Review of « Combining seismic signal inversion and numerical modeling improves landslide process reconstruction », Yan et al.

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General comments

This work deals with the reconstruction of landslides dynamics using seismic signals and numerical modelling, in the case of the Baige landslide. It clearly has the potential to be a very good paper, as results are, in my opinion, convincing. The manuscript is, besides, well written. However, I think some shortcomings should be addressed to improve the quality of the manuscript:

- The contribution of this work, in comparison to other studies, is not clear. Using seismic data to calibrate numerical models has already been done before. If the analysis of high frequencies in seismic signals (besides, define what you mean by high frequencies) is innovative, it think it is not clear enough in the manuscript how it helps better constrain the landslide dynamics. If the main contribution of the article is using seismic data and numerical modelling to better constrain the dynamics of the Baige landslide specifically (which is perfectly okay), it should be stated more clearly.
- The methodology is not described precisely enough, and some methodological explanations are given in the Results section instead of the Methodology section. In particular, the method for estimating the landslide dynamics from seismic signal, and the method for calibrating model parameters, should be more detailed.
- The authors state that using deposits and seismic data to calibrate the model improves the quality of the simulation results, but do not illustrate it. As this is, if I'm correct, a key point of their work, this aspect could be further developed.
- The figures, and their caption, can be improved.

Specific comments

- Abstract.

The abstract could be shortened. In particular, l.22 to 33 is a too detailed state-of-the-art for an abstract. It is not until l.34 that the aim of the paper is given, I think it should be stated earlier.

You could also try to be more specific about the novelty of your approach, in comparison to previous study. When you write « Seismic signal dynamic inversion results are used to verify the numerical simulation, and then the numerical simulation is dynamically constrained and optimized to obtain the best numerical value », this has already been done in other studies, e.g. (Moretti et al. 2012; 2015; 2020; Yamada et al. 2016; 2018).

- Introduction

l.52 : If a remember well, this estimation is for non-seismically triggered landslides.

l.55-58 : You should be more specific about the « new methods ». If you mean the combination of simulation and seismic data, this is not really new (cf references above).

l.92 : It is not clear what « the two main approaches » refer to. The reader is led to believe that you mean « block model » and « numerical simulation », while I guess you refer to two numerical approaches.

l.99 : I would also mention thin-layer models as a third approach. It is formally a continuum approach, but the equations are averaged over the depth of the landslide. They are less precise than full 3D models (continuous or discrete), but allow faster runs (see refs above).

l.107-108 : As stated in my comment on the abstract, it is not clear at this point how your work is different from previous studies combining seismic data inversion and numerical simulations (Moretti et al. 2012; 2015; 2020; Yamada et al. 2016; 2018). You mention in the abstract that you use both high frequency and long period seismic data, but this is not apparent in the introduction.

- Study Area ad Data Sources

l.114-122. I would be more specific on the contributions of previous studies on the Baige landslide. It would highlight the novelty of your work. Besides, I found at least one other article on the Baige landslide that you do not mention (An, Ouyang, et Zhou 2021).

l.133 : You also have Digital elevation models and ortho-photographs. They should be mentioned, with their source, in the Data section, not in the Simulation section (l.200 – 202).

l.135-136 : You should detail your criterion of a good azimuth coverage. It is only slightly more than 180°, so one could argue it is not that good… Did you also have a criterion on the distance, explaining why you do not use stations to the south-west of the landslide ?

l.137 : Add a reference for the probabilistic PSD, or explain it, it is not clear to me how « probabilities » are associated to PSD.

l.138 : « low background noise », you should be more specific and quantitative. What is the criterion of a low background noise ? Besides, you only show the PSD of one station, so we have to take your word that other stations also have a low background noise.

