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Expansive Soils - General Report

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1. Introduction

In the early planning stages for this Conference there was some debate on whether the more wide-ranging term "unsaturated soils" should be used for this technical session. After abstracts had been submitted it was decided to stay with the more usual term expansive soils, even though in the international scene the more general term is now being used. This was most significantly demonstrated last year when the 1st International Conference on Unsaturated Soils, UNSAT '95, was held in Paris under the auspices of the International Society for Soil Mechanics and Foundation Engineering. The latest ISSMFE Newsletter, reporting on that Conference, noted that it was symbolic of a strongly emerging position of the geotechnical community that there is a need to embrace a general framework of unsaturated soil mechanics and to recognise the existence of unifying theories that can bring together researchers and practitioners who have traditionally focussed on specialty topics of unsaturated soil mechanics such as expansive and collapsible soils, arid soils, and compacted soils.

At UNSAT '95 a number of keynote lectures and state of the art reports set the stage for a wide range of unsaturated soil topics including geomorphology, laboratory and in situ testing methods, foundation and mitigation design, pavement design, constitutive model development and unsaturated flow. Many papers were of excellent standard but two, the keynote address by Professor D.G. Fredlund on "The Scope of Unsaturated Soil Mechanics: An Overview" and a review paper on "Suction Measurement: a Review of Current Theory and Practices" by A.M. Ridley and W.K. Wray are particularly relevant for researchers and practitioners in Australia. As some of the points they raise have been addressed by some of the authors to the papers to this session I would like to take a little time in going over them.

Professor Fredlund emphasised the point that the theory formulations developed for the behaviour of unsaturated soils can be shown to include the behaviour of saturated soils as a special case, leading to a generalisation of the mechanics for soils. Leading from this point his paper concluded that the soil-water characteristic curve can be shown to be a key soil function which can be used to approximate the behaviour of unsaturated soils. He showed relationships between the soil-water characteristic curve and the coefficient of permeability, shear strength and the volume change characteristic.

The paper by Ridley and Wray is extensive in its discussion of soil suction measurement in the laboratory and field. The session at which it was presented was most valuable because Professor Wray, in reviewing the papers for the UNSAT '95 Conference had found that a great many symbols, units and presentation styles had been used for total suction, matrix suction and osmotic suction with some authors often appearing to confuse the terms or not delineating which suction was being measured or used in an analysis. We have something of the same problem with papers to this session. In Paris, in an attempt to standardise the terminology the authors introduced a debate on the subject. After a fairly lively discussion on the merits of the various symbols the following nomenclature was recommended and will I think, be used for future papers on this topic.

total suction	ψ
matrix suction	(u_a-u_w)
osmotic suction	π

In the discussion on the units of suction measurement, pF, cm H_2O, psi, kPa, bars and atmospheres had all been used. Again discussion ensued on the various merits of these symbols but, knowing that the metric SI unit of pressure is the Pascal (Pa) the overwhelming choice was for the use of Pascal, (kPa/MPa).

In future authors of papers should recognise these decisions and it is hoped that in notes to authors this nomenclature and these units will be specified as being necessary for paper acceptance. UNSAT '95 produced papers on many topics. There is much excellent work being carried out around the world and the number of papers seems to be increasing almost exponentially.

In this session we have 11 papers almost all of which have a practical bent. It has been notable over the years during the international conferences on expansive soils which preceded UNSAT '95 that contributions from Australia often seemed to be more concerned with the practical application of theories to the design of pavements, footings and other structures on expansive soils. I think that we are respected in the international community for this aspect but we must of course keep up to date on what is going on in other places.

As the papers for this session are varied for review purposes I have found it convenient to group them into the sequence that a practitioner would adopt if needing to design or rehabilitate footings on a reactive clay soil site in an unknown area. These steps would be

1. an assessment of the site soil characteristics which would start with a study of the regional information;
2. sampling and laboratory testing to determine the soil characteristics and variability;
3. either design involving a deterministic approach with numerical modelling to predict the soil movements, soil structure interaction and hence footing sizes; or
4. if trying to rehabilitate an existing structure then consideration of the techniques available.

2. SESSION PAPERS

2.1 Regional Studies

The regional study of the Newcastle Hunter Valley region is now in its 4th year. The study is being undertaken by the geotechnical research group of the Department of Civil Engineering and Surveying at the University of Newcastle and commenced with detailed investigation and monitoring of a site in Maryland near the City of Newcastle. The study is now in its second stage, which has involved the establishment of a reactive soil monitoring network throughout the Newcastle/Hunter Valley region. The paper **"The Network of Field Sites for Reactive Soil Monitoring in the Newcastle/Hunter Valley" by Delaney, Allman and Sloane**, presents the results of the second stage to date.

