V. F. B. de Mello *(Brazil) : I would begin by saying that I have been very impressed by the number of points on which we all agree, which is a matter for congratulations. On the other hand, every time that we come to a specific detail, there are obvious differences, and that also is obvious to anybody. I would like to mention, for instance, the fact that there hasn't been, with respect to the land use downstream of a given dam, any real concerned revision in policy of population resettling, or in revising legislation regarding terrain of foreknown risks. This was pointed out here by one of our colleagues. Now, if we do have flood control, and we are not as happy as our Pakistani colleague to be able to control completely floods for forty years, in Brazil for instance, we do provide protection from average floods, but nobody tells those people who obviously want to settle closest to the water, nobody tells them that all of a sudden, at the moment when they least expect, and while they have been trusting our dam to protect them, all of a sudden, a much huger flood than they ever imagined comes, and they get wiped out. Such problems are very well known, and it seems to me we have been discussing resettlements of people that would be slowly and gently submerged or might be within the area around the reservoir, between the normal and the maximum water level, which is quite obvious : but to some extent such resettlement fails in a country like Brazil, because after you have bought all the land, you cannot keep control on the hundreds of people who just keep moving closer and closer to the water because they want that water for their typical uses. So, there are some problems that we really have to aim at as citizens, and thereby with respect to the comment made by Mr. Roberts, I agree entirely we are not specialists on population — fortunately we have the pleasure of having here a member from the World Bank who will give us some information — but we are citizens, and we have a very important function within our societies to carry to them our information and to interact with them on their information. When we discuss, for instance, cutting down forests for log and for cooking, I beg our dear colleagues to forego putting the automatic adjective " tropical forest ". If forests are good and deforestations bad, they should be bad or good all over the world. Now, if the only possibility is of cutting down a tropical forest or not, that's another point; it has to be analysed specifically.*

Isn't it in part the fact that most of the north-temperate forests have already been cut down or are being damaged by accidental fires, chemical pollutants, acid rain, etc.? In the tropics, we, who studied hard all our lessons from the advancing (predatory) civilizations, are only doing, late and poorly, what we were taught by preaching and practice. All our dam projects were headed by internationally advanced design companies.

So, let's not put adjectives unless we get down to really analysing the specific case. For instance, in our case in Brazil, whereas all civilisations had gr/ *obviously started to progess from the coast, we do have polluted rivers coming way into the hinterland because of mining and because of sewage, because all the cities are closest to the headwaters of the streams that spring in the mountain range close to the coast and flow inland. So the fact that in Pakistan (or in so-called developing countries) they may not have industrial pollutants, that's not true in many other places. Forgive me for saying things that obviously everybody knows, but let's get down to specific cases, individually, with the very well-evidenced knowledge that belongs to this audience.*

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pp. 93-94

"SEVENTEENTH CONGRESS ON LARGE DAMS" - Vol. V, pp.93-94; 270-271; 484; 542-543;554-555; 571-572; 588-589; 611-614. VIENNE/ 1991 - CIGB/ICOLD. (Intervenções em plenário)

modulo 16/01

V. F. B. de Mello *(Brazil) : I am terribly sorry that I was not present at the presentation by Dr. Charles, yesterday afternoon. But I gather that the topic of hydraulic fracturing is also going to be discussed in Question 66, where it should belong more appropriately, because it should not fit into our concept of the slowness of an " ageing " process. However, hydraulic fracturing is drawing very much attention, which it must.*

I am much reminded of the fact that the common cold and " flu ", which are the most common travails of mankind, have ended up being found to have very many causes. Analogously, as a beginning Terzaghi's effective stress equation has been used as a single very simple index for hydraulic fracturing. When you get $\sigma - u = \sigma'$ *(effective) tending in the tensile direction, then you have (primitively postulated) hydraulic fracturing. Well, that is a necessary condition, but not sufficient. Why? Because obviously such a simple equation of STATICS presumes instantaneous, infinitely rigid behaviour. So, all the stress-strain-time behaviours come in between.*

If there is a change of u, everything depends on the rate of change of water inflow, minus compressibility of pore fluids (water plus air), and so on, versus outflow at a given point. If there is a rate of change of effective stress, there should be a change of volume of the corresponding material. It could swell if it is a very slow change, but if it is a very fast change, it cannot swell, therefore it cracks.

