From: Carroll, John (BOS)
To: bobpyke@attglobal.net
Cc: "Angulo, Sunny (BOS)"; Board of Supervisors, (BOS)
Subject: FW: My response to comments made at the November 4 hearing on 301 Mission Street - File No. 210954
Date: Tuesday, November 9, 2021 11:11:00 AM
Attachments: Pyke letter to H & D.pdf
Pyke Response to Millennum Tower VSU Supplement No. 188 - addendum.pdf
Pyke Response to Millennum Tower VSU Supplement No. 188 - updated.pdf
Pyke paper 20ICSMGE - with final edits.pdf
Bob"s P-Y Diagram (004).tif
Pyke Paper for ASCE Lifelines Conference 2021-22 - final.pdf
image001.png

Thank you for your message and attachments. I am adding your communications to the public file for this hearing, and by copy of this message to the board.of.supervisors@sfgov.org email address, it will be forwarded to the Board members for their review and retention.

Regards,

John Carroll
Assistant Clerk
Board of Supervisors
San Francisco City Hall, Room 244
San Francisco, CA 94102
(415) 554-4445

(VIRTUAL APPOINTMENTS) To schedule a virtual meeting with me (on Microsoft Teams), please ask and I can answer your questions in real time.

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From: bobpyke@attglobal.net <bobpyke@attglobal.net>
Sent: Monday, November 8, 2021 11:16 AM

To: Carroll, John (BOS) <john.carroll@sfgov.org>
Cc: Angulo, Sunny (BOS) <sunny.angulo@sfgov.org>
Subject: FW: My response to comments made at the November 4 hearing on 301 Mission Street

John,

Sunny has suggested that I send this to you for inclusion in the hearing file.

Regards,

Bob

From: bobpyke@attglobal.net <bobpyke@attglobal.net>
Sent: Monday, November 8, 2021 8:50 AM
To: 'ROHamburger@sgh.com' <ROHamburger@sgh.com>; 'Greg Deierlein (ggd@stanford.edu)' <ggd@stanford.edu>
Cc: 'Marko Schotanus' <marko_schotanus@marxokubo.com>; Shah Vahdani <shah.vahdani@gmail.com>; Craig Shields <csshields@rockridgegeo.com>; Aaron.Peskin@sfgov.org; 'patrick.oriordan@sfgov.org' <patrick.oriordan@sfgov.org>
Subject: My response to comments made at the November 4 hearing on 301 Mission Street

Please find the attached letter plus 4 additional attachments.

Actually 5 additional attachments. I have also included the figure that did not make it into my Sydney conference paper which illustrates the correct principles for reversing P-y or load-deformation curves. They are shown as bilinear or elasto-plastic curves because in that particular case, it was sufficient to use a bilinear relationship, but the linear elastic portion could easily be made nonlinear. I would be happy to address any questions you have about this point or anything-else that I have said in the attached letter

Regards,

Bob

Robert Pyke Ph.D., P.E.

Robert Pyke, Consulting Engineer
1310 Alma Avenue, No. W201
Walnut Creek CA 94596

(925) 323 7338

November 8, 2021

Professor Greg Deierlein,

Mr. Ron Hamburger,

Re: 301 Mission Street Perimeter Pile Upgrade

Dear Greg and Ron,

I believe that I should respond directly to your comments about outside critics made at the hearing before the Government Audits and Oversight Committee of the San Francisco Board of Supervisors held on November 4, 2021. This letter should be read in combination with the documents that I will attach to a cover e-mail. While I am at it, I might as well cover several related issues, so that you will find that there are four sections below.

1. **How did I come to get involved in this matter?**

More than anything-else I first got involved because two things surprised me when I saw the press conference announcing the Perimeter Pile Upgrade. One was that a representative of Millennium Partners said that no building in this part of San Francisco had ever been founded on piles driven to bedrock. I knew that was not true because I worked for Dames & Moore in the mid-nineteen-seventies when the piles for One Market Plaza were driven to bedrock. Then Ron surprised me by taking credit for and expressing utmost confidence in the PPU, when he is a prominent structural engineer but this is more of a geotechnical problem - I know that the load transfer in the PPU is a very difficult soil structure-interaction problem and that the earthquake response of the building is more of a structural problem, but even there modeling the foundation compliance correctly is very important. That is what led me to ask around and then write my "press release" back in 2019. But also, I suspected that sooner or later the PPU would run off the tracks. While I warned of possible problems during construction, I did not specifically predict the settlement and tilting that has occurred during the PPU installation. I think that I assumed that the Millennium Partners team knew what they were doing relative to pile installation issues and that they would do any necessary testing at the outset. But I thought at that time, and still think, that the PPU will not solve the settlement problem and that the south and east sides of the building will continue to sink as a result of secondary consolidation / creep bearing capacity failure for the foreseeable future, and that it is possible that the building will be red-tagged after even a moderate earthquake (such as a Hayward fault earthquake which could occur any day). Also, my high school history teacher, an Australian who had lived in England for a number of years, once told the story of how he predicted that the Suez Canal crisis of 1956 would be a long-term disaster for the Brits and he regretted that he never wrote a Letter to the Times of London about it. In this case I thought that it might be marginally better to be able to say "I tried to warn you" when things went off the rails, rather than lamely saying "I knew it" after the fact. Also, I was aware that the people who were and remain most knowledgeable about this matter could not talk about it because of non-disclosure agreements and mediation privileges.

But also, I was a bit puzzled that I had never heard back from anyone after, at their request, I sent a signed and stamped copy of my "press release" to the Department of Building Inspection (DBI). That history is summarized in my 29-page document with the long title "Response to Supplement No. 188, the response dated July 25, 2019, to my press release "The Proposed Millennium Tower Fix is a Farce", dated July 17, 2019". Then, after the news broke that the PPU installation was causing enhanced settlement and tilting I discovered that DBI had in fact asked Ron to comment, which he did, but that no-one had forwarded those responses to me. From my 29-pager: "*The main response, labeled Supplemental Report No. 188, is addressed to Dr. Gregory Deierlein, a professor at Stanford and the chair of the EDRT, and dated July 25, 2019. My press release had seven summary points, but Hamburger responded to these points in two differently numbered lists. His first Point No. 1 refers to a previous Supplemental Report No. 34 and so I have included that in the reproduction of No. 188 below. His first list of three points is not in fact addressing anything that I said in my press release but seems to be addressing points that I must have made in an e-mail to Dr. Shahriar Vahdani when I sent him my own site response analysis results.*" After replying to all of Ron's responses I concluded "*I have already remarked on how I never heard back from SGH, DBI, or the EDRT. They were under no specific obligation to do that, but it would have been a normal professional courtesy. However, to make it worse, I have recently seen that in an e-mail to Greg Deierlein, the chair of the EDRT, from Mr. Hamburger dated July 25, 2019, transmitting his responses to my comments he said: "As explained in the letter, we believe our design evaluations have addressed all of his technical concerns." In fact he had not, so that was untrue. My responses above to his responses in Supplement No. 188 and the discussion below on how the installation of the Perimeter Pile Upgrade turned out to be both complex and unusual provide confirmation that he had not adequately addressed all of my technical concerns.*"

This is what led me to initially agree to appear on camera for Jaxon van Derbeken of NBC Bay Area News. Subsequently I saw an e-mail from Patrick Hannan of DBI to Abby Sterling of CBS News in which he said that the EDRT had accepted Ron's responses and Greg confirmed this in his testimony to the Board of Supervisor's committee hearing on Thursday. There may have been many reasons for that, and I guess that it is the prerogative of the ERDT members to make that decision, but it is not the decision that I would have made in similar circumstances. It is things like this that cause Supervisor Peskin and others to suspect or believe that there is some kind of collusion between Ron and the EDRT. You can deny that all you want, but that will not make the issue go away.

2. My response to the comments about public criticism made during the hearing on Thursday.

These comments were provoked by perfectly reasonable questions from Supervisor Peskin, but I can't help suggesting that on any such future occasions you might be better off saying that you have no comments rather than digging the hole deeper by providing childish answers.

Ron started off by doing exactly as I suggest declining to comment on motivation, which was wise, but then he gave two answers, neither of which made any sense to me. One was that he had spent much more time on these issues than any of the critics. I would certainly hope so! I have had the honor to serve on review panels with distinguished geotechnical engineers like Ralph Peck and Jim Mitchell and distinguished structural engineers like Bob Kennedy and Allin Cornell, and I don't think that we have ever tried to put in more hours than the design teams whose work we were reviewing. What we bring is judgment based on a cumulation of education and experience. Cumulatively we might in fact have put in more time on relevant technical or practical issues but obviously we would not have put in anything like the design team has spent on a particular project.

Ron's second point was that Aaron had only listed about six critics who have appeared in news broadcasts when there are 2000 plus geotechnical and structural engineers in the Bay Area. There are many reasons for this. One is that engineers who are employed by a company don't have the time to do the necessary research, and the company would likely not want them to speak about other projects anyway. Another is that, apart from a few showboats, most engineers would be reluctant to speak on camera anyway. I was certainly reluctant to do that because the guest has no editorial control over what is actually broadcast. Typically, a conversation is recorded for an hour or more and only snippets that fit into the story that the reporter is telling are included in the video or the webscript. So, just as we don't know what you discuss in meetings between the design team and the EDRT, you don't know what is left out. Nonetheless, the reporters that I have spoken to have done a good job on selecting the snippets that they include, which is why I have continued talking to them. Additionally, as I have already noted, many of the best-informed engineers relative to this project participated in the mediation proceedings and are bound by non-disclosure agreements and the mediation privilege so that they cannot speak publicly even if they want to.

Greg made the point, which I had previously heard from Craig Shields, that even if we read the reports, view the comment logs and even see the meeting presentations, we do not know what discussion goes on in meetings between the design team and the EDRT. That's true, but isn't that part of the problem? I don't know why those meetings are not covered by the Sunshine ordinance or why they are not webcast. If you want to address the concerns about collusion, why don't you voluntarily webcast them?

So, if I have said anything that you think is incorrect or unfair on TV, or in my 29-pager and the subsequent 6-page Addendum, which I am also attaching, please write to me. That is the way to respond to critics, not with sweeping irrelevancies. I might note that in my 29-pager I acknowledge that Ron is correct in claiming that "*the upgraded building does not qualify as an "irregular" building under the definition of the building code, at least with respect to torsional loading - it turns out that because of the embedded foundation and the symmetrical forest of existing piles under the Tower, the new perimeter* *piles do not add much to the torsional rigidity of the foundation*", and that critics who claim that the PPU makes the building asymmetric under torsional loads are incorrect. I have no problem with Ron citing the building code in this instance, although I note that otherwise you cite the code when it suits you and performance-based engineering when it doesn't. However, the PPU introduces a very significant irregularity under rocking loads which I will address in the following section.

3. Over-reliance on advanced analysis.

One of the continuing themes among people that I talk to is the design team's apparent over-reliance on advanced geotechnical and structural analyses without sufficient calibration and without the application of judgment, based as always on cumulative education and experience, to the interpretation of the computed results.

To their credit, in the John Egan/S&W/Slate geotechnical report it is stated: "*FLAC3D can model a wide range of soil types and behaviors; however, implementation and calibration of complex behaviors such as consolidation and secondary compression may require significant time and effort. Because of the short timeframe available to perform our analyses …",* which is no doubt why they used linear elastic properties, but whatever the reason, the results of their analyses assuming linear elastic properties for both the mat and the support provided by the piles and the soil must be taken with a grain of salt. Sadly, that also applies to most if not all the analyses I have ever seen done using FLAC or PLAXIS. In any case, I continue to think that there are significant concerns regarding the load transfer mechanism and that the overall solution is "too clever by half". Granted structural engineers might be smarter than geotechnical engineers, but you face the same problem which is that you might get a very exact result but only for the assumed model and earthquake input motions. And you can't escape the uncertainty in the soil conditions and the difficulty in modeling the nonlinear behavior of soils.

One of the questions on which I have seen no discussion at all is how do the p-y curves used to model the lateral resistance to pile movement in your analyses reverse under cyclic loadings? I am also going to attach two peer-reviewed papers that I have had accepted for conferences next year. The one for the 20ICSMGE conference in Sydney touches on this subject but because of space limitations I could not include as an example the reversing nonlinear soil springs that I developed when serving as a consultant / expert witness on a major port project. But the very short story is that a structural engineer, with a well-known coastal and port engineering firm, failed to predict the excessive deformation of a cantilever wall because he used a standard computer program that did not properly account for the sequence of loading and the reversal of the p-y springs that he was using.

Another question on which I and others have seen no discussion is how you treat vertical motions in your PERFORM-3D analyses? These are "time history" analyses, no? I understand that the current building code and even the PEER TBI guidelines may not cover the subject of vertical motions very well, but that is no excuse for not going beyond whatever they currently say in this instance when the PPU so clearly will introduce an asymmetrical response to any rocking motions. Further, if you can't analyze it, you maybe shouldn't be doing it?

4. Settlement and tilting triggered by the PPU installation.

I have written about this in the 6-page Addendum to my 29-pager and suggest that you seem to be unaware or not accounting for a mechanism which I freely admit I did not see as being significant when I wrote the first draft of the 29-pager. But, prompted in part by Shah's questions in the comment log, I looked harder at the excess pore pressure measurements in the Old Bay Clay and realized that pushing the 36-inch casings through the sand layer into the Old Bay Clay appears to be increasing the load on the Old Bay Clay and contributing to both immediate and delayed settlement. I also think that you are mistaken in talking about vibration ever having been a cause of the enhanced rate of settlement. I happen to know something about compaction of sands caused by cyclic loading - somewhat to my regret, since I seem to be asked to review every new paper written on the subject! My views on this issue are also covered in the 6-pager as well as the second paper that I am attaching, which will be presented at the ASCE Lifelines 2021-22 Conference, now to be held at UCLA in February 2022. This paper outlines the origins of my involvement in this subject and key factors involved in the mechanism of the compaction of sands by cyclic loading.

I should also share with you two additional comments on Thursday's hearing that I have already sent to Supervisor Peskin:

But perhaps the most amazing revelation was that Greg Deierlein said that there were no estimates of the settlement and tilting that might be caused by the PPU. Because they never thought about it! What else have they not thought about?

And the most embarrassing thing was Ben Turner's slide that said: "negligible settlement = success". See the attached three figures from the monitoring report that was released yesterday, which show that the rates of settlement and tilting after the installation of three casings have returned to about what they were before the pause in late August. It is great that Ben is now monitoring the water level in the casing and the depth of the plug more closely, but why was that not done from the getgo? And is this success?

Again, please let me know if you think that I have said anything that is incorrect or unfair

Best regards,

Bob

Robert Pyke Ph.D., G.E.

Response to Supplement No. 188, the response dated July 25, 2019, to my press release

"The Proposed Millennium Tower Fix is a Farce", dated July 17, 2019

By Robert Pyke Ph.D., G.E.

Addendum - October 25, 2021

"The required construction is neither complex nor unusual." Ronald Hamburger

The master document with the long title above started as my response to the responses by Ron Hamburger to the comments that I made in what I called a "press release" in July 2019. That history is summarized in the master document. But I then expanded upon my response to the Hamburger responses by adding a section called Subsequent Events, in which I said: "*I have recently seen that in an e-mail to Greg Deierlein, the chair of the Engineering Design Review Team (EDRT), from Mr. Hamburger dated July 25, 2019, transmitting his responses to my comments he said: "As explained in the letter, we believe our design evaluations have addressed all of his technical concerns." In fact he had not, so that was untrue. My responses above to his responses in Supplement No. 188 and the discussion below on how the installation of the Perimeter Pile Upgrade actually turned out to be both complex and unusual provide confirmation that he had not adequately addressed all of my technical concerns. It also suggests that the design team has never understood some of the complex issues involved in the Perimeter Pile Upgrade*."

There is now additional information that has been obtained from the test installation of a 36-inch casing on October 12 and 13 to see whether changes in the installation procedure reduced the amount of settlement and tilting that had been observed in the earlier production installation. That test only continues to raise doubts as to whether the design team has ever understood some of the complex issues involved in the Perimeter Pile Upgrade (PPU).

Interpreting the results of this test was always going to be difficult for at least two reasons: (1) as will be explained subsequently, there is a cause of additional settlement which cannot be seen on the day of installation but can only be seen later; and (2) because of that it is difficult if not impossible to separate out the additional settlement that was caused by the installation of a single casing when a number of casing were installed one after another. Nonetheless, Mr. Hamburger then wrote to the Department of Building Inspection (DBI): "*Settlement was negligible throughout, never varying by more than 0.002 ft (0.024 inches) from the baseline reading. The accuracy of the measurement process appears to be +/- 0.001 ft as the readings fluctuated by this amount throughout the test. The final reading indicated settlement of the northwest building corner of 0.001 ft (.012 inch). This is roughly 20% of that experienced during previous casing installations along Mission Street*."

However, Monitoring Report No. 025, which includes data through October 19, tells a different story, indicating once again that Mr. Hamburger appears to be over-optimistic, a Pollyanna who always puts

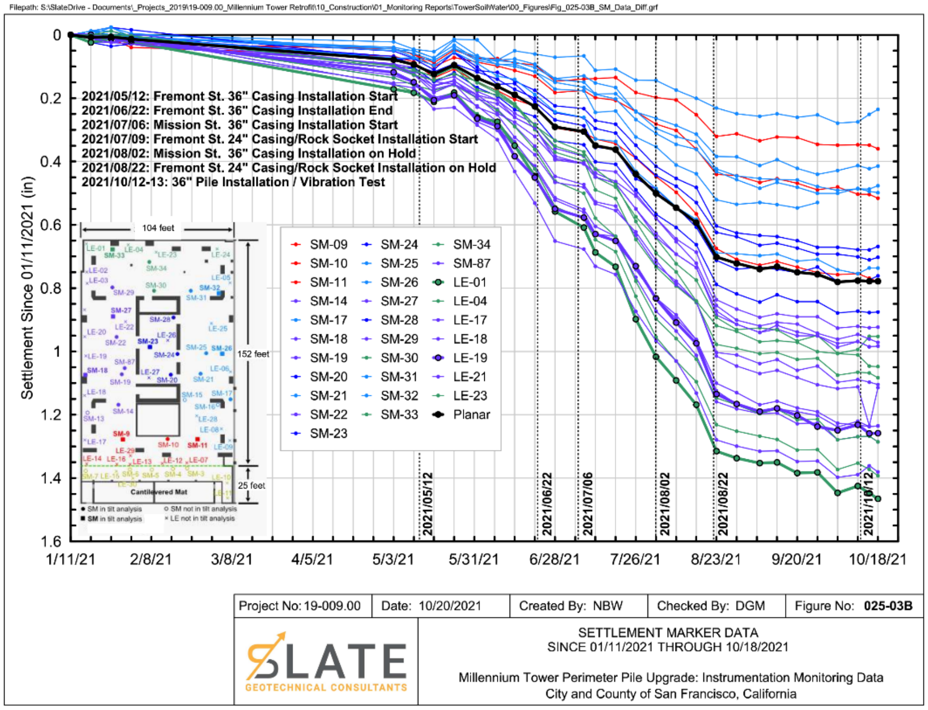


Figure A1 – Settlements

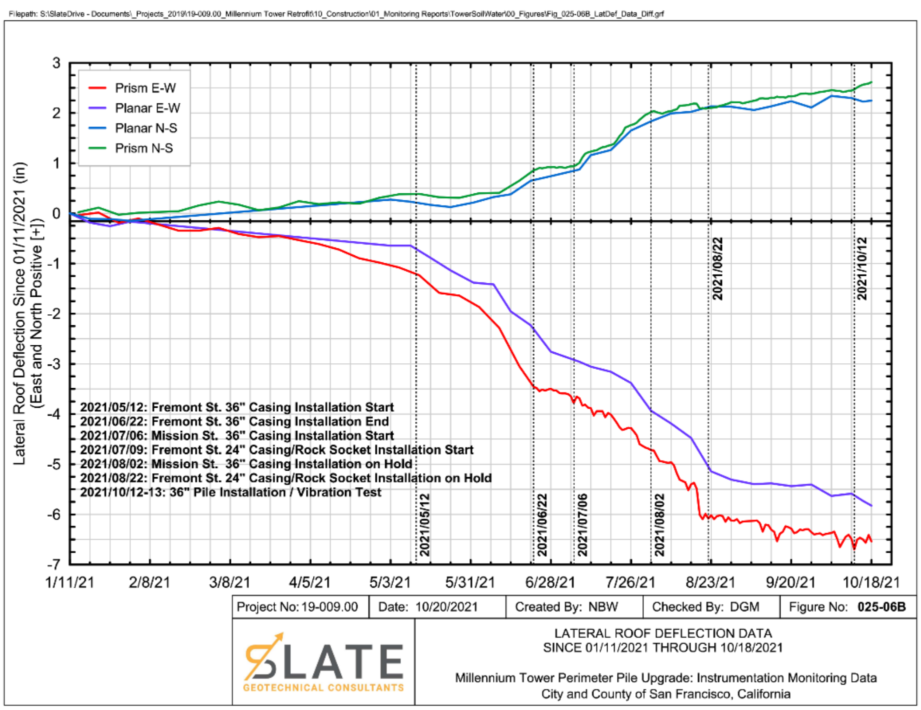


Figure A2 – Lateral Roof Deflections

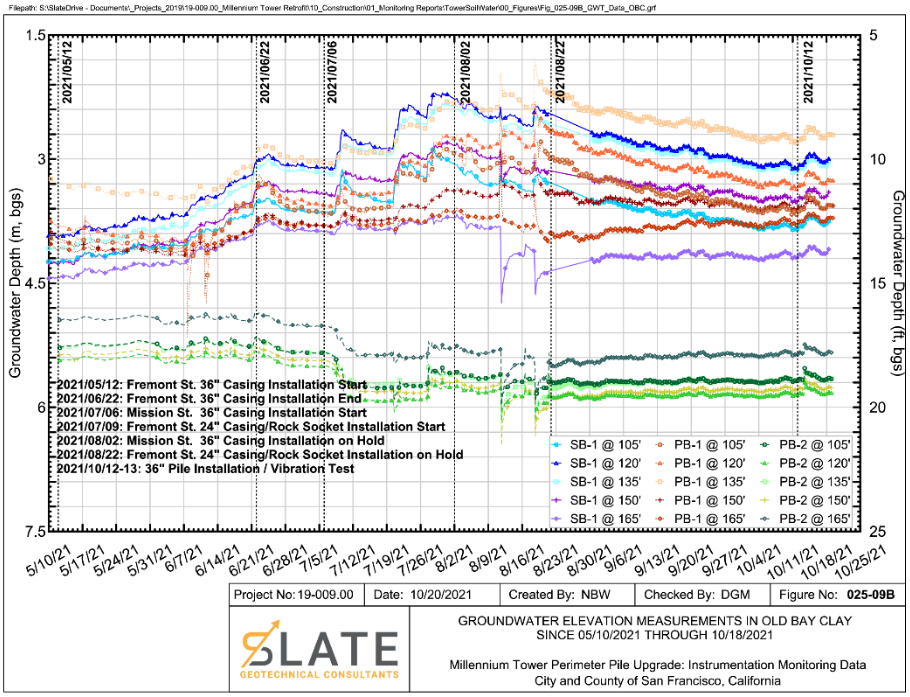


Figure A3 – Excess Pore Pressures in OBC

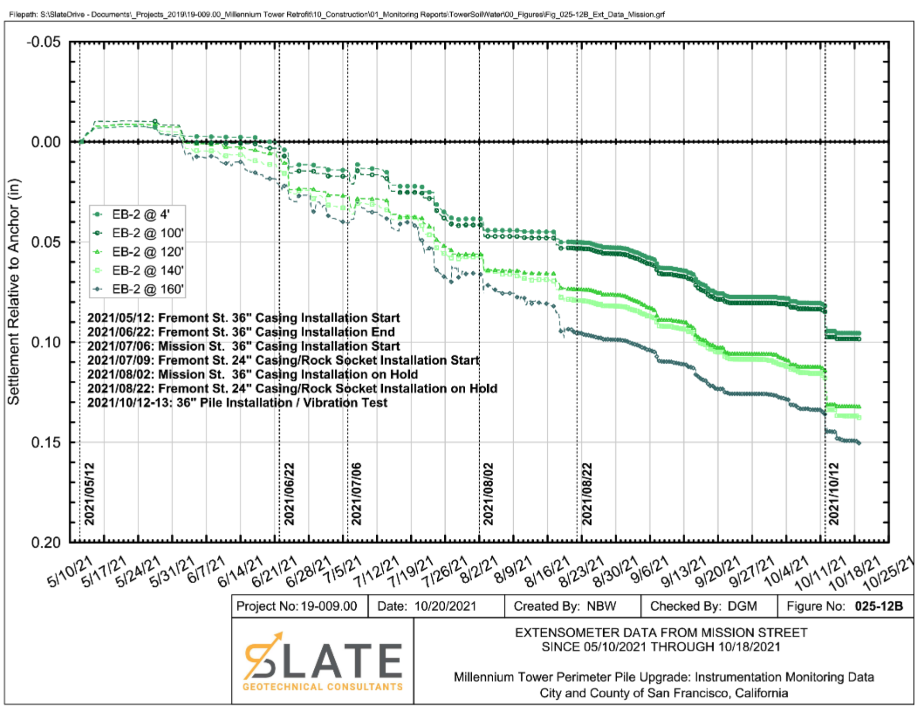


Figure A4 – Extensometer Data

the best possible interpretation on events, rather than emphasizing the uncertainties and the unknowns. The data presented in Figures A1 and A2 indicates that both the settlement of the NW corner of the building and the lateral roof deflection to the west (over Fremont Street) kicked up in the week following the installation of the test casing. Not by a lot – these are still pretty small numbers – but enough to be significant. My reading of the data is that the NW corner settled about 0.05 inch and the westwards lateral roof deflection was about 0.25 inch, that is ¼ inch. The ratio of westward lateral roof deflection to settlement was 5, consistent with what has been seen in the production pile installation. The lateral roof deflection, an indication of the "tilt", is of course greater during the PPU installation than it was in previous ongoing settlement and tilt because the enhanced settlement is only occurring on the north and west sides of the building, so that the differential settlement is greater. Mr. Hamburger may not understand this, because when he was predicting that the settlement would not exceed 1/8 inch, he said that the lateral roof deflection would not exceed ¼ inch. He was lucky that the settlement was well under 1/8 inch (0.125 inches), otherwise the "tilt" would have blown through his maximum of ¼ inch instead of just hitting it.

