Review of “Hydrogeological controls on the spatio-temporal variability of surge induced hydraulic gradients along coastlines: implications for beach surface stability”

Summary

A numerical groundwater flow model is used to quantify the vertical hydraulic gradients within a coastal unconfined aquifer experiencing a storm surge event. A “liquefaction” potential index is used to characterize where and when upward groundwater fluxes could lead to moments of minimal effective stress on sediment grains. Critical values of this index are shown to exist near the intersection of flood waters and topography, and flooded areas with longer flooded conditions resulted in more extended critical conditions.

General comments

1. Quick sand is not the same thing as liquefaction. Quick sand describes a state of the material when it has no effective stress acting upon it. Liquefaction is the process of liquefying such a material with an outside force acting on it. Static liquefaction is possible (Sadrekarimi, 2014), but it also requires an outside force and describes a process. I do not believe that the term liquefaction is necessary for this study to be useful. Using incorrect and inaccurate terminology as the foundation is not useful. Quick sand more accurately describes the state being modeled, but perhaps that label is unwanted. No label is needed once the state is described. It may be most accurately termed a “colloidal solution”. The state could be connected to the many potential hazards it can respond to/cause with external factors considered, including liquefaction, piping, and uplift. Groundwater modeling has been performed to understand these effects using the “Factor of Safety” ratio, which is 1/SLF as defined in this study for coastal settings (e.g., Yang & Tsai, 2020) as well as numerous applications to levees (as already referenced in the manuscript). Liquefaction potential related to sea-level rise causing higher water tables (i.e., changes in saturation) require specific earthquake events (e.g., Grant et al., 2021 and references therein). Including vertical head gradients could provide additional information to these analyses, but none of this was developed in the current manuscript. (Aside: A commentary on how sea-level rise could lead to more areas at risk of high head gradients with surges could be added to the implications discussion section).

2. Overpressures and excess pressures are not used in a context that I am familiar. This generally implies greater than hydrostatic pressure (i.e., a confined or artesian pressure). I do not believe this is what is intended. Rather, I interpret these terms to mean raised water levels from the surge event that then need to drain away. The text does not make this clear, but it affects one of the main conclusions on the “intermediate” elevations playing an important role in setting low effective stress conditions.

3. I do not find the “Field Evidence” portion of the manuscript to contribute to the overall purpose of the study. It appears to be based on previously published results (Housego et al., 2018). Referencing this study and moving on within the introduction to set the hydrogeologic context seems sufficient. If the model were more directly connected to the field site, then retaining such information could be useful. The conclusion that storm surge head gradients at the field site “substantially affect the stability of beach surfaces”

is conjecture and no data are presented on beach stability or vertical hydraulic gradients (at least based on the information provided in Figure 1b). The response of the water table to a storm surge does not provide information on vertical gradients.

4. The difference between the submerged and saturated unit weights are not sufficiently clear to make them distinguishable other than that they are somehow different. Part of the problem is that the γsat and γfw are never defined. It is unclear how γsub would be different from γsat – the water level or pressure is not part of the equation, so it should not matter if the land surface is submerged or saturated. What is different about these two terms other than something to do with freshwater? Is $\gamma fw = 1$? All of these unit weights need to be defined more clearly. I assume that these values depend on the modeled porewater salinity?

5. Varying the hydraulic conductivity a few times and considering two synthetic topographies represents a meager exploration of the parameter space. Interestingly, the parameters listed in Table 1 do not match the values used in the results figures and no justification for the ranges are provided (e.g., K in the 10-100 m/day range from Freeze and Cherry (1979) with no indication of sediment type and then a few values of Kz are reported in the later figures). How many models were performed? If only a few for each, then listing the exact values of the parameters seems feasible and useful. Using only one value of porosity for the critical index value seems overly simple. At a minimum, it could be useful to present what the range of the SLF index is for realistic coastal geomaterials.

6. Conceptually, I feel like the main conclusion that "topography" is important for setting the colloidal solution development is not fully explored. No alternative hypotheses or more nuanced hydrologic explanations are tested, even though the modeling produces the outputs to test them. The elevation of the land surface and ability to drain to the coast are invoked as the primary driver of the "intermediate" elevation cases where the lingering higher pressures occur. This is reasonable to me, but I believe the mechanism is more related to two controls 1) the initial water table depth setting the infiltration capacity and rate(s) as seen with tsunamis and storm surges (Cardenas et al., 2015), and 2) the horizontal vs vertical gradients allowing the pressure to dissipate in those areas. Despite the relative complexity of the synthetic topographies, it should be possible to normalize model inputs and outputs and explore these controls more deeply.

Specific comments

Lines	Comment
Title	I do not find that the title explains the study well. "Topographic controls on surge-induced critical groundwater gradients and potential surface instability"
24	Revised to … - explain what the revision should be "to include steeper portions of …"
26	Typo
37	Pressure distributions do not induce surface failure. An outside force/stress is required to induce this.

