

expansion, then you will certainly have destroyed the structure in that process. There are examples in other materials which, I think, show this quite clearly. So that anything I said is subject to that problem: a partly saturated soil will expand; a saturated clay does not expand if it can carry the necessary suctions.

A residual soil will always expand on sampling. How much damage does that do? I don't know, and it could, I agree, be significant.

CHAIRMAN

Thank you. Any other comment on that from the panel members, please? No? We have Prof. de Mello down on the left.

PROF. VICTOR DE MELLO (BRAZIL)

I would like to take this opportunity to mention that Dr. Brand and the S.E. Asian Society are conducting a special study on Undisturbed Sampling and Testing of Residual Soils. It has long been concluded that there is no single technique of undisturbed sampling for all soils.

Prof. Vaughan has just mentioned that in saprolitic, unsaturated residual soils the potential for expansion would obviously be different than that of saturated soils, which immediately develop negative capillary pore pressures.

Taking advantage of Dr. Brand's presence, I would like to mention that, to my knowledge, for residual saprolitic and lateritic soils, there has never been a systematic study to distinguish between intact, perfect, undisturbed (to different degrees) and remoulded samples (such as conducted by Skempton, Lambe, Ladd, Seed, Davis, Poulos, and others).

One of the reasons for that might be the attempt to emulate something like the famous SHANSEP (Stress History and Normalized Soil Engineering Properties), from the MIT group, which is an extension of what Taylor did in 1947. I would take the liberty of saying that the name should be ASHANSEP, where A would stand for "assumed", because we know essentially nothing about initial

stress states. And we know even less about soils derived from rock weathering, laterization, etc.. Within this context, and recalling what Peter Vaughan has said about yield stress and destruction of bonding, I would ask whether it is even licit to think about that sort of logical reasoning, which originated in Taylor and is behind SHANSEP (or ASHANSEP): that is, to test at significantly high stresses (even using multiple stage tests), later to extrapolate back to the region of (lower) stresses of interest to a specific project. Would Dr. Brand please inform us whether some progress has been achieved in this respect? In Stockholm I had the opportunity to suggest that we should substitute quantifiable disturbance indices for the current qualitative descriptions. This is not being done yet, not even for sedimentary soils. I recently learned that MIT is developing a technique for classifying degree of disturbance on the basis of X-rays, which I repute wrong: there may be pre-fissure states that are not visible to X-rays, so that an unquantifiable state persists.

CHAIRMAN

Thank you, Professor de Mello. You seem to have addressed some of those questions to me. I am not really sure I am the proper person to answer them. Very briefly, indeed, the work that we did on sampling in residual soils - I'm sorry to say, in view of your comment - was largely of a qualitative type. We were looking for intactness of sample from relict joints, sample recovery and the like. And I can just report that, as far as that is concerned, I think we made a lot of progress. I think we know how to obtain a complete sample, as far as its constituent parts are concerned. As to quantifying how good those samples are, no, I'm afraid I have no information. We did do some in situ shear box testing, by the way, that did give us some interesting comparisons between in situ block strengths and laboratory strengths, but I'm sure that is a topic that others here are going to touch upon.

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CHAIRMAN

Thank you very much for your contribution. Our time seems to be too short to discuss all the topics presented by Mr. Barata. Many other important problems have been raised here. For instance, the life span of filters and their position inside dams. Since the President of the International Society is a man of a deep knowledge on these problems, I would like to call him for his contribution.

PROF. V.F.B. DE MELLO (Brazil)

Thank you very much, I am really going to address you as the President of the International Association. We always learn pretty much with ample discussions, nevertheless I would like to remind, more specifically for the works of the International Committee of Lateritic and Saprolitic Tropical Soils, that our intention would be to try to find out which are the behaviour phenomena that are somehow striking, unusual, in tropical soils, when compared with other soils, other dams which were not constructed with lateritic and saprolitic tropical soils. Otherwise, we would get lost trying to cover all soil mechanics or all subjects related to dams.

