Summary of Panel Disscusion
on
Testing and Prediction Methods

Session No. 2, Monday, 16 June 1980

4th International Conference on Expansive Soils

Panel Members

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V. Escario, Spain
D. G. Fredlund, Canada
R. L. Lytton, U.S.A.
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Peter Mitchell, Australia
A. A. B. Williams, South Africa

Panel discussion in Session No. 2 (Panel Members, l to r) Tony Williams, South Africa, Session Chairman; R. L. Lytton, U.S.A.; D. G. Fredlund, Canada; R. G. McKeen, U.S.A.; P. W. Mitchell, Australia; V. Escario, Spain; J. E. Holland, Australia; and L.M. Krazynski, U.S.A., Panel Moderator and Session Co-Chairman.

L. Krazynski

Thank you very much. It just occurs to me that our title perhaps is not totally complete, we talk about testing and prediction methods, clearly in that should be included the sampling--the sampling frequency and the selection of samples. What normally destroys or damages our structures, gentlemen, is differential movement. It is the judgment of how much movement is going to be differential that essentially lies at the heart of our problem. Clearly, tests on one sample don't allow you to make a judgment on a differential behavior, unless you have a great deal of experience with that particular formation. Our next panelist is Professor D. G. Fredlund who has been introduced to you in the morning being the principal author of a paper that was presented then. You will notice from the program that he is also co-author of three other papers presented at this Conference. He is a very widely published author on a variety of subjects in Soil Mechanics and therefore, we look in anticipation to his statement. Professor Fredlund.

D. G. Fredlund

Thank you. I am sure we have all been made well aware once again of the many methods that have been proposed for the prediction of heave. They have been proposed in different parts of the world and have been used in those specific regions. We will also notice that there is very little attempt made to have an objective comparison of these methods. Some of the methods have been used quite extensively in practice. We note that there is getting to be quite a good data bank of experience on the methods, while other methods have been used very little since the time they have been proposed. We also realize that some methods consistently produce predictions which are far too low of what really occurs in the

field while other methods produce predictions which are quite reasonable. My objective, though, is to try to emphasize once again the need for a theoretical context or framework in which the various methods can be visualized. Whether we like it or not, it is important to be able to visualize the different stress paths that are used in the various methods. On the other hand, I do not want to advocate one and only one method for the prediction of heave. This morning I outlined a procedure based on the one-dimensional oedometer test, a procedure we use in Central Canada. There are reasons why we use this procedure in Canada, but I am sure there are equally justifiable reasons for using other procedures in other parts of the world. Let me just outline briefly why we want to use the odometer test in Canada. We find first that most of our soils labs, well all of our soil mechanics labs pretty well have consolidation equipment in them our highway departments and our government agencies, have this equipment. The heaving of light structures is merely one of a number of very important problems that these consultants have to be involved with. There is need to have a procedure and a method that they can use which they can very readily relate to. For years they have made attempts in Canada to try to use the results of free swell consolidation tests and constant volume oedometer tests to predict heave. To say the least, the results have not been too encouraging because their predictions have always been on the low side and this is not the side you want to be on, especially if you are a consultant. The question that we addressed, though, was why are the predictions too low? Because of positive answers we obtained to this question, we have once again revived the use of the oedometer test for the prediction of heave. The answers to that question came about because of two main things. Because of the research into the theoretical framework for the behavior of unsaturated soils. Secondly, because of the research into the effect of sample disturbance on the interpretation of the test data. So once again, I want to emphasize the need for a theoretical framework that will embrace all the methods that will allow us to impose various boundary conditions and see the stress paths that have been followed. I think that I would like to leave you with some questions that should be given consideration in trying to select a method that would be suitable for the prediction of heave. These questions are:

What are the environmental conditions in the area? In other words, will the testing equipment perform satisfactorily in the environmental conditions under consideration? Second, what equipment is available in the soil mechanics laboratories? But I would also add that even if the equipment is not available, there should still be consideration given to purchasing the equipment from a cost-benefit standpoint. And from the theoretical standpoint, point three, what are the empirical aspects or limitations associated with the procedure under consideration for the prediction of heave? And fourthly, what case histories are available to verify a particular procedure? Past experience was one method that must take precedence over other methods until the necessary comparative studies have been performed.

