Replies to REVIEWER#2

The manuscript titled "Tsunamigenic potential of unstable masses in the Gulf of Pozzuoli, Campi Flegrei, Italy" provides a numerical study of four submarine (three) and subaerial (one) landslide scenarios and their corresponding tsunamigenic impact in the Gulf of Pozzuoli. The work is looking at past evidence of collapse to reproduce modes of failure and tsunamigenesis with the largest potential attributed to subaerial collapse. The codes UBO-BLOCK and UBO-TSUIMP/JAGURS are used for simulating mass failure and tsunami respectively. The dispersive potential of the wave characteristics is also investigated in the study. The authors conclude that only some of the scenarios examined have an impact on the adjacent coastlines. This is a comprehensive investigative work that can strengthen the existing knowledge of tsunamis in the region. However, some key points need to be addressed.

We thank the reviewer for the interesting and productive comments, that will help to improve this work. Specific answers to the remarks are reported below in red.

General comments

1) P.2 L45-46, P8 L196-198, P17 L355 and elsewhere

Although it is noted that the masses have been selected based on previous deposits and present morphology, it is not clear why these are the worst possible scenarios that could happen in the region. Can the authors affirm this with certainty? This needs to be carefully addressed as it can be misleading for future policy and hazard mitigation efforts as the current work indicates that there is not a significant risk from submarine but only subaerial landslide tsunamis in the region. Future failures may not replicate past events and an increase in volcanic activity, variance in the location of failure, and higher collapse volumes may increase tsunamigenic potential and the impact may be larger, even more so if the possibility of such events is ruled out.

We totally agree with this view. The cited “worst-case credible” approach bases on the known and potential sources, or on credible scenarios built on the existing evidence. Due to the poor knowledge of the seabed morphology and to the rapidly changing conditions of the area, mobilization of bigger volumes can’t be ruled out. We will add these considerations when citing the worst-case credible approach, evidencing its limitations in this case.

2) S2.2

Subaerial landslide tsunamis generally have a greater tsunamigenic potential than submarine ones and have more complexity due to the interactions between the solid mass, water and air. Modelling the high impact, and complex slide kinematics often requires 3D Navier-Stokes solvers, SPH, CFD or VOF models. It is not clear whether this complexity is captured with the study's numerical codes, an issue which could underestimate the hazard. It is not clarified in the manuscript how the authors distinguish between the modelling of the submarine and subaerial failure, besides the different solvers used in the propagation modelling.

The impact of the sliding mass with water is a very complex process, involving highly non-linear phenomena that are hard to capture with numerical simulations. At the same time, they are mainly confined to the “splash-zone”, the area around the impact which has typically limited extension. Moreover, the generated waves can be very high but have usually high frequency, then dissipate quickly. The perturbation propagating at higher distance in the Gulf of Pozzuoli, on the contrary, results from the longer components, which can be associated to the landslide motion along the submarine portion of the sliding surface. This is adequately captured, in our view, from our modeling approach. Some citations of cases of subaerial landslide-tsunami for which our approach produced very good results will be added (for example, Scilla 1783 landslide-tsunami).

Specific comments:

P.2 L45-46 The worst-case referenced approach is not yet published and therefore hard to understand and verify. The authors should expand more on the methodology.

As stated above, the reference to the worst-case will be removed.

P.2 L127, 128-130: Air Entrainment is of importance when focusing on subaerial landslides, the generation involves a triple phase interaction.

We will adjust the sentences, even though in our approach we will not model such interaction.

P3 L139-141. What are the underpinning equations in UBO-BLOCK? The model seems to have been primarily used in the modelling of submarine landslide tsunamis

Indeed, it has been tested also in the subaerial environment, for example in the 1783 Scilla landslide-tsunami, or in the 1963 Vajont landslide. References to these two cases will be added.

P6 L167 As JAGURS is nested, it is worth clarifying that the nested approach is omitted in that case.

Ok

P6 L167 Please also give more details on whether the simulations were run locally or in a cluster, CPU time and time of the event.

The simulations are run on local computers, with order of magnitude of ten of minutes for the landslide dynamics and some hours for the tsunami propagation. In this last case, JAGURS is not used with the parallel computing version, so the computational time is not optimized, then not particularly significant.

P10 L231-233 This statement should be substantiated by references.

Ok

P10 L240-254 Although the authors mention the locations of the deposits and the corresponding volumes it is not clear how these volumes were estimated based on the observed deposits. A few sentences explaining the approach would help here.

The approach used to reconstruct the scenarios is described more extensively in the Discussion section, under the paragraph *Landslide scenarios*. We will add some sentences on this regard also in the scenario description.

P11 L263-265 If any, what kind of rheology is assumed for the sliding mass?

Due to the few information available for the area, we did not describe in detail the landslide rheology. We assumed for them a moderate translational behavior, based on the scarce information available: for example, for Scenario 1 the position of the final deposit is estimated, and this is assumed as a constraint on the dynamics and the rheology. This is not the case for the other scenarios, for which a similar behavior has been assumed. Of course, there is a big uncertainty on the landslide rheology and geometry: this work aims also at stimulating further investigation in the area. Some of these considerations will be added in the scenario description and in the conclusions.

P17 L355 This statement standing alone reads quite strong and I think it cannot be backed up by only assessing 3 scenarios of collapse, it should be better rephrased to the specific case studies as in P19 L408.

Agree, the sentence will be modified.

Figure 10 I think xlabel is mistyped and '(m)' is wrong?

Yes, thanks for the observation, we'll fix it.

P20 L427-429 I agree with the authors, and I think this is not clear throughout the manuscript so far.

The discussion about the scenario individuation has been included in the Discussion session to avoid repetitions and group all the criticalities. In our view, in this way the paper is clearer and better organized. However, some words about this will be added also in the scenario description.

P22 L472-L479 I believe an important addition to the discussion would be how parameterisation and probabilistic approaches, as for example surrogate models, can help when it comes to future hazard assessments in the region and the capability of assessing multiple scenarios of collapse with large variances in location, volume and mode of failure.

This is an interesting point, we will add some considerations about this in the conclusions.