For instance, when we talk of shrinkage cracks, everybody knows that materials for the ceramics industries, and clayey soils, and so on, are made to shrink when dried. But if they are made to dry and shrink very slowly, they don't have to crack. If they have to shrink fast, they have to create new shorter flow paths for the internal flow of the water to match the evaporation, and therefore they crack in order to alleviate the excessive suction state. So, all of these are questions of the rheologies of the materials, set aside in the equations of STATICS. For instance, if there is an increase of the settlement, but meanwhile there has been a thixotropic regain of strength of the wet clay, with accompanying brittleness gain, and meanwhile the settlement continues, you begin to have ageing effects which were not foreseen (e.g. in unusually wide puddled cores).

So I'm very glad that this very important topic is being handled, and I recommend that our geotechnical community give it much closer thought : the fact is that a simple, intuitively obvious, idealized index is only a first step. There is much to investigate and learn about the rheology of all of the materials at play, under different combined actions, with different rates of change. Incidentally, the bituminous material would come under the same general recommendation.

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pp. 270-271

V. F. B. de Mello *(Brasil) : In hearing of grouted columns, ballast columns, displacement versus replacement, all of these items are always a question of trade offs. I would like to share with you a reflection that suddenly struck me. In our investigations, we have been concentrating on the structural aspects, for quite comprehensible genetic and historical reasons. But what we really build is not dams, we build reservoirs. And if we're discussing reservoirs, we're discussing changes of geohydrological conditions. I was really shocked to know of that case in which the initial geohydrological condition was at elevation minus 240, whatever it was, and suddenly I thought that we should emphasize this problem that water divide does not always follow the topographical divide. Unfortunately that is the difficult foundation condition, and that is the only reflection that I believe is entirely different, because I haven't heard it. The geohydrologic change from prior to post reservoir conditions and to what extent can we really achieve that. The dam is a means, very important of course, it should not fail, but it's not an end in itself.*

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pp. 484

V. F. B. de Mello *(Brazil) : Prof. Schober, congratulating you on this magnificent idea, may we try to clear some points? You mentioned very definitely that engineers, that's ourselves, fear the last few meters. Not so much, but anyhow. But on the principle of always introducing a factor of safety, and also for practical reasons, why don't you use a bentonite suspension such as slurry walls and so on, instead of water? You will get a little bit of a factor of safety. That was question number one.*

Question nº 2 : Let's try to put the classical Mohr's stress numbers. You are making an equilibrium with what we call the σ_2 stress. If σ_1 is the principal unstabilizing stress, due to overburden, etc., then σ_2 would be the upstream-downstream stresses. These are the ones with which you establish the proven equilibrium, and it seems to me that we are most concerned about σ_3, in the direction of the dam axis, which would establish the upstream-downstream cracking, if any to be feared. Of course, after you will finish your construction, if you introduce a concrete wall inside these two membranes, that is handled so easily.

May we be cleared on some points to these two questions?

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pp. 571-572

V. F. B. de Mello *(Brazil) : I would like to ask the Japanese colleague that mentioned the weathering tests on weatherable rock one question, very much in line with what Mr. Klablena just emphasized, that tests have to be really carefully aimed at a specific weathering process, and not at random. I am terribly concerned about the great number of areas in the tropical world, etc., that only have very poor and weathered rocks. If we begin to dismiss them because of testing criteria that are too stringent it would be very serious. Were any tests of that weatherability done under confining-stress conditions, or were they all exposed at atmosphere? That is a very important question because we have satisfactorily used many of these highly weatherable materials that crumble, as shown in the slide that the audience just saw. On some of them we even started with test fills. For instance, back 20-25 years ago we were having nontronite in the basalt and at depths of more than 4-5 m in the rockfill there was absolutely no effect at all, because of much smaller changes of temperature and weathering parameters, and presumably because of being under sufficient confining stresses.*

So I repeat my question : I would like to know whether or not the tests were run under different stresses, and whether you could distinguish between a condition totally exposed at atmosphere versus under given confining stresses?