Representative, Soil Exploration Co., St. Paul, Minnesota

This question is directed at Professor Fredlund, I am concerned about proper seating of samples for measuring swell pressure with oedometers or similar equipment. How do you determine what load to use so that you don't get a fair amount of stress release in the voids between the porous stones and sample.

D. G. Fredlund

I think that far more important than being too concerned about the seating and what loads you are going to put on for seating, I would be more concerned about the effect of sample disturbance on the data. For seating, if you use a token load or else the overburden pressure, there is very little difference in the deformation. We used to run some tests with just a token load, then we started running them with the overburden load put on the sample as the point when we immersed them. But, really there is very little difference in the test results if you make the correction for sample disturbance.

Representative, Soil Exploration Co., St. Paul, Minnesota

We're dealing primarily with a very stiff, heavily overconsolidated clay or dessicated shale, the overburden is only one tsf, or so, and we estimate that the preconsolidation pressure is five or six tsf. If you put a seating load of two or three tsf on it, in one particular case we measured swell pressures of about five tsf and increasing the seating load to about seven tsf we measured a swell pressure of about thirteen tsf.

D. G. Fredlund

At the Second International Conference at Texas A & M University, I presented some data on the Bearpaw shale tested in constant volume tests We showed there the correction for sample disturbance was more than a magnitude of two as far as the way it effected your swelling pressure values. So it is still far more significant. Could I just take a minute to make a point of clarification. I think with respect to some of the statements made by Professor John Holland. I would just like to emphasize, we do not use the oedometer test to get any indication of the final boundary conditions. The fact that the sample is not immersed with water in the field and fully saturated, does not really effect what we are trying to do with that test. The purpose of the test is to get the soil moduli, a property of the soil, not a final boundary condition. We can do this, we can move from a suction plane over to a total stress plane because our rebound surface is unique, as long as our deformations are monotonic. As long as we always have volume increase, swelling, the surface is monotonic. It has been proven over and over again in the literature by the work of Bardon, Sides, and Matador; Rhaddskrisna; Mattias; and Morgenstern and myself, we did testing in that respect. Because of that, we are then using the oedometer test to get a soil property. The determination of your final boundary condition is something completely separate. So I think that the statement that was made that the only worthwhile method for predicting heave is a full-scale field test is probably a slight overstatement of the fact.

Activities in Session No. 3, (l to r) V. Escario, Chairman; R. G. McKeen, Co-Chairman; and D. G. Fredlund, Speaker.

Question is from Donald Eskridge, Senior Engineer with Wrights and Jen, St. Louis, Missouri. Concerning my portion of the question, I believe is directed at the soil suction testing equipment and procedure that I outlined in my paper yesterday. The equipment is all commercially available, thermocouple psychrometers and psychrometric microvoltmeter, from several different firms. Our equipment was purchased from a firm in Utah, both the psychrometers and the microvoltmeter. We can set up a full set of the equipment to do the soil suction test that I described for approximately $1,500. The major item of cost in that set of equipment is the microvoltmeter because it has two special features on it, one is a reference junction for measuring temperature and the other a cooling current circuit for condensing moisture on the thermocouple junction. Psychrometers themselves cost about $15 each. They do require some care especially the ceramic tip type. They now have models available where you can remove that ceramic tip, clean the thermocouple junctions and service the psychrometers more efficiently than in the past. I anticipate from my conversations with one of the gentlemen with the manufacturer that we possibly will be looking towards replaceable thermocouple tips in the future. Someone mentioned the wire screen covering, there are other types as well. I know of at least four manufacturers in this country where you can buy different models of thermocouple psychrometers. The equipment is not that delicate, it can stand up pretty well against moderate mistreatment and does not take a highly trained technician to run. With a little bit of careful training any soils laboratory technician can run the test, maybe not with the judgment that would be needed to pick some problems that might develop. Del, did you have any comment relative to your portion of that question?