- Methodology

l.167 : 0.006 – 0.2 Hz. This frequency bandwith contains only low frequencies, while you mentioned high frequencies in the abstract. Or maybe I'm missing something (what is your definition of low and high frequencies ?).

l.174 – 175 : Are your particles spheres ? Can there be overlap between particles ?

l.180 and onwards : It would be helpful to state explicitly the parameters that need to be set in the model. In this perspective, Table 1 could be added directly in this section. Besides, some parameters in this table are not explained in the main body of the text, such that their role is not clear. For instance, what is the difference between the internal friction coefficient, and the intergranular friction coefficient?

l.208-209 : By « cell size », do you mean particle size ? What do you mean by « real-world » time?

l.210-239 : This paragraph is difficult to follow because you go forth and back between the different steps of the parameters calibration. However, the flow-chart of Fig. 5 is clear. Thus, I recommend you write again this part. If I understand well, there are three steps : 1) choice of some parameters without calibration (explain how they are chosen), 2) Calibration of some parameters by reproducing the extent of the deposits, 3) Calibration of the last 2 parameters by reproducing the inverted force. Just explain these three steps, in three successive paragraphs. Besides, you should give more details on the calibration method : is it simply trials and errors ? What is the range of initially tested parameters ? In the following points I have some more specific questions.

l.210-211 : I don't understand what you mean in this sentence. How do you determine deposition characteristics from seismic signal ? You should be more specific than just using the word « characteristic ». What do you mean by « reference for the discrete element landslide motion simulation » ?

l.212-213 : « accumulation state », « the range of landslide accumulation » : do you mean the geometry of the deposits ?

l.212-215 : You state that the fracture displacement, the initial shear force, and the friction coefficient between particles relate to the bond strength between particles and friction coefficient. Why do other parameters not relate to them (e.g. elastic modulus and stiffness) ? I would add a small explanation in addition to the reference.

l.216-218 : Explain more clearly why you did not calibrate the Poisson ratio and the Young modulus. You should also give the macro/micro conversion formula.

l.218-220 : You should add a reference here, and explain why there is such a difference between laboratory and field scale parameters. You should also justify the 40% value, where does it come from ?

l.220 – 221 : It is not clear, at this point of the manuscript, how you estimate the velocity and displacement of the landslide, from the inverted force. It is mentioned in the Results section, but it should be explained in the Method section.

l.233 : How did you choose your « key points » ? How many are there ?

l.236 : shouldn't s^2 be the sum of $(X_s-X_i)^2$?

- Results and Analysis

l.249-250 : In the methodology you said you deconvolve the seismic signals to obtain displacements. But here you mention velocities. If displacements are used only for inverting the source force, this should be stated clearly. It would be very helpfull in Fig. 1 to display clearly the different stages of the landslide (initation, sliding, blockage and deposition)

l.253 : « The main driving force of the landslide is gravity » . Well, isn't it always the case ? What else could it be ? « the landslide surface » : do you mean the surface of the sliding mass, or the surface on which the mass slides ?

l.254-255 : « … and the SNR of the V component (…) appears high ». I don't see the link with velocity changes in the longitudinal direction. Do you mean that the landslide movement has a significant dip (35°), which implies important vertical velocities in seismic records ?

l.252 : « south-east-to-south », do you mean in the south-south-east direction, or from the south-east to the south (which is strange). To avoid confusion, you could give an azimuth. Besides, from Fig. 1 it seems the landslide went to the East until it reach the valley bottom. So I don't understand the direction « south-east-to-south » you give.

l.255-256 : Throughout the manuscript, I think « deposition stage » could me accurate than « accumulation stage ». At least, it seems more natural and self-explanatory to me. And from Figure 1, I have the impression that the landslide propagates to the North and to the south-East once it has reached the valley bottom. So I don't understand the « easterly direction » you mention.

l.265-271 : This passage should be in the Methodology section. Besides, it should be more detailed : how do you relate the seismic signal to the landslide velocity and mass ? « Roughly determined » is too vague…

l.272-276 : This passage should at the beginning of the Results section, to introduce the seimic signals with their description. By « three phases of velocity », do you mean three phases of acceleration ? It is not clear to me : 1) how you determine the points of velocity changes as they do not always correspond to maxima or minima, and you did not pick some extrema, 2) why these « velocity changes » in seismic signals necessarily correspond to transitions from acceleration to deceleration phases for the landslide.

l.278 : I think the fact that the seismic signal is longer than the event itself results from seismic wave scattering, rather than from « superimposition of vertical and horizontal waves ».

l.281 : Be more specific, the inversion of what ?

l.282 : « The analysis here », do you mean the analysis made before, or the analysis that will be done afterwards ?

l.287 – 305 : This passage is somehow a repetition of what has been said before. You analyze the spectrum content to identify acceleration and deceleration phases, but if I'm not wrong you had similar results from direct observations of the seismic signals (though, as stated above, it is not clear how you draw these conclusions). Besides, the link between the two parts is not completely clear because the 7 times spotted in Fig 6 are not reported in Fig 7.Thus, I was a little lost, as the loigcal links between paragraphs is not clear.

