Reminiscences

Professor Andrew Schofield

Interviewer: Professor Kenichi Soga, University of Cambridge

Date: January 24, 2008

Place: Engineering Department, University of Cambridge

KS: Andrew, after you graduated what did you do?

AS: Prof. J. F. Baker suggested I see London consultant engineers Scott and Wilson (SW), where his former pupil Henry Grace worked. I thought I might work on concrete shell design but I was interviewed by Guthlac Wilson and offered work as a junior engineer in Nyasaland (now Malawi) where Henry Grace was the local partner, and I accepted. On leaving the interview I asked his secretary where Nyasaland was! Henry Grace had been at Harvard and a graduate pupil of Casagrande before WWII, in which he served in airfield construction. He was an excellent engineer to work for. Clayey laterite had been used as pavement material for Cheleka airfield. There were failures and he wanted to see how Lime stabilisation could improve the bearing capacity of future pavements. He asked me to perform California Bearing Ratio tests on lime and cement stabilised Laterite in the SW soil mechanics laboratory, and then set me to find from air photos, sources of clayey laterite for use in low cost road pavements. I constructed trial pavements and prepared the Specifications and Bills of Quantities for contracts for lengths of road to be constructed in the Nyasaland Protectorate in 1954.

KS: After this, you decided to do your PhD or did somebody else ask you to do a PhD?

AS: Ken Roscoe had lectured to me in soil mechanics and was then beginning research. He wrote to me from Cambridge inviting me to return from Nyasaland to be his first research student. When I came back he got me to write up my road work as an essay for the Cambridge University 'John Winbolt' Prize; I won that prize and then rewrote the essay as three 1957 Colonial Road Notes for the UK Road Research Laboratory, which then won ICE Miller Prizes.

KS: How did your PhD research topic come up?

AS: Roscoe offered me two topics; he wanted one student to get data with his simple shear apparatus and another student to collaborate with Prof. Baker's structures group in short pier foundation design to provide moment fixity for the stanchions of welded mild steel frames. Moment fixity at each base would make the best use of the steel in the welded frame. A horizontal force at the base would be carried by a concrete floor slab, but the full plastic moment at the base must be resisted with the moment fixity of a short pier. I chose the collaboration with Baker's group and began my study of earth pressures. In my PhD experiments I measured the development of lateral earth pressure on a plate with an axis of rotation at the surface of sand in a test tank.

KS: During this work you became very interested in plasticity and I guess this led to Cam Clay?

AS: Yes, I read everything I could about plasticity and was impressed with what I found in the Russian literature. I only had enough Russian to be able to read one word after another in journals in the University Library with a dictionary. I later managed to get some funding to get the books translated. But theory of plasticity did not fit test data of soil strength. I asked Roscoe for help and he gave me his translation of Hvorslev's PhD thesis from the German into English (as a prisoner of war throughout WWII he knew German and he later insisted that all research students read it). Hvorslev's equation led me to make what is now called Hvorslev's surface, bending copper wire into a sloping surface, with a curved line edge; it seemed to me that as test paths progressed each must arrive eventually at this edge, the Critical State (CS) Line. That was the beginning of CS discussions with Roscoe. His simple shear apparatus was designed to study the changes of volume of soil in test paths. By this time a second research student had arrived, Peter Wroth. He had no Civil Engineering experience, having been an artillery officer in military service, and had difficulty in getting Roscoe's simple shear apparatus (SSA) to work because it had a basic flaw. The specimen had upper and the lower rough surfaces with equal and opposite shear stresses. For a uniform state the soil needed complimentary shear stresses on the vertical ends. In order to allow the soil to dilate had used lubricated rubber sheets to eliminate shear on the rotating end flaps. The stresses in the interior sample could never be uniform, making endless difficulty with the SSA apparatus. A change came. Prof. Baker advertised a post for a Demonstrator to work with Roscoe and begin the Cambridge Soils Group. Both Peter Wroth and I applied and were interviewed. Having worked with SW in Africa I was appointed, but Roscoe hoped that Peter Wroth would finish his PhD thesis, get industrial experience, and then return to the Group.

Reminiscences (continued)

Professor Andrew Schofield

KS: Then you did triaxial testing with Thurairajah, rather than simple shear tests.

