the Water in the Soil – Part 4

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What I want to do in this article is tidy up a few things about liquefaction before I move on to trying to sort out pore pressure generation within a saturated aggregation of soil particles. First, I'll suggest why silts do not seem as prone to liquefaction as sands. Then, I will look at some good triaxial testing to see if there is any support, or conflict, between this hypothesis and those laboratory findings. After that I'll touch on the possible different effects earthquake induced shear waves and surface waves might have on liquefaction behaviour and structural responses.

Can Silts Liquefy ?

It is in attempting to answer questions such as this: "Can silts liquefy ?", that the utility of a new physical model of two-phase soil behavior can be evaluated. So here I'll attempt to use the L-factor and the "soil" components of the drag force to see if I can explain what's special about silts when it comes to situations where sands would be expected to liquefy and silts seem not to. With this in mind I'll use the pieces put in place already in earlier articles to see how this question might be answered.

In Figure 6 (Part 2) I suggested that particles of a size which can reach their Terminal velocity [v_T] within a fall distance of less than 29% of their diameter are inherently vulnerable to liquefaction. The reasoning behind this is that this is the distance uniform spheres move downward while changing from the loosest packing arrangement to the densest. Silt sizes are all well below this red line, so the implication is that silts are extremely likely to liquefy when going from a loose to a dense packing.

As can be seen in Figure 8 (Part 3), for velocities at v_T, the L-factor is zero over the full silt size range. That means, according to this model, that the Pressure component of the Drag force plays no part in silts under liquefaction conditions, and hydrodynamic resistance to relative velocity (particle movement) is fully accounted for by the Bearing component alone. It implies that when a silt size particle falls enough to have liquefied it does not result in the generation of pore water pressure, as a sand in similar circumstances would, but instead results in a viscous response which I have equated to bearing resistance of a cohesive soil. So, rather than liquefying, a loose silt deposit would tend to consolidate. And since there would be no generation of excess pore water pressure to produce a critical gradient, there could be no manifestation of concentrated venting through local weakness at ground level.

Along this line of reasoning one might wonder if the same way of looking at the behaviour of fine particles might have some involvement in permeability, consolidation and creep.

Before leaving silt there is a point I want to make: The abrupt discontinuity I have shown for the L-factor at R_E = 0.6 doesn't seem quite right to me. There isn't the gradual change from one mode of behaviour to the other that I expect to see in physics where there aren't very different material properties across the boundary. What I'm inclined to think is that this discontinuity is perhaps because our colleagues in Fluid Mechanics didn't, or couldn't, define the values accurately in this area of conjunction. But, as I don't have the ability to come up with my own values, I will settle for theirs and trust that someone else may follow up on this. I suppose I'm basically expecting to see the gradual sort of changes we get in silt and sand sizes as we cross the 200 mesh sieve – nothing startling.

A final word about silt liquefaction: It has been reported that the loess flow slides which resulted form the great 1920 Kansu earthquake in China involved silt liquefied in air. But with no water present, "liquefied" is hardly the word for it – perhaps "fluidized". In any event I think this behaviour is consistent with the above understanding since the viscosity of air is less than one fiftieth that of water. In consequence, the air would not have the bearing capacity to prevent the coarser silts from falling downwards, thereby leading to generation of increased pressure in the pore air beneath them. And I imagine that if such pneumatically charged air were entrapped within the body of the sliding mass it would add mobility to the motion.

Triaxial Testing

James K. Mitchell

Sometime after Professor Mitchell moved from Berkeley to Virginia Tech he was kind enough to write me with a critique of some of these ideas. While he was encouraging about my bearing capacity analogy for small particles he was troubled about what seemed to be a logical inconsistency between laboratory results and saying that motion was the source of pore pressure. The point he raised was: In an undrained triaxial test, when there is an increase in pore water pressure recorded during a test, how could that pressure increment still remain after specimen straining was stopped, if motion was the only reason for pressure generation in the first place? I believe this is a question which is likely to arise again, so I feel the need to address it now.

One response might be just to point out that what's going on inside the membrane of an undrained test is directly analogous to stopping the piston's advance in a hydraulic cylinder which is not leaking. But I think it is more useful to look at the triaxial apparatus itself. The cell pressure is transmitted across the membrane to the soil particles and also to the water inside the sealed specimen enclosure. The force radially inwards at any stage is equal to the membrane surface area times the cell pressure. The outward balancing reaction to this force is the summation of the pressure increments on each particle in contact with the membrane, plus the pressure on the remaining area of membrane in contact with the pore water. During the test, while straining is being imposed, these forces and pressures change depending on how the soil-structure dilates or contracts. But the moment straining is halted these values are "frozen", and apart from any subsequent creep of the soil-structure which might occur within the membrane, I can see no reason for the pore pressure at the end of the test to diminish in value. It just sits there.