The duty of this Technical Commission, according to the Terms of Reference proposed by the President of the Commission himself, and agreed upon by me, appointing this Commission, the aim would be basically to compare the behaviour of the conditions of lateritic and saprolitic tropical soils with other soils. Therefore, as I have said today when we discuss "landslides" we have a representative of our own, or several of them, from Colombia, Peru, or Brasil etc, in the Committee of Land Slides, a Congress of Landslides has been held in Toronto, we also have several representatives of different subjects of our countries in other Committees and vice-versa. The basic goal of this Committee should have been this comparison and as I mentioned this morning when we compare landslides, what matters in fact is not only restricted to that specific area subjected to slides, but also the surrounding area why it did not slide down also. When we are

discussing certain behaviours, it would be necessary to see to what extent, other ordinary soils treated, non tropical , behave in a similar manner. It is not our purpose or ambitious aim to talk about soil mechanics as a whole. I just would like to ask that for the Final Report we should not go beyond the point where critics may start to take shape.

I am sorry for addressing you at this moment, but I think that we are enthusiastically discussing a subject, and that we all agree with several theses but it would be noteworthy to see to what extent these theses are actually peculiar and not applicable generically for all cases and all soils.

Thank you.

CHAIRMAN

Thank you very much Prof. de Mello. Unfortunately our time is over and I would like to call our General Reporter to make a brief comment and to point out some conclusions on today's session.

Mr. MEJIA (Colombia)

I would like to make a summary and state some of the basic topics for discussion. Among these is laterization of filters that I consider a phenomenon we have little knowledge of, and that may turn into a problem. Also the position of filters themselves. It is also important to see to the necessity of having an organized Sub-Committee effort to better define what laterite really is. That leads us to a purely semantic question. When you see a title of a paper and also the text in it, three or four people understand it differently first because we have led ourselves to an exaggerated tecnic level. It is necessary to simplify it and speak the same language. I believe this shall settle most of our main worries.

I was about to forget another extremely important topic which is the necessity of getting laboratory tests that may represent field conditions in a better way. That is all. Thank you very much.

CHAIRMAN

Would the panel like to comment on hardening?

PROF. NOVAIS FERREIRA (Portugal)

In 1963, I spent forty days to define what is laterite. I appreciate very much your summary of your definition of the laterite, but the question is not this; if you classify laterite as a tropical soil, the definition is a scientific one. But in this congress many people are speaking about soils with lateritic behaviour. It is not the same. I think it is not possible for engineers to turn back the science and the scientific concepts. It is for this reason that I say that the geological definition and the pedological definition are a good help, and it is necessary to take them into account. But in this congress there are people speaking about lateritic behaviour, and I don't know exactly what type of soils have lateritic behaviour, if I don't test them in geotechnical terms.

CHAIRMAN

Is there any other discussion on that?

PROF. DE MELLO (Brazil)

I'm saying just a couple of words - the philosopher here is Milton Vargas - but it seems to me that the Holy Ghost has descended, and everybody is speaking in a multitude of languages, according to that biblical event, and everybody understands everybody else. But the biggest problem that we face is, I think, that science and technology give us a road that could be reverted.

Imagine somebody like, let's say, Marco Polo or somebody arriving in China in 1400, on whenever it was, and all of a sudden, doing some work that's supposed to describe all the soils in China, or something. For instance, let's suppose that we talked of all the soils in Russia as being the 'double ultra' soils (a few friends of mine know that I know just one word in Russian which means good morning). Well, from then on, you leave, go away, and leave that

Whole mass of people discussing just what is a double ultra soil. Well, and of course everybody will have millions of descriptions of how to know it, etc. etc... and everybody will be right and nobody will be useful. Well, it is very sad. It seems to me that Professor Novais Ferreira mentioned something that we should use, considering the day and days at which we are. We could invert a process. Let's forget for a moment exactly how to describe or how not to describe a lateritic or saprolitic, soil and so on and so forth. Let's begin by doing good, real soil engineering testing. And first start from the soil engineering testing results and then go backwards.