But it is Figure A3 that provides some insight into why at least some settlement is delayed. This figure indicates the excess pore pressures in the Old Bay Clay (OBC) that is found from depths of approximately 100 feet to 200 feet. It is this layer that is the primary cause of the settlement of the building because the net weight of the building is transferred by the forest of concrete piles under the core and the perimeter mat to the sand layers above the OBC, and this applies enough pressure to the OBC that it has apparently been pushed back onto its "virgin consolidation curve". That does not cause immediate settlement because of the low hydraulic conductivity of the OBC, but settlement occurs over time as the excess pore pressures slowly dissipate. The vertical axes on Figure A3 are labelled "groundwater depth, bgs", which is an odd, but not incorrect, way of representing the total pore pressure. The numbers that are plotted are the depth below the ground surface (bgs) that water would theoretically rise to in an open standpipe with its bottom at the depth of the measuring device. The excess pore pressure (in feet of water) is then the difference between that elevation and the present elevation of the groundwater. I am not sure what that is at the relevant locations, but alternately, if the values in the sand layers above the OBC are hydrostatic, the difference between those values and the values reported for the OBC, assuming of course that the measurements are correct, are an indication of the excess pore pressures in the OBC. The "ground water depth" in the marine and Colma sands appears to be in the order of 20-21 feet bgs while the bulk of the data in the OBC now lies between 9 and 14 feet and was 13 to 15 feet back in May before the PPU installation commenced. Thus, the excess pore pressures in the OBC increased in response to the PPU installation and then started to drop off once the installation was paused. This means that the PPU installation applied at least some load to the OBC and that some settlement occurred as the corresponding excess pore pressures were dissipated.

In the master document I reorganized the three possible causes of settlement previously cited by the design team into four causes and the design team independently did much the same thing. But neither I nor the design team included added pressure being applied to the OBC as a possible cause. Even though I recognized that the PPU installation might apply some additional pressure to the OBC, I thought at the time that this would only result in increased excess pore pressures and not immediate settlements. However, the additional excess pore pressures drain off more quickly than I would have expected. There are two possible reasons for this. One is that the detailed boring log from around the NW corner where the indicator piles were installed notes clayey sand, then lean clay - very soft, low plasticity – and then at 102 feet, decreased sand, so maybe the top of the OBC drains off the additional excess pore pressures relatively quickly. A second is that the loaded volume, or pressure bulb, from a 36-inch casing is much smaller than the loaded volume from a 100 x 150-foot area (the base of the forest of concrete piles in the sand layer). Therefore, the increment of excess pore pressures that is created by the pile installation can dissipate more quickly in all directions. This mechanism is likely more important than a slightly more pervious top to the OBC.

The other four possible reasons for the enhanced rate of settlement listed in the master document are (with minor edits):

1. *Straight over-excavation, which they now restrict to overdrilling for the 24-inch casings.*

2. *What was called, "over excavation of Marine and/or Colma Sand during the 36-inch casing installation due to hydraulic gradient" on August 19 and now appears to be lumped into what Hamburger calls "over-excavation." Great idea to bury this because I think that it is perhaps the most likely cause and it results from a basic error – the failure to maintain enough soil plug and water in the casing to prevent soil from outside the diameter of the casing entering the tip of the casing! I don't know whether they are claiming that this has now been ruled out, but since we have not seen the recent borings, I remain skeptical.*

3. *What was called "heave of Old Bay Clay (OBC) into the 36-inch casings at the end of installation due to stress relief" on August 19 and is now described as "after the casings are installed, soil from beneath the building may be heaving into the open casings." If the enhanced rate of settlement is due to OBC squeezing into the casing, as seems to have been suggested by Dr. Vahdani in the comment log, doesn't this suggest, along with the overall pattern of continuing settlement, that a creep bearing capacity failure is occurring in the Old Bay Clay?*

4. *Vibration. I don't know whether this is a deliberate obfuscation or whether they really believe it. But I don't believe that this explanation is credible. I did my Ph.D. thesis on compaction of sands by cyclic loading and like to think that I know something about this subject. The sands in question are in part clayey and in any case have been there for centuries and will have been shaken by earthquakes from time to time. On Treasure Island it was found that the shoal sands, as opposed to the hydraulically placed sand fill, could not be densified even by heavy vibratory loading. The chance that the sands in which the existing piles under the Tower are embedded have been densified by vibrations caused by the installation process (after they have already had a forest of concrete piles driven into them) is negligibly small.*

I continue to believe that mechanism 4 is most unlikely and that mechanism 3 is possible, but not confirmed. Mechanism 1 has yet to be explored further by a test installation of a 24-inch pile. But the data in Figure A4 suggests that mechanism 2 might still be operative because, in addition to the delayed increase in the rate of settlement, there was some immediate settlement on October 12th and 13th.

But does the enhanced settlement and tilting that is caused by the PPU installation matter? Maybe not in the long-term, because, if the PPU is completed, the north and west sides of the building will be underpinned and the fact that they have settled more just means that it will take slightly longer for the south and east sides to catch up before they exceed the settlement of the north and west sides and the direction of tilt reverses. But, apart from the question of whether there are available funds to complete the PPU, the following questions remain:

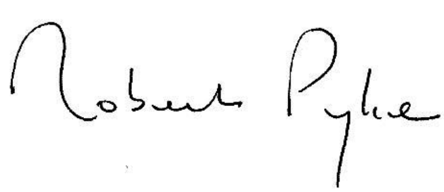
1. Why didn't the design team conduct this research on installation techniques back in May when the indicator piles had already triggered enhanced settlement?

2. Why did the PPU continue for so long until, as best as I understand it, the EDRT pressured then to pause and also to bring in an expert on pile installation?

3. Why doesn't the design team still not have a clear explanation of the causes of the settlement and tilting?

In addition to the questions that I have raised in the master document about the PPU overall, there are also the questions regarding what is going to happen when they excavate down below the current grade in order to install the mat extension; how much are the 24-inch piles going to compress and bow when load is transferred to them; what will the load redistribution on the perimeter mat, the core and their supporting piles actually be when they do that; and how will the perimeter mat respond. And doesn't this all suggest that the design team's continuing confidence in the Fix might be misplaced?

To repeat my final paragraph in the master document: "*Throughout this whole affair, the EDRT has been asking some good questions. So many in fact, that that alone has to cast some doubt on the credibility of the SGH design team and their calculations. However, the EDRT then gets the runaround from the design team. A mixture of gobbledegook and evasion, like Mr. Hamburger's response to my 2019 press release. This is not a good process. So, my final question is: Why don't the DBI and the EDRT put their collective foot down more firmly? A good first step would be to suspend the installation of the PPU until the decision to approve it in the first place is reviewed.*"



Robert Pyke, Ph.D., G.E.

Response to Supplement No. 188, the response dated July 25, 2019, to my press release

"The Proposed Millennium Tower Fix is a Farce", dated July 17, 2019

By Robert Pyke Ph.D., G.E.

Updated October 5, 2021

"The required construction is neither complex nor unusual." Ronald Hamburger

I am an individual consultant on special problems in geotechnical and earthquake engineering and have served as an external reviewer of geotechnical and earthquake issues on a number of high-rise buildings in both San Francisco and Seattle. I have had no formal involvement with the Millennium Tower design or subsequent activities, but in February 2019 I wrote a press release titled "The Proposed Millennium Tower Fix is a Farce." This document found its way to the Department of Building Inspection (DBI) who forwarded the content to the Engineering Design Review Team (EDRT) as "some questions being asked by the public." Several months later I received a message from the DBI commenting that the press release was not signed and stamped, so it was reissued signed and stamped in July 2019. That version is reproduced below.

I was not otherwise contacted by either the DBI, the EDRT, or the design team to discuss my comments, nor did I receive any written reply. However, I have recently found that Mr. Ronald Hamburger, the leader of the Simpson Gumpertz & Heger (SGH) design team, did in fact write responses to the EDRT. The main response, labeled Supplemental Report No. 188, is addressed to Dr. Gregory Deierlein, a professor at Stanford and the chair of the EDRT, and dated July 25, 2019. My press release had seven summary points, but Hamburger responded to these points in two differently numbered lists. His first Point No. 1 refers to a previous Supplemental Report No. 34 and so I have included that in the reproduction of No. 188 below. His first list of three points is not in fact addressing anything that I said in my press release but seems to be addressing points that I must have made in an e-mail to Dr. Shahriar Vahdani when I sent him my own site response analysis results. Also, the EDRT's Comment No. 188 was not directly about anything I had said but was about points raised by Drs. Karp and Kardon in a letter addressed to the Board of Supervisors. I am including the EDRT's comments here because they overlap in part with the comments in my press release.

EDRT Comment No. 188

Please confirm and briefly summarize how the concerns raised in the public service letter by L. Karp and J. Kardon (dated 7/10/2019) have been addressed in the proposed design, including:

a) Effect of dewatering during construction and loss of soil due to pile construction on settlement

b) Torsion caused by plan Irregularity of the new piles

c) Structural integrity of existing precast piles due to mat settlement, rotation and dishing

d) Use of tiebacks in the proposed construction on adjacent properties

e) Capacity of existing mat foundation to resist the redistribution of gravity and seismic loads

f) Significance of cracking that has been recorded the existing basement construction

SGH Response to Comment No. 188

Supplement 188, attached hereto contains our formal letter to the EDRT chair responding to technical issues raised in Dr Pyke's communication. All of the technical points raised both by Dr. Pyke and Mr. Karp have already been addressed in an exhaustive manner as part of our original design evaluations and also in direct response to prior questions from the EDRT. Please note that we have specifically annotated in this log where specific comments relate to Mr. Karp's concerns. We restate below:

a) No dewatering will occur during construction. The water table is presently only modestly above the proposed depth of excavation. We are confident infiltration of water into the excavation can be controlled by the use of jet grout improvement of the surrounding soils, as shown on the Shoring Permit Set.

b) Torsion of the Tower's foundation with the perimeter pile upgrade in place has been explicitly modeled in our analyses. Although installation of the new perimeter piles results in minor shifting of the center of foundation rigidity relative to center of mass, this does not result in creation of a structural irregularity and does not produce noticeable torsional response. In fact, our analyses show that at the extremities of the foundation (the corners and sides) peak displacement in response to MCE shaking with the perimeter piles is substantially less than without them.

c) Rotation of the existing mat, under the influence of tilting has indeed imposed some moment on the pile tops. This has the effect of preloading the piles with lateral demands (shears and moments). We explicitly modeled this effect in our analyses of the foundations and structure's response to earthquake shaking, considering tilting and settlements that is both double and triple that which has occurred to date and also considering bounding assumptions as to soil stiffness, and multiple suites of ground motions. With the perimeter pile upgrade in place, none of the ground motions produce sufficient additional displacement into the piles to cause failure.

d) No new tiebacks are installed as part of the Perimeter Pile Upgrade. However, in order to perform the construction, it is necessary to cut tie backs that were installed as part of the excavation shoring for the original Millennium Tower basement and foundation construction. These tie backs ceased to serve any function once construction of the Millennium Tower basement was completed (14 years ago). They were originally intended to be temporary and sacrificial. Cutting these tiebacks will have no impact on adjacent construction or the Millennium Tower.

e) We have explicitly modeled the existing piles and mat foundation together with the new piles and mat extension both in CSI Perform and CSI SAFE, industry standard tools for evaluation and design of such systems. We have evaluated the adequacy of the foundations for all load combinations specified in the building code, as well as additional load conditions evaluating what-if scenarios associated with additional building settlement and realistic modeling of earthquake effects. Our calculations demonstrate the foundation is adequate to redistribute the building weight induced by settlement and tilting as well as that associated with jacking, which tends to counter the effects of tilting.

f) Most cracking reported in the "Millennium Tower" basement actually occurs in the basement walls of the adjacent podium structure and garage. It is a result of the settlement that has occurred across the site. Cracking within the basement of the Millennium Tower itself if limited and has been mapped and studied both by Arup and ourselves. The cracking in the occurs because the combined system of the basement walls, foundation mat and first floor slab act as a deep (story-high) orthotropic grillage. As the building settled and the mat foundation dished, this orthotropic grillage followed the mat's curvature and experienced stresses The mat and first floor slab experienced tension and compression stress associated with flexure of the system. The walls, acting as webs in this grillage experienced shear stress. In some locations this shear stress was sufficient to yield reinforcing steel and crack the concrete which actions relieved the accumulation of additional stress. It is worth noting that:

1- Neither the first floor slab nor mat foundation exhibit any signs of distress (cracking)

2- The core walls which provide the primary earthquake resistance of the structure do not exhibit any indications of cracking or distress

3- The core walls and mat foundation were originally designed to resist required earthquake stress without reliance on the basement walls.

We conclude the observed cracking has no significant impact on the building's structural safety.

My responses to Hamburger's responses are inserted in the reproduction of Supplemental Report No. 188 which is included below. But to put these responses in context, it is appropriate to make four general observations prior to kicking the football around.

1. It has always been a bit of a mystery as to why the DBI did not add an appropriately qualified geotechnical engineer to the engineering design review team for the design of the Millennium Tower, but this is explained in a letter from Tom Hui, then head of the DBI, to Angus McCarthy, President of the Building Inspection Commission, dated October 27, 2016: *At the time DBI was reviewing 301 Mission, DBI did not have the authority to require the developer to retain a geotechnical engineer as prescriptive code requirements—the design submitted for this project—did not require it… The developer's engineer of record rejected DBI's explicit request to fund the addition of a geotechnical engineer to this peer-review panel. Nonetheless, Professor Moehle issued a letter to DBI dated January 29, 2006, stating: "On the basis of my review, it is my opinion that the foundation design is compliant with the principles and requirements of the building code, and that a foundation permit can be issued for this project."*

2. The decision to go with the SGH proposed solution, officially called the Perimeter Pile Upgrade, or PPU, rather than an alternative solution developed by a team led by LERA, was made as part of a mediation process set up to try to resolve the numerous lawsuits that had been filed. I am told that over 50 lawyers participated in these mediation proceedings. A good outline of the two competing schemes was given by Drs. Larry Karp and Josh Karmon in a letter addressed to the San Francisco Board of Supervisors dated July 10, 2019. Karp and Kardon call the SGH solution the "external asymmetric plan" and the LERA solution the "internal symmetrical plan." Initially the LERA solution was estimated to be very expensive, which is why the developer Millennium Partners (or perhaps more strictly their subsidiary Mission Street Development) asked SGH if they could develop a cheaper solution. Not better but cheaper. However, by the time the mediation process was nearing completion the LERA solution had been revised in two major ways that brought the estimated cost down to be slightly less than the SGH solution. However, Millennium Partners then put their thumb on the scale and offered to manage the implementation of the SGH fix on a turnkey basis and to guarantee it, by for instance taking out appropriate insurance policies, thereby relieving the Homeowners Association (HOA) of the considerable hassle and potential liability of managing this work themselves. That was an offer no HOA would be able to turn down. I do not know why Millennium Partners chose to do this, although it was no doubt helpful in bringing the mediation process to a close. But, if a camel is a horse designed by a committee, a horse designed by a legal mediation process is likely to be an ass.

3. This is getting into the weeds a bit, but it is important to understand the kind of obfuscation that SGH used to deflect my questions and continues to use to deflect questions from the EDRT. The Building Code just sets minimum standards to protect life safety for conventional buildings. Provisions regarding tall buildings and the effect of embedment and the responses to rocking and torsional loadings are not adequately covered in the Building Code. These things are better, but still imperfectly, covered by the PEER Tall Buildings Initiative Guidelines for Performance-

Based Seismic Design of Tall Buildings. Performance-Based Engineering basically requires advanced analysis tools that can follow nonlinear behavior in order to make reliable and accurate estimates of performance. But these are still just estimates. They still require some simplifying assumptions and you can get any answer you want within reason. So, the results should not be taken literally but used to obtain insight before making final design decisions, which should be based on experience and judgment. However, what usually happens is that you try some variations and stop when you get the answer you want. It is also very difficult for anyone else to follow the calculations. Reviewers can ask questions, but it is usually easy to deflect even good questions. The only way to check an advanced analysis is to have an independent third party solve the same problem to see whether they get the same answer. Thus, it is fine to use FLAC 3D and PERFORM 3D, but the results should not be automatically accepted as handed down from God. But there have been no independent third-party checks of the FLAC 3D analyses performed by Slate, SGH's geotechnical consultant, nor of SGH's PERFORM 3D analyses and Mr. Hamburger jumps back and forth between arguments citing the Building Code and reliance on performance-based engineering.

4. Written back and forths of this kind are difficult to follow and are never as satisfactory as a face-to-face discussion and it is regrettable that the DBI, the EDRT, and the design team chose not to talk to me in 2019 and have not approached me since this matter blew up several weeks ago.

My Press Release

The Proposed Millennium Tower Fix is a Farce

By Robert Pyke Ph.D., G.E.

July 17, 2019

While I respect the four engineers who are reviewing the proposed fix for the City and am sure that they will ask many good questions, I am concerned that consideration of this fix will drag out. This is I believe in fact the intention of the developer who wants to avoid full discovery and a trial at any cost, and I feel an obligation to speak out at this time and call the fix what it really is, which is a farce.

While I have had no formal access to any documents in this matter, the publicly available information is adequate to come to some general conclusions, and, while the summary comments below are entirely my own, I believe that they are shared by many other responsible engineers.

The following summary points are just that. A summary of key points without any detailed backup or calculations, but I have a high degree of confidence that they are generally valid and in line with what is generally accepted as an expert opinion.

The following seven points are not necessarily exhaustive with respect to all the issues regarding the Millennium Tower and the proposed fix, but they should give the reader a feel for my concerns.

1. The suggestion that a disproportionate fraction of the weight of the building is carried by the perimeter columns, fails to take into account the sequence of construction, and is wrong.

2. The suggestion that the transfer of 20 percent of the load from the existing forest of piles to the proposed new piles along the north and west faces of the building would result in immediate rebound of about 1 inch of the north and west sides of the building[1] defies basic soil mechanics principles.

3. Arresting the settlement of the north and west sides of the building while the center and the south-east corner of the building continue to settle can only increase the stresses in the mat that underlies the building and the outriggers[1] when the mat is already dished and cracked and the condition of the outriggers is uncertain.

4. The proposed fix cleverly provides for backing off the underpinning of the north and west sides of the building should the settlement of the south-east corner catch up with and overtake the settlement of the north-west corner, but that means the building just continues to settle and there is no fix!

5. The proposed fix creates an asymmetrical foundation which is bad enough under static loads but will create unpredictable and likely adverse responses under seismic loads, especially since the performance of the "outriggers" under earthquake loads is already questionable[1].

6. The proposed fix requires complex and difficult construction on City property which houses many existing utilities and tie-backs and will require new dewatering, which is alleged by the developer to be one of the causes of the existing problem.

7. In summary, the proposed fix is too cute by half. I am aware of at least two alternate fixes which are simpler and more robust and may well be cheaper to implement than the proposed fix.

Robert Pyke Ph.D., G.E.

Hamburger Response

(With some highlights in red and my responses inserted in blue)

25 July 2019

Project 147041.10 – Millennium Tower, Perimeter Pile Upgrade

Comments from Dr. Robert Pyke

Dear Dr. Deierlein:

This letter responds to comments raised by Dr. Robert Pyke in an e-mail, and attached memo, forwarded by the City Attorney's office to Mr. Peter Meier on 23 July. I prepared these responses in consultation with Mr. John Egan, who serves as my principal geotechnical consultant for our work on this project.

The e-mail raises three primary points associated with Mr. Egan's characterization of the site and recommendation of MCE_R ground motion spectra. Specifically, these are:

1. *Characterization of the site as Site Class D rather than E*.

   This point was extensively reviewed by the EDRT and is addressed in the comment log under comment 34.

The response under Comment 34 is reproduced at the top of the next page.

There are multiple issues involved in this point and the following two points. The issue of whether the Site Class should be D or E might be less important than whether the acceleration histories used as the input to the structural analyses are appropriate and whether they are applied correctly to the structural model, but it is still significant because of the language of the building code. The issue of site classification is relatively straightforward and it is hard to understand why Hamburger is so evasive in addressing this and why the EDRT signed off on the rambling response to Comment 34.

Hamburger's response to Comment 34 refers to shear wave velocities measured in Boring TTB-08 drilled for the Transbay Terminal, which was included in Supplemental Report No. 34. I am including that below, following the response to Comment No. 34, as Figure 1.

But in 2018, in the Egan/S&W/Slate geotechnical report, it is indicated that the shear wave velocity profile that they used for analysis of the site response was as shown below in Figure 2 - note the softer top which makes it Site Class E – I know that the input motions for the structural analyses were defined at the base of the perimeter mat, but the building code says that site classes are "based on the upper 100 feet (30 m) of the site profile."

