Calgary Geotechnical Society Presentation

Topic: Analysis of Static Liquefaction with PLAXIS: 1974 Tar Island Slump
Speaker: Mike Jefferies, *P.Eng.*
Date: Wednesday, November 17, 2021, 12:00pm - 1:30pm (MST)

Questions from the audience:

__General__

1. **Do you have a reference for the dam failure in Portugal - especially the part about the loading set up by the bulldozer?**

A: It is a confidential Golder project and not released for publication; it can be accessed by those working for Golder on a confidential basis – please email me from within Golder for the link. But, there is no real need for the data as the point I was making was simply to support the apparently surprising observations reported by Plewes et al from my own observations elsewhere.

__Critical State and Modelling__

2. **Casagrande's theory is that the critical state line is unique. This makes sense for common sand. If one increases the amount of clay within the soil, is the critical state line still unique for the material?**

*A: **YES !** Please go to this link for the open-access (ie free download) documentation* www.icevirtuallibrary.com/doi/full/10.1680/jgeot.20.P.228

3. **How much error would be caused by assuming the cone tip penetration as a rigorous spherical expansion in the interpretation?**

A: A lot. As engineers, we know this rather more in the case of clays where the theoretical cavity expansion is Nkt ~ 9 whereas the practical range is 12 < Nkt < 18 from calibration to other strength measurements; thus, there is the potential for getting the undrained strength unconservatively wrong by a factor of two. Whether drained or undrained, cavity expansion needs scaling to actual CPT geometry.

The basic problem is that the friction on the CPT sleeve and extending up along the rods for at least 500mm shows up as an increase in the apparent confining stress on the tip – and which we cannot deal with, at least at present, in a spherically symmetric solution.

What I did not mention in the talk is that some workers are now moving beyond cavity expansion and directly simulating CPT soundings with the real tip geometry. Mason Ghafghazi at UofT is getting interesting results, as is also next weeks speaker (Marcos). I think this is the way forward.

4. What is the uncertainty in the model input parameters? Is it important to know this?

A: I do not see a great deal of uncertainty in the M, N, lambda etc soil properties themselves. But, we did not explore the effects of, say, changing M on the computed failure. Rather, we felt that there were three far larger uncertainties which needed explicit consideration:

i) *The tailings ψ, which includes issues around the role of loose soil within a distribution of soil state: the EuroNorm 'characteristic' value; and, you may make a different judgment to us. What we did was to apply everything at face value with no "corrections" (ie fiddles)*

ii) *The nature of the stratigraphy and now drowned 'Beach Above Water'. The slump was pretty much the entire length of the cell and when I look at Ed's photos from the era I wonder about whether the stratigraphy shown in Plewes et al might be a little too idealized. Of course, we (or you) could run more scenarios but our goal here was not to keep going to get a perfect match but instead simply model using things at face value.*

iii) *Gmax, which was a huge surprise to us. We have good measurements of Gmax, but a mismatch between what we need to match lab tests and what was needed to capture the field-scale liquefaction. This is an issue for all of us, and our paper does not present "The Answer"; rather, we conclude there is a factor that is presently not considered in the liquefaction literature.*

Thinking as I write this reply, I wonder if we should not have explored uncertainty in the plastic hardening modulus H. None of us would be surprised by the idea that soils insitu have time to develop "structure" and which will manifest itself as a stiffening of the plastic modulus.

