We have received the second referee's comments (RC2) on our paper, titled: **Hydrogeological controls on the spatio-temporal variability of surge-induced hydraulic gradients along coastlines: implications for beach surface stability** (A. Paldor et al.). We thank the referee for their valuable feedback, which we believe will improve the revised paper. Below is a list of the referee's comments (in red) and our respective replies (in black). Text cited from the manuscript is in black italicized, with suggested revisions highlighted in blue with tracked changes.

General comments:

1) Line 32: I do not interpret the beach groundwater observations of Sous et al. 2016 as soil failure, please check.

Agreed. In the revision we will leave only Stegmann et al. (2011) as a reference for that statement.

2) Line 34: I do not fully agree with the definition given for liquefaction. A zero-stress soil needs an external force to be liquified.

We agree, and will revise all *liquefaction* in the MS to *quicksand*, which, as the reviewer correctly points out, is the more suitable term in this context.

3) l.42: depends

We agree and will correct to plural rather than changing the verb:

Laboratory experiments (Sous et al., 2013) suggest that the magnitudes of hydraulic gradients in the beach due to infiltration from sea-swell and infragravity waves depend on the wave frequency, cross-shore position, water table overheight, and the presence of standing waves.

4) l. 53: Mory et al. 2007, I would emphasize here that liquefaction events were related to the presence of a rigid structure in the soil, and rather use Michallet et al. 2009 (JGR) for the same site but finer analysis.

To address this comment, we will edit this sentence and add reference to the suggested study:

Observations, theories, and simulations have shown that the pore-pressure changes owing to energetic ocean waves can reduce effective stresses and may cause ~~liquefaction~~ failure of structures and surfaces (Chini & Stansby, 2012; Mory et al., 2007; Sakai et al., 1992; Sous et al., 2013; Yeh & Mason, 2014 Michallet et al., 2009).

5) l.121: What is meant by "seepage vector" ?

We will revise the sentence according to this comment:

The magnitude of the hydraulic head gradient ~~(Figure 2)~~, which according to Darcy's law is the magnitude of the seepage vector divided by the hydraulic conductivity, is denoted i (Figure 2). The seepage vector is the specific discharge, which is computed as the outflow vector at top nodes of the domain. In 2D, this vector has two components – a horizontal ($-Ki_x$ in Figure 2) and a vertical ($-Ki_z$). This work focuses on the vertical component. Other variables used in the following calculations are shown in Figure 2 and summarized in Table 1.

6) I do not see the input provided by Section 2. The data analysis has been already published, and the results presented here do not bring real insight (no vertical gradient, nothing new than much older works) and certainly do not show the statement in the Conclusions section ('may substantially affect…')
We agree. In the revision we plan to remove Section 2 entirely (and Figure 1).

7) l.163: Please detail the definition of unit weights and more generally provide a unified and clear discussion about saturation vs submersion effets (e.g. l. 292).
To address this comment, we will edit in the explanation given for these two quantities (after equation 3):
The use of γ_sub as the representative unit weight of simulated soil is appropriate for soils that are fully submerged, as it accounts for the buoyancy effect, considering the unit weight of the overlying water column (γ_w). However, for the parts of the model landward of the inundation line, the saturated unit weight may be more suitable. This means that adopting γ_sub uniformly may be an underestimate of the actual unit weight in real systems ($\gamma\_sub= \gamma\_sat-\gamma\_w$)
We will also add these to Table 1.

8) Can you justify the anistropy in K ?
To address this comment and the following one, we will edit the text in lines 222-223:
The homogenous hydraulic conductivity Kx is 50 m/d for the baseline simulation, and values of Kx = 10, 25, 100 m/d were also simulated as part of a sensitivity analysis~~and Kx varied between 10 and 100 m/d in sensitivity analyses~~. In all simulations, the anisotropy was 10 (i.e., the vertical hydraulic conductivity, Kz, was 10 times lower than the horizontal hydraulic conductivity, Kx). This range of hydraulic conductivity with a porosity, n, of 0.3 is typical for sandy beach environments (Freeze and Cherry, 1979).Anisotropy of porous material may represent the presence of horizontally-extended low-K lenses (e.g., localized compacted clay lenses), which reduce the conductivity in the vertical dimension preferentially.

9) Can you describe in detail your sensitivity analysis (parameters and ranges) ?
See previous comment for the suggested revision to address this. Additionally, we state these in lines 248-250:
The sensitivity of the results to the topography and hydrogeologic parameters was tested, including freshwater influx ($0.01< q\_0 < 0.04$ m/d, Figure 3 and Table 1) and hydraulic conductivity ($10 < Kx < 100$ m/d, Table 1, typical values for sandy beaches (Freeze & Cherry, 1979)).

10) The surge imposed here shows the same typical height and time scales than typical macrotidal areas. Does it mean that the potential "liquefaction" predicted here can be observed in any comparable macro-tidal coast ? Please comment.
No, because in macro-tidal areas the dynamic steady state is different since the frequency of the fluctuations is still diurnal. Surges that occur over decadal time scales may induce quicksand as the sediment relaxation time scales is smaller. We will add this comment where the simulated surge height is reported:
For the transient surge simulations, the coastline head was varied over 8.5 hours between zero and a 3 m maximum surge height (inset in Figure 3). A sea level of 3 m above the mean represents a combined high-tide and surge event with a projected return period of 100 yr by the year 2050 in the East Coast of the United States (Tebaldi et al., 2012). The ocean surface was assumed to be spatially constant at any time, and effects of wind waves were not simulated. The simulated surge height is comparable in magnitude to macro-tides, but the differences in frequency (macro-tides are diurnal) mean that macro-tidal beaches are likely in equilibrium with respect to sediment mobility, which is not the case for storm surges.

11) - l.308, 368 etc: What is meant by "overpressure" ?
We mean the pressure induced by the inundation water which is higher than the pre-surge pressure (steady state). To address this, we will change all *overpressures* in the manuscript to *increased pressures.*

12) The role played by horizontal gradients is not explored, and this may significantly affect the interpretation.

We agree that it is important to discuss horizontal gradients and we will revise section 6.1 as follows: *Thus, the simulations suggest the areas most susceptible to destabilization (i.e., deep critical layers) are those where topography is low enough to be inundated widely, and high enough that the pressure release is limited. An important factor that likely plays a role in this relationship between intermediate topography and critical gradients is the horizontal gradient. In places where horizontal hydraulic gradients can develop, a more efficient dissipation of surge-induced pressures may be expected, and therefore critical gradients are less likely. This may explain the absence of critical hydraulic gradients from the steepest areas in the model, since these areas develop horizontal gradients. Horizontal gradients are important also when considering other modes of surface instability, such as shear failure. To assess the potential for shear failure, a Coulomb criterion must be derived, which is beyond the scope of the current study.*