

Interactive comment on “Pull-apart basin tectonic model is structurally impossible for Kashmir basin, NW Himalaya” by A. A. Shah

Anonymous Referee #1

Received and published: 9 February 2016

This paper almost reads like a personal diatribe. The author is adamant that the Kashmir Basin is not a pull-apart basin as proposed by Alam et al. (2015, 2016) and the paper is essentially an earnest attempt at refutation. The author calls the pull-apart model ‘impossible’ 15 times (including in the title and in 110 lines of text) and also states that the pull-apart architecture ‘could not exist’, is ‘problematic’ and ‘inconsistent with data’. If one of my undergraduates had written this paper, I would have sent it back with advice to remove the redundancy, improve the English, remove absolute terms like ‘impossible’, eliminate the undercurrents of emotion, and just stick to data-based arguments. This paper is poorly written and should not be published as is.

Now for the science:

1) The author has a limited understanding of oblique deformation and thinks a pull-

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apart basin has to have the architecture of the simple cartoon shown in his figure 2. The same holds true for his model-driven views of horsetail splay termination zones (his fig. 3). Transtensional and transpressional fault networks can be highly variable as documented all over the world. Positive and negative flower structures can have a wide variety of fault patterns. Transtensional flower structures do not have to have pull-apart geometries. The authors architectural arguments against a transtensional basin are weak.

2) Neither Shah nor Alam present focal mechanism solutions for earthquakes from the Kashmir Basin. If earthquake focal mechanism solutions revealed any transtensional or extensional events, then the Alam et al. pull-apart models would be more convincing. I have not been able to find any transtensional earthquake events in the Kashmir Basin from my web trawl of relevant literature.

3) Both Alam et al. and Shah should look more carefully at the GPS data in Schiffman et al. (2013). Figure 13 in Alam et al. (2015) is an inaccurate representation of the actual data. It should never have been published. Some of the arrows in their figure are incorrectly oriented and the vector lengths are all the same which is misleading. The Schiffman et al. (2013) GPS data indicate that south-directed motions in Zaskar are oblique to the NW-striking Balapora Fault and Central Kashmir Fault. The obliquity suggests significant components of dextral slip. GPS velocities in Zaskar have higher S and SW velocity components than the data from the Pir Panjal Range. Thus the boundary in between - the CKF – is also under compression. Therefore, the GPS data from Schiffman et al. (2013) suggest dextral transpression within the Kashmir Basin, not transtension. Neither author raises this point correctly, nor mentions the term transpression at all.

4) Shah should point out the unconvincing images of strike-slip related features in the Alam et al. (2015) paper – e.g., their figure 8. I am not convinced of any of their visual ‘signatures’ of strike slip features and visual offsets.

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5) In conclusion, Shah is correct to question the evidence for a pull-apart model for the Kashmir Basin. It does not appear well supported. But he should use concise scientific arguments focusing on the evidence from the historical earthquake record, a more careful analysis of the GPS data base, a less rigid approach to what a transtensional basin has to look like in terms of fault geometries, and a more rigorous scrutiny of the tenuous lineament and apparent offset evidence presented in the Alam et al. papers.

Interactive comment on Solid Earth Discuss., doi:10.5194/se-2016-4, 2016.