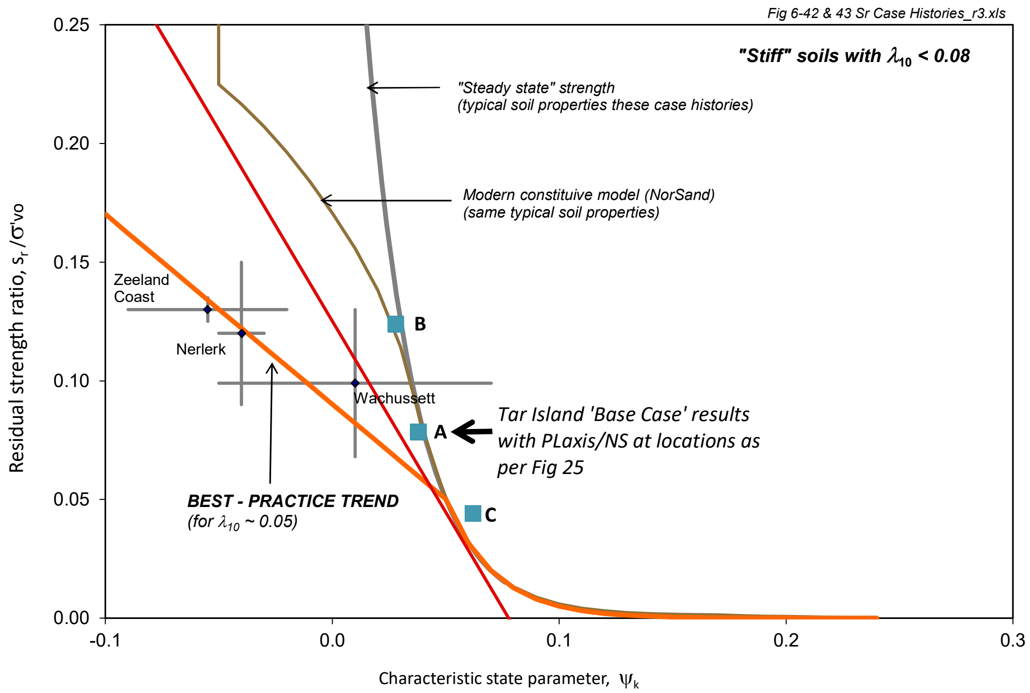
Keep in mind that we started this Tar Island study as an exercise in validation of numerical methods rather than an optimization of understanding the details of the event. More can be done to 'fine tune' the understanding, but that was not our goal. Rather, we 'nail' most aspects on the first-pass with an unbiased assessment of the 'facts', and that is validation of the methodology.

5. How do the strength ratios computed compare to case history data, as plotted e.g. in Jefferies and Been 2016?

A: Very insightful question. We pulled the numbers for the 'Base Case' scenario from the Plaxis/NS output, as things evolve during the drained stage-construction, which are summarized in this table:

Location	*At end of drained loading*		*At completion of liquefaction*	
	ψ_k	σ'_{v0}*: kPa*	s_r *: kPa*	s_r/σ'_{v0}
Point A	*+0.038*	*228.4*	*17.9*	*0.078*
Point B	*+0.028*	*141.7*	*17.6*	*0.123*
Point C	*+0.062*	*35.2*	*1.6*	*0.044*

These values are plotted up on the 'Book' figure below, and fit nicely on the NS trend (which depends a little on λ, with this Book figure using 0.05 whereas Tar Island analysis used 0.065).



There is an interesting corollary of this figure. What we see with Plaxis/NS is a very detailed capturing of the event and the post-liquefaction movements in particular – with the mechanics using all at face value. Conversely, the standard for assessing s_r from case-histories, introduced by Seed senior and followed by many other workers, is limit equilibrium on the post-failure geometry (with or without inertial corrections – depending on the investigator) – and these methods produce strength ratios that have little resemblance to the basic measurements we can all make. Seed and those following him have asserted this is because of errors/simplifications in critical state theory. The reality is, based on the Tar Island simulations, is that the error largely lies with the extreme simplification of mechanics within the limit equilibrium method. An additional factor that may also be involved is soil mixing.