AS: Yes. At that time Imperial College began publishing Geotechnique papers about the pore pressure development in clay. Casagrande's original paper on critical void ratios was concerned with liquefaction and with the prediction of the pore pressure in sand that was unable to change its volume during rapid shear at constant volume. His paper to the Boston Civil Engineers showed a way to find the pore pressure, assuming a critical void ratio with a constant value. However the work of Hvorslev and the Imperial College data showed that the CS varied with the effective pressure. We needed test data of soil on the 'wet' side of the CS to study the change of volume or of pore pressure. Roscoe wanted this to happen in his SSA but drainage from clay means that each test takes a long time. Peter Wroth could test sand quite quickly but there was no chance of quick SSA on clay. So Roscoe took Peter Wroth up to Imperial College and asked Prof. Skempton for access to the PhD theses that were the basis of the new Geotechnique papers. Skempton was very confident of the excellence of Imperial College and let Roscoe and Wroth take the theses to Cambridge. I had analysed the Geotechnique papers and now Peter Wroth as the research student could study the theses in detail. By early 1957 he had new test path data on the wet side of CS. It was an exciting year because of the London Conference of the International Society in which Skempton played a very prominent role. Our Group was not ready to make a contribution but we discussed what we should do with the new CS line. Slides were made showing what we had found with drained or undrained test paths approaching a CS line from 'wet' or 'dry' sides. Roscoe went up to the conference in London with this box of slides in his pocket so that he could get up and show them if needed. Nobody said anything about pore pressures or our CS stuff and he came back from London saying we were far ahead of everyone else, with 10 years in which to get good data from the SSA I decided to begin triaxial testing. Then in November 1957 David Henkel stood up at a meeting at the Institution of Civil Engineers and began to suggest the same sort of CS idea as ours. Peter Wroth sitting one side of Roscoe and I on the other side both nudged him with our elbows and said 'unless you get up and say something now we will lose all our work'. Roscoe stood up and said 'we are preparing a paper on this in Cambridge for submission to Geotechnique'. Dr. Cooling, Editor of Geotechnique, told Roscoe that if we put our paper in quickly it would be published, so we quickly wrote our paper. When it reached Cooling he sent it for review at Imperial College by David Henkel who saw that his student's PhD data had been interpreted and he had got no credit for the work they had done, and asked Skempton to ask how it had happened. I believe Roscoe told Cooling that unless Geotechnique published our paper immediately he would send it to the ASCE Journal. Our paper, only written just before Christmas, was published in the March 1958 issue of the Geotechnique.

KS: So Andrew, this present year 2008 is the 50th anniversary of the Sputnik paper in Geotechnique that launched the Cambridge Soils Group on the international stage.

AS: After 1958 Cambridge had plenty of good applications from well trained students and got UK Science Research Council grants.

KS: And this led to Cam Clay model?

AS: Well, it led to the student, Thurairajah, from Ceylon. There were quite a number of small problems in the 1958 paper but one difficulty was that peak strength in a drained test and in undrained test fitted the same Hvorslev's surface. This meant that no plastic work is dissipated in volume change. We needed Thurairajah to check this with next research student making careful calculations for every step in his triaxial tests. He came up with a remarkable dissipation function; the energy dissipated by aggregated soil grains depends only on shear distortion and not on the volume change. It was a striking finding but not what you might expect by thinking about micro mechanics. However, at that time Calladine, a former undergraduate at Cambridge who had gone to work at Brown University with Drucker, came back to Cambridge he wanted to see and to interpret Roscoe's data (much as Roscoe's student had interpreted David Henkel's data) but Roscoe refused. I realised that I could combine Thurairajah's dissipation function and the Associated Flow Rule of Theory of plasticity that Calladine had talked about, to obtain an equation that could be integrated. So Roscoe and I could then publish two papers. One was a Geotechnique paper on the work of Thurairajah (who at that stage had just gone back to Ceylon and had become Professor at the University of Peradeniya). The other was a paper for the European Regional Conference of the International Society in Wisbaden in 1963 that was a purely theoretical paper based on an assumed dissipation function and the plastic normality law.

KS: At the time of the development, did you predict that Cam Clay would be used so much in the world, especially in computational geomechanics?

