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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Land Subsidence due to groundwater pumping: Hazard Probability Assessment through the Combination of Bayesian Model and Fuzzy Set Theory" *by* Huijun Li et al.

Anonymous Referee #1

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Dear Editor, I would like to thank you for inviting me as a reviewer for the paper "Land Subsidence due to groundwater pumping: Hazard Probability Assessment through the Combination of Bayesian Model and Fuzzy Set Theory" by Huijun Li et al. I really appreciated the authors' approach on land subsidence topic, and I hope my comments would open an interesting and constructive discussion with the authors to improve the paper.

General comments

In this paper the authors proposed an interesting method to evaluate hazard probability of land subsidence in the Beijing Plan, by integrating Bayesian Model and Fuzzy

Set Theory. By using InSAR data, groundwater levels and geological characteristics of the area, authors analysed, modelled and validated the changes in land subsidence rates from 2003 to 2017. They also highlighted the differences in the resulting maps, comparing models with and without fuzzification of the factors' weights, which provided interesting findings related to the importance of the characterization of uncertainties when dealing with the subsidence phenomenon. Addressing uncertainties in approaching natural hazards is of primary importance and necessity, as well their subsequent communication to the administrations in charge. The introduction section provides an extensive framework of the subsidence topic in relationship with other works and approaches. However, it needs some aspects to be clarified, especially when authors point out their approach is quantitative while some previous cited papers are qualitative. The method section, despite its intrinsic complexity, is well presented. The workflows presented in Fig. 1, Fig.2 and Fig.3 are certainly useful for the interpretation. However, some statements should be revised to improve clarity. The case study section appears as a hybrid between method and results; in my opinion this should be avoided. This is the part for which I think more work should to be done. Some operational choices need to be further clarified and explained. Finally, I appreciated the results section as it is concise and very well subdivided in sub-sections according to the different aspects of authors' findings. In particular, the comparison between models with and without fuzzification is meaningful, which translates into the comparison between accounting -or not accounting- for the uncertainties of the influencing factors. Also, the comparison between the two modelling approaches with InSAR derived subsidence rate changes, in terms of overestimation and underestimation is a really appreciable result. However, I would suggest to change the heading of the section in Results and Discussion. In my opinion, the work is worthy of publication, unless the aspects listed in the specific comments are addressed.

Specific comments

Introduction Section

Line 52: You are pointing out that the method adopted in the papers from line 50 to 52 are subjective and qualitative. You should clarify why, especially for the paper of Tafreshi et al. (2019), in which fuzzy theory is used too. Alternatively, this part could also be added in the results section, as a possible discussion of your findings in relationship with previous literature.

Line 78: I do not understand why you stated that your procedure could be implemented in an early warning system. This statement is also repeated in the conclusion section without any further explanation. Do you mean implementing in the procedure future groundwater scenarios or something else? I think you should clarify this point, otherwise I would change it in "prediction".

Methodology Section

Line 89: It is not clear if the mentioned steps were addressed by the authors or if they were already available.

Line 123: Is equation (3) a new formulation by the authors or could it be associated to some references?

Line 149: Standardization has a precise meaning. I would recommend to clarify, otherwise change this word if the meaning is that you have just created homogeneous size grids for InSAR data and not further processed them.

Case Study Section

Line 181-182: Could you further add some characterization to the geological units mentioned? E.g. Soil type, origin etc..

Line 183: This part is crucial, as you are basing your work on calculating different weights for the different features of each factor. Thus, the criteria used for the classification of each factor should be further clarified and justified (i.e. each class represents a factor's feature with its own computed weight, consequently crucial for hazard probability calculation). In my opinion is not enough stating that "the four factors are classified

according to the data characteristics".

Line 192-193: You have set the threshold between subsiding and stable points at 10 mm/year. Usually, this choice should be based on standard deviations analysis of the subsidence rates in a stable area, otherwise, if this threshold came from previous works in the area, it should be mentioned. Moreover, the threshold you have chosen should be used to define colours in your map in Fig. 6c. Indeed, in this figure, you have chosen the green colour also for subsidence rates from 0 to 10 mm/years.

Table 1: It could be interesting if you would provide in the discussion section some considerations about the factors which resulted in the highest difference between the computed fuzzy and no-fuzzy weights.

Line 212: Does it means that, differently for 2011-2014 and 2015-2017, for the period 2003-2010 the prior and posterior probability are computed on the same InSAR data? Could these make a difference in the three resulting hazard probability maps?

Results section

Line 226: Is that right that SrD (i.e. subsidence rate decrease) is located in the region with the highest groundwater level decrease between 2011-2014 and 2015-2017? I would say that where the subsidence rate decreased between these two periods, the groundwater level was restored accordingly, thus the area is SrD, and in a lower hazard class for 2015-2017. If I misinterpreted what you wanted to communicate, I suggest clarifying this part by using shorter statements.

Line 245-247 and linked Fig. 8: Could you provide an explanation (also with the help of previous works) for why after a peak of hazard probability is reached both for compressible layer and Quaternary layer at a certain thickness values and then, for higher thicknesses, the hazard probability is much more lower? In my opinion, an interesting discussion on this could be done.

Conclusions Section

Line 288: As I have previously mentioned in the comments for the Introduction section, I would suggest clarifying the statement regarding the early warning system.

Technical Comments Line 37 and Line 41: The use of “the” before land subsidence should be avoided.

Line 41: The use of “but” in the statement is unnecessary.

Line 55: I would recommend changing “validity of the data” into “data quality”.

Line 89: I would suggest changing “technology” into “processing”.

Line 104-105: the meaning of Y is not specified. Moreover, I think there is a mistake in the statement: “P(Y|S) is the posterior probability of Y subject to S”. S is land subsidence, thus it is the phenomenon for which you want to calculate the probability influenced by the factor Y.

Line 129-130: You stated that the FWBM method contains three parts, but you just enumerated one, so the statement is incomplete. Figures: measurement units in figure 6c and 8 are missing.

About the English language, I think it needs some improvements (singular/plural and punctuation need to be checked, tenses need to be homogenised). If possible, I suggest a professional proofreading.

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