

Interactive comment on “Landslides Data Assimilation Using TRIGRS Based on Particle Filtering” by Changhu Xue et al.

Anonymous Referee #2

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Summary: Landslide stability modelling is conducted using a number of methods, including the USGS-built TRIGRS system. Numerical models of this sort have been in some instances improved by using 'data assimilation' methods, meaning, as I understand it, reinitializing model parameters during model runs with observational data to better represent reality. Here the authors describe the use of a data assimilation method to improve the performance of the TRIGRS model on a specific test case in Central China. They suggest that the model outputs in terms of landslide internal friction angle are improved, and that the groundwater pressure head is another output that would thus be improved.

Recommendation: I cannot recommend this paper for publication in its current form. I think there are numerous details that are absent which makes it impossible to fairly

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judge the work the authors have undertaken, and as such it is inappropriate for publication. While the topic itself is of interest and suitable for the journal, there is insufficient information provided to assess whether the results are significant or if the methods are original. Below I include some specific comments that may help the authors to amend this study.

Specific comments:

1. Throughout the paper there is not enough detail provided about the methodology. For example, it is impossible to judge the particle filtering method when the authors say "The assimilation experiment starts on the first day of observation data and occurs daily using the improved PF". This is simply not enough information! Even if the method is published elsewhere, there must be at least a basic explanation of how this process works and the way it was incorporated. It would be impossible for another researcher to replicate almost every aspect of this study based on the information provided, and until that is possible I cannot recommend this for publication. Specific locations where detail is needed: - How were the parameters for TRIGRS obtained? How was hydraulic conductivity measured? - How was rainfall measured? - What is the particle filtering method? - What are the specific soil conditions used to define the three blocks? - How were InSAR measurements corrected? If these methods are original to this study, then every detail of the methods will be necessary to assess the validity; this could be attached in supplementary material if necessary. Where standard or previously published methods have been used, citations are also definitely necessary; I found this study alarmingly lacking in citations.
2. I do not think it is possible to judge the accuracy or validity of the model outputs given the authors' approach, and moreover it is not clear whether the model is over-fitted to the observational data. As far as I can tell, the GPS monitoring data and Sentinel data is used as input to the data assimilation method and as a comparison for the model output. This seems somewhat circular; how can the outputs be independently validated using the same data that is used as input? I can see that the TRIGRS model alone does



not provide outputs close to the observational data, and that the use of DA methods with the observation data gets this closer – but given that the same observational data is used as input and validation seems to me to be circular. It is not clear whether the observations and the assimilation model outputs are similar simply because the DA methods over-fit the model to the observational data, which I would argue is the null hypothesis. As an example, the TRIGRS model outputs data on the groundwater pressure head (phi). The authors could independently validate the model outputs by using field observations of the pressure head as comparison with model predictions; this would test whether over-fitting is an issue.

3. Nowhere in the study or in the supplementary material do the authors ever describe the error margins (levels of uncertainty) attached to their results. It is impossible to assess whether the results are statistically significant given the data presented. I think it is imperative that the authors include error margins on all of their results when revising this study. For example, figures 4,6,7,9,10,12 and 15 should include error bars, and the map figures should at least contain a description of the uncertainty associated with the estimates. I would not consider this study for publication until error margins are fully described.

4. The authors often use jargon that makes it unclear what is being discussed, and I would suggest revising the text to reduce complexity and explain some of the more complicated details. For example, the sentence: “A great number of approaches of DA have been developed in recent years, of which sequential algorithms like particle filtering (PF) are increasingly popular. PF is based on Bayesian theory and originally introduced by Arulampalam into DA (Arulampalam, et al., 2002). It is developed in many DA studies due to the advantage of being unconstrained by state Gaussian distribution and linear assumptions” The terms ‘sequential algorithms’, ‘Bayesian theory’, ‘state Gaussian distribution and linear assumptions’ are not necessarily clear to all readers (this is not, after all, a journal focused on statistical methodology) and should be explained. There are other jargon-y terms used in various places, particularly in the

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abstract, where jargon is used extensively, and it should be removed and explained in more detail. Additionally, the authors should not use acronyms before explaining what they are (e.g. 'FS' in the abstract). Finally, there are a number of copy-editing issues that would need to be addressed before final publication. I would be happy to assess and correct these issues in a revised version of the manuscript, but at this stage I think it is inappropriate to list them all given the scale of the other changes required to bring this to a point acceptable for publication.

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