These studies in the Newcastle/Hunter Valley region have great potential to give a wealth of information about the region, information which must lead to a greater understanding of many aspects of soil distribution, behaviour and classification and identification procedures. This will be of benefit to the practising engineer and the community as it is obtained and published. The authors cite the need for a closer examination of the soils of the region due to the trend to masonry construction combined with large population growth in the area. Information is being obtained from 19 sites in the region which were specifically targeted to cover a range of the regional geological sequence, residual soil profiles and various rock types, alluvial and colluvial deposits, broad geographical coverage and include a range of terrain, vegetation and site development conditions.

Details are given of the instrumentation and monitoring of the site which basically consists of surface and subsurface movement points together with a neutron probe access tube to allow soil volumetric moisture determination at specific depths. The authors present the results in a number of plots and tables. Table 3 lists soil index properties but as these are from a large number of sites I wonder at the relevance of the mean and standard deviation. The usual correlations between plasticity index and liquid limit, and linear shrinkage and liquid limit are shown but, as is often the case, poor correlations were found with the shrink-swell index and the various basic index properties.

Most of the results are summarised in Table 4 which gives soil profile characteristics and predicted and actual soil movements. When these movements are compared the authors find a range from 7% to 187%. I think that they are being a little hard on the data as the 187% compares 5.6 to 3 mm, both of which I would have classed as 5 mm as, given the uncertainties in the Iss values, we can only predict movements to the nearest 5 or even 10 mm.

Close examination of the table indicates that to date the maximum movements appear to have occurred in what appears to be the more permeable clays. Could it be that the more reactive and less permeable clays have not undergone large moisture changes to date? This points to one additional column which I would have found most valuable, that of the soil suction changes during the period. The variability in the shrink-swell index values, the authors consider was not generally discernible on a visual tactile basis and this has certainly been noticed in other studies. However without some more specific site and depth information for the data it is hard to comment on the wide range in Iss seen in the table.

2.2 Laboratory Testing

The range of shrink-swell results from the region is discussed by **Fityus and Welbourne in "Trends in Shrink-Swell Test Results in the Newcastle Region"**. These authors examine a large database of 902 individual test results obtained from two local consultants and the University of Newcastle Civil Engineering Department. The data was sampled on a suburb by suburb basis with those suburbs with the greater number of results sampled in preference to those with a few results. Suburbs were also selected to give representations of geological formations which commonly weather to produce reactive clays as well as alluvial clay areas. The authors note that efforts were made to select suburbs without bias with those suburbs selected, considered to fairly represent both the higher and lower typical soil reactivities in the region. They also mention that the earliest test records used in the study were adjusted to obtain an instability index in accordance with current procedures.

In discussing the trends in soil reactivity the authors present a histogram of all 902 results and comment that it is apparent that the clay soils in the greater Newcastle region span a wide range from very low to very high Iss values. They draw the conclusion that it is apparent that there is a similar likelihood of encountering Iss values anywhere from 1 to 5 % for any randomly selected site. I think that further information needs to be presented before that conclusion can be drawn. If the results from the selected suburbs have been plotted separately and almost identical histograms obtained then it would follow. As shown, the histogram only shows the frequency of occurrence for the Iss values obtained from the tests and shows that the reactive clay soils of the region can vary from stable to very high reactivity.

The authors also look at the depth of testing and the variation of Iss with depth. They give a frequency plot for the average depth of testing (Figure 5) and the distribution of average Iss vs average depth of sampling. The conclusion they draw is that there is only a slight decreasing trend in Iss with increasing depth. I think that this needs to be qualified as Figure 5 indicates that possibly 5 or less samples were taken below 1.75 metres for each sampling interval. It is apparent that at many of the sites, weathered bedrock was present below about 1.5 metres and thus there was no need for sampling of this material. Because a few samples from greater depths have been tested and show Iss values of between 2 and 3% it cannot be inferred that in areas where weathered bedrock is present that similar Iss values might be obtained at depth.

When comparing the results of alluvial and residual soils the authors do make the point that the data as presented is from a much greater number of residual soil samples(847) than alluvial samples(55). This should be kept in mind when looking at the distribution of Iss values for soils from the different origins as presented in Figure 8.

Empirical correlations are always useful to the practitioner and with such a large database the authors considered it worthwhile to test measured Iss values against the saturated moisture content after the swell test had been carried out. I agree with the authors that for the plot as presented there is very little correlation. However, I wonder if the authors tested for any trends from the individual suburbs or geological sequence.