Forget what that Ghost may have been called, you can then remember all the other names in the world? Oh, everybody intended so. Forget, start the other way round. First get the real good engineering behaviour and then see to what it can be associated; and the second test is you work on. Not that when you predict something, that will happen, but when you predict something, that should not happen in other cases, because classification should meet both these tests. So in other words, we should not only test whether or not certain properties are really associated with a certain group or position in a plasticity chart or any rubbish like that, but also see whether or not the parts that are so outside are significant. Using those two tests in a scientific and technological manner, little by little perhaps you will come around to subdividing consciously this huge mass of soils into significant subdivisions. Nobody has tried to describe temperate soils for all of the developed world. Has it? Why should they be so ironical as to describe lateritic and saprolitic tropical soils for all of the tropical world? I think there is a little bit of this business of planting confusion by the Holy Ghost somewhere in us.

CHAIRMAN

Would our panel like to comment on that? That's what I would call the engineering approach, starting from the end and coming back.

PROF. M. VARGAS (Brazil)

I'm a philosopher, so I would like to give some philosophical comments about the identification in science or technology.

very good data on that and certainly I will be happy to provide it at that time to anyone who is interested. On the second point, do I understand you to say that you feel for design purposes we need information on actual infiltration and rainfall on the spot?

PROF. VICTOR DE MELLO (Brazil)

Well, let's suppose you have a high rainfall intensity, precipitation intensity. From a certain point on, your capacity for infiltration has been all taken up. Thus, from a certain point on the importance of precipitation intensity should level off, because all of it will be turned out into runoff. It seems to me that for lack of better data (as you pointed out) we are working with a coefficient that is presently the best one available. However, would you agree with the possibility of later on correcting some of your data, which is presently related to precipitation and not to infiltration?

DR. BRAND

I would agree, of course, that there might be instances in which the soil becomes fully saturated and from then on higher intensities simply run off. But, either failure will occur when it becomes fully saturated, or it will not, and if it will not, then it does not presumably matter what the intensity is. So obviously a particular intensity in Hong Kong occurs before saturation has occurred, presumably, that brings about our failures. So, it is possible that we could reach some threshold intensity, above which it would not matter what the intensity was. That's quite possible. But, as I say, the information that we have now gathered very painstakingly, shows us that our threshold is about 70 mm an hour, and if we go higher to 100 mm an hour, then we get a very large number of failures.

DR. SANDRO SANDRONI (Brazil)

There is a work (referenced in a book by Guidicini and Nieble), based on data collected once a day, in which it is concluded that if less than a certain percentage of the yearly average rainfall takes place within a 24 hour period, accidents are likely to happen. However, if that percentage is higher, then accidents are almost certain. I believe these Brazilian data support both points of view expressed this morning. I would, however, like to have some clarification on the 70 mm/hr. threshold. Does it mean that no slides occur below that threshold? Are there instances in which no slides occur even beyond that threshold?

DR. BRAND

Above 70 mm per hour, in Hong Kong, we always get some slides, and the number increases rapidly as the intensity in one hour increases. Below 70 mm an hour, we still sometimes get some

DR. WILLY LACERDA (Brazil)

I agree with the comments by Prof. Vargas regarding the type of movement. I myself observed several colluvium movements which occurred long after periods of rainstorm: in most cases this was due to relatively impervious surfaces, underlain by more pervious layers, which absorbed water from distant regions, thus requiring a long time to build up de-stabilizing pore pressures.

DR. BRAND

We do, very occasionally, in Hong Kong, have a failure that occurs some days after the rain has stopped, but that is very much the exception; it usually occurs where we have a very large catchment area up behind the slope, and probably by the mechanism that you described, where water spends a long time reaching the point where it does the damage. But I would still maintain that the majority of failures occur suddenly and they occur during the rainfall. That is my observation not just in Hong Kong, by the way, but in a number of other countries as well, certainly the whole of S.E. Asia. I would be interested to hear your views on this, especially your experience in Brazil.