S. Aoki *(Japan) : I am sorry, I cannot understand your question.*

V. F. B. de Mello *(Brazil) : As far as I could see, most of the tests on durability shown generally spread the rock on a tray, open atmosphere. Whereas, if you wanted to keep them under a stressed condition, for instance you could place them in a triaxial cell and apply a confining pressure; then you could have the circulating fluid or whatever it be, or have air/humidity, etc., go in and out in cycles, at will. In short, under given confining stresses we often find that things change considerably. Did I make myself clear?*

S. Aoki *(Japan) : No stressed conditions.*

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pp. 554-555

V. F. B. de Mello *(Brazil) : There are many topics I would like to come up with later. I just wanted to pick up on what Mr. de Fries said, and remind you that I ventured a thought in my Rankine Lecture 1977 about geotechniques, that it is most difficult for the engineer to play around within the intermediate range, where you do not know what is happening. You either avoid slips altogether by having very flat slopes, or else you force complete definite slips even by introducing very soft materials or very slippery materials — let us call them that way — in order to avoid the rich diversions of the tensile stresses above. This was the basis of the discussions with respect to the contacts of cores against the gravity walls, when we in Brazil had a tremendous debate between wrap-around cores and front-end cores against gravity walls. That is why our colleague mentioned that the Brazilians have a lot of experience. I am happy to note that his experience does prove that. When you really have an inbetween slope — let's say 1 on 5 or 1 on 4 or 4 vertical to 1 horizontal — you are somewhat within that range, where you do not really know what is going to happen. Whereas, if you really make it steep as concrete engineers make things always very steep because of saving concrete, they did not bother too much about dirt. When you really make it steep, things behave very much better and we try to investigate why, and that were the data he was mentioning. We got very interesting results proving that sliping is very favourable on these steep floors.*

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pp. 605-606

V. F. B. de Mello *(Brazil) : I am very happy about this immediate discussion of the presentation by Mr. Megla. To begin with, I must state that back in 1946 we inherited directly from Terzaghi the idea of the homogeneous compacted clay dam with the vertical chimney filter-drain (the Brazilian Terzaghi Dam named in his tribute). We have used that very, very much, but I agree fully with the idea that we should not have anything vertical within a dam to begin with. Secondly, all sliding surfaces that in the 1940's and 50's used to be simplified as circular, are not circular. They are subvertical at the start and top because of much more brittle behaviour in tension, and then swerve into being subhorizontal at the bottom of the sliding mass accompanying critical conditions of increasing stresses and plastification; that is, they tend to be cycloidal, shapes that for the past 25 years have become easily handled in generalized sliding analyses. So, we have had one case of a dam with downstream slope failure at end-of-construction, where the top of the sliding surface followed the vertical chimney downwards for 20 m : in circular failure analyses, as the sand is reasonably resistant because of the friction angle, such a real failure condition never shows up. The friction resistance down the chimney depends upon the lateral stresses, and any dam tends to spread out a little bit; the horizontal stresses decrease, we do not know exactly how much, nobody has measured it, but they do decrease, easily to a conditioning degree. So I would very strongly recommend, do not use the vertical chimney plane that was associated with so brilliant a name as Terzaghi.*

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pp. 588-589

V. F. B. de Mello *(Brazil) : I also would like to comment on Mr. Fry's presentation. To begin with, I beg excuses to request that we adopt a different index than the compaction water content being optimum plus 2 or optimum minus 2, etc., because in our experience we deal with materials that go all the way from optimum of the order of 7 or 8 % to about 45 %, and plus 2 or minus 2 makes a tremendous difference whether it is on an 8 % optimum, whereas no difference at all on a 40 % optimum. So we've begun to adopt a fraction of the optimum, in other words, a wet material would be placed at about 1.05 of the respective optimum. That gives a much better index of what range are we really working with.*

The reason why I mention this is because (I hate to repeat myself) but over 30 years have gone by since, against my will, I was forced to go up with a core that had to behave as a shell on a clay of 1.34 ton/m^3 maximum dry density, optimum water content of the order of 42 %, which was the Paranoa dam for the construction of Brasilia, 1958, 59. This has been published in great numbers of papers. We went up 35 m in 42 days on 1:1 slope, and although it had been predicted that it would fail according to all stability computations, etc. at 1:4 slope, we went up at 1 on 1, and there was absolutely no problem. Why? And that is where I would like to request that we begin to give much more attention to pores, pore sizes and porosymetry. In sedimentary materials there is a tendency for things to be uniform, not only the grains, but also therefore the pores, whereas in most of our soils, saprolite soils, which are bad soils for most of you, but which are very nice, sweet soils for me, very intimate soils for me, what we have in saprolite soils is such a wide range of pore sizes that the macropores are able to permit internal drainage and thereby not have any pore pressure, or at least absolutely insignificant pore pressures, which has nothing to do with the poor British, my very dear colleagues, who always have excess pore pressures, no matter what they do. But anyhow, the fact is that we have to move into somewhat more particular concern for the only thing which is compactable, which is the pore, and the air porosymetry. Unless we do that, we're really fooling ourselves. We are investigating parameters that have nothing to do with it. So, that was the second point.