AS: It's hard now to remember how recently the computer has come in. When I was a research student analysing the data of my earth pressure tests, simple calculations were performed on a big sheet of paper with columns and a calculating machine with a handle that was physically rotated to multiply numbers of 5 decimal places. All my graphs resulted from work on these big calculation sheets. There were no electronic calculators at that time. The computers which were coming into existence were being used in analyses of structures for study of deterioration of stiffness with deflections and the onset of the instability in columns under compression. No computers had software such as they have now.

Reminiscences (continued)

Professor Andrew Schofield

What was clear to me was that the CS concept would apply widely in practice. In 1958 immediately after our publication, I visited the Swedish Geotechnical Society in Stockholm and then Gothenburg where failures of the quays had been discussed in a Geotechnique paper. I saw that the large movements of the soft post glacial clay must have generated excess pore pressures. After the failure the harbour was reconstructed and new work built over damaged ground it would show excessive settlements in the regions which had sheared. The crane rails for handling goods on the quay side had had to be continuously levelled; the harbour engineer pulled out the settlement records of the quays and I could see that big dips in the settlement records coincided with the locations where the slides taken place, as predicted.

KS: Another significant achievement you have made is in geotechnical centrifuge testing. So can you tell me what led you to start centrifuge testing?

AS: While reading the Earth Pressure book of V.V. Sokolovski, I found a footnote about 'the well known modelling method of G.I. Pokrovski'. I followed it up and found several English language publications. A paper at the Harvard Conference of 1937, and also one or two papers in an English language journal called Technical Physics of the Soviet Union. The technology was sound and the ideas fitted in well with critical state soil mechanics, but I felt there must be something wrong, or the Russians would have published more. I was reluctant to begin work but I got a small modelling apparatus made to fix on to the end of a hydraulic turbine. My first experiment in centrifuge modelling with little specimens about 2" x 2" worked well and I was puzzled as to why G.I. Pokrovski published little work. I delayed making applications for a centrifuge but I got a research grant, and I and a new research student got access to a centrifuge in Luton, once used for a British aerospace program. We made models in Cambridge and drove our models, about a metre in overall external length, to Luton and the research student succeeded in making tests. I introduced the idea of centrifuge modelling at a British Geotechnical Society (BGA) meeting. Others began to take interest, in particular Peter Rowe at Manchester who obtained funding and decided he would have a centrifuge. When my book with Wroth was published in 1968 I was invited to become Professor at UMIST. I made applications for funds for centrifuge modelling in Manchester, and when I went there in January 1969 I built a UMIST centrifuge. Prof. Rowe at that time was a major consultant on dams. He built his centrifuge about a year later, so there were two centrifuges in Manchester.

The next big excitement was in 1973 with the International Conference in Moscow. By that stage, having a lot of evidence from the work we had done in Britain that the technique was very good, I wanted to learn what had happened in Russia. I decided that a good idea would be to display our work in the Moscow conference. A simple way was to hire a commercial exhibition space in which to show our work. I wrote to the Moscow Chamber of Commerce, booked a commercial space and got the English centrifuge modellers (my former students in Cambridge and Rowe and I in Manchester) to prepare displays. We went to Moscow and put up all our photographs. The Russians looked at what we were doing and asked what we were selling. I said we hoped to get research contracts for using the centrifuges. Then the Russians asked if we would agree to exchange information and arranged that at the end of the conference everybody who was interested in centrifuge modelling could have a meeting. G.I. Pokrovski was there, an elderly man who was clearly a very distinguished engineer. When I looked round the Soviet centrifuge centre it was clear that they photographed models with a stationary camera in the wall of the centrifuge pit as they flew past. I realised that the event photographed must be an explosion and the reason that the Soviets did not publish must be that it had become military research. In fact G.I. Pokrovski was a leading scientist on weapons effects concerned with multi-megatons weapons, with the rank of General in the Red Army. All that the Soviets knew about explosions and craters was a military secret.

KS: I guess that your centrifuge testing concentrated largely on civil engineering applications and that then led to many people working openly on centrifuges.

AS: Yes, the military secrecy made it essential to have some way to get centrifuges into the public domain where we weren't regarded as being spies and had no problems in travelling or visiting. This became possible at the time of the Stockholm conference at which Victor de Mello was the President. He agreed that there should be a centrifuge committee TC 2.