The general conclusion in the paper appears to be that there is little point in attempting a local mapping of reactivity as changes between individual sites can vary so greatly. This does not appear to fit with the authors initial suburb selection criteria which gave the impression that some suburbs were known to consist of soils mainly with higher or lower soil reactivities. Perhaps the authors would like to comment on this.

The paper by **McManus and DeMarco, "Variability of Expansive Nature of Clay on a Site"**, explores the problems of site soil variability and examines another test which might help to determine its range. They examine three sites in each case carrying out methylene blue adsorption tests on samples to obtain the apparent surface area of the soil. The conclusion they draw from the results, that the average apparent surface area for each borehole was roughly related to the movement of the structure, needs I think to be qualified. For volume change you need not only a reactive clay but a moisture change. The test certainly appears to have indicated the extent of possible reactivity of the clays in that area but there is no information on soil suction change. Without that information it is difficult to draw any conclusions regarding the movements of the footings. It is quite possible that in those areas where little or no movement has occurred that little or no moisture change has occurred.

The authors also conclude that from a practical point of view it was deemed sufficient that the MBA test was a consistent indicator of the relative reactivities of the soils on various parts of the site. This conclusion appears to have been drawn without any measurement of soil reactivity or soil volume change potential. The reactivity appears to have been based totally on the relative movements of the structures without any knowledge of the moisture changes which have occurred beneath those structures. The test appears to show great potential and can be easily carried out. It would seem that seem some further research is required, particularly some soil suction data, before the results and conclusions can be firmly stated.

One of the authors of the regional Newcastle study, S. G. Fityus, examined, as a final year student at the University of Newcastle some of the problems inherent in carrying out and assessing the results from the shrink-swell test for the determination of Iss as described in AS 1289.7.1.1-1992, **"The Effect of Initial Moisture Content and Remoulding on the Shrink-Swell Index Iss"**. Although the main aim of the study as stated in the paper was the experimental assessment of the value of the factor alpha (normally taken as 2) used to divide the swell-strain component, the results of the study are also useful as they address the problems of sample preparation and moisture conditioning and the problems of soil loss in the conventional consolidation apparatus if used for a swell test. Moisture conditioning of CH clays, using compacted, remoulded material is always difficult and really shows how much the higher measured field permeability must be influenced by soil structure, discontinuities and infilled shrinkage cracks etc. In the laboratory specimens, none of these features are present and it can take months to prepare a wetted sample in comparison to at most a few weeks in the field. The adaption of the odometer ring with an extension to stop soil loss from samples undergoing large swell strains is a simple way of overcoming a perennial problem. This writer used a similar ring many years ago when preparing samples of normally-consolidated clay from a ring filled with clay at the liquid limit.

The results from the tests indicate that the value of 2 given in the Standard is about right, given all the other factors which can influence the final result. It is interesting to note from the data that the slopes of the shrink and swell strains presented in Figure 3 are about the same. This would indicate that for these samples at least the index could be as reliably determined using shrinkage strain only, as described in AS 1287.7.1.3.

Finally the author presents some interesting data on the effect of remoulding of the clay on the Iss value. If the results as indicated from Maryland clay are an indication for other clays of high reactivity, then the problems of testing dry or disturbed samples can be easily overcome by some laboratory preparation. Certainly an avenue for further research on this topic, as the author suggests.

Additional basic research on the measurement and apparatus used for the determination of the moisture characteristic curve for an expansive clay has been presented in the paper "**Soil-Water Characteristic Curves from Drying and Wetting of a Residual Soil**" by **Leong and Rahardjo** of Nanyang Technological University, Singapore. As indicated by Fredlund at UNSAT '95, the moisture characteristic curve for a soil is an important factor in understanding the changes that occur to shear strength, permeability and volume changes in unsaturated soils. The authors are obviously aware of the need for the establishment of these relationships when they discuss the problems of slope stability and infiltration rates into unsaturated soil slopes.

The authors describe an automatic pressure plate apparatus which addresses most of the problems associated with the conventional apparatus. The sample volume change and moisture change is constantly monitored and any leakages in the system can be quickly detected. The apparatus can presumably be used with one sample which can be cycled through a series of soil suction changes. The authors present data from both compacted and slurried samples of similar soil and show that the historetic effect is greater for compacted material. This they attribute to the presence of larger pore spaces in the compacted material. I would tend to agree with this.

