

Interactive comment on “Linked thick to thin – skinned inversion in the central Kirthar Fold Belt of Pakistan” by Ralph Hinsch et al.

Anonymous Referee #1

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This paper uses a combination of seismic reflection profiles, field observations, and structural modelling to examine the central part of the Kirthar fold belt in western Pakistan. The main result is that the region represents a combination of thick-skinned reactivation of normal faults and thin-skinned deformation of the overlying sediments. At present, I think significant clarifications will be necessary in order to make the paper suitable for publication, as described below.

1. As a general point, I struggle to see, and judge the robustness, of some of the interpretations of the seismic data (e.g. the images in figure 8). I think that it would be helpful if the authors: (a) provided un-interpreted, as well as interpreted, images of the sections; (b) provided zooms of the key features discussed in the text; (c) marked on the locations of the wells that seem to be key to the correct identification of some

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horizons, (d) provide in the text a discussion of the reasoning behind interpreting the structures, and the horizon identifications. For example, at the eastern ends of the seismic sections, and in places where it is buried, where the pick of the top of the Kirthar formation is key to the subsequent discussion of regional level, a detailed discussion about the reasoning of the pick would be useful. On some seismic sections I can't tell what the basis is for interpretations (e.g. faults 1 and 2 on fig 9). In general, I think a much more thorough analysis and justification of the seismic data is necessary.

2. I think the authors would benefit from a clearer consideration of the seismicity. Focal mechanisms are provided in Figure 2, but they are wrongly attributed (the ISC only estimates locations, not mechanisms, so these mechanisms must be sourced from elsewhere). The depth of these events is not discussed (i.e. are they within the deformed sedimentary sequence, or the underlying basement?). I think the authors would benefit from searching the literature for well-constrained locations, mechanisms, and depths for earthquakes in this region, and discussing the relationship between the geometry of the active faulting and the structural models they propose. In addition, Ambraseys and Bilham (Bulletin of the Seismological Society of America, Vol. 93, p. 1573–1605, 2003) contains much useful information on the historical seismicity, including the 1931 event close to the Krithar range-front, which will have important implications for the kinematics of the shortening. Some of the arguments based upon the dip of the faulting (e.g. that thrust faults can't form at dips of 45 degrees; section 5.1) are incorrect, based on observations of faults this steep being newly-formed in oceanic outer rises (e.g. Craig et al, EPSL, 392, 94-99, 2014).

3. Little detail is given of the structural reconstructions (Figs 12-15). For example, what is the justification behind each step in the reconstruction, and how many other interpretations are possible which match the observations? The authors acknowledge that the solution is not unique, but I have little feel for how many different configurations are possible, why these models were chosen, and how alternative models would affect their conclusions. I think these issues need to be discussed in detail (particularly the

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final one), and I think that for each stage in the reconstructions a reason should be given for why that deformation has been chosen (e.g. in order to match feature X, we now need to undertake deformation Y).

4. Although I can see why the authors have suggested a combination of thick- and thin-skinned deformation in this region, it's not clear to me why this definitely needs to be the case, rather than just one of a range of possibilities. The pattern of folding is described as being analogous to an array of normal faults, but I don't see why this necessarily needs to be the case – the folding looks fairly similar to that in the Zagros mountains, where it is thought that the folds are decoupled from the underlying basement by the Hormuz salt. The second paragraph of section 5 simply states their view, without justifying it. For example, how have they ruled out the possibility of more thickening in the deeper parts of the sedimentary layer in the western parts of the section giving the change in structural level? Given the thickness of the sediments, this seems equally plausible? If the authors are going to pick a preferred viewpoint, I think they need to give a detailed justification.

5. In general, I think the manuscript would benefit from many of the statements being backed up with observations and/or reasoning. For example, in section 4 the lateral thickness change in the Ghazij Shales is stated. However, we are not told what information this was based on (i.e. where are the well or surface observations, or how is the top and bottom of this unit recognised in the seismic data). This thickness change is key to their suggestion of the reactivation of normal faults. There are many statements like this in the text, which leave the reader wondering what the conclusion is based upon. I think it would be very helpful to the reader if the authors provided supporting logic or observations of all statements they make.

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