D. G. Fredlund

The equipment required for the procedure I outlined, of course, is the conventional consolidation equipment and if you don't have it available in the lab, it will cost on the order of approximately $1,500 to purchase that equipment. But if you already have it for doing consolidation tests for settlement analysis, there is no outlay. If you are just getting the swelling pressure and the rebound curve just past the

swelling pressure point, the cost for running a test like that in Canada is anywhere from $35 for that test up to complete consolidation test being to the order of about $80. The short test takes on the order of about five days to get results whereas the complete consolidation test would take at least ten days. Technicians are used to using the consolidation equipment, so I guess you can decide for yourself whether or not it is a delicate piece of equipment. A person would require approximately three tests on a particular site and so you would be running up to a cost of somewhere from $100 to $250 to get the information required for prediction of heave analysis.

R. L. Lytton

I think the question could be broadened somewhat, Don, you were talking about psychrometers. Del was talking about some kind of consolidometer device and I know there are other devices that have been mentioned like the filter paper that Del and Gordon McKeen have used to measure suction. I know Gordon, at his laboratory at the Civil Engineering Research Facility at the University of New Mexico, is able to measure these instability indexes or the soil response to suction change. I would like to hear what Gordon has to say about the filter paper procedure, the COLE measurements done by the Soil Conservation Service and his modifications. I would also like to hear what Del has to say about the filter paper procedure.

R. G. McKeen

We have attempted to establish the same kinds of soil properties, a soil volumetric response to suction change, with the filter paper method. I think we spent $15 for all the filter paper we have used in the last 2-1/2 years. The advantage of filter paper is it is cheap. It is a test that requires a seven-day equilibration period. Following equilibration you have to obtain moisture content and if you are going to make a volume change measurement which would be required to evaluate the moduli, that would perhaps require another couple of days, so I would say a two-week test to obtain these moduli. One of the advantages of the filter paper is that you can run a tremendous number of tests simultaneously because the technician is not involved in monitoring anything. He sets up the sample and then leaves it to equilibrate

D. G. Fredlund

We are still at a very preliminary stage in using the filter paper technique. We have really just undertaken a pilot study where we attempted simply to measure the correct suction values using filter paper. We have two soils in Saskatchewan; namely, the Regina clay and the glacial till that we have tested extensively. We know the relationship between water content, dry density, matrix suction, solute suction and total suction for those soils. We have that very well defined so when we come along with another technique like the filter paper technique, we can see if we get the correct answers. Some of our first tests were not very encouraging, but some of the latter ones have been. Recently we have been working with compacted samples and we would drill about a 3/4 inch diameter hole into the compacted sample and then curl up two pieces of filter paper, one inside the other, and insert them in the hole and leave them for five to seven days to equalize, take them out and measure the water content of the filter paper and go to our calibration curve for the filter paper and the results have been very encouraging. So much so that we have plans to take on a more extensive research program in that area. We have not gotten so far as to look at how we are going to determine the moduli for the soil or what procedure would be used; that is something we still have to look into.

W. G. Holtz

This is not a question, but a discussion which has been submitted by Mr. Carroll Coffee, one of my colleagues from the Bureau of Reclamation. He would like to solicit comments from Drs. Fredlund and Bocking. First he would like to discuss the technique used at the Bureau of Reclamation for soil suction measurement using the axis translation technique and compare these with those discussed by Dr. Fredlund.