l.298 : « in the longitudinal direction », what do you mean ? How do you relate a direction to a PSD curve ?

l.306-309 : Do you have references to support this affirmation ?

l. 309 – 310 : You should be more specific than just stating that there is a « clear difference », especially as there are no figure to support this statement.

l.315 : You should give these empirical relations in the Methodology section.

l.330-335 : This should be in the methodology section, and the method must be clarified I think. It is not clear to me how you infer « sliding trajectories » from satellite data, and how you « minimize discrepancies » between these observations and inverted positions (and you did not mention satellite data in the Data section, this should be corrected). What is exactly that you invert ? The mass and the initial position (the displacement being then deduced from Newton's law) ?

l.336 : You already have a masse estimation at l.316, this is confusing. I would only keep one in the Results section, and discuss it in the Discussion section by comparing it to other estimations.

l.337-338 : From this sentence I understand that the displacement is deduced from the result of the method explained l.330-335, but isn't displacement a direct result ?

l.344-352 : You should add more references to the figures and subfigures to illustrate better your description of acceleration/ deceleration phases. They could be indicated more clearly in Fig. 8 (I guess it is the dashed vertical lines, but it is not said in the caption). Besides, the link with the acceleration/deceleration phases observed in the seismic signals is not clear.

l.352 : « the relative high frictional force », what do you mean ? High in comparison to what ?

l.354-356 : Are these stages deduced from the simulation, from seismic data or from the inverted force ?

l.361 : What do you mean by « variance results » ? Variance of what ? Is it the s^2 mentionned in the Methodology section ? It should be more explicit.

l.362 : « after which they match », well in Fig.12 I wouldn't say that the inverted and modeled velocities match after t=18s.

- Discussion

l.401-402 : You should reformulate. Reading the sentence we think you compare your volume estimations to the estimations of Chao et al. (2016) and Ekström and Stark (2013), while you actually compare your volume estimations to topographic surveys estimations. I would simply say that your volume estimation depends on the frequency bandwidth, it would me less misleading. Besides, write frequency bandwidths in Hz or s, but not both, it is otherwise difficult to compare them.

l.411 – 412 : Well, if with a different frequency bandwidth you have a better volume estimation and similar estimations of the trajectories, the advantage of using a higher frequency bandwidth is not clear. I understand that it allows to model sharper increases/decreases of the force, but isn't it more interesting to have a better estimation of volume ?

l.419-423 : I would have liked a more detailed discussion on the calibration method : choice of parameters calibrated for deposits and then dynamics, influence of calibrating dynamics

before deposits, method of calibration (trial and error, gradient descent, …), sensitivity of results to parameters variations, … You also state that combining deposits and dynamics helps improve the simulation results, but we have to take your word. What would you have obtained if you had only ised deposits ? To what extent would the modeled dynamics be different ? Besides, there are no details on the ressources needed to run simulations. How long is one simulations ? How many needed to be run for the calibration step ?

l.432-434 : I don't understand, if you do not model fluidization, shouldn't you under-estimate the velocity, instead of over-estimating it ?

l.436-497 : It is not clear, in this section, what you deduce from your inversion and simulations, what you deduce from field observations and/or previous studies, and what you hypothesize. Clarifying these points would help highlight your contribution to the understanding of the Baige landslides dynamics, in comparison to previous studies. For instance, you go through a lot of details for the initiation stage, and I don't always understand how you can deduce them from your results.

l.504-533 : In these paragraphs you only state relatively broad statements, without references : in this perspective, it helps understanding the contribution of your work, but it would be more helpful in the introduction. In the discussion, I would expect that you compare more specifically your work to previous studies on the Baige landslides, so that the reader can see if your description is in agreement with what has already been done, if your work sheds lights on some points that previous studies had overlooked, or if you disagree with previous interpretations. Besides, you could also discuss other landslides where you think your methodology could help better constrain their dynamics.

Another point that could be discussed is the difference between your work and previous works using seismic data to calibrate numerical models (Yamada et al. 2016; 2018; Moretti et al. 2015; 2012; 2020). If I'm correct, they compared the force inverted from seismic signal, to the force provided by simulations, whereas to you compare trajectories. Do you expect the results to be different?

l.507-508 : Quantify what you mean by low-frequency and high-frequency.

