the main conditons of Mr. Engel, who is in charge of the Deltadienst. He said with the work and the work load we have. I can hardly ask my people to make this extra effort, so we will have to see when we have some time to organize this.

We have seen now and we know now how much work has been carried out by the people in addition to their normal work and we are very grateful to them for the trouble they took not only during these 4 days but especially during the six months previously. We know how much work it was and I think we all appreciate very much the effort and trouble they took to explain to us their studies and their findings. First of all the Organizing Committee. They did a wonderful job, they organized it even in such a way that their programme was not entirely correct. Maybe that is because they organized as consultants, and as a consultant you must always be on the safe side, and so they advised us always to take a rain coat with us. Yesterday I don't think we needed that rain coat, but as a consultant it was good advice and that you always need. So the weather yesterday was fine, and maybe the atmosphere in this room was not always that fine, but it was very pleasant to be here. I also would like to thank the Scientific Committee for the work they did in the timely preparation and the publishing of the many papers, and we are looking forward to volume 3. Of course we must also thank the technicians here in the hall and the various officers who took care of the organization here in this building, and also yesterday during the excursions. I must make an exception for two people and call them by name, and that is in the first place for Mr. Reginald de Vlugt of the Delft Hydraulic Laboratory. He did a wonderful job by his expert experience, and he guided, as the man who knows and the man who has experience. Well we know from soil mechanics people how important experience is, so we appreciate that very much.

The second exception I make is for Mr Harm Houweling who was actually the managing director of this conference. He devoted for at least six months all the time he had for the preparation of this symposium, and he was running after everybody to get things done. He did a wonderful job, although he had little experience, he managed things extremely well.

Then of course the authors, the chairman, you, the participants and also the ladies, many many thanks for your contribution and for your interest shown in our country and especially in the project of the Deltadienst.

I personally would like to thank also the Deltadienst itself and the Delft Soil Mechanics Laboratory. They have made a tremendous contribution, thank you very much.

Thank you.

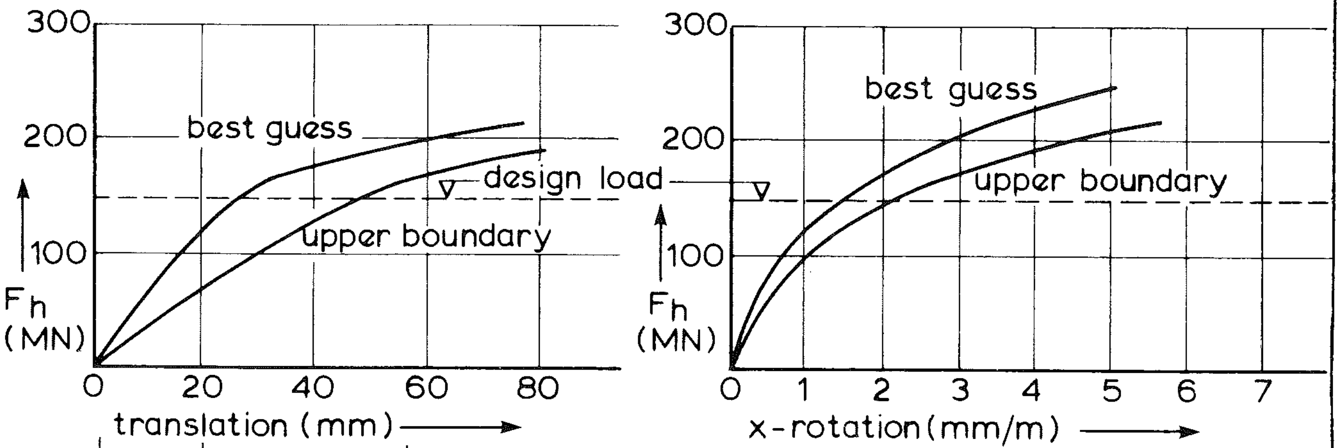
Chairman:

Thank you Prof. Van Weele. Ladies and gentlemen the last words have been said now. I am closing the symposium. I thank you very much for coming. I hope we will see you back once again and I wish you all a good return home. Thank you.

Corrigenda Volume 1 & 2

paper	page	line	correction	
contents		V. 1b.	F.J. de Mulder, F.F.E. van Rummelen	→ E.F.J. de Mulder, F.F.F.E. v. Rummelen
II.1.	1	18	thorugh	through
	1	28	mathermatical	→ mathematical
	2	43	sompared	→ compared
	2	48	has s strength	→ has a strength
	6	15 - 16	to be published	→ Proceedings of ASCE, Journal of the Geotechnical Engineering Division, Vol. 105, NO. GT 6, June.
III.1.	16	table 2 dimension γs	MN/m^3	→ kN/m^3
	18	30	$\gamma = 0{,}11 \log pf + 0{,}55$	→ $\gamma = - 0.11 \log pf + 0{,}55$
III.3.	4	23	$f((v + \omega b - v_r)/u_v) \mathrm{tg}\, \Theta_v$	$f((v + \omega b - v_r)/u_h) \mathrm{tg}\, \Theta_h$
	15	6	$\frac{1}{f \;\; \mathrm{tg}\, \delta_h}$	$\frac{1}{f \;\; \mathrm{tg}\, \Theta_h}$
		10	$f \;\; \mathrm{tg}\, \delta_h$	$f \;\; \mathrm{tg}\, \Theta_h$
		13	$\frac{1 + \sin\phi\, (\cos\, (\chi + \psi)}{1 - \sin\phi \cos\, (2\,\chi)}$	$\frac{1 + \sin\phi \cos\, (2\,\chi + 2\,\psi)}{1 - \sin\phi \cos\, (2\,\chi)}$
IV.2.	3	fig. 2.1.	$q = \frac{\sigma_1 + \sigma_3}{2}$	→ $q = \frac{\sigma_1 - \sigma_3}{2}$
		19	$\varepsilon_{Am}\ \varepsilon_{Bm}$	→ $\varepsilon_{Ap}\ \varepsilon_{Bp}$
		form. (6)	n_σ	n_δ
	4	1	$K,\ G\ (:)^y$	→ $K,\ G\ (:)\ \sigma^y$
	7	table 2.3. air content	$n_a = 1$	→ $n_a = \sqrt{10}$

paper	page	line	correction	
		21	table 1.1.	→ appendix 1
	11	fig. 3.5.	horizontal axis "swing"	add: 2 * amplitude
	12	fig. 3.7.	change "downwards" and "upwards"	
		fig. 3.8.	swing	→ swing amplitude
	16	fig. 3.18.	replace figure by the next one	



paper	page	line	correction	
VI 2	1	24	prescribed	→ described
		24	stresses	→ stress difference
	2	14	stress-path	→ stress-strain
	3	25	material	→ materials
	9	23	isotrope	→ isotropic

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