C. Coffee

I am Carroll Coffee and I can say from a long-standing study of soil suction that this Conference is most gratifying. I can remember a time when I was working in research on soil suction when it was a very popular subject in about the latter 50's and early 60's that the only way I could talk to some of my colleagues about soil suction was to go into their office and get between them and the door and pull my chair so they couldn't get away from me. So, it is very gratifying to hear as much discussion about soil suction as there has been in this Conference. I have also stated because of my long association with soil mechanics that if I had two requests to make, I would like to live a long and useful life in a thirst for knowledge in the subject of soil stress and then when I go to die, I would like to be planted in a subsurface excavation of CL clay which has about 5 atmospheres, that I would be protected by about 5 atmospheres of soil suction. So you can see the subject is dear to my heart. When Dr. Bocking presented the paper by he and Dr. Fredlund, "Limits of the Axis Translation Technique," the experience that I had in my early study brought up some comments that I would like to make in the most humble and respected manner. On Page 119 of your Proceedings there is a statement made. I believe in the first paragraph, top of Page 119, the author has mistakenly applied Coffee and Gibbs work of 1963 and 1969, not in principal but in technique. We were trying to correctly measure suction pressures which in practice could not be measured on soils which had high suction pressures by the method of translation which Hilf, Bishop, and others had tried. I am sure, from the research that they did, that this was because the soil had a very low permeability to air. Therefore occluded air did hinder the ability to successfully make measurements of suction pressure greater than about -13.8 psi which is 2 psi below atmosphere here in the Denver area. I believe in England, the direct measurement through the translation of the axis method was about a -18 psi which would be comparable to the amount at this elevation. Now in conclusion No. 2, Page 131, the statement for soils containing sufficient occluded air was ...."Use of the axis translation test to measure soil suction is

theoretically correct for soils with totally interconnected pore-air voids." That, I believe, in the test that Hilf, Bishop, and others such as Langfelder and Olson performed is theoretically correct. "For soils containing significant amounts of occluded pore air, the measurement can be erroneous." I disagree with this latter statement. Based on the use of Coffee and Gibbs method, if error exists, the pressure will be underestimated. Using the Hilf, Bishop, or others method the statement can be true. Now, I would like to show some overhead slides. If you will note, this is a diagram of the apparatus that Bishop used in about 1960 before the 1960 Shear Conference in Boulder. Hilf, in his work in the translation of the origin method, used somewhat the same set up and you will remember the diagram that Dr. Bocking showed on the board of the way of measuring suction pressure. What he did was place a fine ceramic material, a fired clay or commercially available fine ceramic material, that should have a bubbling pressure or the ability to form a concave menisci across the surface equal to that in the equilibrated soil enclosed in the membrane. Then Hilf and Bishop would place the soil specimen on the plate and as the soil suction was formed in the end plate it would put tension in the measuring system. As the tension in the soil water began to go close to a minus one atmosphere they would inject air in at the top of the soil specimen and as you injected the air, the soil suction would continue at the bottom. As an example, stating that u_c or the suction or capillary pressure is equal to minus the quantity u_a minus u_w. If you would put 60 psi pressure at the upper part of the soil specimen and there was only say 20 psi available at the bottom to offset the origin and you would say there was 60 in there, then you would overestimate the soil suction that you would have at the bottom, because you didn't know exactly how much pressure was effective on the measuring system. There was another problem involved in this. I have tried many of these tests and at that time no one knew exactly what the soil suction was at a given moisture density relationship. So, while you were getting the soil specimen set up, the water column would cavitate; water would be taken from the measuring system up into the soil and you

would get a volume change as you applied the pressure on the outside to hold the specimen from consolidating. To hold the specimen from expanding, you would apply the internal pressure, then you would have to apply a pressure to the outside of the membrane to hold the specimen at a given volume. There was volume change taking place and therefore the uncertainty was that you did not know the pressure of the air at the measuring media between the end plate and the soil. Therefore the sensing device would measure what pressure was left to be measured in pore pressure measuring device. Now then I would like to show the next transparency. Now basically, if I remember correctly Olson and Langfelder did something similar, only they used a specimen about one inch thick which covered the complete end plate. Therefore, if you did have a soil that had a high suction pressure in a fine clay and the moisture content was above 85% saturated, then you could still have difficulty translating the origin through that. If the soil was less than about 85% saturated, where air was interconnected, then the theoretical reasoning is absolutely correct. Now then I want to look at some diagrams showing characteristics of ceramic end plates and testing for capillary pressure. In the upper diagram A, if you had a saturated ceramic end plate that had the possibility of supporting concave menisci without penetrating that end plate of say 60 psi or about 5 atmospheres then you could obtain a measurement of the soil suction of about that amount, that is what the bubbling pressure means. Now then if you took an end plate with a sensing device on it, in a chamber that had been completely saturated, and you apply an air pressure in that chamber through a saturated sponge so that you didn't dry the surface of the measuring media out any, then if you apply

40 psi pressure in that chamber, you would read instantly 40 psi on your no-flow cell if you were completely de-aired and your water column behind the saturated stone had no expansion in it. Another characteristic of measuring systems is this, if you would take that same ceramic end plate and allow it to evaporate across the surface to form a concave minisci completely across the surface equal to a minus one atmosphere, the water column in the sensing device would cavitate.

