Referee: 1

Comments to the Author

The authors presented a hybrid numerical model to capture the soil-fluid-structure interaction. The Material Point Method and Implicit Continuous-fluid Eulerian framework are adopted for large deformations in porous media and complex fluid flows, respectively. The theoretical formulation and numerical implementations are sound. The model feasibility is demonstrated through comparison with analytical and experimental results. The application to earthquake-induced submarine landslides is particularly interesting.

Thank you for your positive feedback on our hybrid numerical model for soil-fluid-structure interaction. We appreciate your recognition of the sound theoretical formulation and numerical implementations. Below is our response to your specific comments.

Some specific comments are given below.

1. Please double-check Equations (3) (the definition of Terzaghi's effective stress), (12), and (13) for oversights.

The reason for the difference in sign between our equation and Terzaghi's equation is based on our assumption regarding stress. Terzaghi assumed that the stress is positive during compression, while we have assumed that the stress is positive during extension and fluid pressure is positive in compression. Hence, this variation in assumptions leads to a difference in sign between our equation and Terzaghi's equation. However, this comment leads us to double check the equations; we found a couple of signs mistake in equations (16) and (53).

Indeed, equations (12) and (13) are modified to be consistent.

2. Numerous Typos are detected, for instance,
a) page 11, the paragraph before Eqn. (11), ("stretch" instead of "sketch");
b) page 12, the paragraph before Eqn. (17) ("model" instead of "mode");
the paragraph before Eqn. (20), (contact law instead of "contactlaw");
c) page 19, the paragraph below Eqn. (39) ("the" instead of "he") and the phrasing before the same equation;
d) page 33, the last paragraph ("shaking" table instead of "skaing" table, can "occur" instead of "occured").
f) page 35, the paragraph below Figure 17 ("pressure" instead of "ressure").

We have thoroughly reviewed the document and made the necessary corrections to address these typos.

3. What is the value taken for the Smagorinsky constant Cs for the numerical examples? Please specify.

In our numerical examples, we used a value of Cs equal to 0.1. This value was chosen based on previous studies and the specific characteristics of the simulated flows. It is important to note that the selection of Cs depends on various factors, including the specific turbulence model employed and the nature of the flow being simulated.

4. On pages 13-14, it is not clear how the expression of the hydraulic conductivity is obtained from Eqns. (25) and (28). Please double-check.

We have thoroughly reviewed the mentioned section and provided a more detailed and explicit explanation of how the expression for hydraulic conductivity is obtained from the given equations.

5. On page 15, the momentum exchange coefficient is 1E5 (in the text) or 1E15 (in Fig.3)? There are two values of the momentum exchange coefficient in the model. The momentum exchange coefficient between the structure and seawater/air is set to 1E15, while the coefficient between water and air is set to 1E5. Thanks for your comment, it shows the need for clarity in the manuscript. We have made the necessary updates, ensuring that this information is clearly stated.

Referee: 2

Comments to the Author

The manuscript presents an extended version of the MPMICE algorithm to account for porous media using an implicit formulation. The overall framework is presented and validated using two analytical solutions. Two more benchmarks are presented, and results are compared with FEM solution and experimental data. Finally, the framework is used to demonstrate the ability of the model to capture the whole deformation forces of earthquake-induced submarine landslides. The topic is relevant for the Geotech and Offshore communities. However, I have a few comments and suggestions that need to be addressed before this manuscript can be accepted.

Thank you for your positive feedback on our manuscript. We appreciate your recognition of the overall framework, validation with analytical solutions, comparison with FEM solutions and experimental data, and the demonstration of the model's ability to capture the deformation forces in earthquake-induced submarine landslides. We agree that the topic is relevant for the Geotech and Offshore communities. We appreciate your valuable comments and suggestions, and we carefully address them to meet the requirements for acceptance. Below is our response to your specific comments.

- The title and abstract bring high expectations in terms of the ability of the model to deal with earthquake motions. Although the final application shows a landslide triggered by shaking (which is well appreciated by the reviewer), the model does not capture filed earthquake field conditions. The numerical algorithm is not particularly validated for this type of application (e.g., no validation of site response, boundary conditions are not well explained). The formulation is dynamic, but this does not imply that it is ready to deal with these types of applications accurately.

I understand that the expectations regarding the model's ability to handle earthquake motions were high. While the application of the model to simulate a landslide triggered by seismic loading (shaking table) was appreciated, it is noted that the numerical example does not fully capture field earthquake conditions. We acknowledge your concern about the lack of validation of the site response the inadequate explanation of boundary conditions. To our understanding, the lack of validation of site response could be (a) validating seismic loading generated in the model with actual recorded ground motion data from earthquake events and (b) validating the accuracy of the numerical model in replicating the actual response of the soil under seismic loading conditions.

For (a), ground motion can be set at bedrock (or rigid material in our numerical model) as ground motion at the rigid materials is typically less affected by site-specific soil properties compared to the surface ground motion. This motion can be obtained from recorded data at a nearby bedrock station or estimated using ground motion prediction equations that relate earthquake characteristics to the expected ground motion at the bedrock. We do not aim to simulate the earthquake process, but we just assume to use directly ground motion as input motion. For the simplicity, we use the input motion with constant magnitude (1g) and constant frequency (2Hz)

For (b), we have validated the response of the slope under seismic loading using the same approach to generate seismic loading in an additional numerical example of earthquake induced slope failure in the section “Validation of soil response to the seismic loading”.