Gonzalo Castro

Dr. Castro's research work at Harvard must surely rate amongst the best and most significant geotechnical laboratory work yet performed. Figure 10 is a photocopy of his "Fig 22" from his

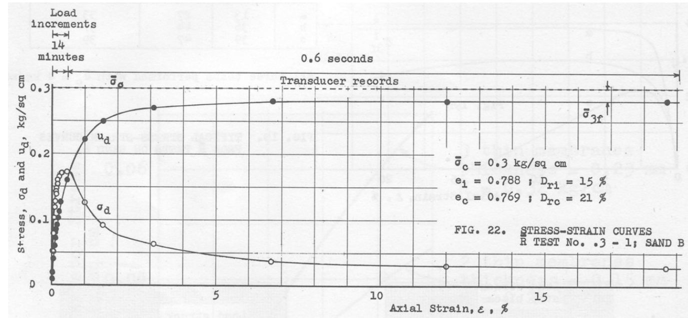

Figure 10: Castro's Fig. 22 from his Harvard Soil Mechanics Series No. 81

work published in 1969 as *Harvard Soil Mechanics Series No. 81.* It shows the stress-strain record of a consolidated undrained triaxial test performed on a specimen of his sand type B. Liquefaction was brought about by monotonic axial compression. Here u_d is the pore pressure change induced by application of deviator stress σ_d . The axial load was increased gradually over a 14 minute period by adding dead load increments. When the load exceeded the strength of the soil-structure it failed in an instant. One thing that I find particularly informative here is that approaching the point of failure the pore pressure is only about half its final value; it is only after failure of the soil-structure that it rose to about 93% of confining pressure. In fact I believe the pore pressure increase prior to liquefaction may be attributable to changes in the proportions of the membrane interface with particles and with water as the soil-structure tries to accommodate the increasing load. It is also apparent on this data record that much of the pore pressure increase happens while the particles are collapsing.

Yoginder P. Vaid

The work done at the University of British Columbia under Professor Vaid's guidance in the early 90s was most useful and enlightening to me. Consolidated undrained triaxial testing of Fraser River sand showed quite clearly that whereas this uniformly graded natural material was dilative in compression at even the loosest (pluviated) densities, it could be brought to liquefaction at relative densities up to 40% when subjected to axial extension. The importance of this radically different behavioral response to stress path will be discussed below with respect to wave forms created during earthquakes.

Triaxial Results in terms of Fall-to-Diameter Ratio

In Part 2 the Fall-to-Diameter ratio [F/D] was introduced as a numerical criterion for assessing the opportunity of individual spheres to reach v_T based on their diameter, in comparison with the amount of space available for them to fall through water as their packing arrangement changed from a loose state to a dense state. In fact what I used were the maximum and minimum void ratios (e_{max} and e_{min}) of idealized arrays of uniform spheres. There I gave the value of F/D as 0.29, which is the numerical value of the exact mathematical solution, $1-1/\sqrt{2}$, for this change in position. Another way of arriving at this same value is to consider the ratio of downward displacement of the centre of gravity of a saturated mass of uniform spheres per unit height of the initial assemblage. This can be expressed as $(e_{max} - e_{min}) / (1 + e_{max})$ which is also equal to $1-1/\sqrt{2}$, since $e_{max} = 6/\pi - 1$ and $e_{min} = 6/\pi\sqrt{2} - 1$. Taking advantage of this correspondence I decided to plot the results of both Castro and Vaid in terms of what their specimen void ratios suggested about this type of equivalence to F/D, by replacing e_{max} in this relationship with the loose void ratio at which the specimen was prepared.