Now, I saw the curves that we were shown as dry and wet, etc. and Duncan, and the hyperbolic relations they said; they don't really work with materials compacted dry of the optimum, where they really give a peak and go down again at very, very small strains. So in all stability analyses, whether we accept automatically a rigid block analysis, everything working at the same percent strain and so forth, we're really making idealizations that don't work at all. So, kindly take that into account and moreover, finally, God be thanked, in materials compacted dry of the optimum we have high residual stresses from compaction, and I don't believe, or at least if I do, I would like to request that it be confirmed, whether or not internal residual stresses were included in those analyses, and if they were not, we're just discussing changes of stresses, just static changes of stresses and not the real total stresses or effective stress.

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pp.542-543

DISCUSSION

V. F. B. de Mello *(Brazil) : Incidentally, with respect to the three dimensional finite elements analysis, may I request if Dr. Schober would be willing to introduce shifts of the mesh along the siderock. Has he done it already? Because if we begin to get settlements, movements of the order of 3, 4, 5 %, the mesh on the sides might also have to require adjustments.*

Well, one of the problems I did mention a couple of days ago and that worries me considerably, is the fact that we may leave behind some false impressions to a great number of keenly interested geotechnicians or dam engineers. Well, that is a problem of names. Unfortunately many questions use the plasticity idea referring to clay as being plastic, merely because it has a high plasticity index, which is really nothing but the range of water content through which the soil would deform without cracking in that very, very rudimentary test, in which we roll the thread at atmospheric pressure. What that has to do with the plasticity behaviours we look for, I do not know. I am terribly sorry to say, it has absolutely nothing to do with it. Very high PI clay needs to be compacted well dry of optimum, you know if it's to be compacted at all. Let us distinguish between remolding and compacting. A lot of our friends at water contents higher than 1.1 of the respective optimum are really remolding and not compacting. If you need compaction, then you have to go to fairly dry conditions, high suction. It becomes very tough and very brittle. Now, very quickly we compare the compaction water content with the optimum and normal range with which we work. What we have to do, if we want to discuss fragility or cracking at low pressures, would be to compare the W optimum with the plasticity limit water content. I did that for a range of plasticity chart characteristics for the clays we have dealt with, from about liquid limit 20 to about 100. That is about the widest range in the world. I did that in Madrid in 1973, showing that most clays as they get higher PI-values become less plastic, very much less plastic in behaviour as compacted. Then what we really want is plastic deformation behaviour. If you make it one-dimensional, there is a very big difference between the so-called plastic behaviour, which is what we aim at hopefully, and the brittle type of behaviour to which most materials get in the range of high over-consolidation ratios.

Incidentally, I mentioned the thixotropic regain with time, that Moretto demonstrated with long-storage unconfined compression tests on remolded clay, back at Urbana Illinois in 1948. I am very happy to learn of the added results on the clays in Mexico, as just shown by our colleague Moreno. Exactly the same type of phenomenon has been occurring and therefore, there is a problem on first filling. That is one thing, and there may be problems on not merely the first filling, but subsequent fillings. To continue, again begging your pardon, everbody knows that effective stress is total stress minus pore pressure, and what we are interested in, is the σ'_3 effective, the weakest one of the three, σ_3. Incidentally I have had wet seams in the Jaguara Dam back in 1964, which *fortunately did not develop wet seams, developed a sort of muddy condition close to liquid limit, which suited perfectly, because the clay had the conditions for swelling to the point that it really did not crack at all. That was stress release and swelling in the vertical condition. So the vertical stress can become*

PLASTICITY

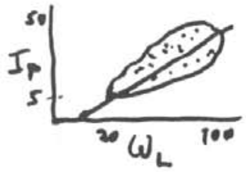
1) $I_P = PI = \underbrace{\omega_L - \omega_P}_{*}$

* RANGE OF ω THROUGH WHICH SOIL DEFORMS WITHOUT CRACKING**

** AT ATMOSPH. PRESS.

2) $\omega_{comp} \simeq \omega_{opt} = \left\{ \begin{matrix} 0.92 \\ \downarrow \\ 1.1 \end{matrix} \right\} \omega_{opt}$

ω_{opt} compared with ω_P ?