KS: This was one of the main reasons for developing at TC 2. The committee became really big now.

AS: Yes, it turned out in 1973 when we went to Moscow that there were two centrifuge modelling groups in Japan; one at Tokyo Institute of Technology but the first one had been at Osaka City University where Prof. Mikasa was one of the Japanese engineers who had been a WWII aircraft designer and in the demilitarisation program had to move into civil engineering. He had the scientific and technical background to develop his own very special centrifuge and original modelling techniques.

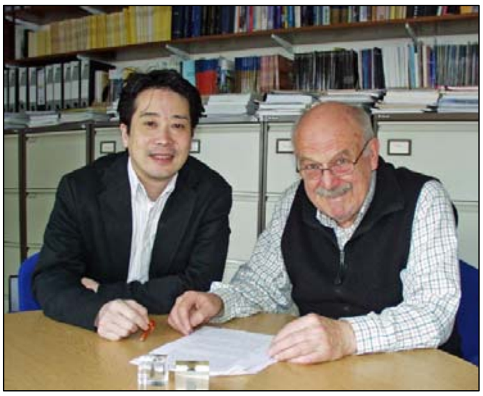
KS: I would like now to hear from you about the future of the geotechnical engineering. What do you think first in terms of ISSMGE, what is the future of this society, do you have any thoughts?

AS: I do not think that international cooperation is as good as it should be. It was very striking in the time that I was a research student that Terzaghi was contemptuous of Russian engineering and simply didn't respect any Russian work. His dismissal of G.I. Pokrovski's paper was contemptuous. I hope that ISSMGE can get full collaboration both with the Chinese and the Russian research establishments. The next generation of young Western engineers should be familiar both with clever Russian and with clever Chinese engineers.

Reminiscences (continued)

Professor Andrew Schofield

KS: To these young geotechnical engineers and academics, what technology fields in geotechnical engineering are mostly needed at present in your opinion?



AS: I'm struck by the fact that the soil mechanics in Terzaghi and Peck's text book is about the post glacial deposits of Northern Europe and the Northern United States. I saw soils in Africa that were transported to form a catenary from the top of mountains down the slopes to the valley bottom. The soil sequences that developed involve chemical transport as much as the physical transport of grains. CS soil mechanics is about aggregate of grains and has not developed a place in it for the pore water and soil surface chemistry. For soils of central Russia or China or South East Asia, like their Laterites, it is not clear that CS soil mechanics and Casagrande's soil classification techniques are the right starting point. It is clear that Coulomb's equation is not the best starting point. We have a major problem in Europe because we already know that the Mohr-Coulomb equation is wrong but Eurocodes are being rolled out in industry with no rational discussion of Mohr-Coulomb's equation and cohesion and friction. I have made a beginning with my 2005 book with Thomas Telford Limited. The International Society needs to be able to revise the mechanics of soils; not simply micro mechanics but including the transport of the chemical nature and thermal effects in soils. As far as I can see much has to begin again.

KS: Thank you Andrew, it looks as though there is a great future in Soil Mechanics and our horizons will continue to expand. So lastly, would you please say a word to the members of the International Society?

AS: Well, my experience of soil mechanics has been that the International Society has played the central role in providing the academic freedom for discussions, publications and contacts. Terzaghi played a vital role in setting up the whole system and creating many opportunities. It is as important today that the International Society has many forums in which young people and older people can discuss their work and can publish as it was 50 years ago in 1957 - 1958 when many scientists came to the 1957 London conference. Whatever enables engineers to get funds, make experiments and discuss them internationally is a very desirable thing. The development of new theories that can lead to new teaching in geotechnical engineering gives the ISSMGE new opportunities.

KS: Thank you Andrew for your insightful thoughts.

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PUBLICATIONS: (with C. P. Wroth) Critical State Soil Mechanics, 1968; (ed with W. H. Craig and R. G. James and contrib.) Centrifuges in Soil Mechanics, 1988; (ed with J. R. Gronow and R. K. Jain and contrib.) Land Disposal of Hazardous Waste, 1988; Disturbed Soil Properties and Geotechnical Design, 2005; papers on soil mechanics and civil engrg

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