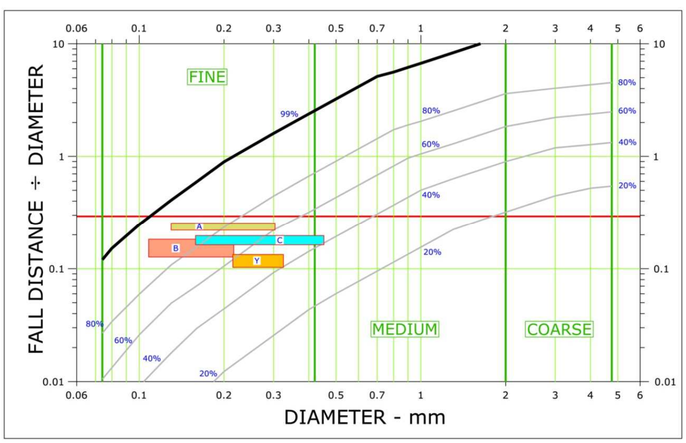

Figure 11: Castro and Vaid results on Fall ÷ Diameter plot

Figure 11 is based on the same computational approach used to make Figure 6 in Part 2 of this series. Here it has been drawn to a larger scale since it is only sand sizes I want to look at. The heavy black line labeled 99% identifies the ratio of the distance a spherical particle must fall in relation to its diameter [F/D] in order to transfer 99% of its buoyant weight to the water it is falling through. Similarly the other percentage labels are for lesser amounts of weight transfer to the water.

The three rectangles, labeled A, B and C are thus derived from Castro's three sets of triaxial tests over the range of densities where the specimen liquefied under monotonic loading. The letters are the same as those used by Castro in naming the three different sands he used. The rectangle labeled "Y" is for the extension tests which resulted in liquefaction (steady state stress-strain) of the Fraser River sand Vaid used.

Table of Equivalent "Fall-to-Diameter" Ratio Limits (values plotted as rectangles on Figure 11)

Sand Type		Size mm		Fall ÷ Diameter	
Label	Source	D_{85}	D_{15}	Upper	Lower
A	Salt Lake earthfill	0.304	0.130	0.254	0.220
B	Ottawa Banding	0.217	0.108	0.183	0.127
C	Huachipato Beach	0.452	0.159	0.197	0.163
Y	Fraser River	0.325	0.215	0.134	0.103

The vertical sides of the rectangles are at the D_{85} and the D_{15} gradation sizes for each particular sand type. The upper and lower horizontal sides of the rectangles cover the range of void ratios at which specimens were made, and which resulted in liquefaction failures. These values are listed in the Table, where it can be seen that soils fall into the category of fine sands and the equivalent F/D range lies between 0.1 and 0.25. Here it is necessary to point out that although some of these numbers are quite close to the red-line value of 0.29, and might be considered as providing some support for this proposal, this is not the case. Instead, they need to be compared with the values along the black/grey curved lines representing the percentage of weight transfer to the water.

As may be seen from this mode of presentation the losses in effective particle weight range from

40% to 90% with the average being somewhat less than we would expect during a liquefaction failure. I believe a better interpretation of this plot requires consideration of the Crowding-factor [K], since the % transfer lines are based on single particle responses, whereas here we are for the first time dealing with the soil mass. "K", which will be subsequently introduced and developed in Part 5 of this series, is essentially an amplification factor on relative motion. As such it has the effect of reducing the amount of fall necessary to achieve a particular level of weight transfer, and therefore, should bring the curves more into line with these laboratory results.

Shear Waves and Cyclic Loading

Computer programs which deal with the transmission of shear waves through soil, such as SHAKE, have proven very useful (and surprisingly accurate) in predicting how tall buildings move/sway about in response to earthquake vibrations. These programs are based on how small strains of different frequencies would be either amplified or attenuated as they pass through a stable/intact soil-structure. I doubt if the original authors would have condoned their use for soils which were strained to the extent that they were collapsing. However that may be, what is known for sure is that shear waves cannot pass through a fluid, and this presents a problem when dealing with soil we expect to liquefy. Presumably that part of the vulnerable deposit closest to the excitation would be fluidized first. Then the question arises as to how and why would liquefaction trespass beyond that boundary. Surely it couldn't.

The complementary laboratory testing, which involves cyclic loading, I find equally difficult to accept inasmuch as it bears on liquefaction. Apart from believing that such testing would have application only in the case of shear wave transmission, the idea of subjecting saturated sand inside a sealed membrane to as many as a 1,000 stress reversals has always struck me as some kind of abuse of specimen: For some reason or other it makes me think of those bad days in medieval times when confessions were extracted by torture.