The comment seems to be aimed at understanding why Site Class D has been selected instead of Site Class E. Section 20.3.2 of ASCE 7-10 and 7-16, Soft Clay Site Class E, states "Where a site does not qualify under the criteria for Site Class F and there is a total thickness of soft clay greater than 10 ft (3 m) where a soft clay layer is defined by Su < 500 psf (Su < 25 kPa), w >= 40%, and PI > 20%, it shall be classified as Site Class E." Ground motions for the structural analyses of the Millennium Tower are being applied at the elevation of the base of the Tower mat, so approximately 25 feet bgs. As such, to estimate V_{S100} (aka V_{S30}) and Site Class appropriate to characterizing map-based seismic parameters for comparing Code requirements to the site-specific ground motions, we have considered the soil profile extending 100 feet from the bottom of the mat. As indicated by the comment, there are some zones within the subsurface profile that have shear wave velocities (V_S) less than 600 fps, notably within the Recent Bay sediments (i.e., Young Bay Mud and Marine Sands). This is illustrated by the attached figure of shear wave velocity measurement obtained using suspension logging in Boring TTB-08, drilled on Fremont Street within about 25 feet of the Tower. We note that there are two zones, each approximately 5-ft-thick, for which the shear wave velocities (V_S) fall below 600 fps. We note however, that even with these thin low-velocity zones, the V_{S100} for the profile exceeds 600 fps and falls within the V_{S100} range (600 to 1200 fps) for Site Class D. In addition, examination of the measured undrained shear strengths (S_u) for the soils within the profile (see attached figure) indicates that none of the laboratory- or field-measured undrained shear strengths within the profile below the bottom of the mat are less than S_u=500 psf and the average S_{u100} is ≈ 1000 psf. Given that the calculated V_{S100} for the profile exceeds 600 fps, none of the measured undrained shear strengths are less than 500 psf (a requirement of ASCE 7-10 Section 20.3.2 for designating a site as Soft Clay Site Class E), the average S_{u100} is ≈ 1000 psf, and the average SPT penetration resistance for the cohesionless soils, N_{ch100} is ≈ 40, we are of the opinion that Site Class D is the appropriate Site Class designation for the soil profile. See Supplement No. 34 for visual comparisons. Lastly, we also note that CSMIP designates the profile as Site Class D for the strong motion station (CGS # 58411) installed at the Tower.	5/15/2019 - Resolved pending submission/review of the revised/final geotechnical report 8/26/2019 - Resolved

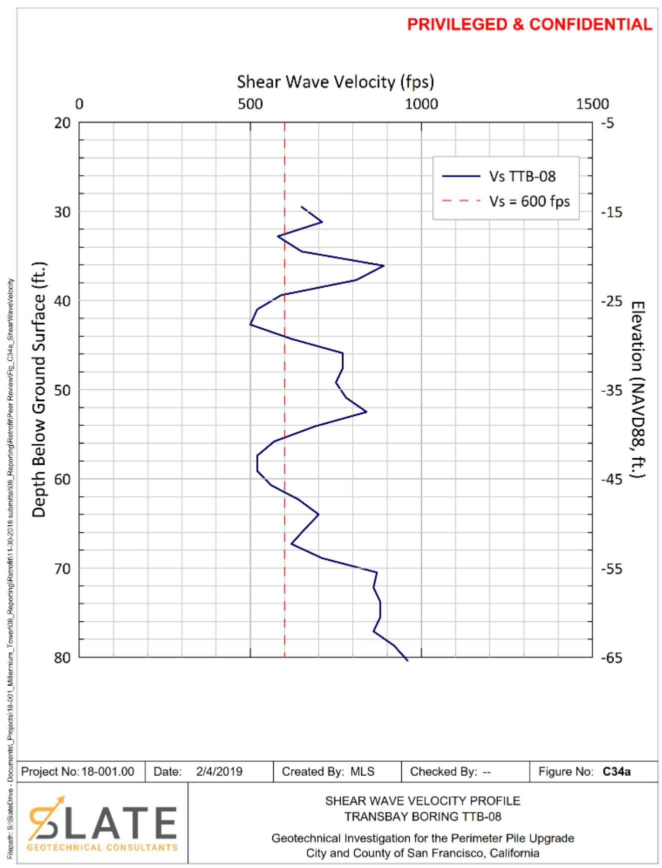


Figure 1 – Vs profile from Supplement No. 34

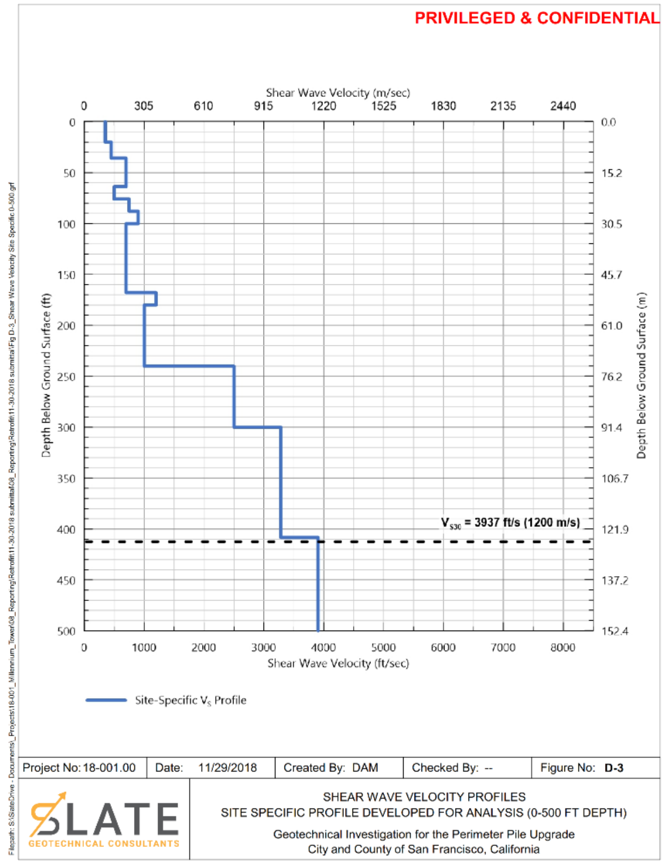


Figure 2 – Vs profile from Egan/S&W/Slate Geotechnical Report

So, why this misdirection? Likely answer: because for the default response spectra in the building code the longer period motions are less severe for Site Class D than they are for Site Class E. However, the current building code requires that a site-specific seismic hazard and / or site response analysis be conducted to establish the longer periods motions for Site Classes D and E. In any case, this should be done for a building of this significance. A site response analysis should also be done for a profile of this kind where the shear wave velocity in the upper 100 feet does not adequately capture the effect of the soil profile on earthquake ground motions. My own analyses of sites both in San Francisco and Foster City, as well as for the new East Spans of the San Francisco-Oakland Bay Bridge, suggest that in profiles with significant depths of Old Bay Clay, the effects of the Old Bay Clay in damping out shorter period motions but amplifying longer period motions can be quite significant. Thus, to claim that this site is a "stiff soil" site, which is the building code description of Site Class D, and not a "soft clay soil" site, which is the building code description of Site Class E, is a bit of a stretch. And, this is still significant, even when a site response analysis is conducted, because the building code limits the drop from the standard code spectra to 80 percent of that spectra (or 70 percent in the case of buildings with embedded foundations).

The results of my own site response analyses, using input motions matched to the building code Site Class B spectrum for this site and applied at the top of the Franciscan formation bedrock, and using the Egan/S&W/Slate shear wave velocity profile, are summarized in the next two figures showing the computed ground surface response spectra. Figure 3 shows the results for the "free-field" profile, and Figure 4 shows the results for a profile that stops 25 feet below the ground surface, or at the base of the perimeter mat. The effect of adjacent buildings is not taken into account.

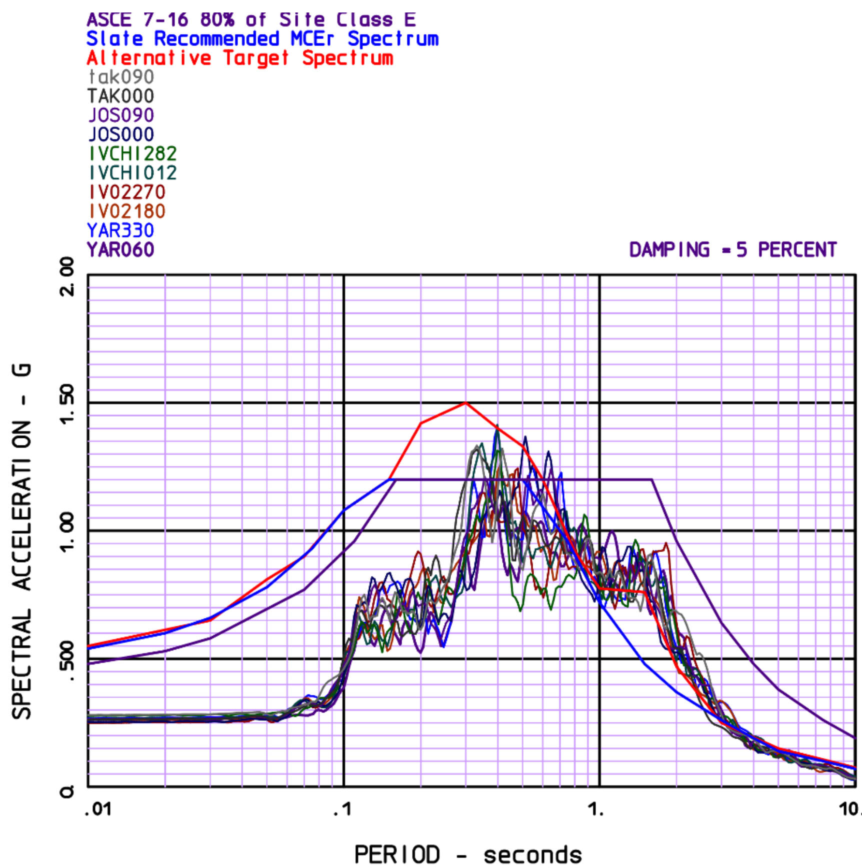


Figure 3 – My Computed Free-Field Ground Surface Response Spectra

It may be seen that, except at very short periods, there is not that much difference between the computed spectra in Figures 3 and 4, confirming that the Old Bay Clay layer has more impact on the site response than the top 25 feet of the profile.

For comparison, three other spectra are shown in these figures. Shown by a blue line is the response spectra recommended by Slate and based on their own site response analyses. These analyses were "equivalent linear" analyses that used entirely inappropriate modulus reduction curves for some layers and had generally quite low longer period responses so that the recommended spectrum was

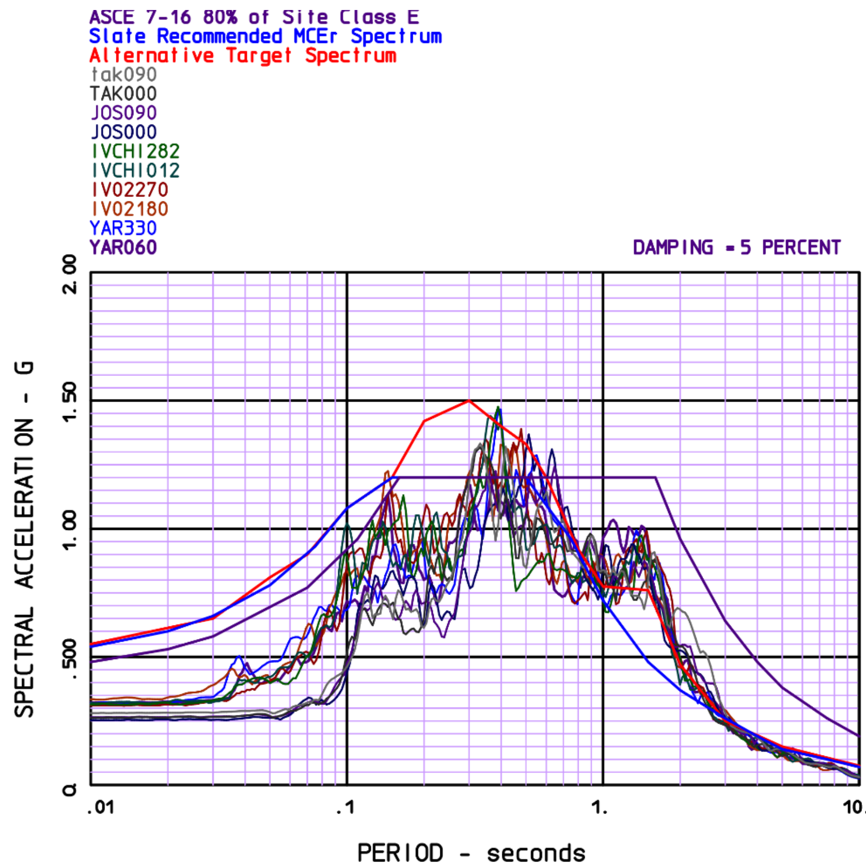


Figure 4 – My Computed Ground Surface Response Spectra for 25 feet BGS

controlled by 80 percent of the then building code spectrum for Site Class D. To their credit, SGH recognized that this spectrum was inadequate and so they adopted the spectrum shown as a red line, labeled Alternative Target Spectrum on the plot, which had been developed by ENGEO for the competing LERA proposed solution. It may be seen that although the ENGEO analyses appear to have used slightly stiffer "backbone curves" for their nonlinear site response analyses than I used in mine, the results are not dissimilar. Although I am using the same shear wave velocity profile, my analyses are pretty much an independent third-party analysis and I would conclude that while the ENGEO results are a little less conservative than mine, they are in the right ballpark. As I understand it, John Egan then fitted his choice of acceleration histories to the ENGEO spectrum and those acceleration histories were used to drive the PERFORM 3D nonlinear structural model at the base of the perimeter mat.

However, the third spectrum, shown in Figures 3 and 4 as a grey line, is 80 percent of the current building code Site Class E spectrum, which is the normal minimum allowable spectrum even if site-specific analyses are conducted. I believe that that spectrum may be unnecessarily conservative but current homeowners and potential future homeowners should understand that the performance of the building has not been checked for the minimum loading that would be required under the current building code. The difference at a period of 5 seconds, the approximate fundamental period of the building, between the minimum building code spectrum and the results of the site response analyses is quite striking and would remain so even if the 80 percent were reduced to 70 percent as allowed by the current building code for buildings with an embedded foundation. This occurs partly because the site response analyses assume vertically propagating shear waves and they may be unconservative at longer periods, since the longer period motions in earthquakes are principally generated by surface waves. Further study would normally be required to justify the use of spectral accelerations at 5 seconds any lower than 70 percent of the standard building code spectrum.

But it must also be recognized that there are many approximations involved here. One is that the building will actually be driven by a complex set of motions that vary from whatever drives the existing forest of piles under the building, to the base of the core, to the base of the perimeter mat, to the ground surface, and that the ground surface motions will not be "free-field" on the south and east sides of the Tower. Ideally, this would be accommodated by specifying multiple support input motions as is commonly done, for instance in the analysis of offshore structures. A second major approximation involves the modeling of the stiffness of the soils around the embedded portion of the tower and its foundation. I have seen some elements of Slate's recommendations regarding this subject but I have not seen all the details of how SGH have included these recommendations in the PERFORM 3D model and I am skeptical that the soil behavior in both loading (or passive pressures) and unloading (or active pressures) and the reversal from one to another has been modeled correctly. It rarely is. When I have been involved in reviewing the design of high-rise buildings in Seattle with deep basements there has always been pushback from the structural engineers regarding these two issues with them saying something like: "we can't use multiple support input motions or model the soil behavior in more detail because we need all the available degrees of freedom to model the structure in more detail." All this applies only to horizontal motions. I will get to vertical motions later on as another issue, but the overriding point here is that the results of the PERFORM 3D analyses cannot be precisely correct no matter how good the structural modeling if the input motions and the modeling of foundation stiffnesses are not precisely correct.

2. Use of 80% of the default spectrum specified by the building code, rather than relying on site specific study, noting that ASCE 7-16, which will be adopted by the City of San Francisco in January 2020 will require site specific study.

> In the course of their geotechnical study, Mr. Egan and his support team did indeed perform site-specific response analysis to develop a response spectrum appropriate to the foundation level of the Tower. ASCE 7 requires that when site-specific response analysis is performed, the resulting spectrum cannot be taken as less than the 80% of the default spectrum. Mr. Egan's site-specific response analysis resulted in a spectrum with spectral ordinates generally less than 80% of the default spectrum, but with longer- period (i.e., 2 sec ≤ T ≤ 4 sec) energy content exceeding 80% of the default spectrum; thus, the greater of the 80% limit or the site-specific response study was adopted as the recommended spectrum, as required by the building code. This was reviewed by the EDRT and is logged as comment 3 in the log.

I don't know why Hamburger was still talking about Egan's site-specific response analysis when the 2018 Egan/S&W/Slate geotechnical report makes it clear that the ENGEO spectrum developed for LERA had been adopted by SGH. Hamburger's response to Comment 3 simply states: "MCE spectrum is revised based on discussions during 21 Dec. 2018 meeting. Please see Supplemental Calculation 03." I actually had a copy of Supplemental Calculation 03 and a letter that Mr. Hamburger wrote to Gary Ho dated February 20, 2029, obtained under a "Sunshine Act" request when I wrote my press release. That was where I obtained the information to run my own site response analyses, the results of which are shown in Figures 3 and 4. Supplemental Calculation 03 is 53 pages long and details the development of a design response spectra and time histories by Egan and Slate for SGH but then says:

At the request of SGH, we compared the target spectrum for time history selection from the ENGEO analysis (Table 9.2-1, ENGEO 2018) with the target spectrum for time history selection from the analysis described previously in this report. This comparison is shown in Figure D-10 and demonstrates that the ENGEO target spectrum is significantly higher at a majority of spectral periods values between 0.01 and 10 seconds, with the 25-foot depth within spectrum exceeding the ENGEO target spectrum by about 10-15% over the periods of 1.8 seconds to 2.3 seconds. Based on the results of this comparison, and for comparison with the earlier analysis conducted by the LERA retrofit team, SGH requested that we perform spectral matching of the ENGEO time history suite to the ENGEO target spectrum, for application in the SGH structural model.

There is no point in going over the shortcomings of the Egan/Slate site response analyses in detail, but this must at least raise a caution about their judgment on other matters.

3. *Dr. Pyke's personal belief that characterization of ground shaking at the site using the Vs-30 parameter will underestimate the likely energy content of shaking in the period range 1 to 1.5 seconds. Dr. Pyke notes that Engeo's proposed design spectrum did have increased energy content in this period.*

   We note that the building's fundamental period of response is approximately 5 seconds and more than 60% of the building's mass is mobilized in modes that have periods in excess of 3 seconds. Only 20% of the building's mass participates in the period range between 1 and 1.5 seconds. Regardless, in the course of our design, we evaluated the building for Engeo's ground motions as well as those recommended by Mr. Egan. The building performed adequately for both sets of ground motions.

I don't understand Mr. Hamburger's characterization of my "personal beliefs," but I did understand at that time that the building's fundamental period of response was approximately 5 seconds and that the bump in the spectra between 1 and 2 seconds, as can be seen in Figures 3 and 4, might not be highly significant to the response of the building, but my bump was larger than ENGEO's and I would still like confirmation that that is not significant. But more importantly, since Mr. Hamburger is chair of a key ASCE committee that oversaw the latest updates to the building code, how would he explain away the fact that the 2016 update to ASCE-7, which forms the basis for the current California and San Francisco building codes, specifically made a point of increasing the longer period motions for Site

Classes D and E because studies by Dr. Charles Kircher had indicated that they were previously unconservative? I have already discussed this issue in my response to Point 1 above. To reiterate, current homeowners and potential future homeowners should understand that the performance of the building has not been checked for the minimum loading that would be required under the current building code. As shown in Figures 3 and 4, the difference at a period of 5 seconds, the approximate fundamental period of the building, between the minimum building code spectrum and the results of the site response analyses is quite striking and would remain so even if the 80 percent were reduced to 70 percent as allowed by the current building code for buildings with an embedded foundation.

Dr. Pyke's memorandum dated 17 July raises the following technical points:

(Note that these are paraphrases of what I actually said.)

1. *An allegation that our team purports that a disproportionate fraction of the building's weight is carried by the perimeter columns, and this fails to take into account the sequence of construction.*

   We are not sure what Dr. Pyke is referring to. We have never made statements suggesting that a disproportionate amount of the building's weight is carried by the columns. We independently computed the amount of building weight carried by the individual columns and the central core and compared these with similar computations made by DeSimone Consulting Engineers in their original structural design. Our calculations suggest that roughly 45% of the building's weight is carried by the central core and 55% by the perimeter columns. This is consistent with distributions of load we have observed in other tall buildings.

I am happy to join with Mr. Hamburger in saying that I am also not totally sure what I was referring to two years ago, but I do remember reading or hearing somewhere that he had been emphasizing that more of the weight of the building was carried by the perimeter "super-columns" than by the central core, which was why it made sense to install the additional piles at the perimeter. Accepting Mr. Hamburger's statement that 45 percent of the building's weight is carried by the central core and 55 percent by the perimeter columns, using the dimensions provided by Slate in their monitoring reports for the area of the core and the perimeter mat, if the core and the mat were uncoupled the pressure applied at the base of the core would be about 40 ksf and the average pressure applied at the base of the perimeter mat would be about 8 ksf. In fact, the core penetrates through the mat, and the two are structurally connected, resulting in some load redistribution from the core to the mat, but how much is unclear. I understand that Slate have conducted advanced analyses using FLAC 3D to study this and other issues, but Slate used linear elastic properties for both the structure and the soil so that their results cannot possibly be correct. To their credit, in the Egan/S&W/Slate geotechnical report it is stated: "*FLAC3D can model a wide range of soil types and behaviors; however, implementation and calibration of complex behaviors such as consolidation and secondary compression may require significant time and effort. Because of the short timeframe available to perform our analyses …*", which is no doubt why they used linear elastic properties, but whatever the reason the results must be taken with a grain of salt. [1] [2]

I mentioned the “sequence of construction” in my press release because it had been explained to me that “the mat,” meaning the combination of the base of the core and the perimeter mat, had settled and dished during construction in part because, as is normal, the core advanced ahead of the frames that support the perimeter of the building. The record of this settlement and dishing has been obscured by reporting only settlement results since 2007. Settlements prior to that date have effectively been zeroed out. But the fact that there was dishing is acknowledged in the response by Mr. Hamburger to Comment No. 188 in the design comment log: *The cracking in the (basement) occurs because the combined system of the basement walls, foundation mat and first floor slab act as a deep (story-high) orthotropic grillage. As the building settled and the mat foundation dished, this orthotropic grillage followed the mat's curvature and experienced stresses.*

I have also been told that this dishing was believed by the LERA team to have damaged the tops of the existing piles under the perimeter mat, as discussed by Drs. Karp and Kardon in their letter to the Board of Supervisors. The SGH team argue that, based on their analyses, they do not believe that significant damage has occurred, but I remain skeptical as to the accuracy of those analyses and continue to believe that the dishing and integrity of the mat should be of concern.

2. *The suggestion that transfer of 20 percent of the load form the existing piles to the new piles would result in immediate rebound of about 1 inch*.

   Geotechnical analysis conducted by Mr. Egan and his team confirm that approximately an inch of rebound will occur when the load is removed from the building. We concur that this will not occur immediately, but rather may take approximately 1 to 2 years to occur, consistent with the time-dependent rebound behavior of clay soil when overburden confining stress is reduced. The expression of immediate recovery of settlement alluded to was made in the context of the 40-year period over which our team has evaluated the building’s future settlement behavior.

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[1] Karl Terzaghi, the "father of soil mechanics," once said: "Unfortunately, soils are made by nature and not by man, and the products of nature are always complex… As soon as we pass from steel and concrete to earth, the omnipotence of theory ceases to exist. Natural soil is never uniform. Its properties change from point to point while our knowledge of its properties are limited to those few spots at which the samples have been collected. In soil mechanics the accuracy of computed results never exceeds that of a crude estimate, and the principal function of theory consists in teaching us what and how to observe in the field."

