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Comment on nhess-2021-35

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Community comment on "Spatial and temporal subsidence characteristics in Wuhan (China), during 2015–2019, inferred from Sentinel-1 synthetic aperture radar (SAR) interferometry" by Xuguo Shi et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-35-CC1>, 2021

The present manuscript focuses on the spatial and temporal distribution of land subsidence hotspots across the expanding and developing urban footprint of Wuhan in China.

At the moment there is a growing InSAR literature investigating land subsidence and karst collapse hazard in Wuhan. Therefore, the authors should contextualise their results and compare with published InSAR results achieved by processing either (nearly) the same Sentinel-1 dataset used in this paper or other SAR datasets.

It is with regard to this important aspect that my comment is made.

The authors seem not to have accounted for the following study:

Han, Y.; Zou, J.; Lu, Z.; Qu, F.; Kang, Y.; Li, J. Ground Deformation of Wuhan, China, Revealed by Multi-Temporal InSAR Analysis. *Remote Sens.* 2020, 12, 3788.
<https://doi.org/10.3390/rs12223788>

Han et al. (2020) have processed and analysed basically the same Sentinel-1 dataset i.e. April 2015 to June 2019, with SBAS-InSAR technique. So, there is a straightforward opportunity for the authors of the present manuscript to make a comparative discussion of their results with those published by Han et al. (2020).

Another very recent paper that the authors should also consider is:

Jiang, H.; Balz, T.; Cigna, F.; Tapete, D. Land Subsidence in Wuhan Revealed Using a Non-Linear PSInSAR Approach with Long Time Series of COSMO-SkyMed SAR Data. *Remote Sens.* 2021, 13, 1256. <https://doi.org/10.3390/rs13071256>

In this paper, my collaborators and I have processed and analysed the longest time series of COSMO-SkyMed data that has been published so far over the city of Wuhan. Because our paper and the present manuscript share the common interest on correlating the observed land subsidence with soft soil consolidation, it would be interesting if the authors would enrich the discussion of their results vs. those published in our paper.

Further line-by-line comments are appended here below:

- Lines 34 - 44: these sentences are very common knowledge for the journal readership and can be removed, alongside the cited references. This should help the authors to shorten the manuscript and save space for the discussion later on.

- Lines 52-54: with regard to the mention of COSMO-SkyMed, the whole archive of COSMO-SkyMed 2012-2019 has been analysed and very recently published by Jiang et al. (2021) - see comment above. This should be acknowledged to keep the state-of-the-art section updated with the very recent literature
- Figure 1: karst collapses and levelling points are barely visible. The authors should consider the addition of a zoomed view.
- Line 74: The authors should specify what "The Rise of Central China" is
- Lines 112-116: these sentences are very common knowledge for the journal readership and can be removed, alongside the cited references. This should help the authors to shorten the manuscript and save space for the discussion later on.
- Line 124: why did the authors choose 500 m as the upper limit of bperp, given that Sentinel-1 ref. tube deviation is +/- 100 m (<https://sentinels.copernicus.eu/web/sentinel/missions/sentinel-1/satellite-description/orbit>)?
- Line 125: The section lacks of information about the software that has been used to process Sentinel-1 data or, instead, if a proprietary code has been used.
- Line 148: this spatial intersection should be better displayed by combining the InSAR subsidence rates and geological datasets in the same figure.
- Section 4.4. Houhu area: how do the present results and time series compare with those published in Han et al. (2020) at equal SAR data processed?
How with Jiang et al. (2021) who processed X-band high resolution data with non-linear PSInSAR technique?
- Section 5.2, Relationship between karst subsidence and river water level/rainfall: how do the present results compare with those published in Han et al. (2020)?
Han et al. (2020) found that the changes of land subsidence near the bank of the Yangtze River are generally consistent with the variations in the river water level over most of the monitoring period. However, they also noted a time delay with respect to the time of water level changes, suggesting the complexity of and variation in the hydrogeological condition along the Yangtze river in Wuhan. What is the authors' opinion in this regard based on their data?
- Lines 292-295: please revise this last sentence in the context of the future direction of the present research