If you would allow it to continue to evaporate it would pull the water out of the measuring system and air would penetrate the measuring system and would decay the measuring system. Now then, if you take the same end plate and you would open the end plate to drainage into a sealed triaxial chamber like you would use in a triaxial shear test and apply 40 psi pressure to it and allow drainage rather quickly you would read 0 on your sensing device, because you would have drained concave menisci and the air would support the applied pressure. Now continuing this discussion, if you would take the same end plate that I am talking about and take the rubber membrane completely off the soil specimen and expose the end plate, then place the soil on the end plate, and keep the end plate saturated (zero pressure at the sensing device) until you put the soil specimen on it. Immediately the soil, because of the zero pressure in the end plate and say a suction pressure of 4 atmospheres in the soil, will start forming concave menisci across the surface of that end plate. At the time you had allowed this to proceed to about 3/4 of an atmosphere; if you would come in and apply an air pressure completely around the soil specimen and if you could do this quick enough then you would read zero on your sensing device because you have completely translated the origin if you have no expansion in the system. At some intermediate time when it had progressed to where you had a concave menisci in the soil, of 10 psi, then you would be reading -10 on the bottom and you would have progressed along the line until you had water pressure in the end plate of 30. Now at an equilibrated time the test procedure states (Gibbs and Coffee, and Knodel and Coffee) that at the time you had reached equilibrium in this test you would be reading a -7 left to read at the sensing device. You would have applied 40 psi in the chamber around the soil specimen which is the value less than the suction pressure of the soil and you have only taken the amount of water that it takes to form the menisci across the surface of the specimen. You are reading a -7, a +40, you would have the equivalent of a -47. Now this is a nondestructive test. This test can be performed on undisturbed samples or remolded samples in the case of triaxial shear, you can use the same specimen for your triaxial shear and know where the pore water pressure began and as you apply compression force, then it becomes a measurable range, you can measure it. Now I would like to show the next slide. Here is a test performed that had a suction pressure of about 40 psi and it was performed in the manner in which we let it go down 5 then apply 5 in the chamber and finally came down to -45 which meant we were reading about -7 or -8 in a measuring system at 40 applied in the chamber. I suggest that this is a systematic significant number that can be used in evaluation of the soil suction in a soil. There is a test method in the Earth Manual of the Bureau of Reclamation whereby we perform this test, we get the initial suction pressure, then trace the suction pressure from initial condition through a volume change basis to zero or complete saturation. Thank you.

D. G. Fredlund

Let me say first that in the reference on Page 119, we were simply referencing some of those who had used the axis translation technique to measure suction. We go on in the paper to explain that we do not use the procedure as outlined by Coffee and Gibbs; we used a procedure, that I believe was used by Olson and Langfelder. It is a procedure used at Berkeley where the sample covered the whole baseplate. Now that may be an inferior way of doing it but that is the procedure that we have used for years. It is a procedure used other places and that is the procedure we chose to simulate with our theory. I suppose if we had tried to simulate the theory outlined here, just a few minutes ago, the first thing we would realize is that we would have a problem because the problem becomes three-dimensional and is much more difficult to analyze first of all. So, we are using samples that are on the order of 1/2 to 3/4 inches in thickness. They are not the large lengths, long samples as used by Bishop. But, if we do have occluded air and if we go to the extreme where we may have 95% saturation, and have essentially air/water menisci around the entire sample, then we make the assumption that there will be no diffusion of air through the water phase. This is the case that we have simulated and under these conditions, we feel that then there will be compressibility of the soil structure because your applied air-load acts on the entire sample and you will have a sample with a compressible pore fluid and it will start

to compress under this configuration. We have not thought through the theory related to the procedure used by Coffee and Gibbs and that is what I think we should do. I think that is all I have to say about it, thanks.