[2] A friend who has much experience with offshore structures writes: “Amazing the building guys are still only just trying a 1000-year earthquake and doing a horrible job of foundation modeling, which is fundamentally the most important part of their model - they should model the rest as linear, with p-delta, then remodel as demand requires.”

This is an amazing response centered on the definition of immediate. I understand that, in theory, relieving the load on the Old Bay Clay might lead to a small amount of rebound but this requires sucking in water which would likely take more than one or two years. And Slate's prediction of future settlements shows no rebound at all, as can be seen in Figure 5.

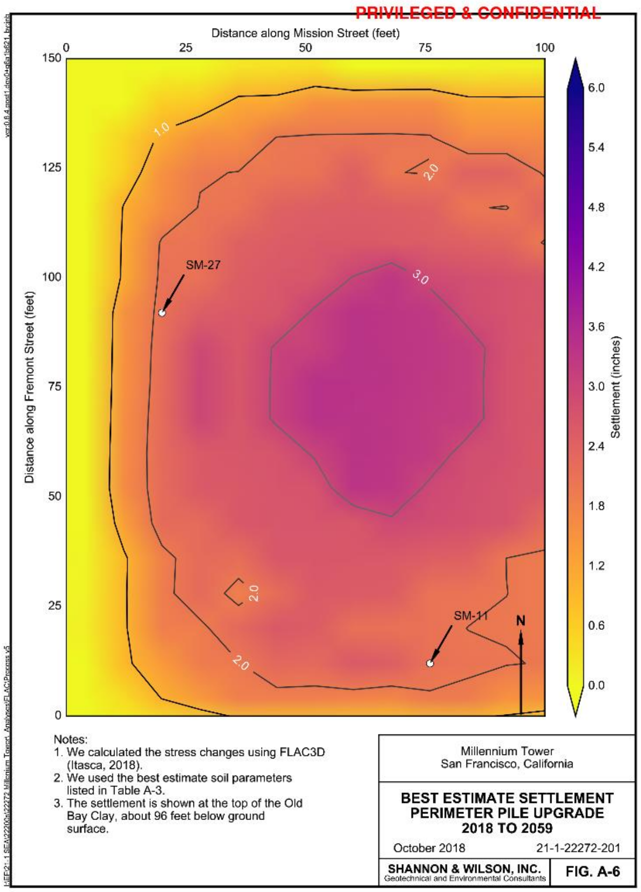


Figure 5 – Slate's Prediction of Future Settlements

3. *Arresting the settlement of the north and west sides of the building while the center and the south-east corner of the building continue to settle can only increase the stresses in the mat that underlies the building and the outriggers when the mat is already dished and cracked, and the condition of the outriggers is uncertain.*

   In the course of our design, we conducted extensive analyses of the post-retrofit settlement of the building, and the effect of this settlement on the mat foundation and structure. These analyses suggest that post-upgrade settlement will counter the settlement that occurred to-date and in the process of doing so, tend to relieve, rather than increase, stresses which have accumulated to-date. We have demonstrated through our extensive analyses, reviewed by the EDRT, that the mat is capable of resisting stresses associated with the addition of the new piles, as well as the building's response to MCER shaking, as specified by the building code.

I am pretty sure that SGH could do an analysis that says I am tall, when everyone can see that I am short! This goes back to my third introductory point. Advanced analyses are difficult to follow and review and the only meaningful check is an independent third-party analysis. For what it is worth, LERA apparently obtained significantly different results. In the letter from Mr. Hamburger to Gary Ho dated February 20, 2019, it is stated that:

We identified the probable reasons that LERA's analyses predict substantially different behaviors for the building than do ours. These are:

1. *LERA's use of amplitude-scaled as opposed to spectrally matched motions.*
2. *LERA's use of an excessively conservative estimate of pile hinge length.*
3. *LERA's assumption that pile prestress is fully effective at the pile top.*
4. *LERA's failure to consider the effect of interaction with the adjacent podium structure.*

Mr. Hamburger may be correct on all these points, but he also might not be correct. Expert structural analysts that I have talked to have many questions about the SGH analyses as described in documents that I have obtained. They say that the calculations need to be validated against observations and/or by independent third-party analyses. But to the extent that the SGH analyses are consistently less conservative than the LERA analyses, this demonstrates one of my main points, which is that there is no single correct answer that can be obtained from analyses. Further, making a series of unconservative assumptions to support a preferred narrative is not a responsible approach. But Mr. Hamburger's apparent willingness to do this might explain why Millennium Partners put their thumb on the scale during the mediation process in support of the SGH solution.

But here is an example of the complications in conducting and interpreting advanced analyses from the 301 Mission Perimeter Pile Upgrade EDRT Meeting 2021-09-16:

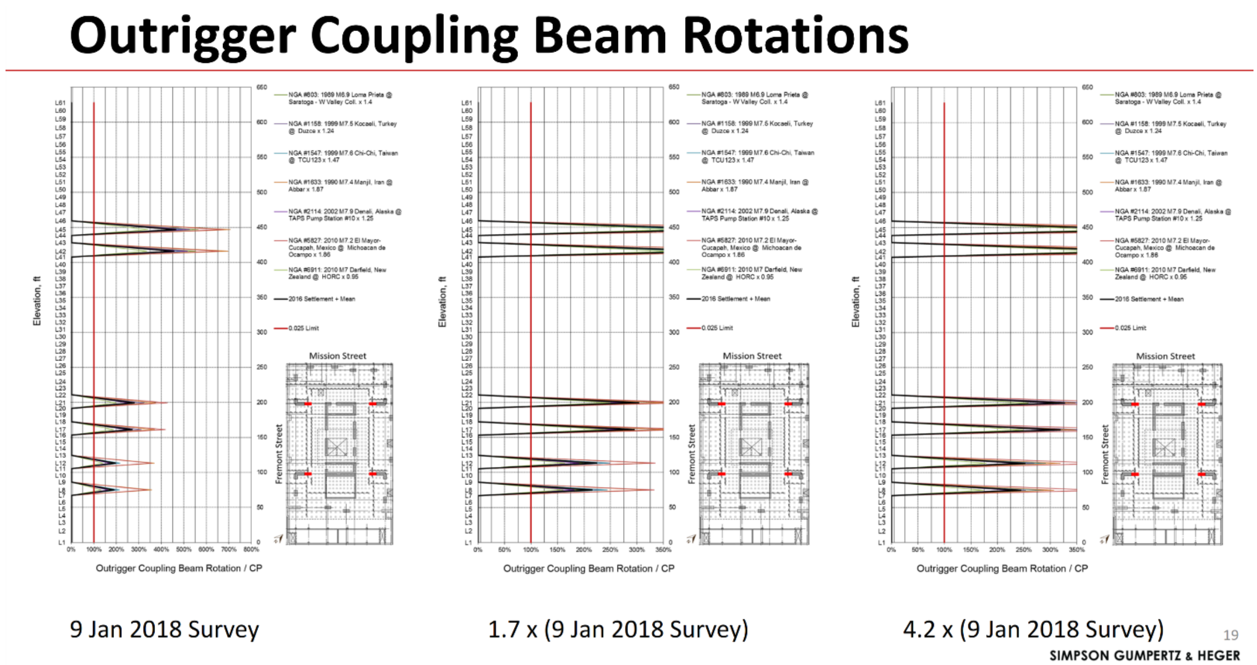


Figure 6 – From SGH Presentation to EDRT September 16, 2021

While narration and questions and answer during the presentation might have more fully explained this figure, it is difficult to get over the first impression that there are real issues with the outriggers, as has long been suspected.

And here is another example from the SGH Supplemental Report for Foundation Investigation, dated July 26, 2017, which was prepared to address questions raised by the EDRT.

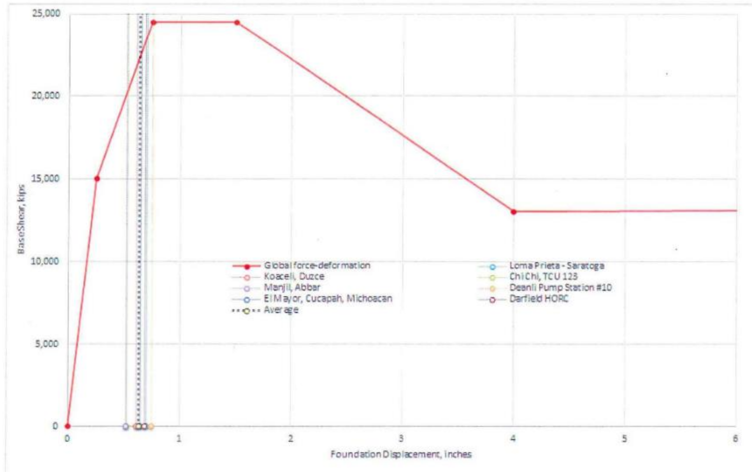


Figure 37. Peak East-West Pile Lateral Displacement Demand from Seven ground motions

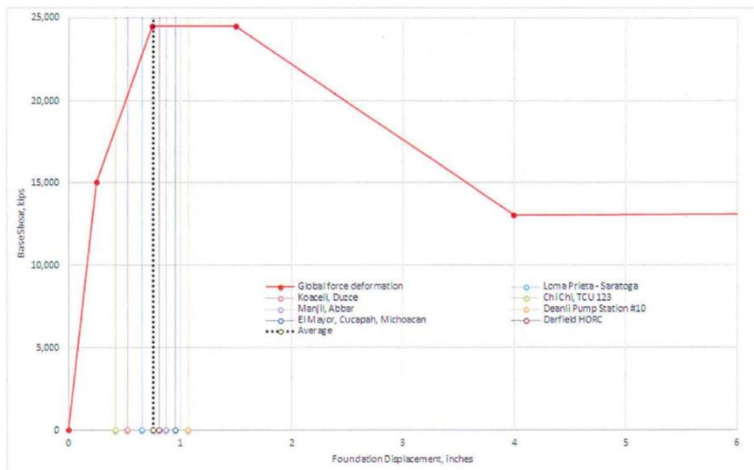


Figure 38. Peak North-South Pile Lateral Displacement Demand from seven Ground Motions

Figure 7 – From SGH Supplemental Report dated July 26, 2017

The report's description of these figures is: "*Figure 37 and Figure 38 respectively present the peak lateral displacement demands on the pile cap in the east-west and north-south directions, overlain on the global pile nonlinear force-displacement behavior previously shown in Figure 22. Predicted pile lateral displacement is typically less than 1 inch and does not approach the displacement at which foundation strength degradation initiates.*" The development of the global force-displacement relationship for lateral loading is described in the report but it is less than clear why it ends up having a significant drop from the peak capacity and it is not clear at all how this relationship reverses when the direction of shaking reverses. And there were necessarily a number of approximations made in constructing this relationship. So, the answers obtained in each direction are just one possible answer and are not necessarily the correct answer. But, if these force-displacement relationships are correct, it is curious that the peak demand falls just short of the capacity. The implication of this is that, given even a small increase in the earthquake loading, the Tower might end up in either Fremont or Mission Streets. I really don't think that will happen because the passive pressures provided by the embedment of the foundation do not appear to have been included in this calculation so that the only real effect might be (further) damage to the tops of the existing piles.

The two previous figures in this report, which are too detailed to include in this commentary, show a similar result relative to the axial capacities of the piles. In both compression and tension, many of the demand-to-capacity ratios are close to 100 percent. That might be explained by the values that are shown being the maximum of seven runs using two horizontal components of motion, but why are so many of them close to 100 percent but not equal to it, and why are there sometimes adjacent values that are much lower? There is nothing wrong with an individual pile reaching a demand to capacity ratio of 100 percent, because that just means that there will be load re-distribution to the adjacent piles, but that does not appear to be what happened. And what would happen if the full vertical component of motion was included in the analyses?

4. *The proposed fix cleverly provides for backing off the underpinning of the north and west sides of the building, should settlement of the south-east corner catch up with and overtake the settlement of the north west corner*.

While it is true that the design would accommodate reduction in the amount of jacking applied along the north and west sides, this was never the intent of the pile head detail. Rather, the intent of this detail was to allow jacking of additional force onto the piles if rebound resulted in reduction of the effective jacking force. We note, however, that since the settlement experienced to-date is due to consolidation of the underlying soils, as the building settles, the consolidating soils will ultimately become normally consolidated and the rate of settlement will naturally diminish significantly with time. In fact, this behavior is evident in review of settlement data collected over the past 18 months.

The first part of this response is very good, but the last part was not credible in 2019 and is even less credible today. Plots of the settlement and the variation in the elevation of the groundwater with time from the latest monitoring report are shown in Figures 8 and 9. Figures 8 and 9 strongly suggest that there was a small increase in the rate of settlement from 2015 to 2018 when the groundwater elevation was drawn down as a result of nearby construction activities and that the rate of settlement then slowed down from 2018 through 2019 as the groundwater elevation returned to normal. But to suggest that meant that the overall rate of settlement was slowing down is wishful thinking. By 2021, before the Perimeter Pile Upgrade commenced, the average rate of settlement over ten years was about ½ inch per year, with no real indication that it was slowing down. The reasons for this are not entirely clear but secondary consolidation / creep bearing capacity failure in the Old Bay Clays are widely believed to be the most likely cause. These are mechanisms that might go on for some decades.

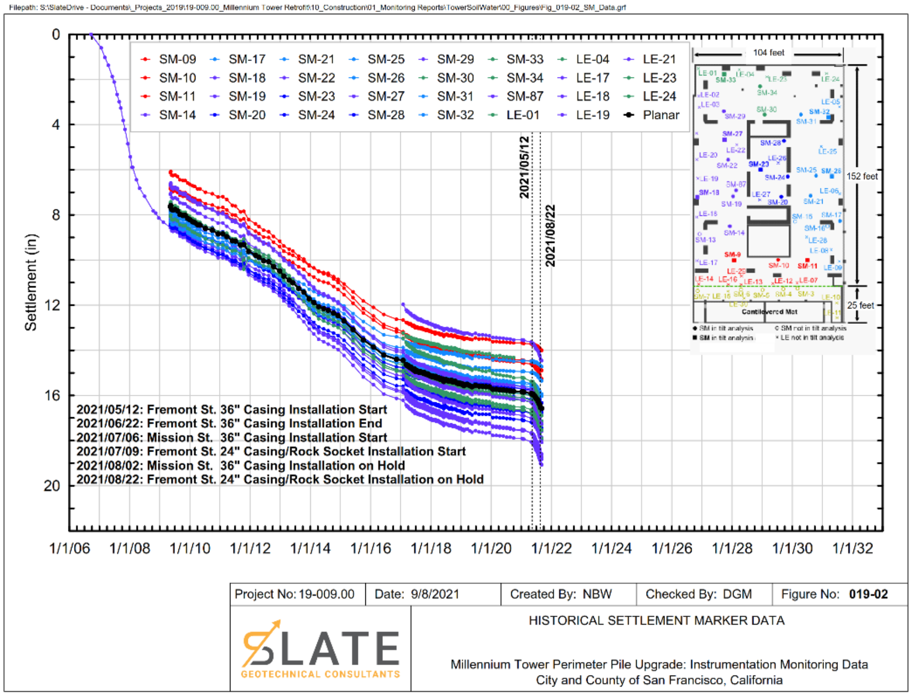


Figure 8 – Historical Settlement Data

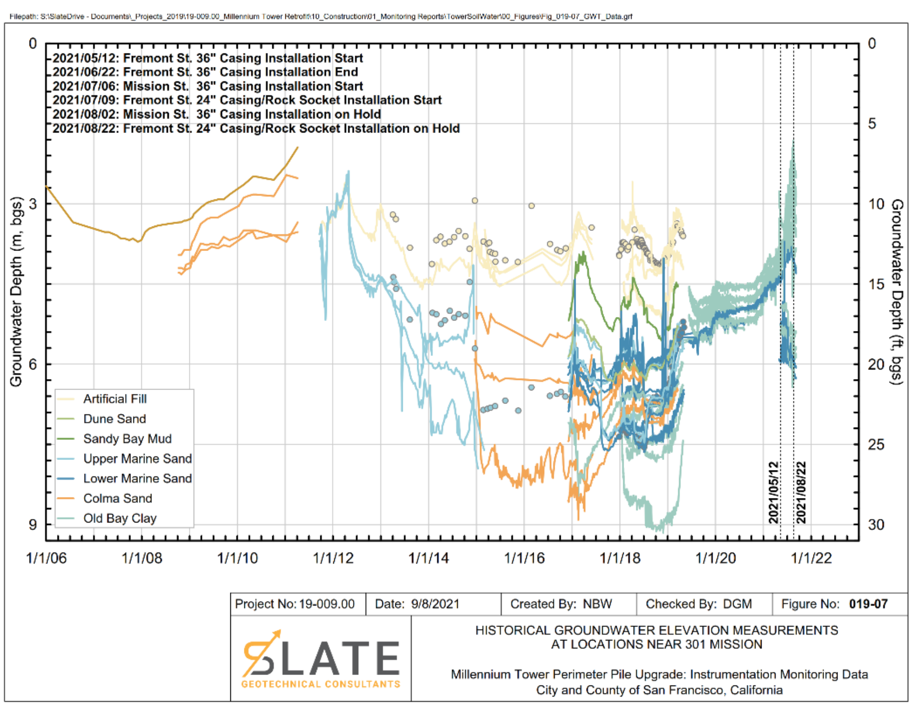


Figure 9 – Historical Groundwater Elevation

5. *The proposed fix creates an asymmetrical foundation which is bad enough under static loads but will create unpredictable and likely adverse response under seismic loads.*

The perimeter pile upgrade adds vertical and lateral stiffness and strength to the foundation along the north and west sides of the building foundation. We have extensively and rigorously studied both effects in our analyses of the design. The upgraded building does not qualify as an "irregular" building under the definition of the building code. Further, the building's response to earthquake motion is superior with the perimeter pile upgrade in place, compared with that of the un-retrofitted building.

Again, the first part of this response is not unreasonable, but the last part is laughable. There are two rather different kinds of response that are impacted by asymmetry – torsional and rocking. Even well-educated and experienced structural engineers, let alone lay people, think at first glance that the behavior of the Tower with the PPU is likely to be problematic in both torsion and rocking under earthquake loadings. However, I believe that Mr. Hamburger is correct in claiming that the upgraded building does not qualify as an "irregular" building under the definition of the building code, at least with respect to torsional loading. It turns out that because of the embedded foundation and the symmetrical forest of existing piles under the Tower, the new perimeter piles do not add much to the torsional rigidity of the foundation.

The major asymmetry due to the PPU, however, is in the vertical direction, which is important especially for rocking of high-rise buildings. Unfortunately, building codes have traditionally tended to downplay vertical motions, and hence rocking, and so it is not addressed in the same way as other effects of asymmetry. Further, the treatment of the vertical component of motion in the SGH analyses is something of a mystery. One of my structural analyst friends, after reviewing a recent SGH presentation to the EDRT, wrote, "Vertical EQ - guessing maybe they used +/-20%, but it is not even discussed, nor included in Load Combination, nor even mentioned. I have read the discussion of 4.3.2.9 Soil Springs several times and have NO IDEA what they actually did. Has anyone seen where they describe vibration frequencies and contributions to base reactions? Any indication of vertical and torsional modes with/without PPU? Is there any discussion of validation of their foundation model?"

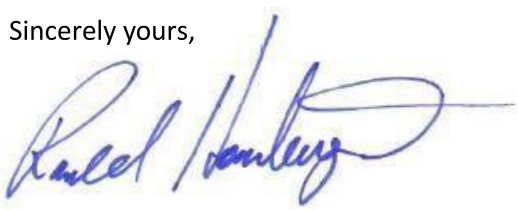
So, the claim that, "the building's response to earthquake motion is superior with the perimeter pile upgrade in place, compared with that of the un-retrofitted building" is highly unlikely to be correct. The rocking behavior has to be worse and the existing condition of the mat and the outriggers, whatever it is, is highly likely to worsen. While I don't think the building is in any real danger of collapsing, it is not a stretch to imagine that it might be red-tagged after even a moderate earthquake.

6. *The proposed fix requires complex and difficult construction on City property which houses many existing utilities and ties backs and will require new dewatering.*

The required construction is neither complex nor unusual. It requires installation of drilled piles around the perimeter of the building. Piles of this type are routinely employed in building construction. The tie-backs, which will be cut, were installed to permit the original excavation for the building's construction. They serve no purpose at this time and were intended to be sacrificial when installed. No dewatering will be required to enable the construction.   Ground water will be controlled by soil grouting as has been successfully done in the construction of other nearby projects.

My response to Point 6 is included in the next section which is titled Subsequent Events.

Sincerely yours,



Ronald O. Hamburger, SE
Senior Principal

CA License No. 2951

Subsequent Events

I have already remarked on how I never heard back from SGH, DBI, or the EDRT. They were under no specific obligation to do that, but it would have been a normal professional courtesy. However, to make it worse, I have recently seen that in an e-mail to Greg Deierlein, the chair of the EDRT, from Mr. Hamburger dated July 25, 2019, transmitting his responses to my comments he said: “As explained in the letter, we believe our design evaluations have addressed all of his technical concerns.” In fact he had not, so that was untrue. My responses above to his responses in Supplement No. 188 and the discussion below on how the installation of the Perimeter Pile Upgrade turned out to be both complex and unusual provide confirmation that he had not adequately addressed all of my technical concerns.

But this is just one of many instances in which Mr. Hamburger appears to be over-confident and less than truthful. A good example of his misdirection is that in lectures given at both the University of Kansas and the University of Minnesota he has said that the length of the building along Fremont Street is 200 feet and that the average bearing pressure is thus 11.2 ksf, whereas, as shown in Figure 8, the length of the perimeter mat is actually 152 feet (with an additional 25-foot cantilever supporting a PG&E substation), so the average bearing pressure under the mat is actually more like 14.7 ksf.

