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Discussions

Interactive comment on "Quaternary lava tubes distribution in Jeju Island and their potential deformation risks" *by* Jungrack Kim et al.

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The manuscript by Kim et al. presents an original application of various and current methods as such InSAR, machine learning, and field data. The objective is clear and particularly interesting for several readerships ranging from Earth observation to spatial exploration. I am enthusiastic about the possible applications and the estimation of risks on Jeju Island. The choice of Jeju Island is ideal for this study allowing the identification of many targets (lave tubes), supported by robust previous studies. However, the excepted signals should be low or spontaneous, such as loading effects and/or brittle deformation. The InSAR observation is therefore adequate (spatial/temporal revolution) and the application of both methods (PS/SBAS) allows to have a cross-validation of results, needed on low signals. But the manuscript contains many weaknesses: 1) The organization of the manuscript is missing: for example, the 3.2 section contains

results and methods. Overall, the results and methods are mixed in sections. So, I recommend completely restructuring the manuscript using simple sections (geological context/method/results/discussion/conclusion). 2) The core of this manuscript is InSAR method, and I am unable to understand the used methods from the text. For example, I did not understand the compensation of orbital errors and why to use this substraction: (line 253). I think the InSAR section needs many improvements adding the accurate workflow (my main questions concern InSAR and PS/SBAS processors, used DEM, used orbital files, the processing parameters, etc). Why are there two periods for PS and SBAS? Why are the ascending data not used? Specifically, some descriptions in the manuscript are wrong: line 130, the optimal data set is defined as being Sentinel-1, this is only true for this study; line 134: there is a shortest revisiting time than Sentinel-1. line 134: the adjective "unique" is used to describe the IW mode while the StripMap mode is available. Moreover, I am not convinced by the use of SBAS: the SBAS processing allows to increase the spatial resolution of results, by adding some source of uncertainties (e.g. multilooking) and other issues currently discussed in the literature, (e.g. bias). My recommendation is a proofreading by an InSAR specialist to rewrite the method sections and correct errors. For the SBAS, I recommend extending the network and using more interferograms. Finally, I can propose to add the ascending data even if the island is not fully covered. This addition should improve the machine learning results without strong assumptions about the displacement components. 3) The method of spatial analysis of lava tube distributions is also not clear for me. The uses of kriging and interpolations require to have assumptions concerning the spatial evolution of the variables and nothing allows me to verify these hypotheses. 4) Laser scanning methods are not described and are seldom used in the manuscript. 5) All the figures of results are difficult to read. The units of maps are missing, and the legends are not complete. Firstly, a modification of color scales is mandatory to allow a good visualisation of displacements. However, the LOS velocities from PS results are low < 1 mm/yr and therefore less than uncertainties. But most noticeable is the difference between the PS and SBAS results. Even though the observation periods are different,

I presume a similarity between the PS and SBAS results but the SBAS map shows velocities between <-10 mm/yr and > 10 mm/yr on a period covered by PS results. These transitory signals would be visible on time-series, but it is not clear. I recommend to propose a real comparison between PS and SBAS results, on a single figure for example. 6) The distinction between the previous results and the new results provided by this study is not clear. 7) The difference between the results and interpretations are not clear. It is mandatory to separate observations and interpretations/speculations. 8) The example of Manjang cave is very interesting to describe the study and the Laser Scanning result is an asset. 9) The last part of the manuscript is not acceptable as is. But I am confident that the modifications of the InSAR parts (methods and results) should improve the discussion section. To summarise, I recommend to better structure the manuscript and to propose a precise description of methods (mandatory when the study focus on low signals). I also suggest modifying the SBAS network and adding the ascending data to improve the time series results and allow a cross validation of InSAR displacements. Another solution is to not use the SBAS results and just use the PS results in both directions (ascending and descending). I actually think the ascending data should be a strong improvement for this study. This simplification could improve the clarity of the manuscript. After these modifications, the understanding of the results and interpretations should be clearer. The potential interest is very high for a large audience (and for public actors). I hope my recommendations will improve the quality of the manuscript making a high-quality publication. Major revisions required.

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