At Cadia, the insitu tailings showed some layering with sandier zones within a more average profile. The large movements during the liquefaction mixed it all together, and that changed the CSL. The figure below is taken from the ITRB report and compares the insitu CSL for 'sandier' and 'predominant' gradations with the CSL of the mixed soil found in the run-out zone. The effect of this mixing is an apparent shift in the CSL – and thus ψ – at constant void ratio. What Seed and those following his lead have done is to compare apples with oranges: the effect at Cadia is a shift of about $\Delta\psi \approx +0.04$, and if you apply that shift to the case histories on the above figure things get a whole lot closer to theory.

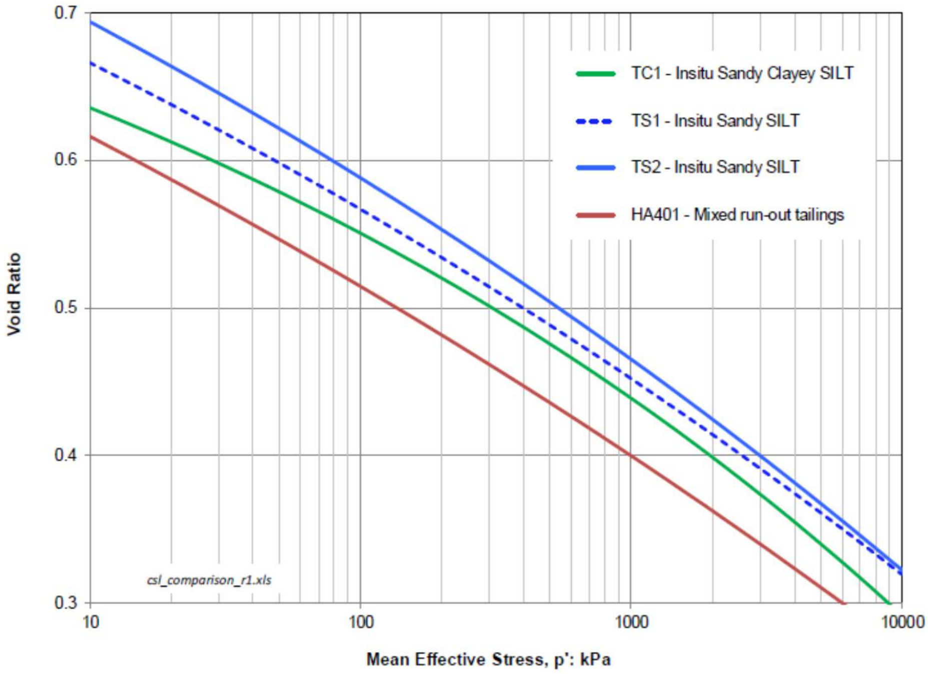


Figure 5-14: Comparison of CSL for NTSF tailings

Of course, the Tar Island slump was really rather contained (indeed, Tar Island is properly referred to as a 'slump' rather than 'liquefaction slide') and its 4.8m of settlement is a vastly different situation to the large slide at Lower San Fenando dam and many other of the 'standard' case-histories. So, perhaps less surprising that the large-strain mode of Plaxis/NS did a very good job of modelling the Tar Island slump as it was well-within the capability of the method.

6. Based on the plot of Ghafghazi's work, it seems the calibration of CPTwidget was done in dilative materials with negative state parameters. Does the CPTwidget still have a good correlation in contractive materials?

A: The Cadia validation I showed, which can be found in the ITRB report online as Fig E4-5 with associated text about what we did, is for contractive soil at approximately $\psi \approx +0.07$. I have not found any CPT calibration chamber data in truly contractive sand.

In a way, this is the importance of the Tar Island case-history: it tests all of our procedures, including the inferred state using the widget. And, for contractive soil of principal interest for liquefaction.

7. Why use a continuum-based program such as PLAXIS, rather than FLAC or UDEC from Itasca suite of products which might model the same thing with more simplicity?