But the prime example of his over-confidence is his assertion in Supplement No. 188 that the installation of the Perimeter Pile Upgrade was “neither complex nor unusual.” Equally surprising is the statement on television by the past-president of SEAONC that, “underpinning is done all the time for houses in San Francisco.” That is true, but not for 600-foot tall condominium buildings and not with over 200-foot-long piles. Drs. Karp and Kardon and I gave warnings that the PPU installation would not be straightforward. Karp and Kardon focused to some extent on renewed reduction of the groundwater elevation. Mr. Hamburger stoutly denied that this would happen, but Figure 9 shows that it did. Neither Karp and Kardon or I predicted exactly what has in fact happened, but on the basis of our experience we

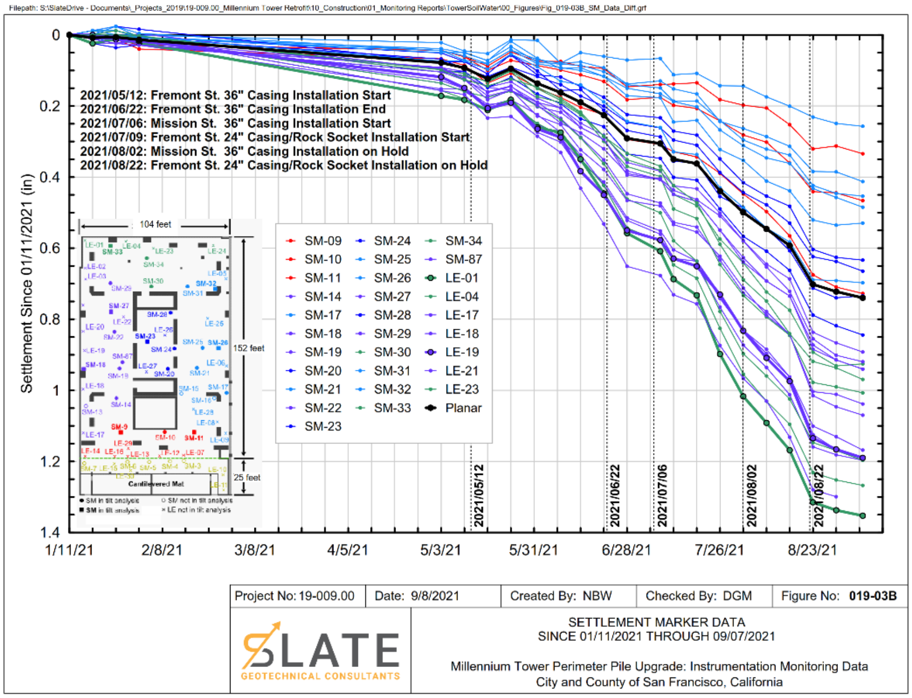


Figure 10 – Enhanced Settlement as a Result of PPU Installation

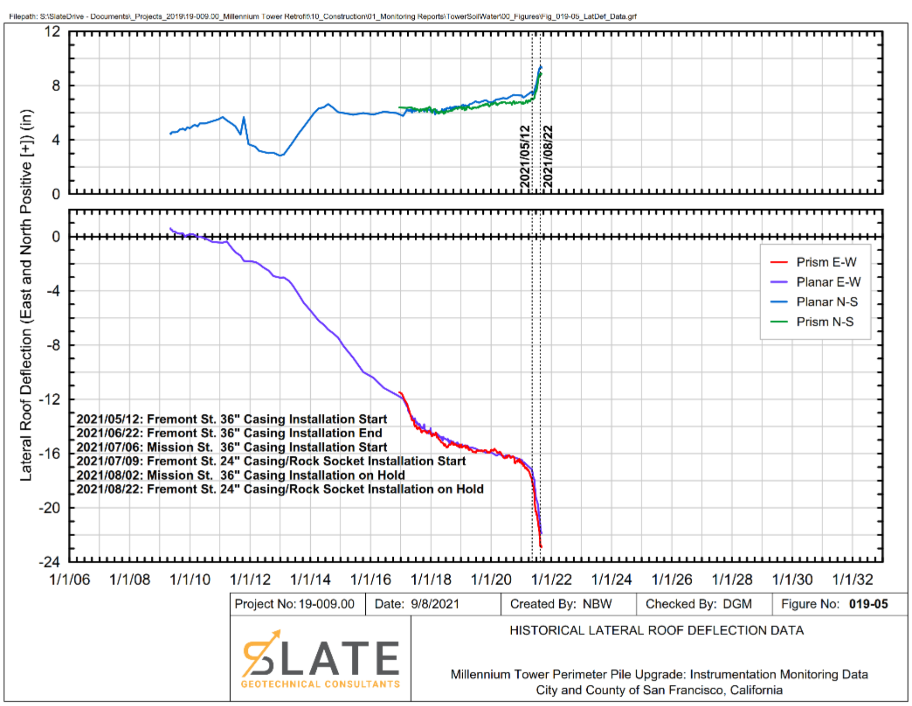


Figure 11 – History of Lateral Roof Deflections

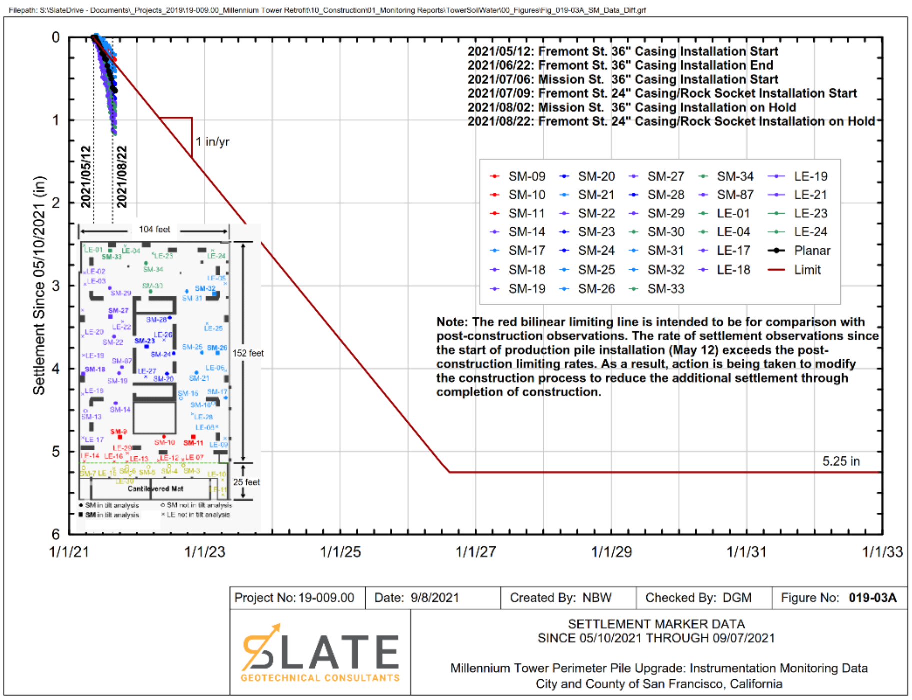


Figure 12 – Slate Prediction of Future Settlement

knew that there was a good chance that Murphy's Law would likely apply. If anything can go wrong, it will go wrong. That is what I meant when I said in my press release that the fix was "too cute by half."

But even worse, there is some reason to believe that the design team tried to cover up the installation issues and keep going. Certainly it was only when the story was broken on NBC Bay Area News by Jaxon van Derbeken that the public became aware of the problem.

What has actually occurred is illustrated in the above three figures. Figure 10, taken from Monitoring Report 19, shows enhanced settlement starting in May 2021 and continuing settlement after both casing and pile installation was put on hold in August. A subsequent monitoring report shows some rebound after that, but more time is required to see how things settle down. Figure 11 shows the full history of lateral roof deflections. Because of the scale of the plot, this figure looks very scary, however, I believe that the design team is correct in saying that the enhanced rate of deflection is only temporary. The question is more why this was allowed to happen, why was disclosure delayed and what are the implications for the design team's credibility on other issues?

Figure 12 shows the enhanced rate of settlement due to the PPU installation relative to Slate's prediction of future settlements. This too looks kind of scary, but the comments in my previous paragraph still apply. Note that the red line in Figure 12 is not Slate's best estimate of future settlement but that their 1 inch per year is their estimated maximum rate of future settlement of the south and east portions of the perimeter mat. This is not inconsistent with my estimate of about ½ inch per year. But where we differ is that, according to Slate, the settlement magically stops in 2026. The load redistribution that will occur if the PPU is completed and some of the weight of the building is transferred to the new piles is uncertain, but my guess is that the contact pressures under the core and the south and east portions of the perimeter mat will be little changed and that secondary consolidation of the Old Bay Clay could continue for decades, resulting in the building eventually tilting to the south-east. And, to the extent that the Tower is restrained from settling on the south and east sides because of the adjacent buildings, it is possible, even likely that the dishing of the perimeter mat will increase.

But, back to the question of the design team covering up the installation issues and continuing work on the retrofit construction. It appears that the installation was only halted as a result of pressure from the EDRT. It had been obvious since at least the end of June 2021 that there was a problem and on July 29 Greg Deierlein, the chair of the EDRT, wrote to the DBI as follows:

"*Because of uncertainties involved related to relative contribution of the above factors and to develop the most appropriate/practical measure(s) to mitigate the accelerated settlement, we understand that the Design Team has suggested to the 301 Mission homeowners association representative and others managing the retrofit project that the installation of 36-inch diameter casings along Mission Street be paused and the rate of settlement be carefully monitored during installation of the 24-inch casings. The benefits of this approach would be: (1) to allow more time to investigate the issues related to installation of 36-inch casings and (2) to separate accelerated rate of settlement between installation of the 36-inch casings and that of the 24-inch casings.*

We support the suggestion to pause in 36-inch casing installations to evaluate the situation; however, we understand that this suggestion has not been acted on and the project is continuing to move forward without any pause in construction. We are bringing this to your attention as a point for DBI to be aware of and perhaps raise with the 301 Mission Street building owners.

We would request a meeting with DBI staff to discuss this issue as soon as possible."

The installation of the 36-inch diameter casing was then halted on August 2 but the installation of the 24-inch piles and rock sockets continued until August 22, three days after a meeting with the EDRT.

At the August 19 meeting with the EDRT the design team offered the following possible reasons for the enhanced settlement and tilting:

- *Heave of Old Bay Clay (OBC) into the 36" casing at the end of installation due to stress relief*
- *Over excavation of Marine and/or Colma Sand during the 36" casing installation due to hydraulic gradient*
- *Soil densification due to vibration*

The remainder of this presentation in my view was very weak and indicated a lack of experience and judgment.

More recently, Mr. Hamburger has said in a letter dated September 28, 2021, to the HOA:

"*We have identified three potential sources of the increased settlement and tilting that has occurred as casings and pilings are installed. One of these is associated with installation technique that results in removing a greater volume of soil at depth than is replaced with pile, which I term over- excavation. A second is that after the casings are installed, soil from beneath the building may be heaving into the* *open casings. The third is vibration-induced densification of the sand layer, which supports the existing piles. Shimmick has identified revisions to its installation technique, both for casings and pilings, that will reduce the amount of over-excavation. In the last week, the project geotechnical engineers performed on-site testing of soils within and adjacent to the casings, and have determined that heaving is unlikely to be a cause of the settlement and tilting."*

There are actually four explanations buried in whatever three they list:[3]

1. Straight over-excavation, which they now restrict to overdrilling for the 24-inch casings. They may have also overdrilled for the first 20 feet of the 36-inch casing, but I have now figured out that after that they were pushing these casings in (I think they were just pushing them – if they were driving them with a hammer that would cause more "vibration", which maybe they did and that might explain why they are worried about vibrations – I have not seen anywhere a clear explanation of what they were actually doing).

2. What was called, "over excavation of Marine and/or Colma Sand during the 36-inch casing installation due to hydraulic gradient" on August 19 and now appears to be lumped into what Hamburger calls "over-excavation." Great idea to bury this because I think that it is perhaps the most likely cause and it results from a basic error – the failure to maintain enough soil plug and water in the casing to prevent soil from outside the diameter of the casing entering the tip of the casing! I don't know whether they are claiming that this has now been ruled out, but since we have not seen the recent borings, I remain skeptical.

3. What was called "heave of Old Bay Clay (OBC) into the 36-inch casings at the end of installation due to stress relief" on August 19 and is now described as "after the casings are installed, soil from beneath the building may be heaving into the open casings." If the enhanced rate of settlement is due to OBC squeezing into the casing, as seems to have been suggested by Dr. Vahdani in the comment log, doesn't this suggest, along with the overall pattern of continuing settlement, that a creep bearing capacity failure is occurring in the Old Bay Clay? (This might explain why settlement continued for at least two weeks after the pile installation was put on hold – at that point I think the 36-inch casings were all tipped in Old Bay Clay – and why the extensometers show continuing "settlement" in the Old Bay Clay.) Mr. Hamburger is now ruling this mechanism out, but I would like to know what Dr. Vahdani thinks.[4]

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[3] The day after Mr. Hamburger wrote to the HOA with the quote that I excerpted above, that is, September 29, he wrote to the DBI with a revised list of possible reasons for the enhanced settlement and tilting which has four points, that are similar to mine but numbered in a different order!

[4] The mechanism that Mr. Hamburger variously calls "over-excavation" or "heave" and includes my mechanisms 2 and 3, is still ruled out in his September 29 letter, but not having seen the results of the new site investigation, I am skeptical of that. A boring was apparently only drilled into one 36-inch casing. Another friend who is supportive of Dr. Vahdani's line of questioning has written: "My perception of the problems from early on involved a plunging failure of the mat supporting piles such that the actual pressure bulb hardly resembles an elastic half space. If you don't evaluate the potential for bearing capacity failure involving tens of inches of settlement, you would have done poorly in the foundation engineering course I was taught and thereafter taught in graduate school.

4. Vibration. I don't know whether this is a deliberate obfuscation or whether they really believe it. But I don't believe that this explanation is credible. I did my Ph.D. thesis on compaction of sands by cyclic loading and like to think that I know something about this subject. The sands in question are in part clayey and in any case have been there for centuries and will have been shaken by earthquakes from time to time. On Treasure Island it was found that the shoal sands, as opposed to the hydraulically placed sand fill, could not be densified even by heavy vibratory loading. The chance that the sands in which the existing piles under the Tower are embedded have been densified by vibrations caused by the installation process is negligibly small.[5]

I still have the following questions:

1. Why was installation of the perimeter piles not paused by the end of June at the latest when it was already obvious that there was an enhanced rate of settlement?[6]

The modern monitoring results of large pile rafts supporting rigid mats shows extreme loads on the perimeter piles of the mats — virtually no shear friction on the shafts of the inner piles. I believe the current fix design team still does not recognize the metastable nature of the foundation. Their modeling of the building's response to a major earthquake with piles already in the plunging mode fails this course."

[5] I was only thinking about the sand in which the perimeter piles are being installed when I wrote this. As a third friend has pointed out, "the contractor could only drive piles readily to the design toe elevations in the NW corner of the Tower. Elsewhere, there were areas where the piles were driven to refusal short to well-short of the design toe elevations, regardless of how deeply they predrilled. With 900+ piles mostly driven 3 ft center-to-center, whatever sands they reached or penetrated through were pounded into a much denser configuration."

[6] Monitoring Reports through No. 022 are now available on the DBI web site. They show continuing settlement and tilting since the pile installation was paused. Of particular interest is the fact that even the soil borings conducted two weeks ago apparently had an adverse impact on the settlement and tilt of the building. The approximate increase in lateral roof deflection to the west was 0.3 inches, and the approximate settlement of the north-west corner of the building was 0.04 inches, a ratio of about 8. This is higher than the same ratio was during the perimeter pile installation and much high than the ratio was during the ten years from 2011 to 2021, because the difference between the settlement of the north-west and the south-east corners of the building is much greater. In the letter that Mr. Hamburger wrote to the DBI on September 29 he suggests that the additional settlement caused by installation of a test pile at location 33 should not exceed ¼ inch. That does not sound like much, but it might translate to a lateral roof displacement to the west of as much as 2 inches. That is kind of scary! Why? Because in his response to Comment No. 67 Mr. Hamburger says (spelling mistakes are his, not mine): "*Please refer to the 16 September EDRT meeting demonstratives for our analysis of the building response to gravity loads in combination with MCE shaking. These analyses, which were presented to the EDRT as part of the original permit process, demosnstrate that the sturrre and its foundations are stable and safe for tilting of as much as 29 inches to the west and 13 inches to the north, while present tilt is 22 inches to the west and 9 inches to the north. These analyses including all of the effects requetsed. Also note, that we evalutaed the building for as much as 58 inches of tilt to the west and 26 inches to the north and found that the building would resist MCE shaking, though perfomrance was starting to degrade. We are confident that present levels of tilt have not created an unsafe condition.*" These numbers are repeated in the September 29 letter to the DBI, except that the 13 inches has fallen to 12 inches. Notwithstanding my reservations about the detail and the accuracy of the SGH calculations, Mr. Hamburger has thus gone on record as saying that exceeding these numbers for the lateral roof displacement may create an unstable and unsafe condition during a major earthquake. He also says that the lateral roof displacements are already 22½ inches to the west and 9 inches to the north, so that there are only 6½ inches and 3 inches left to reach his own limit for acceptable behavior. But even installation of a single test pile using improved installation

2. Why had they not sampled the material at the bottom of the plugs in the casings to see whether it is sand or Old Bay Clay (OBC) that is entering into the bottom of the casings at an earlier point?

3. Why does the design team continue to talk about compaction caused by vibration being a major contributor to the enhanced rate of settlement when that is not credible?

4. If the enhanced rate of settlement is due to an adverse hydraulic gradient causing sand to enter the casing (which at one point seemed to be the design team's favored theory), why was that allowed to happen? This is Drilling 101.

5. Doesn't this all suggest that the design team does not know what they are doing and that their assurances relative to other issues involving future performance may be worthless?

Conclusions

To repeat what I said at the beginning of the preceding section: Mr. Hamburger in an e-mail dated July 25, 2019, transmitting his responses to my comments said: "As explained in the letter, we believe our design evaluations have addressed all of his technical concerns." Meaning *my* technical concerns. In fact he had not, so that was untrue. Well, he might have believed it, but his belief was incorrect. My responses above to his responses in Supplement No. 188 and the discussion above on how the installation of the Perimeter Pile Upgrade turned out to be both complex and unusual, and in fact a fiasco, provide confirmation that he had not adequately addressed all of my technical concerns. I believe that I am a bit of a pushover when people sit down and explain things politely to me. More than once, I have said something like: "I don't think that I would do that, but if you are confident that it will work and you are signing and stamping it, good luck." But the failure of either SGH or the EDRT to respond to me suggests that they did not have adequate explanations and that they knew it. But this is just one of many instances in which Mr. Hamburger appears to be over-confident and less than truthful.

I believe that the fiasco involving the enhanced settlement triggered by the PPU installation and the design team's changing explanations for it indicate that the design team in general, but Mr. Hamburger in particular, do not have a good grasp on what they are doing overall. I continue to believe that the

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procedures might add 2 inches to the displacement to the west! Of course these are kind of "worst case" numbers, but if, for instance, the additional settlement caused by completion of the perimeter pile installation is a total of an inch, which might be a low estimate, the total lateral displacement to the west might be increased by up to 4 inches, using a lower value of the ratio of roof displacements to settlement based on the data since May 12th, and the total lateral roof displacement to the west would become 26½ inches, which is perilously close to the limit of 29 inches. And what if that 29 inches is a little bit unconservative? The SGH computations as to the current state of the outriggers due to tilt and the static loads are confusing but can be interpreted as indicating that they are already close to or at failure so that it would not be surprising if the 29 inches is a bit unconservative. And, given that the lateral roof displacement to the west was already on the order of 16 inches when the perimeter pile installation began, why did the SGH design team allow the contractor to continue for so long as the original 13 inches from the limit dropped in half to 6 ½ inches from the limit?

PPU will not solve the settlement problem and will make the behavior under earthquake loading worse, not better, as Mr. Hamburger claimed in his 2019 response to me: “Further, the building’s response to earthquake motion is superior with the perimeter pile upgrade in place, compared with that of the un-retrofitted building.” That is not credible.

And there is still the question of why the EDRT signed off on this fiasco? The materials that I have seen only recently certainly suggest that Millennium Partners had put their thumb on the scale in the mediation process (see above), so for the EDRT it was perhaps a choice between agreeing to the SGH solution or doing nothing. I also have now seen correspondence confirming that the letter from Greg Deierlein to the DBI signing off on this fix was edited by lawyers for the City prior to being finalized. This was not a surprise to me – it had always looked as if it was edited by attorneys. But it is a pity that the recommendation from the EDRT that they be engaged for ten years to continue to review the matter was not edited out, since this gives the appearance of a conflict of interest, whether or not there is a real conflict of interest. There is also the fact that the two geotechnical engineers on the EDRT had at one time worked for Treadwell & Rollo, the geotechnical engineer for the original design and construction of the Tower. I do not have a problem with this, but I know that for some other people it again it creates the appearance of a possible conflict of interest.

Throughout this whole affair, the EDRT has been asking some good questions. So many in fact, that that alone has to cast some doubt on the credibility of the SGH design team and their calculations. However, the EDRT then gets the runaround from the design team. A mixture of gobbledegook and evasion, like Mr. Hamburger’s response to my 2019 press release. This is not a good process. So, my final question is: Why don’t the DBI and the EDRT put their collective foot down more firmly? A good first step would be to suspend the installation of the PPU until the decision to approve it in the first place is reviewed.

Robert Pyke, Ph.D., G.E.

Modeling of stress-strain relationships of soils from Ted Davis to current practice

Modélisation des relations contrainte-deformation des sols de Ted Davis à la pratique actuelle

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ABSTRACT: This paper summarizes the history of the treatment of stress-strain relationships in geotechnical engineering over the 60-year period from the early 1960's to present day. Key developments in this treatment from the initial separation into elastic and plastic analyses, through the use of hyperbolic stress-strain relationships for static analyses and equivalent linear analyses for earthquake engineering analyses, to more fully addressing cyclic loadings, nonlinearity and three-dimensional geometry are noted. It is concluded that current practice is uneven, with an inadequate emphasis on behavior in unloading and reloading, a persistent failure to distinguish between behavior under slow monotonic loadings and fast cyclic loadings, and the lack of study of the transition from cyclic loading to failure in a particular direction. The complexity of full three-dimensional soil models used in more advanced analyses has the effect of making soil models like religion – everyone believes in their own but is reluctant to accept anyone-else's. It is also concluded that the observation made by the faculty at the University of California, Berkeley, in 1970, that our analytical capabilities were getting ahead of our capacity to evaluate soil properties by use of field and/or laboratory tests, is even more true today.

RÉSUMÉ : Cet article resume les etapes importantes dans l'histoire de l'analyse des relation contraintes-deformations sur une periode s'etendant du debut des annees 1960 a nos jours. Au cours de cette etude, des developpements cles ont ete constates, comprenant la separation entre analyse en domaine plastique et elastique, et passant par l'utilisation d'une relation contrainte-deformation de type hyperbolique pour les analyses statique et de type lineaire pour les analyses d'ingenierie sismique, afin de traiter de facon plus convenable les charges cycliques, l'analyse non-lineaire et la geometrie tridimentionnelle. Il peut etre conclu que les methodes actuelles sont contrastees, incluant une insistance inadéquate sur le comportement au déchargement et au rechargement, un echec constant a distinguer un comportement sous charge monotone lente et charge cyclique rapide, et le manque d'etude approfondie au sujet des transitions entre charge cyclique et rupture selon une direction (un plan) particuliere. La complexite des modeles de sols en trois dimension utilises lors d'analyses plus avancees a pour effet de les faire passer pour l'equivalent d'une religion – chacun croit en son propre modele et n'est pas dispose a accepter celui d'une autre personne. Il peut egalement etre conclu que le constat fait en 1970 a l'Universite de Californie, Berkeley, suggerant que nos capacities d'analyses devancaient nos competences a evaluer les proprietes des sols in-situ et/ou en laboratoire, est d'autant plus vrai aujourd'hui.

KEYWORDS: soils, stress-strain, nonlinear, cyclic, three-dimensional

1 INTRODUCTION.

The author was initially taught soil mechanics by the late Professor E.H. (Ted) Davis at the University of Sydney in 1962 and 1963. Then, after working for 5 years, principally on investigations, design, and construction of Corin Dam, outside Canberra, Australia, he attended graduate school at the University of California, Berkeley (Cal), from 1969 to 1973.

During the period from 1963 to 1969 civil engineering had gone from use of slide rules to use of electronic computers and the programs in structural and geotechnical engineering at Cal were among the leaders in developing modern methods of analysis made possible by the use of computers. Within geotechnical engineering these analyses fell into two general categories: "static", or monotonically loaded analyses of slopes, embankments and excavations, and "dynamic", or cyclically loaded analyses of site response and also the response of slopes, embankments and excavations during earthquakes. Because the speed and capacity of computers was still limited at that time, simplified methods of representing soil properties were used in both static and dynamic analyses for both one and two-dimensional geometries.

Subsequently, as computers became faster and were able to access much larger memories, a variety of more complete soil models have been developed for use in analyses of two and three-dimensional geometries, but there is still no widely accepted 3D soil model. It has been said that soil models are like religion: everyone believes in their own but is skeptical of anyone-else's.

Since 1973 the author has been involved in a wide variety of applications of these methods of analysis and has developed his own simplified nonlinear soil model for use in earthquake site response, liquefaction, seismic settlement and lateral spreading analyses. This paper briefly describes the history of developing soil models for use in geotechnical and soil-structure interaction analyses. It is noted that soil behavior is interesting and complex, which makes this a challenging task. However, many geotechnical engineers have failed to grasp the importance of reversals and have failed to distinguish between behavior under slow monotonic loadings and fast cyclic loadings. The over-use of linearization of soil properties to accommodate structural engineers who do not wish to include representation of the actual nonlinear behavior in their models is also a continuing problem.