2.3 Numerical Modelling

There are three papers in this Session which use numerical modelling to extend our knowledge and understanding of the behaviour of footings on expansive soils. Two of these, "**Soil Pile Interface Treatments of Bored Piles in Expansive Clay**" by **Kilsby, Cameron and Symonds**, and "**Numerical Modelling of Covers and Slabs Subject to Seasonal Surface Suction Variations**" by **Li, Cameron and Mills**, detail work being carried out at the University of South Australia with actual footing systems. The third, "**Two-Dimensional Simulation of Soil Moisture Around a Leaking Water Pipe Adjacent to a Concrete Slab**" by **Fityus, Smith and Kleeman**, gives us some information on the more theoretical work being carried out at the University of Newcastle.

The prediction, numerically, of seasonal influences, extreme influences such as trees and leaking pipes, and of the effect of shrinking and swelling soils on loaded footings is extremely valuable and necessary for the extension of our understanding of what might occur. As a basis for prediction it is the only way for geotechnical engineers to provide answers to many problems. However, when attempting numerical solutions to problems to which we may already have some answers through field testing, long term observation or even simple gut feeling, the answers must come under close scrutiny. I suspect after reading these papers that often the real challenge is in overcoming the deficiencies in the software programs, particularly at the interfaces between the soil and the atmosphere, the soil and the structure, the reactive and the stable zones in the soil.

The paper by Li *et al* recognises that the next generation of computer-driven design methods must be based on an interactive model rather than one involving superposition of the stiffened raft on an estimated mound shape. The foundation and footing movements in this paper are soil suction change generated and this in turn takes into account the changes in the boundary conditions. It seems that this program can take into account a variety of complex factors such as multi-dimensional transient moisture flow, multi-dimensional swelling, skin friction and suction dependent soil parameters.

It is early days in the the use of this program and the authors concede that further research is needed to calibrate the numerical model with field data. On this point I would have to agree as several points need to be more closely examined. These are

(i) the surface boundary suction change of pF2.0 seems rather severe and compares to pF1.2 change required by the Standard. The extreme values of pF 3.0 and 5.0 shown at the surface I have found only rarely in the top 50 to 100 mm of the soil. Below that depth suction change to higher or lower values in accordance with the Standard's guidelines is very rapid.

(ii) the result that the maximum change in surface suction at the edge of the slab was only half that which occurred at the surrounding surface. The authors put this down to the effect of slab cover, but that I think needs further examination as I cannot recall field data showing this effect

(iii) the results for a slab with edge stiffening beams show that after nine years the interior of the slab is still moving up and down seasonally. This does not occur in practice and it would seem necessary to examine the suction changes as predicted by the model in this region.

The other footing system paper, that by Kilsby *et al* provides us with some early data on a group of four piles with different shaft-soil interface conditions in a reactive soil. Mother Nature being unpredictable has not been too helpful in getting early results from the installation and the authors have had to move things along by pumping in some water. The results to date show only minimal pile movements even though rather large suction changes appear to have been initiated even to five metres. It is interesting to note in the figures showing the changes in soil characteristics over the two year period that the soil suction has also changed at depth. Even allowing for the fact that the ground was soaked a change of at least 0.3 pF appears to have occurred at about 5 metres. This of course is well below the depth normally taken as stable by AS2870.

The results of the numerical modelling show good correlation with the field results and the three 3D finite element program MSC/NASTRAN must be useful to engineers faced with the problem of determining the amount of tensile reinforcement. The installation will I am sure provide most useful information in determining what advantages are to be obtained from under-reaming and the various pile shaft treatments for reducing side friction during swelling, and I look forward to some later results from this group.

The paper on the leaking pipe problem by Fityus *et al* gives the results of some of the theoretical modelling being carried out, in conjunction with the field studies by the University of Newcastle. This work involves the development of a computer program to predict unsaturated moisture distribution in layered soil profiles beneath covered areas such as building slabs. The authors describe some of the problems associated with the solution of partially saturated flow in two or three dimensions and explain how they have applied a Type-3 boundary condition to simulate the case of constant moisture content at the upper boundary of the soil. Using the transform methods they propose it seems that a number of simplifying assumptions and manipulations have to be made in order to solve the Richards equation for moisture flow. The proof of their success must be in the ability of the numerical model to accurately predict some field data and unfortunately at this stage that has not been attempted. The formulation of the leaking pipe problem is very adequately illustrated but I do question the infiltration rates that appear to be shown in Figure 2. After 50 days the moisture wetting front appears to have only penetrated to a depth of about 0.5 metres and very little water appears to have leaked from the pipe. This is contrary to normal seasonal moisture movements in a "light clay", which would I suspect have been wetted to over by one metre by that time. I know the authors have described some of the future developmental work required for the program but perhaps the authors would like to comment on how they will be attempting to justify their predictions.