A: The FLAC algorithm, at least to date, has not proved capable of following the propagation of liquefaction. Thus, FLAC is plenty good for looking at the development of triggering (and was used *for this in recent dam failure investigations) but is not up to understanding the propagation of liquefaction. To my eyes, the video I shared (and which can be downloaded from the paper as supplementary data) has a striking similarity to the security camera video at Brumadinho – view the two videos one after the other to see this for yourself.*

More generally, I understand that ABAQUA EXPLICIT works well with NS and is ostensibly the same algorithm as FLAC – where they differ is how the 'out of balance' forces are redistributed. My own preference is the viscoplastic algorithm as that mimics my sense of how soil redistributes load for integration points exceeding their yield stress, but I must admit the current Plaxis method worked very well; contact my co-author Sandro if you have questions about Plaxis. Dawn has implemented NS using viscoplasticity as a further development of the public-domain codes from the Smith & Griffith's text 'Programming the finite element method'; you can download Dawn's code as open-source software from the Soil Liquefaction book website (or email dawn_shuttle@hotmail.com).

8. If you were to apply one of the earlier lifts of Tar Island as undrained, would you have also predicted the development of excess deformations?

A: Not in the early lifts, but if you look at the stress-path for Point B (and which was located not quite at the initiation point) there was not much headroom to avoid liquefaction in the later lifts as the stress-path gets pretty close to the instability locus. My own sense is that the 'perturbation' was the bulldozer and I'd rather like to explore this case-history with slightly denser tailings with a more severe perturbation. It was all rather close, but you can see that too in the Plewis et al 'safe raising rate' plot.

9. Did you also consider G as a function of strain amplitude instead of using G_{max} alone, and would it help in the numerical prediction?

A: Theoretically, true elastic G cannot be function of strain. When people use 'strain dependent modulus', what they are doing is accounting for plastic strains. In a proper elasto-plastic approach (NS, bounding surface, or CASM) we compute the plastic strain explicitly so elasticity must be just elastic – that is, no strain dependence.

However, there is a reasonable question of wavelength of the seismic signal which seems in the order of 200mm – 500 mm for the typical millisecond impulse used and for ~200 m/s velocities encountered. How that relates to the sub-mm particle contacts is an interesting question that I have not seen explored. My own experience, which is SBP dominated, was reflected in Fig 14.

My sense is that the issue of mobilized M at low strain in the laboratory – which I flagged up at the end of the talk – may be the underlying issue. If M is low, we get much less plastic strain from the flowrule and that in turn allows us to use a larger G to get the measured stress-path.

Limit Equilibrium

10. **Does Limit Equilibrium Analysis still have value in new projects and feasibility level studies where sampling, budget and access may be limited?**

A: This is a question for all EoR… what concerns me, and should concern all of us, is that LE did not provide a basis to understand any of the recent large dam failures. From which I infer LE does not have great reliability for design.

What now should occur, if we are a true learned-profession, is that this Tar Island paper should be viewed as a challenge to our design methods leading to an appropriate investigation of the adequacy of LE. Keep in mind that our choice of Tar Island over the Cadia/Fundao etc case-histories is that the loading was simple with nothing else going on. This same simplicity also applies to a LE validation.

I mentioned in my talk than this whole critical state stuff was driven in the 1980's by about 30 engineers in Calgary working for Dome, Esso, Gulf, EBA, Golder and Hardy. It seems to me that a group of you within the CyGS should get together – with at least BGC/KCB/Golder included so that it becomes an 'industry' response – to analyze Tar Island 'triggering' using s_u *in LE. Much of the data need is in the current paper and I'm sure you will get permission to use the digital CPT data to develop the* s_u *(I imagine you might start with the Olsen correlation for "yield" strength, but it will be easy enough to add in strengths developed through critical state approach). The Geotechnical Research journal will provide rapid publication and will welcome a contribution of this nature. I might even use the this question as the title of the paper… !*

11. **Based on your experience, can limit equilibrium post-liquefaction analyses underestimate the required mitigation measures?**

A: I think the opposite: the current liquefaction literature is predicting too small sr if that sr is to be used as a lower-bound strength such that displacements remain small.