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Interactive comment on "Analysis of crustal deformation and strain characteristics in the Tianshan Mountains with least-squares collocation" *by* S. P. Li et al.

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On behalf of my co-authors, we thank you very much for giving us an opportunity to revise our manuscript, we appreciate you very much for your positive and constructive comments and suggestions on our manuscript. Those comments are all valuable and very helpful for revisin and improving our paper, as well as the important guiding significance to our research. We have studied your comments carefully and have tried our best to revise our manuscript according to the comments. The main corrections in the paper and the responds to the comments are as flowing:

Referee #2's Comments: My main concern about this paper is that the results pre-

sented here provide no new insights on the deformation mechanism of the region. The authors concluded that a) the convergence rate in the western part of Tiram basin is higher than in the eastern part, b) the crustal deformation decreases gradually from south to north and c) Wuqia-Jiashiand lake Issyk-Kul are the highest strain rate regions in the area. These conclusions are the same as those derived in (Wang, X-Q et al, 2007) in which GPS data during 1998-2004 was used to estimate strain rate, based on classical method.

Authors' Answer: This manuscript tries to use another method to derive the deformation field in the Tianshan Mountains. Indeed, the deformation mechanism of the region seems no significant changes. This manuscript actually provides a cross-validation of conclusions made by Wang et al, 2007. However, we do have some details and differences, such as: 1)We interpolated the GPS velocity by using a reasonable grid, which can avoid the disadvantage that the strain results depend on station distribution density. 2) We detected a divergent component existing in the interior of the Pamir relative to the Tarim Basin, despite of the presence of thrust faults along their boundary 3) In our paper, we analyzed the relationship between the seismicity and strain rate measured at the surface, however, In Wang's paper, he did not discuss this issue.

Referee #2's Comments: There are two main differences between (Wang, X-Q et al, 2007) and this paper: a) the time span and density of GPS data in this paper is different from those of (Wang, X-Q et al, 2007). But it is not clear to what extent the use of denser and longer time spanned GPS data changes the strain results. That would be nice if authors discussed this issue in more details.

Authors' Answer: We will add new data sets and computation, as well as results derived from the new data sets. Please find the new paragraphs.

Referee #2's Comments: b) The other difference is the use of different methods to interpolate GPS velocity fields. In this paper collocation technique, which is a well known method in geodesy, is used to interpolate GPS velocity field, before computing strain parameters. It seems that using this method does not significantly improve and change strain regime as compared to the results of (Wang, X-Q et al, 2007), obtained based on the classical technique. Furthermore, the comparison between different interpolation methods (including classical and collocation approaches) to estimate strain rate has been already investigated by Wu, Y. et al. 2011.

Authors' Answer: There are a few differences mentioned in the manuscript, for example: 1) The strain parameters in our manuscript were calculated by using Gauss plane coordinate system instead of spherical coordinate system in Wu's paper.2) We analyzed the best value of K in Gauss empirical function.

Referee #2's Comments: Therefore, I do not think that this paper moves forwards our understanding of the crustal deformation in Tianshan region significantly and I think that the main outcome is not new.

Authors' Answer: We have added new data sets and new results. Hopefully, this can convince the reviewer.

Referee #2's Comments: I regret that I cannot provide a more positive assessment, but I hope that the authors will find my comments helpful eventually. Below I will also list some of my comments on different parts of the paper. - The paper overall needs to be significantly polished as many parts of paper is not easy to be read.

Authors' Answer: We have made significant efforts on smoothing the language.

Referee #2's Comments: - The justification of authors in some parts of paper is not satisfactory and convincing. For instance: a) on page 3184, line 1, the authors indicate that:" By analyzing the differences between the strain parameters calculated separately by using the sphere surface and Gaussian plane coordinate system, Shi and Zhu (2006) proposed that we should use spherical coordinates when calculating strain parameters. However, considering the fact that the span of our research region in this paper is moderate and the differences in the results between the spherical co-

ordinate system and Gauss plane coordinate system are not significant, we still used the Gauss plane coordinate system to calculate the strain parameters. "The reasoning of authors here is not conclusive: What is the moderate distance in their opinion and why the difference between two coordinate systems is not significant here? Is there any mathematical proof behind that?

Authors' Answer: As we all known, when we analyze the crustal deformation, if the area of region is not very huge, we can use plane coordinate system in place of plane coordinate. Considering the distance between two stations, when the distance is about one degree, the difference between great circle line length and responding chord length is only about 1.4 meters. The relative error is only 10-4. Many international researches (Pietrantonio G et al. 2004) also do it like this. We will add mathematical proof in our manuscript.

b) On page 3186, line 24, it is written: "Compared with the rotation rates of 0.65°Ma-1 estimated from geologic models (Wang et al., 2000) and 0.5203°Ma-1 from the work of Yang et al. (2008), our result is much smaller. The reason for these differences remains to be elucidated. "The last sentence does not give any satisfactory information. (Wang et al., 2000) used more GPS stations in Tarim basin (see Fig. 1 in their paper) to calculate strain rate. Therefore, I think the difference between results of this paper and (Wang et al., 2000) is due to the interpolation error.

Authors' Answer: We will add new data sets and redo the calculation of rotation rates. As expressed by the reviewer, we also think that the denser observed stations locating in the Tarim basin would decrease the interpolation error.

- On page 3186, line 5, the authors named three stations (CHG4, IO29 and IO59) as the stations with highest misfit values. But there is no information in the paper about the location of these stations. That would be nice if the authors provided a table with some information about names, coordinates and accuracy of estimated velocities of stations.

Authors' Answer: We have now added information in the manuscript.

- The authors in some parts of paper have referred to the references which are uncommon. For instance, on page 3184, line 14, one of the appropriate references for calculation of principal strain, maximum shear strain and surface expansion can be the famous textbook of Ranalli (Giorgio Ranalli. Rheology of the Earth. Springer Science & Business Media, 1995). It is the same for the reference of Least squares collocation on page 3182, which can be more well-known ones (e.g., Moritz Helmut. Least-squares collocation. Reviews of geophysics, 16.3, 1978: 421-430.)

Authors' Answer: We have replaced these two references.

Interactive comment on Solid Earth Discuss., 7, 3179, 2015.