- Conclusion

l.520-521 : I think it is not clear enough, in the manuscript, how the analysis of the high frequency content (i.e. above XX Hz ?) of seismic signals improves your understanding of the landslide dynamics. In the Results section you give successive descriptions of the landslide dynamics, from raw seismic signals, from the inverted force, and from the simulation. However the comparison between these different descriptions is not very clear.

l.524 : « there are often multiple solutions ». You did not illustrate it in your work. You show the result of your calibration process, but you do not quantify its benefits in comparison to simpler methods (e.g. using only depoits).

Detailed remarks

l.44 : It is not clear what « each method » refers to.

l.120 : Use scientific notation for the volume (1.96×10^7 instead of 1960×10^4)

l.122 : If you give the altitude of the deposition zone, you should also give the altitude of the initiation zone.

l.147-149 : Isn't this sentence a repetition of the first sentence of the paragraph ?

l.166-169 : This paragraph on signal processing should be at the beginning of the section, before you explain force inversion.

l.182 : where does the « 9+ » come from ? Is it a typo ?

l.370 : Shouldn't it be s^2 instead of R^2 ?

l.378 : « the velocity variance » -> The variance of velocity residuals

l.421-422 : I don't think these notations are explained in the main body of the article.

l.424 : « different landslide », there is a mistake I think, maybe « different landslide phases » ?

l.450 : You should indicate the « first-level » and « second level » platforms on previous figures.

Figures and Tables

Fig 1 : Give the source of the DEM. (a) and (b), the date could be made more visible (e.g. in black with a wjite background). (c) Give the meaning of SA1, …, SA5. The small insets for I, II, III and IV are too small and without context and scale, they are hard to interpret. I would remove them or create another figure to improve readability. (d) Add the orientation. Are the distances to scale ? Finally, the acronyms DEM and AUV are not explained in the main body of the text.

Fig 2 : (a) I would mention, at least in the caption, the main geological periods corresponding to each symbol. Or you could simplify the figure by regrouping some geological units by periods.

Fig 3 : Explain what white lines are. You could also try improve the grid lines (I'd try a lighter colour, with thinner lines) What is the subgraph, below the PSD graph, with the blue and green lines ?

Fig 5 : The subscript for \delta in the last diamond is very small and hard to read. The legend for the different kinds of \delta values in the grey box is also too small.

Fig 6 : Explain what the different numbers refer to. Change the x-axis not to repeat x-tick labels. The caption says the figure shows signal-to-noise ratio, but only velocity is given by the y-axis, if I'm not wrong.

Fig 7 : The spectral content is not coherent with you pre-processing step, where you filter signal between 0.006 and 0.2 Hz. What are the red dashed vertical lines ? (b) The FFT is

weird looking, as if you had a continuous content in your signal. Did you detrend it ? Otherwise, consider using a log or semi-log scale. (c) The bandwidth considered here (y-axis) should be the same as in (b), for consistency. (d) What is the bandwidth used for the PSD ?

Fig 8 : what are the dashed vertical lines ? You should explicit what you mean by « absolute value ». Is it the norm of the acceleration et velocity vectors ? For the displacement, is it the cumulated displacement (i.e. the curvilinear abscissa of the center of mass of the landslide) ?

Fig 9 : Maybe I missed it, but do you explain in the text why some seismic traces are not used ? If not, this should be included. In any case, a brief explanation would be welcome in the caption.

Fig 10 : I recommend using a colorblind friendly colormap for the time. The northern extent of the deposits is outside of the figure limit.

Fig 11 : Same remark, use a colorblind friendly colormap. It is not the same orientation as in previous maps. Where is the North ? You should also add a scale.

Fig 12 : Use thicker lines and different line styles, to have a colorblind friendly figure. I think R^2 should be s^2, to be consistent with your notations in the main body of the text.

Fig 13 : The northern extent of the deposits, in the river, displays a positive difference (more than 60 m). Shouldn't it be 0 at the edges of the deposits ?

Table 1 : You could add a column where you specify which parameters are deduced from laboratory measurements, and which parameters are calibrated.

Table 2 : It would be better to indicate the different phases between the given times, rather than above them (we do not know if the given times indicate the beginning or the end of the phase written above).

Bibliography

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