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Discussions

# *Interactive comment on* "Coseismic slip inversion based on InSAR arc measurements" *by* C. Wang et al.

**Anonymous Referee #1**

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The discussion paper presents a method that is best described as an incremental advance for interpreting InSAR deformation cause by coseismic slip. This advance can be summarized as using the unwrapped difference between two pixels (arc length), rather than the pixels from an unwrapped InSAR image. The paper suggests that this method avoids the pitfalls of the specific unwrapping algorithm, as well as the problem of ambiguous unwrapping results for coherent pixels surrounded by incoherent pixel regions. The paper also suggests that the proposed arc length can include slip rupture (i.e., observed offset) information directly in the inversion method and that standard inverse methods that use point displacements cannot account for such offsets, because they are not point measurements.

My assessment that the methods presented in this paper are incremental, at best,



because of several critical problems. First, Feigl and Thurber [2009] already presented methods for inverting InSAR data that do not require unwrapping. While the authors of this paper provided a cursory reference to Feigl and Thurber [2009], this paper does a great disservice to Feigl and Thurber [2009], which solved the problem of inverting, without unwrapping, more than five years ago. Furthermore, this paper dismisses the methods of Feigl and Thurber [2009] as 'nonlinear' and 'time-consuming' and that 'only a limited number of fault geometry parameters can be resolved'. While the inverse method is, indeed, nonlinear, I disagree with the proposed limitations (time consuming and limited parameters) of Feigl and Thurber [2009]. The authors of this paper simply fail to support such a harsh assessment of Feigl and Thurber [2009], which was, of course, published via a strict peer review process in the journal Geophysical Journal International.

Second, this paper fails to provide quantitative support for the conclusions. For example, conclusion 1 indicates that "...the new method produces similar results as those from the conventional method that is based on point displacements...". Considering that the noise structure is known, as stated in this paper, the authors should provide clear statistically-based support for such a conclusion. Otherwise, conclusion of "similar results" is meaningless. For conclusion 2, the authors state that "...the simulated test has shown that the new method perform [sic] much better than conventional one...". Again, the authors fail to support this statement.

Third, the paper fails to acknowledge that fault offset measurements are easily included in standard inverse methods that use point InSAR data. Such fault offset measurements may be readily included as a priori constraints directly in the G matrix.

I suggest that the authors conduct a direct comparison of analyses using Feigl and Thurber [2009] versus the methods proposed in this paper. Furthermore, such analyses must be supported by quantitative comparisons of results. Finally, as a user of the code GIPhT (the method presented in Feigl and Thurber [2009]), I found that GIPhT runs sufficiently fast and allows for numerous fault parameters. The authors of this paper fail to demonstrate otherwise.

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