PROF. VICTOR DE MELLO (Brazil)

You mentioned the coincidence of some of these principal catastrophic slides occurring at the time of high intensity of precipitation or rainfall. And meanwhile all of your talk emphasize, and many of the discussers have quite emphasized, that the principal problem is infiltration. Now, since obviously there is the very recognized equation: precipitation equals infiltration plus runoff, I would be very much interested to find out whether you have gathered more information with respect to what I consider to be our principal lack of data, which is how do infiltration coefficients vary with different surface covers, inclinations and so forth, and whether or not you would agree that your initial data should be revised on the basis of some real infiltration corrections on the precipitation data. It seems to me that we are working with the negative of photograph rather than with the positive.

DR. BRAND

I am not sure, Victor, about the second part, but on the first part, it is slightly premature to ask me that question. As you say, there is a total lack of information on infiltration and the effect of different surfaces. In fact, there is almost no information available, and because of that we have instituted in Hong Kong some experimental plots with different kinds of vegetation cover and different kinds of hard surface covering to measure runoff, and infiltration, and suction changes in rainfall, all at the same time, right on the same spot. As yet that is only under way and I haven't got any data to report to you. I would think that in one year's time we should have some

TropicalS - Brasilia 1985, Vol. 3. Geot. Eng'g. in
Tropical Residual Soils, pp.96, 97.

One talus feature is that, it is always or almost always saturated and consequently very instable.

Finally we have the alluvium that occurs on river banks.

These situations are very frequent in Brazil and I believe they are also very frequent in any other countries with big rivers or intense rainfall. Finally, I'd like to make a brief comment on Mr. Cláudio Wolle's presentation on the Imigrantes Highway. I agree that Serra do Mar has natural slopes that are very instable. Actually we have already had the opportunity to discuss the matter.

Serra do Mar should be faced as an unstable slope. However, the problems observed on Imigrantes Highway were mainly due to the construction of inadequate access. That is, what started most of the sliding accidents on the highway was the access road built practically without any protection.

We must also remember the Caraguatatuba case, where there are references of an earthquake happening at the time of the slidings. A small earthquake would have occurred near Caraguatatuba. These are references I have no means of confirming except recalling bibliography on the matter. Thank you.

GENERAL REPORTER

Thank you, Mr. Vaz. There is some confusion of terminology between talus and colluvium. Before I call the next speaker, I would like to call Prof. de Mello to say something quickly.

Prof. VITOR DE MELLO (Brazil)

Thank you very much.

I would like to avoid our wasting time with terminology. The three sister societies: International Society of Soil Mechanics and Foundation Engineering, International Society of

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Rock Mechanics and International Society of Engineering Geology,
have a committee working on a joint nomenclature system and, by
coincidence, within the next two weeks, I will be going to London
and Brussels for the first time, to have an official meeting of
the three presidents.

We have a single committee called the "lexicon committee",working
on terminology and all these words that have been mentioned as
part of the Brazilian experience or not, are Latin words used all
over the world. We are not going to establish a different
terminology here in Brazil when the three societies together
establish a joint system.

So, I'm in favour of not wasting time and I agree entirely that
all of us know more or less what is being said. So, let's go
ahead, because the exact words will be chosen by the committee
members and all the people who have strong opinions on these
matters should send their opinions to the respective committee
members.

Thank you.

GENERAL REPORTER

Thank you very much for the comment. Yes, indeed. It's also with
the symbols. When the International Societies give us the list of
symbols, we'll try to follow the standard and it will make things
a lot easier. Thank you very much for your comment. I use talus
and colluvium interchangeably and probably I am wrong in doing
that.

Let me now call Professor Milton Vargas to speak to us.

PROF. MILTON VARGAS (Brazil)

I am surprised that the expression of slides, of land slides was
mentioned as one single phenomenon. But, in reality, there are