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Natural Hazards and Earth System Sciences
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Discussions

Interactive comment on "Subsidence activity maps derived from DInSAR data: Orihuela case study" *by* M. P. Sanabria et al.

Anonymous Referee #2

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The paper proposes a methodology to create subsidence activity maps and apply the Serviceability Limit State (SLS) criterion to historical buildings, based on ground displacement data from satellite Persistent Scatterer InSAR (PSI). Maximum differential settlement and angular distortion for each building are evaluated using interpolated maps of the PSI displacement data, and compared with SLS to verify whether limits have been exceeded. The methodology is applied in Orihuela in Spain, where historical buildings have been damaged by subsidence induced by aquifer overexploitation. The method proposed by the authors is significant scientifically, although there are some aspects to clarify. These mainly relate to (i) the discrepancy between the resolution and geolocation accuracy of the input PSI data, and the local scale nature of this research; and (ii) to the use of the PSI data and the reliability of their interpolation

across areas of no data. Overall, the manuscript in the current version has a good scientific significance, but major weaknesses (which are listed below in detail) make it of poor quality of presentation and discussion, and these need urgent attention for the manuscript to become acceptable for publication in NHESSD. My detailed comments and suggestions to address the above mentioned aspects are summarized below, by using a section-based order, when possible.

Before the detailed comments, it has to be noted that part of the manuscript has been published by Sanabria et al. in Mathematics of Planet Earth, Lecture Notes in Earth System Sciences 2014, pp 267-270 (http://link.springer.com/chapter/10.1007%2F978-3-642-32408-6_60), although it is clear that the latter is only a 4-page long extended abstract from a recent conference (i.e. IAMG 2013, held in Madrid). Probably the authors should cite it and acknowledge the relationship of this paper with the above publication (e.g. by declaring that it is a longer version of the paper already published).

Specific comments

The abstract is clear and concise, although the authors do not report quantitative information on the results of their research. The latter would be added value to the paper, so I suggest the authors improve the text accordingly.

The first paragraph of the Introduction (page 5366, l.18-25) includes a sequence of statements regarding land subsidence and water withdrawal, structural motions, damages and monitoring, which would benefit from the addition of specific supporting references to scientific and technical literature. The sole reference Tomás et al. 2013 seems not sufficient to support the entire set of statements, although this is a review of the subject 'subsidence measured with InSAR'. Please consider related work and include appropriate references.

Section 2, Methodology (pages 5368-5369): this section summarizes the entire methodology proposed by the authors to create activity maps for subsidence from PSI data. The section lists the various phases of the methodology, but these are mixed with discussion and results from the specific case study of Orihuela, which are then presented again in the following sections. Although it is undoubtedly useful for the readers to have a summary of the methodology at the beginning of the paper, the current content of section 2 creates confusion with sections 4, 5, 6 and 7, where a more detailed description of the various phases and steps are described, together with results. Please make the link between this sections and the subsequent ones more clear and smooth.

Page 5372, l.27 & page 5373, l.1-5: 'The thickness of the soft soils cannot be considered as an additional source of information for improving the displacement data interpolation. Although this fact is apparently contradictory to the results published by Tomás et al. (2010), the observed lack of correlation can be explained considering that the piezometric level variation is also a key variable involved in the consolidation process. The soft soil thickness has a spatial variability, whereas the second one has a spatio-temporal variability.' — please comment further these statements, and use figures or plots to explain the contradiction with the observations by Tomás et al. On the other hand, based on figure 3 it can be noted that motion values seem to be highly correlated with the thickness of soft soils, as opposed to what stated here. Please add further numerical evidences for the lack of correlation, and specify the thresholds for the Pearson coefficient to judge the data as correlated or uncorrelated.

Sections 4-6, PSI data and results, spatial analysis and interpolation: it is apparent that there are critical aspects associated to the used input data and the approach proposed. In particular:

(i) the PSI data for the test area do not cover homogeneously the monitored area, hence the spatial interpolation of displacement data based on points which are several kilometres away is undoubtedly risky and of little reliability (see Fig.4 and sparse location of PSI data across the river plain). PS points located far away are very unlikely to be correlated with areas of no PS, hence are not significant. Please comment this aspect further, and discuss limits and issues relating to this evidence, as well as how

the CGS performs with regard to this issue.

(ii) as per page 5368, l.18-24, the cumulative displacement values used by the authors refer to the satellite LOS component of motions (i.e. the raw output of the PSI processing), and not to the recomputed vertical values. For the analysis of building displacements and the application of the Serviceability Limit State (SLS) criterion, it is clearly necessary the use of vertical components, and not LOS ones. Please first comment this aspect with regard to civil engineering and geotechnical standards. It is also highly recommended that the authors re-compute all the displacement values from LOS to vertical, and recalculate all subsequent parameters for the various buildings by taking into account this conversion. Results discussed in sections 4, 5, 6 and 7 should be rectified accordingly.