2 TED DAVIS.

Ted Davis was a brilliant man who developed the concept of using elastic theory at low strains and plasticity theory at larger strains into a workable approach for practicing engineers. John Christian once told the author of his admiration for Ted, who while on sabbatical leave at MIT when walking across the campus and an interesting problem came up in discussion, would reach into his briefcase and say "oh, I have a solution for that". I am sure that Ted understood that soils were complex because of the care that he would take in repeatedly say "assuming that we have a linear, homogeneous, isotropic material" before detailing an elegant elastic solution. As I recall, Ted taught us that elastic solutions could safely be used up to about 50 percent of the ultimate load, which is sufficient for many practical purposes where one aims to have a factor of safety of two or more. In this range it is possible to select an appropriate average modulus somewhere between the maximum modulus and half that value depending on the nature of the loading. When failure was approached, Ted switched to using plasticity theory. After struggling though Hill's "Mathematical Theory of Plasticity", developed for metals, I was not convinced at the time, and remain unconvinced to this day, that any soil particle has ever read the Mathematical Theory of Plasticity. But at the time this approach was a good starting point. In conjunction with Harry Poulos, Ted developed and published many useful solutions for elastic behavior and in conjunction with John Booker, innovative solutions to plasticity problems. In fact, the Davis-Poulos-Booker approach remains applicable for many problems today provided the engineer shows good judgement in choosing the required soil properties. Indeed, one of the attractions of this approach is that it is clear that there are some assumptions, and thus approximations, involved, so that the engineer might be less inclined to believe that the calculated answer is precisely correct. There continue to be assumptions and approximations involved in more complete and complex models of soil behavior, but they may not be so obvious.

3. WHY SOILS ARE INTERESTING.

Soils are not in fact linear, homogeneous, isotropic materials. "Plastic" behavior starts at very small strains; they are layered and lensed; and they show inherent and stress-induced anisotropy. The stress-strain relationships of soils are strongly impacted by reversals in loading, and by stress and strain history, and by ageing. Cohesive soils act quite differently when they are over-consolidated, and cohesionless soils can change their properties dramatically with confining pressure - so much so that they might be thought of as not a single material but a family of materials with a transition from contractive to dilative behavior as a function of both strain and pressure for a soil with a given density. Evaluation of soil properties is further compounded by differences between field and laboratory behavior. Laboratory measurements of soil properties are almost always impacted by both changes in the soil fabric, because of either sample disturbance or reconstitution of test specimens, and the deformation boundary conditions of the particular test apparatus. A partial answer to this problem is to rely more on field tests to measure soil properties at low strains, but reliance on field tests, and in particular penetration tests of any kind, for the evaluation of properties at large strains is problematic. All these factors should be kept in mind when constructing or using more advanced soil models, but that appears to happen rarely if at all. Ideally a paragraph like this and a warning that the purpose of analysis is to gain insight into the problem at hand, rather than to obtain precise numerical results, should be included as a preface to every geotechnical consulting report.

4. SIMPLE SOIL MODELS FOR STATIC ANALYSES.

Following the development of the finite element method as a practical tool for structural analysis in the late 1960's it was quickly adopted for use in geotechnical analyses. Some of the earliest work used linear elastic stress-strain relationships, capped by the shear strength of the material, usually determined by the Mohr-Coulomb failure criterion. This bilinear elastic-plastic model of soil behavior has come to be known at the Mohr-Coulomb model, which is not a particularly good name but seems to have stuck. However, this model ignored the ability of the then new computerized analyses to follow the transition from elastic to plastic and bridge the two components of the Davis-Poulos-Booker approach. To address that problem Mike Duncan and his students at Cal developed a hyperbolic stress-strain relationship, generally referred to as the Duncan and Chang model, in which the stress-strain relationship was a hyperbola fitted between the elastic modulus at small strains and the ultimate static strength. This remains a powerful approach for many problems today. However, even though this model provides good results throughout much of the loading, it breaks down near or at the point of failure because soils really do exhibit plastic behavior at that point with the strain increments being more or less in the direction of the stress, rather than the stress increment. Thus, current consulting practice has generally shifted to using more modern finite element or finite difference programs using more sophisticated three-dimensional "plasticity" models. However, frequently the old Mohr-Coulomb model, the simplest of all possible plasticity models, is used, which appears to the author to defeat the purpose of spending time and money on an advanced analysis. The defenders of such analyses like to claim that they can get the correct answer in back-calculations of observed behavior, but that is true only if they happen to choose the correct input parameters. There is usually some combination of input parameters that can match key elements of the observed behavior, but that should not give any confidence that the model can be used for forward projections.

Another problem with this class of model is that reversing loads are not always addressed. This is relatively easy to do using, for instance, the Cundall-Pyke hypothesis described below. However, in at least some analyses when the load reverses the stress-strain point simply moves backwards down the same stress-strain curve, which is quite wrong.

5. EQUIVALENT LINEAR DYNAMIC SOIL PROPERTIES.

At the same time as the Duncan and Chang approach to modeling soil behavior for static analyses was being developed, Harry Seed and Ed Idriss developed the concept of using "equivalent linear" properties for use in iterative linear analyses of earthquake engineering problems. This was done partly to accommodate the methods of analysis being used at that time which required linear elastic properties and partly to avoid the complexities of modeling the actual nonlinear behavior.

Such equivalent linear "properties" consist of three elements: the maximum shear modulus, that is the shear modulus at low strains; a modulus reduction curve that plots the ratio of "average" secant shear modulus, typically taken as the secant shear modulus on the 5^{th} cycle of a strain-controlled laboratory test, and the maximum shear modulus, as a function of cyclic shear strain; and the damping ratio, also plotted as a function of cyclic shear strain. The damping ratio is an odd quantity obtained partly by equivalencing the area of a real stress-strain loop to the area of an ellipse described by an equivalent visco-elastic model. These approximations, which neglected the effects of any excess pore pressure development and any hardening effects and the fact that the cyclic strains were constantly changing, were consistent with using iterative linear analyses using properties based on the "average" cyclic shear strain in each layer or element to conduct analyses of nonlinear systems. Figure 1 below, which shows the stress-strain history for a typical layer in a site response analysis using one of the motions from the 1995 Kobe earthquake as the input, suggests that determining an appropriate average cyclic shear strain is in fact quite a challenge. Equivalent linear analyses served a useful function at that time but, as an example, in earthquake site response analyses, one of their main applications, they exaggerate the response at the equivalent natural period and overdamp or filter out the response at other periods. They did allow deconvolution of ground surface motions, but the resulting input motions were not realistic. A typical defense of the use of such approximate analyses is that it is not possible to deconvolve the ground surface motions using a nonlinear analysis, but the author has developed an iterative procedure to do that which converges very quickly on realistic looking base motions. Equivalent linear analyses are stable and reliable because of the inherent overdamping of higher frequencies, but they provide reliably incorrect answers.

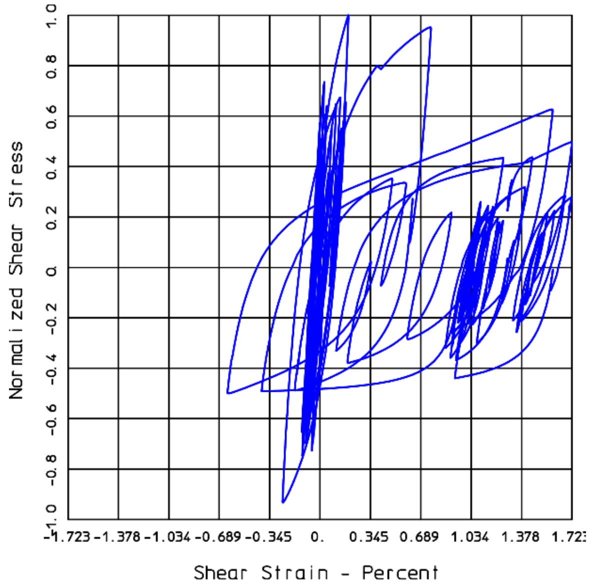

Figure 1. Shear stress – shear strain history for a sandy silt layer in a site response analysis using the NIS000 record of the Kobe earthquake.

6. SIMPLE NONLINEAR DYNAMIC SOIL MODELS.

Simple models of the shear stress – shear strain behavior under cyclic loading can be constructed using the same shear modulus reduction curve that is used in equivalent linear analyses as a "backbone curve". The backbone curve could just be defined by a table of values but is commonly specified to be a hyperbola or a modified hyperbola whose initial slope equals the shear modulus at small strains. The asymptotic limit on the shear stress is often taken to be equal to some vaguely defined shear strength, but, as discussed below, is better thought of as just an asymptote.

Rules are then needed for how this backbone curve translates to unloading and reloading curves under cyclic loading. Initially, and to this day, this is commonly done using the Masing hypothesis, even though Masing's original paper was written in German and few geotechnical engineers have ever read it. The Masing hypothesis was based on the behavior of an assembly of springs and sliders, now commonly called an Iwan model. This kind of model simply doubles the scale of the backbone curve on the first unloading and then maintains that shape on subsequent loadings and unloadings. The Iwan model has been used with some success for modeling, for instance, joint behavior in structural analyses, but it has a big limitation for use in geotechnical analyses and the fact that it complies with the Masing Hypothesis should not be taken as anything other than a circular argument. Masing himself tested his hypothesis on brass cylinders and concluded that it did not work very well.

A clue to the limitations of the Masing hypothesis is provided by the fact that in an application such as that shown in Figure 1, additional rules are needed to make it work – if the shear stress on reloading or unloading crosses the previous loading or unloading curve, it has to follow the previous curve; if the shear stress on reloading or unloading exceeds the asymptotic shear stress, the material suddenly becomes plastic and maintains that shear stress. But the big limitation of the Masing hypothesis for geotechnical studies is that if there is an initial shear stress at the start of a cyclic loading, it is well documented that even with a uniform symmetrical cyclic loading, the test specimen will accumulate permanent displacements in the direction of the initial shear stress. However, even a model that follows the extended Masing rules does not do that – it simply cycles around and around without developing permanent deformations.

This problem led the author to conclude that a better rule was that the scale of the backbone curve should be changed by a factor equal to the normalized distance from the reversal point to the asymptotic stress in the direction that the load was now heading. Thus, if the reversal point was close to the asymptotic shear stress, either positive or negative, the factor would be close to two, and if it was at zero shear stress, the factor would be one. Peter Cundall had reached the same conclusion independently and so when the author published this idea (Pyke 1979) he named it the Cundall-Pyke hypothesis. This basic rule eliminates the need for any additional rules since a model based on it never actually reaches the asymptotic shear strength let alone exceeds it. And, critically, permanent deformations are developed when even a symmetrical cyclic load is applied on top of an initial shear stress.

As noted already, the backbone curve is commonly specified to be a hyperbola or a modified hyperbola whose initial slope equals the shear modulus at small strains and whose asymptotic shear stress is variously defined. Stress strain curves for soils are not exactly hyperbolic and so various workers have suggested fitting parameters in order to make modified hyperbolas that provide a better match to laboratory data. The earliest of these to gain prominence was that of Hardin and Drnevich which used two fitting parameters, a and b. The author used these in his first soil model using the Cundall-Pyke hypothesis which was thus called the HDCP soil model. However, the parameter b led to potentially unstable results and the Hardin and Drnevich a parameter had the wrong signs which meant that sands were more linear than a plain hyperbola and clays were more nonlinear, when the opposite is really true. Darendeli (2000) later suggested a modified hyberbola with a single parameter, also called a, that provides more stable and accurate results. However, it is likely a mistake to try to closely match any particular set of laboratory tests results exactly since those results are impacted by the boundary conditions of the particular test apparatus and do not necessarily accurately represent field conditions. Added to which, the fabric of the tests specimens is likely to be different whether due to sample disturbance or remolding. In fact, many laboratory tests are carried out using washed and screened sands or pure clays, which are rarely if ever found in natural deposits. So, the general trends observed in laboratory tests should be respected, but not necessarily in every detail.

Pyke et al. (1993) constructed families of modulus reduction curves for both sands and clays using the HDCP model with plain hyperbolas and using the "reference strain", calculated as the shear strength or asymptotic shear stress divided by the shear modulus at low strains, that is the maximum shear modulus, to characterize the individual curves. Alternately, for a plain hyperbola, the reference strain is the cyclic shear strain at a shear modulus equal to half the maximum shear modulus. A typical reference strain for a clean sand at low confining pressures is 0.1 percent and for a clayey silt like San Francisco Bay Mud it is 0.3 percent.

Pyke et al. (1993) also provided companion curves for the damping ratio that is used in equivalent linear analyses. The values shown in that report are 50 percent of the hysteretic damping generated by the HDCP model using plain hyperbolas, because a plain hyperbola without introducing features such as the development of pore pressures and dilation at larger shear strains, which make the stress strain loops more S-shaped, leads to hysteresis loops that are too fat. The reduction by 50 percent provided values that were consistent with hysteretic damping values measured in constant strain laboratory tests and generally was found to give acceptable results in equivalent linear analyses.

Curves of damping ratio vs cyclic shear strain are not required as an input to nonlinear analyses because the appropriate hysteretic damping is generated automatically. This is illustrated in Figure 1. This example includes some excess pore pressure development and modest dilation at larger shear strains but the larger loops are relatively fat. However, it can be seen that when driven by an irregular cyclic loading the closed loops are smaller and less numerous that might be expected under an equivalent uniform loading. The overall pattern of behavior is in fact quite different from a laboratory test conducted with either constant cyclic shear stresses or strains. The permanent strains in this example are due simply to the irregularity of the input motion but model will develop permanent strains if there is an initial shear stress.

The original example of the Cundall-Pyke hypothesis in Pyke (1979) showed the development of permanent shear strains under even a symmetrical cyclic load when it was added to an initial shear stress but generated permanents strain too quickly. This was because the stiffening of the shear modulus, or "hardening", that results from cyclic loading was not included. Data from the author's thesis (Pyke 1973) shows both that the settlement per cycle for a given cyclic shear strain decreases with an increasing number of cycles and that the secant shear modulus increased with an increasing number of cycles. Following the suggestion of Geoffrey Martin, the author uses the accumulated volume change, or the latent volume change in the case of a saturated sand, as a measure of the strain history up to that point. These changes in the stiffness as well as the rate of settlement as a function of the previous cyclic loading are quite significant and are illustrated in Pyke (2021a).

The 1979 example also showed sharp corners on the stress strain loops, which is generally not correct. Acting on a suggestion by Mladen Vucetic, Pyke (2004) introduced rate of strain effects which generate the low strain damping which is seen in laboratory tests and helps damp out high frequency noise in analyses. This is very much preferable to arbitrarily introducing artificial viscous damping to control high frequency noise.

A further example of the effect on soil fabric of cyclic loading is provided in Figure 2 which is taken from Dahl et al. (2014).

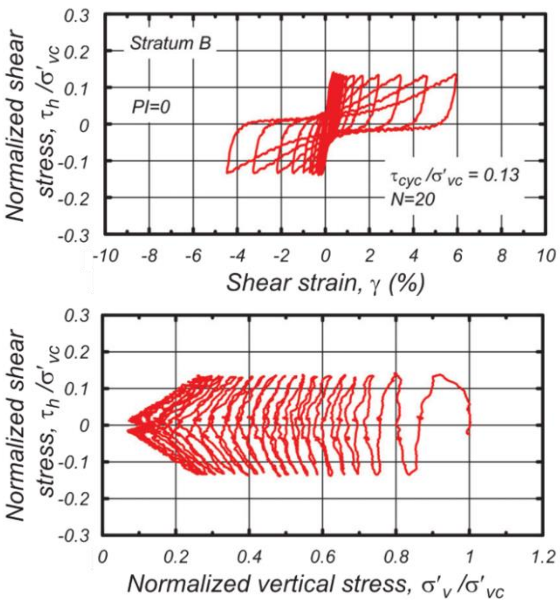

Figure 2. Results from constant height stress-controlled cyclic simple shear test on a sandy silt. From Dahl et al. (2014).

It may be seen in Figure 2, that the shear strain at which dilation kicks in increases with the number of cycles as excess pore pressures increase and the specimen is pushed out further, first in one direction and then the other. The Dahl et al. paper also serves as a great example of the issues involved in sampling and testing real soils. And it includes data on the effect of initial shear stresses in generating permanent deformations, as shown in

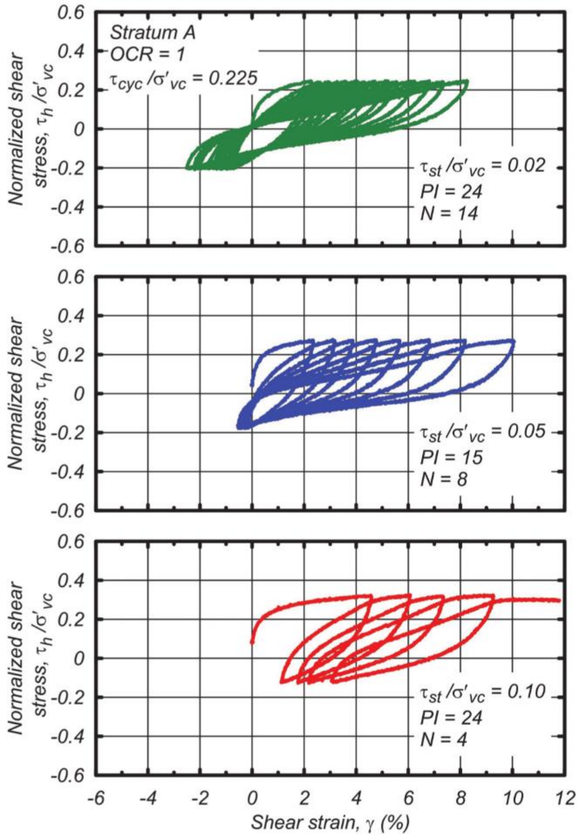

Figure 3. Results from stress-controlled cyclic simple shear tests with initial shear stresses on a clayey silt. From Dahl et al. (2014).

Figure 3. That data was used to calibrate the HDCP soil model for use in analyses of potential lateral spreading for a major land development project.

The phenomenon illustrated in Figure 2, the impact of cyclic loading on the tendency for dilation of a granular soil, is one of the two principal reasons why the "static" shear strength, that is the strength under a monotonic loading, should not be used as the asymptotic shear stress in a nonlinear soil model. The other is rate of loading effects. For example, based on data developed for San Francisco Bay Mud on several large projects in which the author has been involved, the apparent shear strength implied by the projection of shear stress-shear strain curves to large strains can be as much as three times the conventional static shear strength. The consequence of constructing a nonlinear soil model using the conventional static shear strength is that the model becomes more nonlinear and has much larger damping. Soil models or computer programs that limit the asymptotic shear strength to the conventional static shear strength produce results which are clearly overdamped and not consistent with recorded motions. The author's practice is to not specify an asymptotic shear stress when running site response analyses but to specify the reference strain and to accept whatever asymptotic shear strength is implied by the maximum shear modulus and the reference strain.

Constructing a nonlinear soil model which generates appropriate hysteretic damping is not a straightforward task and it cannot be accomplished by detailed matching of the results of a particular element test in the laboratory. Rather it requires checking the results obtained in analyses with observed data in the field as has been done by Afshari and Stewart (2019). The soil model then accommodates not only any differences between the soil behavior under field conditions and in the laboratory but also accounts for elements of the wave propagation in the field that might arise from various inhomogeneities that are not modeled in the analysis.

7. EFFECTS OF MULTI-DIRECTIONAL LOADING

Although most laboratory tests are conducted using uni-directional loadings, most dynamic problems in the field, and especially earthquake loadings, involve multi-directional loadings. The effects of multi-directional shaking on sands have been described by Pyke (1973), Seed, Pyke and Martin (1978), and Pyke (2021a). These effects are most dramatic on the pore settlements of dry sands, and hence the development of excess pressures in fully saturated sands. The principal finding of these studies was that the settlements of dry sand caused by horizontal shaking with two orthogonal components were approximately equal to the sum of the settlements caused by horizontal shaking with each component acting alone. The effect on shear modulus is complicated and deserves further study. The shear modulus initially appears to be lowered by multi-directional shaking, but, at least for dry sands, settlements then progress more quickly so that the shear modulus increases more quickly than for uni-directional loading and may end up being higher. The effects of vertical shaking are often ignored because in a fully saturated soil they should make no difference to the effective vertical stresses, but they should be considered in non-saturated or dry soils. When vertical shaking is superimposed on multi-directional horizontal shaking, the settlements are increased but the shear modulus is increased on some cycles and reduced on other cycles. So, early in the loading the shear modulus remains about the same as for horizontal shaking, but hardening will occur at a faster rate.

These multi-directional loading and hardening effects are ignored by most soil models. Most analyses of earthquake site response are carried out for a column of soil driven by a single horizontal component so that multi-directional shaking could not be addressed in any case. A few use 3D brick elements and are driven by two horizontal motions and perhaps a vertical motion so that it is possible to address multi-directional loading and hardening effects, but only if the soil model includes them.

The author’s own 1D site response program, TESS2, does not include vertical excitation but runs analyses for two horizontal components simultaneously. However, the user can take the vertical motion into account when specifying the parameters for computing settlements or latent settlements. This method of analysis implies that the shear modulus in one direction is independent of shaking in another direction. As noted above, this is not precisely correct, but when the strain history and hardening effects are taken into account, it may not be such a bad approximation. The settlements or excess pore pressures and latent settlements are computed separately for the response to the two horizontal components and are then added together, both to obtain the total accumulated settlement or latent settlement and to adjust the shear modulus when this is specified to be a function of the effective confining pressure. This approach is not as elegant as using 3D elements and a 3D soil model which captures all the necessary features of soil behavior, but it has the advantage of both being relatively simple and forcing the user to think about the input assumptions, and maybe even about the limitations of the approach. Because the HDCP soil model in TESS2 generates permanent displacements, TESS2 can be used for analyses of lateral spreading in addition to analyses of site response, liquefaction, and seismic settlement (Pyke 2019).

8. MORE COMPLETE AND COMPLEX SOIL MODELS

More complete and complex soil models are needed to conduct 2D and 3D nonlinear analyses, especially under complex cyclic loadings. But all geotechnical analyses involve approximations and there is no guarantee that more complex and complete soil models are any more accurate than simplified ones. There is also no guarantee that users in practice will recognize that to justify the time and expense of conducting 2D or 3D nonlinear analyses, whether static or dynamic, there should be sufficient field and laboratory investigations to establish the 3D geometry of the soil deposits or constructed works and to identify inhomogeneities including finer layering, lenses, and anisotropy. Many such analyses appear to the author to be conducted solely to produce multi-colored pictures in reports which might look impressive to laypeople but lack adequate backup and explanation. For instance, example shear-stress shear strain plots should be shown, not to prove that they match laboratory test data exactly, but to show that they are consistent with laboratory test data and common sense.

2D and 3D soil models require a second elastic modulus, usually a constrained or bulk modulus, but surprisingly little attention is paid to the second modulus unless the instantaneous Poisson’s ratio exceeds 0.5 and the solution scheme blows up. That limitation is a problem since real soils can dilate quite strongly especially under low confining pressures. A more complex and complete soil model should also include hardening and rate of loading effects as discussed above with reference to simple models. The common complaint that 3D soils models are too complex or have too many parameters may have some merit, but unless they properly model complex soil behavior soil models are of limited value. The trick is to do that at the same time as making them easily understood and managed by users.