2.4 Effect of Expansive Clays on Structures

The final three papers I have grouped together because they are basically case studies of the behaviour or rehabilitation of structures which have felt the effects of reactive soil movements. The paper by **Barthur, Jaksa and Mitchell, "The Design of Residential Footings Built on Expansive Soil Using Probabilistic Methods"** assesses the information collected on the behaviour of a large number of housing slabs in the Adelaide area which have been built on M or H sites. This information alone is valuable but the authors then show how the data can be normalised to give a series of design curves linking probability of exceedence of crack widths to beam footing size. The curves are most interesting and I would recommend that anyone working in the field of footing design for domestic housing (or soil testing as it is known in this State) study them. The implications are that by using the footing sizes specified in AS2870.1 there is approximately 50% chance that cracks greater than 1 mm (i.e damage category 2, which is what the design engineer would generally regard as the upper limit) will be exceeded. However, the cost of reducing the probability of exceedence to possibly more acceptable values is high with, in the case of solid masonry houses, footing sizes increasing from 800 mm deep to 1200 mm deep for 10% POE) and to 1600 mm deep for 1% POE. How many owners would be prepared to double their footing costs for an almost complete guarantee against cracking?

In their introduction the paper is presented as an alternative design method for the design of residential footings on expansive soils. It is rather I think a complementary method, which provides the design engineer with some guidance to the level of risk associated with the design process - a point the authors make further in the text. The final conclusion to the paper that the estimated crack widths associated with the footing sizes recommended by AS2870.1 are likely to exceed 1 mm with a 50% probability of exceedence and that the footing sizes recommended by AS2870.1 may be somewhat unconservative will I am sure lead to further close scrutiny of the results and to additional surveys of this type.

The paper by **Brown and McManus, "Rehabilitation of Damaged Houses Founded on Expansive Soils Using Moisture Recharge"** and that by **Holden "Some Developments in Urban Root Barriers"** describe in some detail ways in which we can ameliorate the drying effects of trees and restore some buildings back to their original level. For speedy relief, some of these techniques have to be used and it is useful to see the results of some documented work. So often, and especially when carried out by consultants the pressure of sorting out the next problem does not allow the time to put the information together to allow study by others.

Moisture recharge of dry or drying reactive clays can be a very effective stabilisation technique as the Brown and McManus paper shows. The techniques can vary from the crude to the refined and the authors conclude that wherever possible it pays to use a more scientific rather than a hit-and-miss method with a hose and sprinkler . Although more costly, the fixed and automated type of installation has the best chance of success and long term control. Automated sensor-driven equipment is now also available which overcomes the problems of resident and/or landlord education and involvement.

The results from the Werribee site were of particular interest and would perhaps bear more detailed explanation. Why for example had such large movements occurred even though a soil suction of pF 3.9 was still present down to about 1.8 metres. Could the house have been constructed with the surface soils in a very wet condition? Do the authors see the need for constant watering *ad infinitum* to retain very high suctions in the soils close to the surface and what do they think might be the effect on the soils at depth in the long term? Is there a chance that by covering the underfloor area with plastic, drying could be reduced and an equilibrium value established? An interesting case.

The history of the root barrier development presented by Holden is timely as this technique is being used more often in controlling the drying effect of trees without having to resort to the more drastic measure of chopping off their heads. The problems of working in confined spaces, digging and sealing around services, cutting through large tree roots and final sealing at the surface are all discussed and the most effective ways found in dealing with these matters are described. The use of HDPE as the barrier and the ease of backfilling with Liquifill is a real advance and I am now looking forward to my next root barrier case to try it out.

Sealing around services continues to be a problem and it is time-consuming. We need some collars or flanges which can be easily fixed to the services and the HDPE without having to excessively widen the trench. Perhaps some sort of in situ welding may be the answer. I am sure that further research by the VicRoads group will lead to solutions to this and other practical matters.

3. CONCLUDING REMARKS

In conclusion, and at the risk of sounding repetitious, can all of us when presenting information in this field of unsaturated soils aim for the following:

1. a unified set of symbols and units for the description of soil suction.
2. critical scrutiny of the results of any numerical modelling and rigorous testing of the model.
3. the provision of both volume change characteristics and total suction changes when presenting the results of surface distortions.

Adoption of point 1 is necessary for the for the easier dissemination and understanding of information in this field; adoption of points 2 & 3 will allow a more scientific evaluation of that information.