66	Flooding is generally a temporary event while inundation implies a longer-term water coverage. I believe all uses of "inundation" in this manuscript refer to flooding.
86	Add arrows to Figure 1 (if keeping)
89	For how long? Sense of time in Fig 1 is mostly missing.
90	Dune location needed in Fig 1a
92	Then why is this field study here? Not even one example run could be done to connect these observations with the SLF results?
121	Seepage vector needs to be defined and developed more clearly. Not the same terminology as used later in either the methods or results. Assuming seepage means "specific discharge vector"? Why the magnitude? Isn't direction important? Figure 2 could be made more consistent with the methods/equations/variables. Where is this seepage vector being calculated (especially for Fig 2)? At the top of the free surface? At some depth? Figure 2 also does not show the "magnitude of seepage component" but the actual seepage component with changing directions.
144	Quick sand and liquefaction are related but exceptionally different. This is a weak justification. This analysis does not study liquefaction.
146	Sand is not weightless ever. It has mass. "Suspended" and liquid-like or colloidal-like, sure. Inaccurate terminology.
152	Full saturated or flooded?
161	What simulated area? In the generic models. Suggests a field site.
163	The use of however here implies there is a justification preceding it, but there is not. What is the "however" referring to? What is or is not being done in the study? A new paragraph may be helpful or a rearrangement of the two sentences that establish the differences.
168	Enhance/increase/etc (alter in which way?)
178	Vz is the specific discharge according to this equation. Seepage velocity is easily confused with average linear velocity, and I do not see porosity in the equation. Providing units could also be useful to distinguish with the volumetric form.
185	And how are they different? A sentence is needed to explain the difference rather than the existence of a difference.
189	Positive and negative are confusing and dependent on the reference frame. When possible, it would help the reader to use upward and downward to describe flow. It would also be useful to minimize the use of gradient directions and stick with one convention that is clearly explained.
191	Would be simple to provide a range of values
206	Please develop how this is a novel interdisciplinary approach within the context of the previous work (e.g., Yang and Tsai, 2020) and levee-based studies. This is the analysis of the outputs of a well-used groundwater model tool within a context that has been done before. I believe the study is worthwhile, but this statement is a gross and unjustified over-sell.
214-19	Why are these results sentences in the methods? Seem out of place.
258	Are crater topographies a standard coastal type? "Closed-dune" or similar would get the point across without invoking the extraterrestrial.

275	Doesn't Figure 5 show 3D volumes of the SLF? The vertical slices appear to be a visualization technique rather than "the only place we have results".
276	Provide the value of the threshold in this sentence and remove the next.
291	Unit weights difference still uninterpretable
308	Not overpressure.
309	Which head differences? In space or time? From what to what?
313	Not excess head
328	And why does this matter?
348	Justification is "because there's more of them"? Really? The nice idea with the Factor of Safety (1/SLF) is that you don't want to be exactly at the critical value but need some buffer. Explaining this choice within a FoS framework would provide stronger justification.
370	What prevents this release? Slow drainage? Long flow paths? Other? Developing this would allow the next statement on something explaining the similarities (correlation? statistical danger word)
376	Implications for real systems with more anisotropy and heterogeneity could be developed here with reference support on the importance of such things in coastal groundwater hydrodynamics.
383	Arguably, none of these results suggest liquefaction, but it does seem important to develop this further to explain why intruding flood waters with the same pressure gradients lead to different results.
386	How does this pressure (really pressure head) difference relate to those modeled in this study? Would help provide context of 0.01 m to an actual range – does this matter or not?
394	This result is not presented – the "simultaneously reached". Only snapshots of model results are show, and no time-dependent results are provided to support this portion of the discussion.
396	Indication that the flow rates related to infiltration rates are an important control and should be analyzed. This sentence also says that lowering pressure reduces the pressure gradient and is self-evident. Lowering a numerator does have that effect on a fraction.
404	Maximum {vertical} hydraulic gradients. Directions needed.
421-2	This was in no way a result of this study.
Figure 2	A hypothetical system. Need consistency with methods. Missing subscript.
Figure 5	Are the 3D color volumes the vertical hydraulic gradient or SLF? The last sentence of the caption implies they are gradients, but there is no colorbar for gradient.
Figure 7	I suggest removing Figure 7 and incorporating these results into Figure 6, which would benefit from a contoured SLF value and topography included above the depth slices in c).

References

Cardenas, M. B. B., Bennett, P. C., Zamora, P. B., Befus, K. M., Rodolfo, R. S., Cabria, H. B., & Lapus, M. R. (2015). Devastation of aquifers from tsunami-like storm surge by Supertyphoon Haiyan. *Geophysical Research Letters*, *42*(8), 2844–2851. https://doi.org/10.1002/2015GL063418

Grant, A. R. R., Wein, A. M., Befus, K. M., Hart, J. F., Frame, M. T., Volentine, R., et al. (2021). Changes in Liquefaction Severity in the San Francisco Bay Area with Sea-Level Rise. In *Geo-Extreme 2021* (Vol. 0, pp. 308–317). Reston, VA: American Society of Civil Engineers. https://doi.org/10.1061/9780784483695.030

Sadrekarimi, A. (2014). Static liquefaction-triggering analysis considering soil dilatancy. *Soils and Foundations*, *54*(5), 955–966. https://doi.org/10.1016/j.sandf.2014.09.009

Yang, S., & Tsai, F. T.-C. (2020). Understanding impacts of groundwater dynamics on flooding and levees in Greater New Orleans. *Journal of Hydrology: Regional Studies*, *32*, 100740. https://doi.org/10.1016/j.ejrh.2020.100740