A review of the various available models is beyond the scope of this paper, but an important test is whether they can develop permanent strains in response to initial shear stresses. “Failure seeking” models like PM4Silt developed at the University of California, Davis, can do this and Boulanger (2019) provides a good example of the intelligent use of advanced soil models.

But the most common model seen by the author in practice is the so-called Mohr-Coulomb model, which, as noted above, is simply a linear elastic / perfectly plastic model. It is usually not clear how this model unloads and reloads and rarely, if ever, is any effort made to calibrate its ability to develop permanent deformations or to establish what damping it generates under cyclic loading. In at least some earthquake engineering applications the shear modulus is chosen as a function of the “average” shear stress so that the analysis is just an equivalent linear analysis with a cutoff at high stresses. It is true that the Mohr-Coulomb model does develop permanent deformations in approximately the right direction under earthquake loads but that is simply because elements that have large initial shear stresses reach failure and go plastic before other elements do. The calculated displacements should thus be taken with a good dose of salt.

9. ONGOING COMPLEX SOIL BEHAVIOR ISSUES.

Some more complex soil behavior issues such as strain history and hardening effects under cyclic loading, and multi-directional shaking, have been discussed above, but there are further issues which have not been adequately researched to allow confident modeling of them in complex soil models.

Laboratory tests are normally run with monotonic loadings to failure or with uniform cycles of stress or strain, not with a combination of these two types of loading or with irregular cyclic loadings. Some years ago, in connection with the design of an offshore platform, the author conducted what became know as the “tickle test” in which a small cyclic loading was superimposed on the monotonic loading to failure in a triaxial apparatus of a dense silt. Perhaps not surprisingly, the tendency to dilate under the monotonic loading was entirely suppressed. It is possible that this mechanism might, for instance, play a role in the triggering of flow slides in tailings dams by micro-earthquakes or mine blasts, although the conventional wisdom is that the high frequency waves generated by those sources do not create significant strains in a large structure. What is perhaps more likely is something else that is not captured in traditional modeling. Namely, that if seepage and piping have created a locally unstable soil structure which has dimensions in the order of say a meter or less, then that structure will be impacted by higher frequency motions which might trigger a local collapse and initiate a progressive failure (Pyke 2021b). This is a particular case of an inhomogeneity governing the behavior of what can otherwise be modelled as a continuum and illustrates the danger of leaving critical details out of an analysis. More generally, the transition from behavior under cyclic loading to failure in a particular direction has been the subject of little if any study, although the Material Point Method (MPM) appears to offer promise as a framework for such analyses.

Another question is whether behavior in cyclic tests or in the field under irregular loadings is the same as in tests with uniform cyclic stresses or strains. As shown for instance in the paper by Dahl et al. referenced above, in a stress-controlled laboratory test with a constant applied cyclic stress, the strain at which strong dilation kicks in is moved out as a result of the cyclic loading causing pronounced S-shaped shear stress – shear strain loops. This no doubt corresponds to a change in the soil fabric caused by the repeated loading. But does the same thing happen under irregular and multi-directional loadings? Modelers go to great efforts to match this kind of behavior as seen in laboratory tests, but does it occur in the field? This issue also links to the question of the asymptotic shear stress not being equal to static shear strength as noted above, another question that deserves further study. While there is compelling evidence that in sub-failure situations the asymptotic shear stress should be much greater than static shear strength, does that also hold true as failure in a particular direction is approached?

Also, to assume that on average the shear modulus and its variation with strain are the same as under uniform loading in a single direction might be an adequate approximation for some purposes but the limits of its applicability are not clear. The author regrets not studying this issue in more detail at the time of conducting research for his PhD.

Finally, there is a pressing need for geotechnical engineers to educate structural engineers as to the significance of some aspects of soil behavior to soil-structure interaction problems. As one example, although geotechnical engineers have specified nonlinear p-y curves for use in soil-pile interaction analyses for many years, computer programs for the analysis of piles commonly make no provision for the proper behavior in unloading and reloading and thus it is not possible to compute meaningful deflections in response to staged or repeated loadings. A second example is the use of a single set of elastic springs to represent the foundation compliance even for high-rise buildings. This still occurs in cities like San Francisco and Seattle even when the geotechnical engineer may have conducted elaborate studies using nonlinear analyses to determine the foundation input motions. As the capacity of computers continues to grow it should be possible to conduct long-overdue studies of the effects of more accurate modeling of foundation compliance for this and other soil-structure interaction problems.

10. CONCLUSIONS.

The Davis-Poulos-Booker approach to modeling soil behavior using different approaches for low strain behavior and at failure remains valid today. Progress towards constructing a single model of soil behavior that is valid at both small strains and at failure, for both “static” and “dynamic” loadings, is best described as uneven. For many problems, a somewhat simplified approach such as the use of Duncan and Chang hyperbolas with the addition of proper rules for unloading and reloading is adequate for static analyses, and the use of models like the HDCP model for 1D earthquake site response analyses is adequate for dynamic analyses, and more complete and complex models are not required.

Soil behavior is interesting but complex. Soil models should not be required to exactly match the results of a particular laboratory test because any one laboratory test is unlikely to precisely represent field conditions, but soil models should capture the essential elements of soil behavior.

Where use of more complete and complex models is justified, such as in some 2D and 3D analyses, users need to become much more familiar with the assumptions they are making and spell out in their reports both the main assumptions and the sensitivity of the results to these assumptions. They should never report just the result of a single analysis. Ideally, they should show that the model is consistent with relevant laboratory element tests, but it is not necessary to match them perfectly. And, in the author’s opinion, the Mohr-Coulomb model is not appropriate for use in more advanced analyses.

There is a pressing need for more emphasis on basic studies of soil behavior. When the author was a student at Cal in the early 1970’s the faculty were saying that our analytical capability had outstripped our ability to determine soil properties in the field and the laboratory, and that is even more true today.

Particular issues that require more attention are the modeling of unloading and reloading; the modeling of strain history and hardening effects: rate of loading effects; the effect of irregular and multi-directional loadings; and the interaction between monotonic and cyclic loadings.

Even with an improved focus on soil properties, geotechnical engineering will remain very much an art as well as a science, and experience and good judgment will continue to be essential to good practice.

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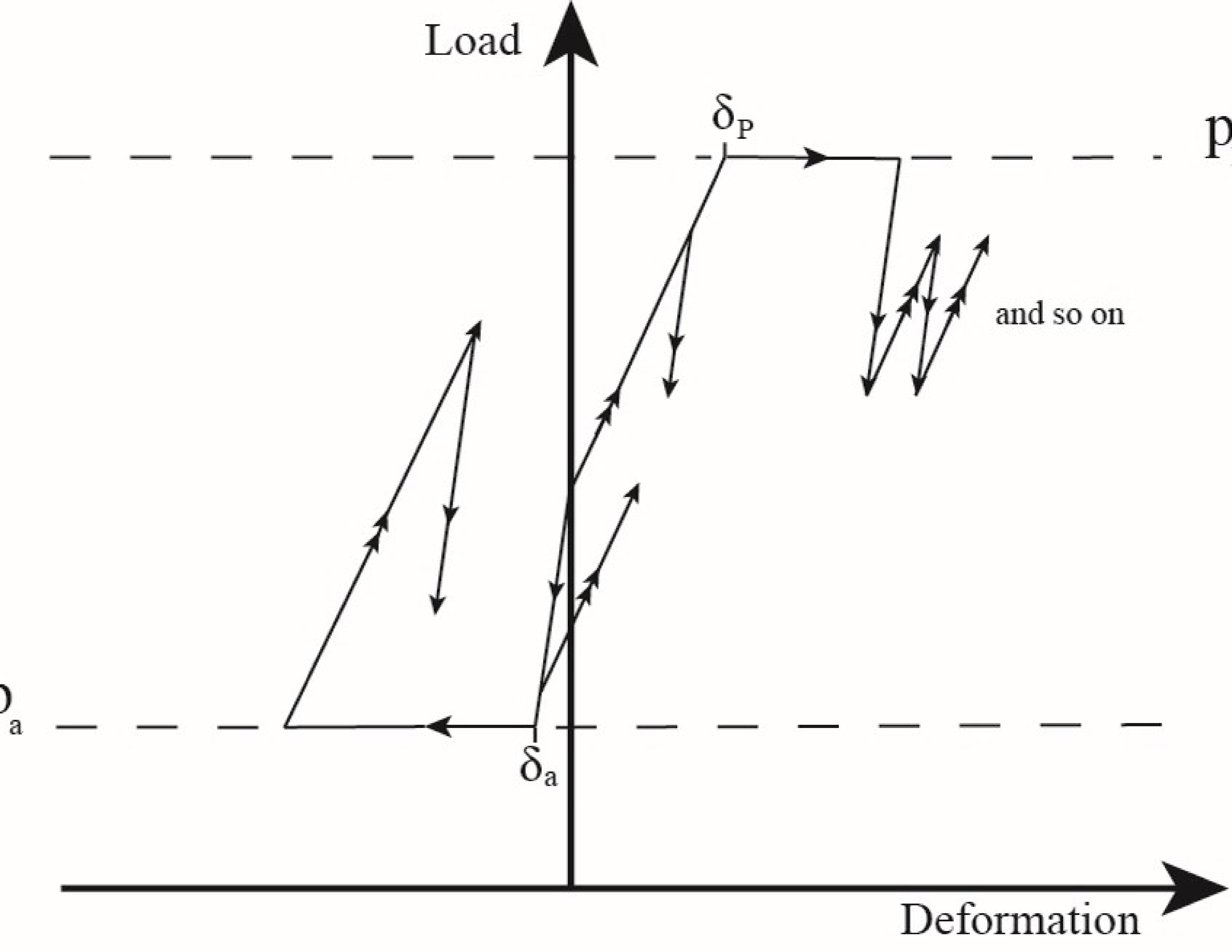

Load
δP
and so on
δa
Deformation

Lessons Learned from the Observed Seismic Settlement at the Jensen Filtration Plant in the San Fernando Earthquake

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ABSTRACT

The Joseph Jensen Filtration Plant of the Metropolitan Water District is located in the area of strongest shaking generated by the 1971 San Francisco Earthquake. One or more concrete basins which had been precisely leveled settled by amounts of up to 6 inches, of which about 4 inches were estimated to be due to compaction, rendering the plant inoperative. The primary lesson that was learned is that the amount of settlement that was observed could not have been predicted based on the calculated history of cyclic shear strains using a single component of the earthquake motion. However, it was found that the settlements caused by each component acting separately could be added to approximate the settlements caused by multi-directional loading. It is now practical to conduct bi-directional nonlinear effective stress site response analyses running the two horizontal components simultaneously and adding the settlements and excess pore pressures that are calculated for each component at each timestep.

INTRODUCTION

The author began research for his Ph.D. thesis under the supervision of the late Professor Harry Seed in the summer of 1971 shortly after the San Fernando Earthquake of February 9, 1971. Although settlements caused by shaking in that earthquake, whether of non-saturated soils or associated with liquefaction, were not as dramatic as the near-failure of the Lower and Upper San Fernando Dams and even the damage to the Juvenile Hall caused by a very shallow landside, concrete settling basins at the Joseph Jensen Filtration Plant of the Metropolitan Water District of Southern California settled as much as 5 or 6 inches at one end, more than sufficient to put this important lifeline facility out of service. The plant had been constructed on a fill that was up to 55 feet deep composed of compacted sandy material derived from the Saugus Formation and overlay a relatively thin layer of recent alluvium. The water table passed through the alluvium so that at least some of the alluvium was susceptible to liquefaction. Studies of this settlement had already been conducted by a local geotechnical consulting firm who concluded that the settlements at the end of the settling basins could be attributed to three causes: (1) reconsolidation of the alluvium that liquefied; (2) compaction of the non-saturated soils; and (3) lateral stress reduction and spreading.

This case history illustrates two important things about settlements caused by earthquake loadings. One is that they are not very dramatic when compared to liquefaction and landsliding although they may be be sufficient to impact critical lifelines. The second is that there can be multiple causes of the settlement observed at the ground surface or under structures, so that it is very difficult to calibrate methods for estimating settlement from case histories. Macedo and Bray (2018) suggested that settlements of buildings associated with liquefaction can be categorized as ejecta-induced, shear-induced, or volumetric-induced, omitting mention of lateral stress reduction and spreading. What Macedo and Bray refer to as shear-induced settlements might be better called “distortion”, or even “bearing capacity failure”, but regardless of the terminology, this mechanism has led to some of the most dramatic failures due to liquefaction such as the overturning of the apartment buildings at Kawagishi-cho in the 1964 Niigata earthquake. Prediction of such failures is outside the scope of this paper but from the practical point of view the lesson to be learned is to not put buildings with shallow (or no) foundations on loose sandy or silty soils, especially when there is a high water table. Most of the literature, and this paper, focus on settlement due to volume change.

Silver and Seed (1971) had in fact performed laboratory testing and developed a simple procedure for estimating settlements due to volume change, and so Professor Seed asked the author to see whether this procedure was able to match the component of the observed settlement at the Jensen Filtration Plant that was thought to be due to volume change of the non-saturated soils, which was on the order of 4 inches. The author used the same cyclic simple shear apparatus that had been modified and used by Marshall Silver and conducted more or less constant cyclic strain tests on samples obtained from the site that were recompacted to the in-situ density by what became known as the “moist tamping” method. The fill had been found to be relatively uniformly compacted to a dry density of 121 pcf which was 92 percent of the maximum density obtained using ASTM D-1557. An important lesson from this case history is that even 92 percent relative compaction using the modified ASTM standard was insufficient to prevent volume change under very strong shaking and that specification of a minimum density of say 95 percent, which will produce average densities more like 98 percent, is desirable for fills in areas in which strong shaking might be expected. The fill had been previously described as a clayey sand but the samples that were tested were a well-graded silty sand with 40 percent passing the No. 200 sieve and less than 10 percent finer than 2 microns.

Equivalent linear site response analyses were then conducted using a modified version of the acceleration history recorded at the Pacoima Dam as the input motion. In his thesis the author, in accordance with the thinking at that time, stated “from a response analysis, the history of shear strains for a layer at any depth in the deposit can be obtained and it is not difficult to interpret these results to determine an equivalent number of cycles of a representative average shear strain for the layer.” Since that time the author’s thinking has evolved and he now believes that the calculation should be carried out cycle by cycle, but that approximation was likely not a major source of error. The thesis went on to say “the surface settlement of 1.47 inches computed for the survey baseline is a little more than one-third of the 4 inches settlement attributed to compaction. It is recognized that various simplifications and assumptions have been made in the analysis and that errors may have been made in the interpretation of field conditions, but the substantial difference between the computed and the observed values invites consideration of whether any significant factors have been overlooked."

The author then proposed that the failure to consider the effects of multi-directional shaking might be a major issue that had been overlooked but it was Professor Seed who had the idea of using the large shaking table at the Richmond Field Station of the University of California to conduct tests with two- and three-dimensional shaking. At that time the table was limited to shaking in a single vertical plane, that is, in one horizontal direction plus vertical shaking, but shaking in the second horizontal direction was added by constructing a lightweight shaking table that was mounted on the large shaking table perpendicular to the vertical plane in which the large table could move. These tests were necessarily acceleration or stress-controlled, but companion strain-controlled tests were also run using the cyclic simple shear apparatus. Because Geoffrey Martin, who at that time was on sabbatical leave at the University of British Columbia, suggested a way to link the settlement of dry sands to the liquefaction of saturated sands, subsequently published as Martin et al (1975), Professor Seed, who knew that settlement was generally a less sexy issue than liquefaction, then became more interested in the implications of these tests for liquefaction rather than just settlement, Thus, detailed results of these tests were reported in Pyke (1973) and in an EERC report, Pyke et al. (1974) and the findings regarding the Jensen Filtration Plant were reported in Pyke et al. (1975). The implications for liquefaction were discussed in another EERC report and Seed et al. (1978), but the full implications for conducting more robust estimates of settlement were never published. This had led to some confusion regarding the findings relative to estimating settlements in the absence of liquefaction and that is the thrust of the present paper. But first, some of the data from uni-directional shaking table and cyclic simple shear tests is presented in order to set the stage for the subsequent methodology and discussion regarding the effects of multi-directional shaking.

RESULTS OF UNI-DIRECTIONALTESTS

The results obtained in the shaking table and the cyclic simple shear tests were generally similar although not identical. All tests were conducted using Monterey No. 0 sand, a uniformly graded sub-rounded dry sand, mostly passing the No. 30 sieve but retained on the No. 50 sieve.

If the results were reduced and plotted in the traditional way in terms of the applied cyclic stress ratio the shaking table results can be plotted as shown in Figure 1. This form of presentation of the data shows the effects of relative density, the average cyclic stress ratio, and the number of uniform cycles at a glance. It is not recommended for more precise calculations but may be convenient for simple "back of the envelope" calculations.

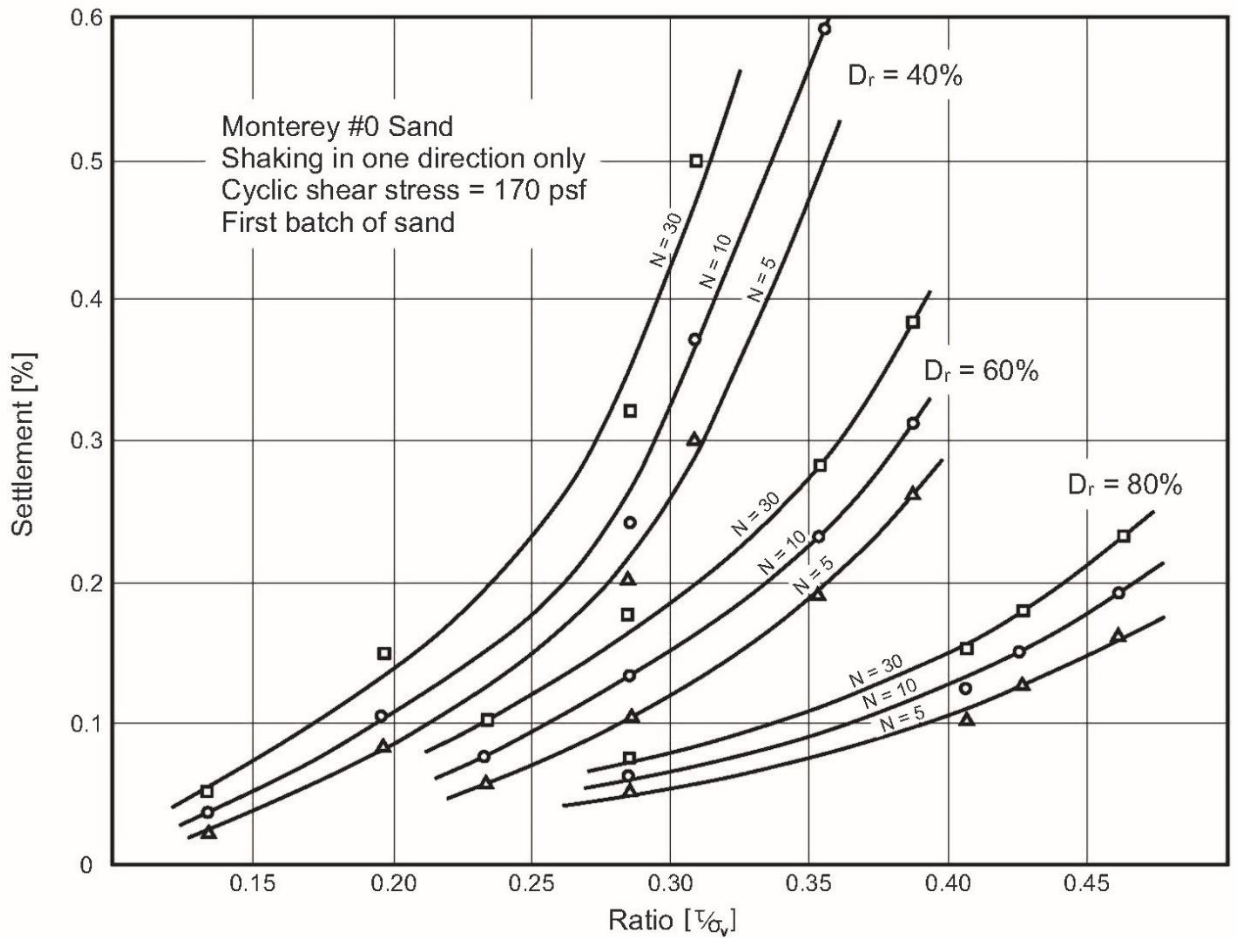

Figure 1. Summary of Results from Pyke (1973) for Uni-Directional Shaking Tests.

For use in more complete and accurate calculations, the data needs to be reduced and presented in terms of cyclic shear strains, rather than cyclic shear stresses, as shown in the subsequent three figures, taken from Pyke (1973), which show data from more or less constant strain cyclic simple shear tests.

For a relatively constant cyclic shear strain, the settlement decreased and the stiffness increased with an increasing number of cycles, as may be seen in Figure 2. Both of these factors should be addressed in any attempt to perform more accurate calculations.

These were of course uni-directional cyclic loading tests. The effect of multi-directional shaking on settlement is discussed below. The effect on shear modulus is complicated and deserves further study. The shear modulus initially appears to be lowered by multi-directional shaking, but, at least for dry sands, settlements then progress more quickly so that the shear modulus increases more quickly than for uni-directional loading and may end up being higher.

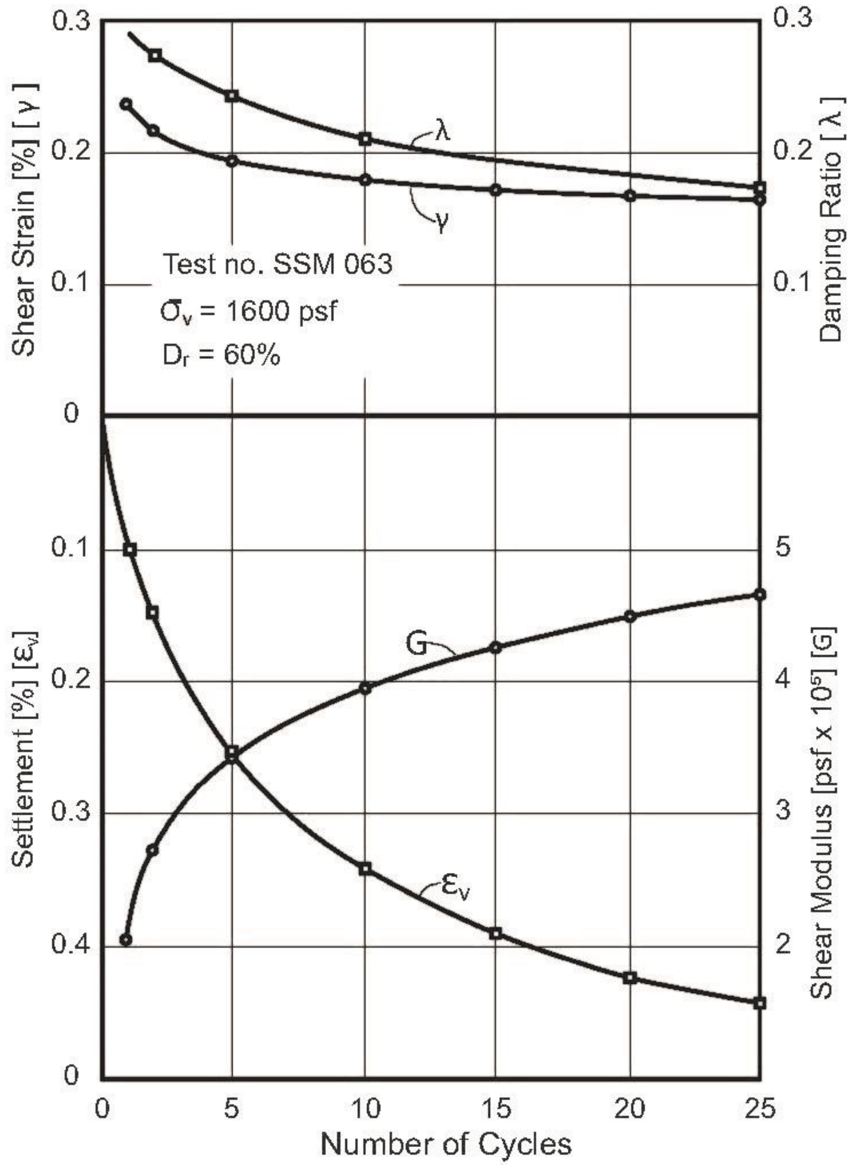

Figure 2. Typical Results of Cyclic Simple Shear Test on Monterey #0 Sand.