As I visualize it, stress reversals result in grain asperities been broken off. These small pieces/dust are not large enough to remain part of the soil-structure. As a result the specimen gradation tends to become gap-graded. After grain damage progresses and the resisting soil-structure's volume becomes effectively smaller, I think it inevitable that the inside of the membrane becomes increasingly more in contact with specimen water that with specimen solids. I should stress here that what I mean by specimen solids are only those solids comprising the soil-structure, and not including the dust from broken asperities. As the balance of water forces across the membrane depends on the summation of the membrane areas resting on either phase, it seems obvious that the opportunity exists for water pressure inside the membrane to escalate. And this, coming about without the need to invoke soil-structure weakening, but rather because of reduced effective lateral confinement. To this we might perhaps add the possible membrane ballooning because of inside temperatures rising from energy expended (work done) by the deviator load repetitions.

I realize this is heretical thinking, but I gained some confidence in this position by the encouragement offered when, towards the end of 1998, Ralph B. Peck wrote me saying: "*I share your feeling that much of what we think we know about liquefaction is an artifact of our tests.*"

Surface Waves and Soil Extension

Just after the Loma Prieta earthquake in October 1989 I went down to California to look at the evidence of damage. Apart from the well reported details, two things struck me as odd and requiring an explanation which didn't seem to fit with the current way of looking at the propagation of energy from the epicenter in the Santa Cruz Mountains to structures in San Francisco about 100 km away.

First was the liquefaction of the dredged sand foundations in the Marina District. As far as I knew attenuation of shear waves were supposed to leave them with little residual energy once they had travelled about 30 km, so how could such destruction be wrought 96 km away? The answer is I believe that it wasn't the shear waves that caused liquefaction, it was the Rayleigh component of a surface wave that did it. Rayleigh waves can travel great distances - they are the geotechnical equivalent of a tsunami. Also, this surface wave, because it causes the ground level to be temporarily super-elevated as it passes would result in elongation of the soil column (ground profile) thus producing a stress path similar to the one replicated by the Vaid triaxial extension work. And as we saw above, soil extension is a most effective way of precipitating liquefaction.

The second situation which made me wonder was what might have gone on in the ground under the Cypress section of the Nimitz Freeway (I-880) to make the upper deck fall as it did. In his geological narrative *Assembling California*, John McPhee describes it thus:

"*The under road is northbound, and so is disaster. One after the last, the slabs of the upper roadway are falling . . . A man in another car guns his engine, keeps his foot to the floor, and races the slabs that are successively falling behind him*."

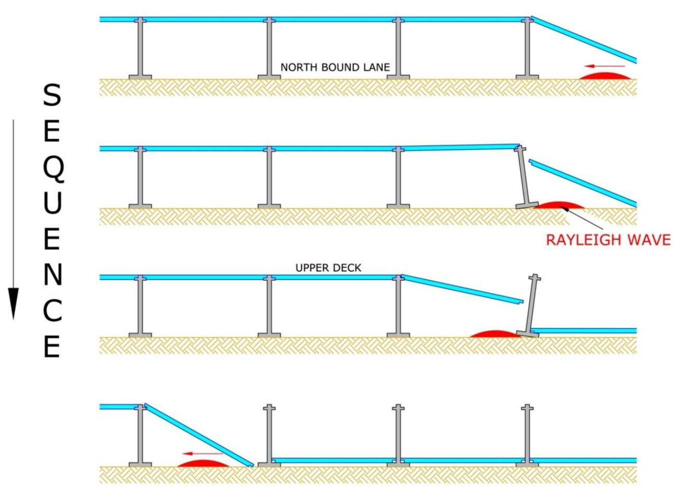

Figure 12: Speculative failure mode for I-880 upper deck

It is very tempting for me to believe this lucky individual was racing the surface wave radiating out from Loma Prieta. The orientation of the freeway is consistent with this idea, so in Figure 12 I've drawn a cartoon of a possible mechanism which seems consistent with the above evidence. The red bump moving to the left (towards San Francisco) represents the surface deformation due to the seismic wave. Unfortunately, I find the math-physics necessary to calculate the speed of the Rayleigh wave in this particular soil column too intimidating to attempt, and therefore can't say if a car could stay ahead of the wave for a while in this surficial geology.

The point I'm trying to make is this: It is far easier to explain these events by looking at the surface wave and soil-structure extension rather than by examining the damage as if it were caused by shear waves and cyclic loading.

in the Next Article

It is now time to move beyond single particles and approach more closely the practical goal of this series, and that is the generation of pore water pressure in real soils. The next necessary move is to account for the magnification effect due to the crowding of particles inherent in an aggregation of grains packed closely together in a mass. So in the next article I will develop what I call the K-factor.