Madrid 1973

$\omega_{opt} - \omega_P$

3) PLASTIC DEFORMATION BEHAVIOUR

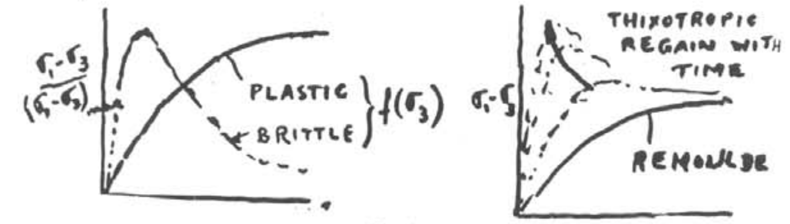


Fig. 1

$$\sigma_{eff} = \sigma_t - u_*$$

$$\downarrow (\sigma_e)_3 = f(\sigma_{e_1}, K_o, OCR, etc..)$$

VARIABLE

$$\sigma_{t_1} < \begin{cases} = \sigma_{comp} & OCR > 1 \\ = \gamma z & 1.00 \text{ NORM. CONSOL} \end{cases}$$

REDISTRIBUTIONS !?

$$* \; u < \begin{cases} u \text{ const.} \\ u \text{ FLOWNET} \end{cases} \quad \int \Delta u \; ? \quad \Delta u = FLN_1 \to 2 \to 3$$

$$\Delta u \to \Delta\sigma_{e_3} \text{ AT CONST } \sigma_t \quad **$$

TIME RATE OF CHANGE OF $\sigma_{e3} \to -ve$

FAST → CRACK

SLOW → SWELLING

** RATES OF RESERV. FILLNGS

(FIRST) (DURING LIFE)

INFL → OUTFL

PORE FLUID ΔV

Fig. 2

a σ_3' due to arching and due to residual stresses from compaction and so on. Now, σ_3' is equivalent to the compaction pressure, under compaction during construction and during the first few lifts above it. That is, we start with over-consolidation ratio OCR higher than one and thereby a very different K_0' coefficient. Incidentally I mentioned that this is variable. Many engineers think that K_0' is a constant of a given soil. It is not. It is far from it. Incidentally we do not know what K_0' are at microstrained conditions, because we have never been interested in investigating microstrained conditions, which are the ones that really matter in this context. As far as a tendency to form a crack is concerned, we are microstrained. Well, over-consolidation was due to compaction stresses, and then it gradually tended to the $K_0' = 1 - \sin \varphi'$, which people use in the simple computations. I am terribly afraid, a lot of people have always simplified for the past 20 years these problems.

The pore pressures u change very much from end-of-construction to your flow nets, so that is the Δu. The Δu itself does change from, what we might call, flow net 1, to flow net 2 to flow net 3, as there is an advance of the wetting front from upstream, and that is one of the reasons why we are concerned about first filling. Of course we are more concerned about first filling because the first time of a different stressing always produces greater effects : but why should the first filling be the only filling we are concerned about? We have been talking of ageing in clays and so on. Well, a refilling after 8 or 10 years can find the material more brittle due to the thixotropic regain. We can find redistributed stresses and so on, and can find the inflow minus pore fluid compressibility versus outflow situations such that they tend to force more water in than can get out. What does nature do then? She says, " I am terribly sorry, I am going to crack on you ". That is all. Well, time changes this problem of Δu as a function of $\Delta\sigma_3$ and so on at constant total stress. Time changes $\Delta\sigma_3$ becoming negative, which is the dangerous condition. If it is fast, it can crack. If it is slow, it can merely tend to swell. To tend to swell we would have to examine the condition of a very simplified logarithmic diagram of consolidation with unloading and swelling. The materials tend to swell very differently, if they are inert filler type or if they are hydrophillic clays. It is the hydrophillic clays that are more generally thought of as being the more plastic, but they have two terrible problems : one is that they can form slickensides during compaction, and on a slickenside the swelling is essentially nil and very much slower. Whereas, if you have a remolded hydrophillic clay, it can swell very much and very well. That was the case that saved us in the Jaguara Dam. So the rates of reservoir filling can and should be important not merely during the first, but also during subsequent unusual cycles throughout reservoir life. So there are problems of hydraulic fracturing that do change somewhat from first filling, which is the worry of most people and also of myself, and subsequent refillings of significant variation. Thank you very much. There are many things to comment on, but I think that the most important thing of all is to find that this audience is so keen on debating.