However, although this was intended to be a constant cyclic strain test, the cyclic shear strains were not constant because of compliance in the test apparatus and this needs to be accounted for in the data reduction. Following the suggestion of Geoffrey Martin, as published in Martin et al. (1975), the data on settlement was reduced as shown in Figure 3 in which the settlement per cycle is shown as a function of the cyclic shear strain and the accumulated settlement. The accumulated settlement turns out to serve as a very good measure of the effects of the strain history to that point. It was also found that when the data was reduced in this way, the settlement per cycle was largely independent of confining pressure, confirming that behavior under cyclic loading is more fundamentally controlled by the cyclic shear strain, rather than the cyclic shear stress or stress ratio.

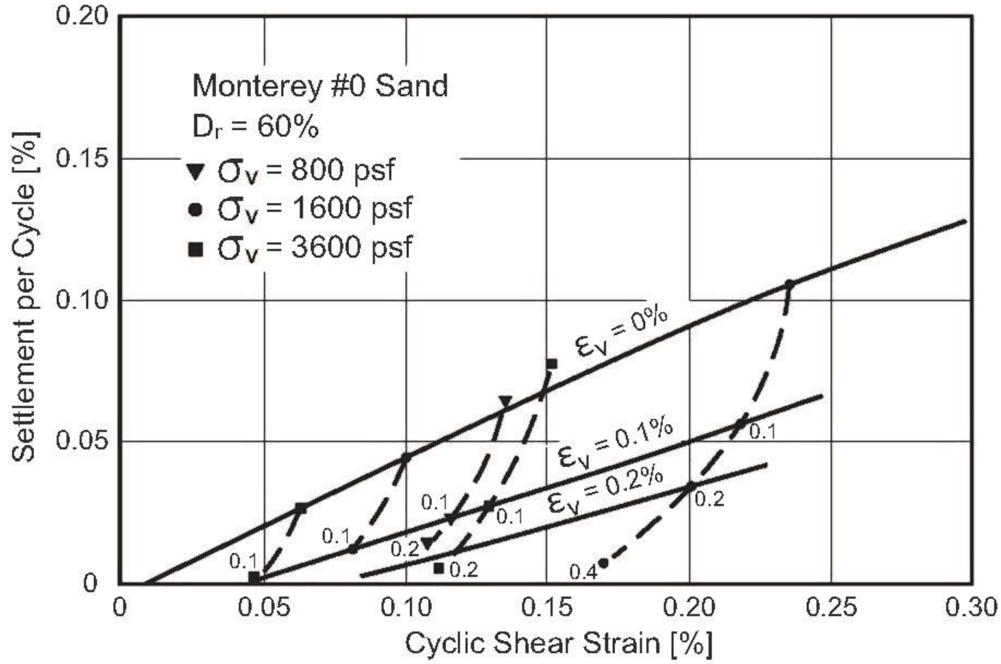

Figure 3. Settlement per Cycle for Monterey #0 Sand.

The data on the secant shear modulus can also be reduced in a similar fashion, as shown in Figure 4. The increase in the secant shear modulus for this dry sand is quite marked, and Pyke (1973) suggests that it still generally applies when there is multi-directional shaking. As pointed out by Vucetic and Mortezaie (2015), this effect can also be seen in undrained cyclic tests on saturated sands although in that case it is quickly overwhelmed by the decrease in stiffness that accompanies the development of excess pore pressures.

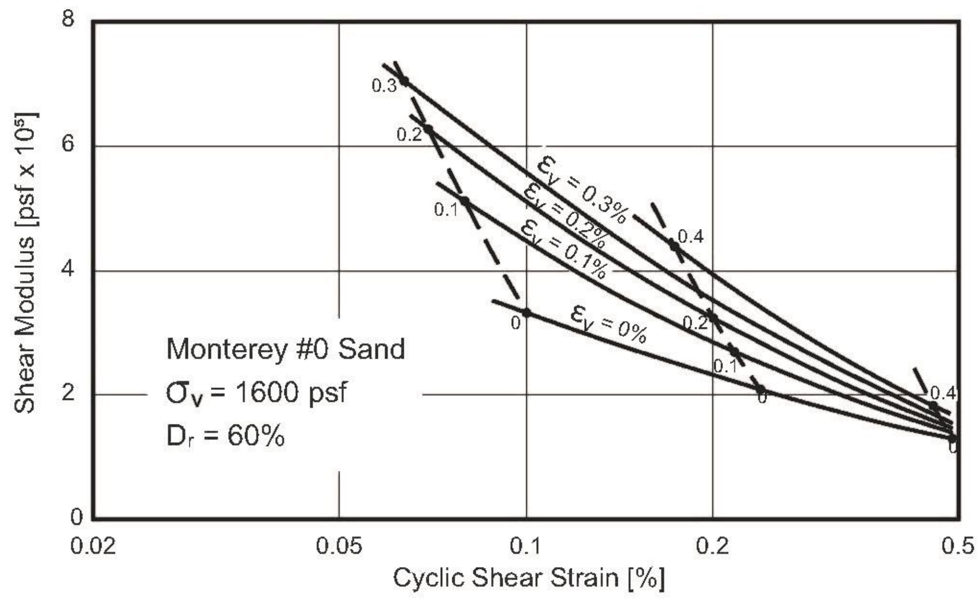

Figure 4. – Hardening of Monterey #0 Sand.

EFFECTS OF MULTI-DIRECTIONAL SHAKING

Because of space limitations the key figures showing the effect of horizontal shaking with two orthogonal components and of vertical shaking are not shown in this paper, but they may be found in Pyke (1973) and Pyke et al. (1974). The latter publication may be found online at https://peer.berkeley.edu/ucbeerc-report-series. But the principal finding was that the settlements caused by horizontal shaking with two orthogonal components were approximately equal to the sum of the settlements caused by horizontal shaking with each component acting alone. If the components were equal, as was the case in some tests conducted with gyratory shear, that is sinusoidal loadings offset by one-quarter of a cycle so that the cap on the shaking table moved in a circular fashion, that meant that the settlements were approximately double those caused by a single component acting alone. In addition to being approximate, these findings also had the limitation that results from the settlement per cycle decreasing with the number of cycles. Because the accumulated settlement increases more quickly under multi-directional shaking, the rate of increase of the settlement slows down more quickly than is the case with uni-directional shaking. Thus, the rule of adding or doubling the settlements from individual components applies more at the beginning of shaking than at the end of a longer duration of shaking.

The effects of vertical shaking are often ignored because in a fully saturated soil they should make no difference to the effective vertical stresses, but they should be considered in non-saturated or dry soils. However, even for dry soils there is an interesting wrinkle. In shaking table tests with only vertical motion there was no visible settlement until the peak acceleration exceeded 1g. But when vertical shaking was superimposed on horizontal shaking, the settlements increased. A comparison of the shear stresses in tests with and without vertical accelerations showed that the effect of vertical acceleration was to increase the shear modulus on some cycles and to reduce it on other cycles. The average shear strains were about the same so that the greater settlements in tests with vertical shaking were primarily due to an increased tendency for compaction. This effect increased with the vertical acceleration and for a sinusoidal vertical acceleration the settlements were about 50 percent greater for a uniform vertical acceleration of 0.25 to 0.3 g. The increase in settlements with irregular vertical motions might be much less than this.

Notwithstanding the various assumptions and approximations involved, these findings essentially closed the gap between the computed and observed settlements at the Jensen Filtration Plant, the computed settlements now being in the order of 3-4 inches compared with the observed settlement due to compaction on the survey baseline of 4 inches. As noted subsequently by Yee et al. (2014), this settlement was still much less than what would have occurred in a freshly deposited clean sand.

AN IMPROVED ANALYSIS PROCEDURE

These findings and the data reduction procedure illustrated above also suggest an improved procedure for estimating likely settlements due to compaction in earthquakes. The key to this improved method is that you need to know the history of cyclic shear strains in each layer in order to make a reasonably accurate estimate of the likely settlement in a given earthquake. That in turn requires the selection of appropriate acceleration histories to use as input motions but that task has been made much easier by the development of the PEER and other earthquake ground motion databases. It is in fact astonishing that the development of modern computers which has made much more precise and useful calculations of the response of structures to earthquakes, has been sidelined in geotechnical engineering in favor of simplified methods that basically could be done by hand. That was understandable in 1970 when the first simplified procedure for the evaluation of liquefaction potential was published because only a handful of engineers could conduct site response analyses, but it is hard to understand today. If the simplified methods forced the user to conduct better site investigations and more carefully analyze the data, that would be an argument in their favor, but the opposite is true. In practice the simplified methods tend to be more automated and the user is not required to study the data carefully.

The improved procedure is implemented in a new computer program called TESS2 which conducts bi-directional nonlinear effective stress site response analyses. In TESS2, the stiffnesses and the settlements or latent settlements are calculated for each half cycle. The two horizontal components are run simultaneously and the contributions to settlements or latent settlements and excess pore pressures are then added. Data on the settlement of dry Monterey No. 0 sand caused by cyclic loadings obtained from Pyke (1973) is built into the program but the user can apply a multiplier to this data or specify site-specific data should that be available. This multiplier can also be used to account for the effect of vertical motions. In lieu of acquiring site specific data, users can refer to Ramadan (2007), Duku et al. (2008) and Yee et al. (2014) for data on other sands. Note that Yee et al (2014) suggest that compaction caused by cyclic shearing is reduced when even the low plasticity fines content exceeds 10 percent. For saturated sands, the settlement on reconsolidation from Ishihara and Yoshimine (1992) is built into the program and the latent settlement jumps to their values when excess pore pressure in any layer reaches 100%. Again, the user can specify site-specific data should that be available. Otherwise the latent settlements, which are the settlements that are only seen on the dissipation of excess pore pressures, of saturated sands are based on the assumption of Martin et al. (1975) and Seed et al. (1978) that, short of the development of 100% excess pore pressure, the settlement on dissipation of excess pore pressures in a saturated sand is the same as the settlement that would occur under the same loading in a non-saturated sand. Because of this jump in the settlement if the excess pore pressure reaches 100% it is important to use an effective stress analysis in which the excess pore pressures are redistributed and dissipated as appropriate. The point of performing the calculations this way is that the strain histories and the peak excess pore pressures make a difference. Thus, the character and duration of the input motions also make a difference. Although these calculations are too onerous to perform by hand or even in a spreadsheet, they provide a more accurate calculation and, if run with a suitable number of input motions, show the sensitivity of the computed settlements to the random nature of earthquake ground motions.

Although there is no hard limit on the total amount of settlement that can occur, it can be seen from Figures 2 and 3 that additional settlements will be small once the accumulated settlement reaches a value on the order of 0.5 percent under uni-directional loading. Since the settlements caused by each component of motion are additive, once the accumulated settlement reaches about 0.5% of the layer thickness, additional settlements caused by motion in either direction will also be small. In other words, accounting for the second component of motion increases the rate at which settlements or latent settlement accumulate but does not have as much effect on the maximum settlement. The data presented above and included as the default in TESS2 was obtained on a clean, washed and screened sand, that is a "baby" sand deposited by dropping it through the air. Pyke (1973) was one of the first studies to explore the effect of the method of sample preparation on settlement and liquefaction under cyclic loading and includes data on the effect of overconsolidation and the application of an initial static shear strain, both of which reduce the settlement per cycle. The upshot of this is that the processes involved in deposition and subsequent ageing in the field are likely to decrease the settlement per cycle, perhaps significantly from those seen in laboratory tests and that Pyke's data on Monterey No. 0 sand likely provides an upper bound on expected settlements in the field.

But the first step in conducting an improved analysis is likely an improved site investigation and careful study of the data that is obtained. An adequate site investigation will generally include measurement of shear wave velocities, drilling borings to obtain samples in addition to pushing CPTs. Also, hydrometer tests and plasticity index tests are required to learn the character of any fines. It should be kept in mind that actual sand layers or lenses are usually offset from the depths indicated by CPTs since the CPT is measuring the properties ahead of the cone, but good practice is to first push CPTs and then to follow-up with borings and SPT measurements and sampling in any sand layers or lenses. The fraction of the sample passing the No.200 sieve should then be determined for each separate material found in the tip and the barrel of the SPT sampler and hydrometer tests and plasticity index tests then should be performed on samples with more than say 30 percent passing the No. 200 sieve. Additional borings or CPTs should be advanced as necessary to confirm that sand layers are not continuous if this is suggested by an initial or preliminary investigation.

MISUNDERSTANDINGS ABOUT THE FACTOR OF TWO

Although it was indicated by Pyke et al. (1975) and assumed by Seed, Pyke and Martin (1978) that settlements under shaking with two orthogonal components, would on average generate twice the settlement generated by one component acting alone, that only applies in individual cases if the two horizontal components are approximately equal. Two recent studies, Nie et al. (2017) and Reyes et al. (2019), have made this point using numerical studies involving complex 3D soil models. Zeghal et al. (2018) have demonstrated the same behavior using biaxial shaking in centrifuge tests. All these results are generally consistent with the earlier conclusions and with results obtained using TESS2. Nie et al. (2017), for instance, concluded that the factor that should be applied to the settlement computed using a single horizontal component ranged from

1.52 to 2.32. However, the concept of adding the settlements caused by two orthogonal components of shaking applies only to more accurate calculations. It is not necessary when the calculation is very approximate and conservative in the first place, as is the case with various simplified methods for estimating seismic settlements. Likewise, the effect of vertical motions can generally be ignored. In addition to the conservatism involved in estimating the cyclic stresses or strains using simplified methods, because the volume change data used in these methods was obtained on "baby" sands the use of this data for naturally occurring sands, which may show effects of fines content, overconsolidation, pre-straining and other ageing phenomena can be thought of as cancelling out the need to increase the calculated settlements in order to account for multi-directional shaking. For saturated sands, methods that rely on Ishihara and Yoshimine (1992) should most certainly not be doubled because the calculated settlements are controlled by the occurrence of liquefaction or the factor of safety against liquefaction and the effect of multi-directional shaking should already be taken into account.

ADDITIONAL COMMENTS ON SIMPLIFIED METHODS

Simplified methods for evaluating both liquefaction and settlement under earthquake loading have been widely used for some years without much comment on their limitations, but now Boulanger et al. (2016) and Pyke and North (2019) have spelt out the reasons that they are generally quite conservative. Pyke (2019) provided a case history involving Lum Elementary School in Alameda CA, in which excessive conservatism led to particularly adverse social impacts. Crawford et al. (2019) provided a case history involving the River Island development in Lathrop CA where estimated seismic settlements of up to 15 inches using the simplified methods of analysis built into the computer program CLiq were reduced to at most several inches as a result of improved site investigations, laboratory testing and analyses.

A more complete discussion of these limitations is provided by Pyke (2020) but the key issues in practice are usually one or more of the following:

- The failure to exclude materials with clayey fines
- The failure to correct penetration resistance to equivalent "clean sand" values and to account for the effect of the presence of fines on settlements due to compaction
- The failure to exclude the "transitions" in CPT data.
- The failure to exclude lenses from the analysis
- Overprediction of the number of layers that might liquefy
- Unnecessary doubling of calculated settlements

More generally the simplified methods for evaluating liquefaction or settlement should not be used unless the engineer is familiar with each step in the procedure, the limits of applicability of that step and whether the site in question fits within the limits of the overall applicability of the method. Even then, simplified methods for estimating seismic settlement should at best be used only for screening evaluations. If a screening evaluation indicates settlements that are not of practical concern, nothing further need be done, but if larger settlements are obtained it should not be assumed that ground improvement is required. If a screening analysis indicates seismic settlements that are of practical concern, then an analysis of the kind conducted using TESS2 should be performed in order to refine the estimate and determine whether or not ground improvement is necessary. However, a “simplified analysis” is not necessarily required even as a screening evaluation. The widespread belief that “one has to show a calculation” tends not to promote better geotechnical engineering practice but rather worse practice. A good screening analysis should emphasize common-sense and experience. Such a screening procedure is described in Pyke (2020).

CONCLUSIONS

The San Fernando Earthquake of February 9, 1971 provided many useful learning experiences with relatively minimal damage. The settlement of the fill at the Joseph Jensen Filtration Plant provided a good case history for studying the factors that contributed to the observed settlement and prompted further studies on the phenomenon of compaction caused by earthquake shaking and the particular impact of multi-directional shaking. But the results of those studies have sometimes been misused and have contributed to the excessive conservatism of simplified methods for estimating earthquake-induced settlement due to compaction. Misuse of simplified methods of analysis has in fact made settlement due to compaction under earthquake shaking appear to be more significant than it is in reality. However, it is now possible to conduct bi-directional nonlinear effective stress site response analyses which provide a relatively simple and accurate method for estimating settlements due to compaction if appropriate judgement is applied in selecting the input parameters.

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From:	Carroll, John (BOS)
To:	Angulo, Sunny (BOS); davidwilliams.eng@att.net
Cc:	BOS Legislation, (BOS); Peskin, Aaron (BOS)
Subject:	RE: Your Nov 4 BoS Hearing - Millennium Tower Upgrade Status
Date:	Monday, November 8, 2021 10:51:00 AM
Attachments:	SF BoS Peskin MT Hrg Nov4 2021.pdf image001.png

Thank you all. I am adding this communication to the official file for this hearing matter, and by copy of this message to the board.of.supervisors@sfgov.org email address, it will be forwarded to the entire membership of the Board of Supervisors for their review and retention.

Regards,

John Carroll
Assistant Clerk
Board of Supervisors
San Francisco City Hall, Room 244
San Francisco, CA 94102
(415) 554-4445

(VIRTUAL APPOINTMENTS) To schedule a virtual meeting with me (on Microsoft Teams), please ask and I can answer your questions in real time.

Due to the current COVID-19 health emergency and the Shelter in Place Order, the Office of the Clerk of the Board is working remotely while providing complete access to the legislative process and our services.

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From: Angulo, Sunny (BOS) <sunny.angulo@sfgov.org>
Sent: Monday, November 8, 2021 10:29 AM
To: davidwilliams.eng@att.net
Cc: BOS Legislation, (BOS) <bos.legislation@sfgov.org>; Carroll, John (BOS) <john.carroll@sfgov.org>; Peskin, Aaron (BOS) <aaron.peskin@sfgov.org>

Subject: RE: Your Nov 4 BoS Hearing - Millennium Tower Upgrade Status

David –

I assume that your intent was to include this public comment in the official record and hearing file, given that you were unable to speak on the record beyond the allotted two minutes?

If that's the case, I'm copying the Committee Clerk to facilitate that request. Please confirm, if that's the case.

Best,
Sunny

From: David Williams <davidwilliams.eng@att.net>
Sent: Saturday, November 6, 2021 11:59:11 AM
To: Peskin, Aaron (BOS) <aaron.peskin@sfgov.org>
Cc: David Williams <davidwilliams.eng@att.net>
Subject: Your Nov 4 BoS Hearing - Millennium Tower Upgrade Status

Supervisor Peskin,

As you know I listened in to the subject Nov 4 Hearing and was prompted to comment. Unfortunately the 2 minute limit for Public Comment made it challenging to do justice to the topic. Accordingly, I have attached a text of my comment to the best of my recollection. I know some was abbreviated and the last sentence was totally cut-off. (I had a problem with the long delay between telephone call-in and meeting broadcast and hearing either, so just raced on.)

Both items are summary comments and can be elaborated as needed. The first is imminent if the PPU construction proceeds. (Aside: I was delighted to hear your questioning regarding the foundation mat concrete pour and associated heat of hydration. I have unsuccessfully sought the MT construction records for 4 years.) The second is only an issue if the building experiences a large earthquake. But that is what it is meant to be designed for.

My motivation?
I have nothing against any of the protagonists; I have high respect for the one person I know well on the ERDT (worked together successfully as co-consultants on a high-profile Bay Area public infrastructure seismic retrofit); I enjoyed my one involvement with SGH which was as an expert witness with Frank Heger (the H in SGH) on a pioneering project in 1980/81. I know one or two others as colleagues or acquaintenences.

My concern has been, and is, for the building and people living in it. I consented to provide engineering assistance to some home-owners in 2017 when the problem first arose, and have closely followed the subsequent unfolding.

I generally prefer to stay under the radar. I reluctantly agreed to make comment to Jaxon Van Derbeken of NBC re the accelerated settlement rate once DBI released the data from the PPU drilling operations in July/Aug; I declined invitations from Abby Sterling of CBS for 6 weeks before she asked if I would help with a pre-hearing segment she had planned, to lay out some history of the story.

I am concerned about the reputation of the engineering profession I have practiced for 50 years (and still learning). The foundation design of Millennium Tower is an unprecedented embarrassment. I have recently published (Nov 2021) a selected article (peer-reviewed) for the quarterly Journal of the International Society of Structural and Bridge Engineers, definitely the pre-eminent international journal for bridge engineering and commented previously on bridge design and construction failures, including exorbitant cost over-runs; but otherwise keep quiet and pursue my interest in pioneering structures.

I try to maintain an independent and objective position. I wish a successful outcome for the Millennium Tower.

Please let me know if you have any questions.

Sincerely,
David

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SF Board of Supervisors, Govt Audit & Oversight Committee Meeting
Nov 4, 2021
Item 4: Hearing - Update on Seismic Retrofit of 301 Mission Street - Sponsor: Peskin

David Williams Comments

Even recognizing the extensive work and reports I've reviewed over the past 4 years, due respect to all, and what I've heard today, I believe implementation of the ongoing voluntary PPU should be put on hold to confirm 2 critical issues related to the upgrade and future performance of the Tower:

1. Mat Condition Assessment

Once the PPU piles are completely installed and the foundation mat has been extended, the next operation is potentially risky – it involves load redistribution on the foundation mat – loading of the perimeter piles by jacking against the extended mat causes a reduction of load on the original piles and increases of load on the mat foundation.

Given the consequences of poor assumptions, the current foundation mat condition should be thoroughly reassessed. There are many indicators suggesting potential for degraded capacity. It warrants detailed field investigation including further non-destructive testing. Any opening of cracks on the underside is a serious durability issue.

2. Asymmetric Foundation and Future Seismic Response

Even if successfully implemented without incident, the resulting "Upgraded" structure with asymmetrical vertical stiffness is likely to result in a less desirable seismic response. Tall buildings founded on soils respond to strong ground motion by swaying and rocking. For slender buildings on deep foundations such as MT, rocking is the most likely response.

For a reasonably uniform foundation, rocking is typically smooth, cyclic and acceptable. For a building founded on stiff piles (anchored in bedrock) along two adjacent sides and elsewhere founded on soft piles (above a deep layer of Old Bay Clay), the seismic response is unlikely to be smooth. It is typically biased in one direction with little cyclic recovery – and this can cause problems. That is why the guidance for voluntary upgrade warns against adding any asymmetry to the structure or foundation. This is beyond code compliance. Many experienced structural designers would have concerns about the performance of any slender high-rise building with vertically asymmetric foundations during strong ground shaking, and avoid them. And a partial PPU fix would be even more problematic.

So, the designer should be obligated to demonstrate, to more independent review, a satisfactory strong seismic response for the asymmetric PPU.