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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.

Anonymous Referee #2

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In this paper, crustal deformation of Tianshan mountains has been investigated. The authors used previously published GPS data collected from 1992 till 2006, applied least squares collocation method to interpolate and estimate velocity field and derived strain parameters.

My main concern about this paper is that the results presented 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-Jiashi and lake Issyk-Kul are the highest strain rate regions in the area. These conclusions

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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. 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.

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.

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.

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.

- 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 be-

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tween 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 coordinate 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?

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.

- 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.

- 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 text-book 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.)

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