-Introduction is well-written, and the motivation and objectives are clear. The authors also highlight the new contributions of this work compared to those already published. The organization of the paper is also clear. However, the writing of the rest of the document (including Appendices) needs a thorough review. There is a number of issues with the English quality, many grammar mistakes and typos, some sentences feel incomplete (e.g., “Solving the linear equation below to obtain the increment of velocity with i,j = 1 : N as”), and excessive repetition (pg. 26, 27, 28).

Thank you for your positive feedback regarding the introduction, motivation, objectives, and highlighting the new contributions of the work. We appreciate your recognition of the clear organization of the paper. Your comments have been noted, and we have carefully addressed these language-related concerns. We have revised the document, paying close attention to grammar, sentence structure, and repetitive content to improve the overall quality of the writing.

To prevent repetition, we present the parameters of water and air which remain consistent across all simulations in the beginning of the section “numerical examples”.

- The numerical implementation section is slightly tricky to follow, in part because the notation is not always consistent or well presented (e.g., check the notation in Figure 5, what is the difference between FC (capital) with fc (lower caption)?). I highly recommend that the authors include a Figure with a diagram summarizing the computational scheme.

We have conducted a thorough review of the notation employed in our paper and taken measures to ensure its consistent usage, minimizing any potential ambiguities. To aid readers in comprehending the implementation process, we have included Figure 6, which offers a comprehensive visual representation of the computational scheme. It provides clarity on terminology, such as "FC" denoting "cell-face" quantity, "f" representing fluid, and "c" indicating "cell center" quantity. The notation "f,c" specifically refers to the cell center quantity of the fluid.

- Pg 18: Can you better explain what you mean by “extra momentum from contact forces”? Do you mean ffric? I also don’t understand the context of “The nodal velocity and nodal temperature are applied boundary conditions”.

Yes, by "extra momentum from contact forces," we refer to the frictional forces acting at the nodes due to the frictional interaction with the structure. We rephrase the sentence to clarify.

Regarding the boundary condition, it is applied after solving the balance equations as depicted in equation (88).

-Pg 19: What is the meaning of “faced-centered” or “face-centered”? The value calculated at the center of the cell? Please, clarify.

Instead of using the term 'face-centered,' we clarified it by using the term 'cell face' instead. By 'cell face,' we refer to the face of the element.

-Pg 25: Can you provide the details of the GitHub repository?

we update the manuscript to include the link of the GitHub repository (https://github.com/QuocAnh90/Uintah_NTNU).

-Pg 30: Provide reference after “Unlike other computational models based on total stress analysis,…”

We made the necessary updates to provide 4 references after the statement.

-Pg 31: “The saturated debris flow … turbulent flow as grains are separated from each other and exhibit no contact forces between grains”. Add a new figure comparing the evolution of effective stress in both scenarios. We should see effective stress going down to zero in the submerged model.

We appreciate your suggestion to include a new figure comparing the evolution of effective stress in both scenarios: the saturated debris flow and the turbulent flow with no contact forces between grains. Figure 13 provides a visual representation of the differences in the behavior of the effective stress between the two models. We incorporated this new figure into the manuscript to further support our discussion.

-Pg 33: “In the final example, we perform numerical analysis of the earthquake-induced submarine landslides”,” The earthquake of this magnitude can occur typically for the earthquake of magnitude of more than 6” (??). This is not similar to an earthquake loading. Boundary conditions are not realistic. Please, rephrase this explanation. Also, refer to the first comment.

We assume a constant acceleration and frequency for the sake of simplicity. However, in reality, ground motion is more complex but can be decomposed into a series of simpler components with varying accelerations and frequencies. By employing Fourier analysis, we can analyze and understand these constituent components, even though the actual ground motion is more arbitrary in nature.

We rephrase this:

An earthquake of this magnitude is possible. For instance, in the case of the 2023 Turkey-Syria Earthquake, significant ground shaking with peak ground acceleration exceeding 1g was observed at numerous locations. This serves as an example of the practical occurrence of such high levels of ground acceleration during seismic events.

-Pg 34: Do you mean Rowe's stress-dilatancy theory? Please, correct and add a reference.

We updated the manuscript to provide the reference for the Rowe's stress dilatancy theory (Soil Behaviour and Critical State Soil Mechanics – David Muir Wood)

-Pg 34: "On all boundary faces, the symmetric boundary condition is imposed" I do not understand the meaning of this sentence.

"Symmetric boundary condition" refers to a condition where the normal component of the velocity at the boundary face is zero, and the tangential component is equal to the tangential component of the (non-existing) "mirror" neighboring cells. This boundary condition is often applied when there is symmetry in the problem domain, and it ensures that there is no flow or movement across the boundary face in the normal direction. We have rephrased this sentence.

-Pg 39: Why is drag force in a summation form?

We emphasize that the total drag force is the sum of drag forces from different materials. For instance, in the water momentum balance equation, the total drag forces comprise three components: (1) the drag force between air and water (multi-phase flows), (2) the drag force between water and structure (fluid-structure interaction), and (3) the drag force between water and soil (porous media).

-Pg 41: What is the difference between superscripts C and FC? Please, clarify.

We have clarified in the Figure 6