(iii) the figures with PSI results need to include locations of the reference points for the two datasets. Were these identified at the same location for both datasets, 1995-2005 and 2004-2008? If yes, please specify the assumptions made for this location, and the selection criterion for this point. If not, it is clear that the two datasets and results of the methodology would be difficult to compare as they are, and these would need calibration to a common reference point, before any statistical and structural analysis. Please clarify and, in case, calibrate the two PS datasets.

(iv) please specify and discuss geolocation accuracy of the two SPN datasets, including internal precision of the SPN points, accuracy with respect to the SRTM DEM, and final external accuracy of the points after the geocoding to map coordinates. In section 4, the authors should also mention the input pixel size of the satellite data of the two stacks of SAR and ASAR data, and multi-look factors used during the SPN processing. They also may want to relate the latter with the final geolocation accuracy of the points.

Section 7, page 5376, l.23-26 & page 5377, l.1-3: the authors use a buffer area of 14m around each building to calculate the angular distortion and differential settlement for the application of the SLS criterion. The authors need to comment about the choice of this size for the buffer areas, in respect of: (i) the resolution of the satellite images (around 30m), the interpolated maps (20m), and the geolocation accuracy of the final SPN results/data (as per comment above regarding accuracy of the geolocation). These aspects and related assumptions are critical, and the above seem a contradiction between the local scale (single building) of the SLS analysis, and the resolution and accuracy of the input satellite SPN data.

Section 7.3: at the beginning of the manuscript, at page 5369, l.9-12, the authors state that they have calculated maximum differential settlement and angular distortions for each historical building of the Orihuela city, however, in section 7.3 only the results for a single church (Santa Justa and Rufina church, see section 7.3) are shown and discussed in detailed. Indeed, there are only final statistics about false and true positives and negatives with respect to a 'damage inventory'. To this aim, please specify and describe the content of the 'damage inventory' (page 5378, l.6-11), i.e. type of information recorded within the inventory. And also the level of completeness of the inventory (at l.5-6, the authors state this is not exhaustive).

As for the entire set of historic buildings, it would be useful to show and analyze results for all 27 historical buildings, for instance, by creating a comparative table with maximum differential settlement and angular distortions for each building. Please also number and show the location of all buildings in one of the existing figures. The authors should also discuss further these results with respect to the total area of each building, and in relation to the resolutions and accuracies mentioned above. Considering the dimension and areas of these buildings, are the resolution and accuracy of the data of this research suitable? Please comment on the significance and suitability of these data. As per above, there seems to be discrepancy between the spatial scale of the two layers (satellite data vs. building dimensions), with medium resolution data that are used here for a local scale, building specific assessment. Please clarify and rework the text accordingly.

Regarding the analysis of Santa Justa and Rufina church, based on figure 11, it seems

that the interpolated motion map is based only on 1 PS, hence the maps used for the SLS analysis and assessment is based on a single point and their reliability appears extremely low. Please clarify and discuss this aspect further, in relation to this building, and all other 26 historical buildings of the city. Show activity maps for some of these buildings, and the number of input SPN points present in their buffer areas. This aspect is very critical for the reliability of the proposed activity maps. Please also add data from the damage survey of the Santa Justa and Rufina church and other buildings, e.g. reported damages, number, aperture and size of cracks.

Other corrections related to presentation

Describe the meaning, mathematical formulae, symbols, and add specify references and comments regarding the following: (i) Pearson correlation coefficient (page 5372, l.24-27); maximum differential settlement and angular distortions (page 5368, l.3 & page 5369, l.6-7; move SLS formulas and parameters from Figure 8 to the main text).

More in general, some repetitions and duplications of various sentences are present throughout the manuscript (e.g., across the above sections, and the discussion and conclusions). These may cause confusion in the readers, and make the procedure difficult to export to other test areas or to allow reproduction by other scientists. I suggest the authors rework the text to restructured the layout, and reduce the above duplications and increase clearness. Section 3 could also be moved before the methodology, to make the description of the approach flow better and smoothly – with no interruptions by the description of the case study area.

Section 8 'Discussion and conclusions': this only includes conclusions, and duplicates sentences from the previous sections. Please add a specific section with detailed discussion and analysis of the results for the historical buildings.

Figure 3: add location of figure 6, 7, 9, 10, 11.

Figure 4: change colours for the last two classes of points, to make them more distin-

guishable in the legend and map.

Figure 8: move the bottom of the figure to the text and describe SLS formulas and parameters into detail, to make the method exportable to other sites and data.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 5365, 2013.