Reply to RC2 by Referee #2 Jonas Kley

We thank the reviewer for the effort of the thorough review of the manuscript. We think that our revisions based on the comments significantly improved the revised manuscript.

Our responses to the specific comments of RC2:

1) RC2

Page C2 Paragraph 1): "In the way of data, the weakest part of the paper is definitely the claim that reactivated normal faults are involved in the deep structure. The seismic profiles do not reach deep enough to show anything conclusive. The earthquake nodal planes except one at 57◦ dip too gently to satisfy the Coulomb-Mohr prediction for normal faults. In fact, the average (arithmetic mean) dip angle of the west-dipping nodal planes is only 38◦ , much closer to an ideal Mohr-Coulomb thrust fault than normal fault. The normal faults of the structural model dip around 50◦ . Judging from the stratigraphic description and the authors' comments, the timing of active rifting isn't very well constrained, either. The same seems to hold true for the depth to and nature of the basement. I therefore recommend to tone down the inversion-related part of the interpretation while maintaining that the basement must be involved in thrusting".

Response:

The interpretation that the required basement involvement in the deformation is related to inversion is not only based on the fact that some of the nodal planes are too steep to represent newly initiated faults but on a combined series of observations. We will elaborated this below and have changed Section 5 accordingly.

Jonas Kley suggest that the average of the west-dipping nodal planes (38°) is close to the ideal angle for Mohr-Coulomb faults. We consider that taking an average is not suitable for this discussion. In our manuscript and in our structural model we suggest that only the steeper events are considered to represent inverting normal faults. In our model there are also newly initiated thrust faults (flats and ramps) in the sedimentary succession, which would also contribute to the events recorded. An average value thus is not indicative.

On page 9 line 25 of RC2 supplement it is commented that steeper faults are also "too gently dipping to represent normal faults".

We do not fully agree to this point. The angles are below the 60° usually assumed for the formation of normal faults. The listric nature of extensional faults in rift systems can lead to rotation of faults in the hanging wall, making them more suitable for reactivation (as proposed for example by Jackson 1980). Direct fault inversion indeed is a complex matter. Fault angles, partly rotated through the extensional history, as wells fluid pressures on inversion may play a role (Sibson, 1995). It is beyond the scope of this manuscript to discuss all the potential parameters - instead, we changed the text to clarify, that we consider the presence of inversion as a most likely scenario based on a line of constraints (reworked Section 5). We clarify that we do not only consider (partly) fault inversion, but also other deformation structures often associated with inversion, where faults are not suitable for direct inversion (e.g. hanging wall shortcuts, buttressing effects, back-thrusts, cf. Cooper et al., 1989; Hayward and Graham, 1989).

We expanded Section 5.1 to explain the regional constraints which we interpret as being indicative of inversion rather than non-inversion basement thrusting. The main argument is the orientation of the fold axes west of the Kirthar Escarpment (NNW-SSE to N-S) which does not fit well with shortening one would expect from transpression with the main left lateral strike-slip faults striking N-S to NNE to SSW.

With all the clarifications given above (and implemented in the MS) we think it is justified to keep our interpretation that the deformation in the study area is related to inversion.

Nevertheless, we clearly state that this model remains an indirect conclusion on several observations and interpretations, rather than a direct observation (6.5 Uncertainty).

Changes on the MS:
Section 5 and subsection have been re-ordered and extended to improve the line of argumentation based on all constraints. This includes addressing the following main questions: a) could a pure thin-skinned (duplex) solution explain the same (regional) pattern? (requested by RC1) b) Which are the indications for inversion in contrast to a (non-inversion) basement involved model?

All constraints from geological maps (Fig. 5 and reworked Fig. 2) and the new regional section (new Fig. 16g) are elaborated in subsection 5.1 “Constraints from regional structures”. The former sub-section “Deformation pattern west of the Kirthar Escarpment is included here). After this, the constraints from the focal mechanisms are elaborated (Section 5.2.). To the sub-section 5.3 “A simplified thick-skinned - thin-skinned inversion model” we added that we consider also other inversion features and a higher complexity (Cooper et al., 1989; Hayward and Graham, 1989). See text as in the track-changes manuscript file.

Section 6.5 Uncertainty, added: “The inversion model is very likely, but remains a conclusion, rather than a direct observation”.

2) RC2
Page C2 Paragraph 2): “When the authors compare their new structural models to Banks and Warburtons passive-roof duplex interpretation they should at least briefly discuss what happens in the more internal parts of the belt, away from the deformation front. The passive roof model was motivated by the need to explain gently folded strata raised well above the regional level for a considerable across-strike distance. I assume that this problem also applies to the central Kirthar Range. If the Kirthar Range is held up by a series of reactivated normal faults, where is the reverse displacement of the more internal faults accommodated that cannot be transferred to the thin-skinned front? Or, in other words, is there enough shortening in the internal Kirthar Range to support its topographic and structural elevation assuming that the basal detachment is in the basement?”

Response:
The question raised is interesting, but difficult to answer. We do not have enough geological data to confidently constrain the amount of shortening all across the lateral mountain belt towards the strike slip faults at the plate boundary. West of our core study area we do not have a high resolution geological map, no bedding dip information and no thickness control on the stratigraphy etc. However we address this question as best as possible in Section 5.1, making use of the new regional sketch section Fig. 16 g (as recommended by the referee, see Point 5 below) as well as a small sketch to explain the consequences of thick-skinned/thin-skinned shortening (Fig. 17).

The regional sketch section indicates a deformation style with some inversion and distributed ductile deformation (including wedging/LPS), similar to what we observe in the study area in the front of the section. The amount of shortening in the gently folded strata west of our study area has only about 10% shortening (very rough estimated as the section is partly oblique to some of the fold axes. Furthermore, there are no plane strain conditions in a regional section in a transpression zone). Due to the poor input data, we assume that the actual shortening is in the order of 15% if one would include the non-resolved deformation (wedging/LPS, our sections show 18-20% shortening). We consider that this amount of shortening can be locally accommodated and does not need to propagate to the deformation front – However, with the present data at hand, we cannot define this any better. With 15% of shortening a thick-skinned solution for the structural elevation west of the Kirthar Escarpment can be justified. A thin-skinned solution can be ruled out.

Changes on the MS:
Section 5.1: Description of the added regional sketch cross-section (Fig. 16g as well as a geological map background in Fig. 2). The regional shortening and how that can be accommodated are discussed (using new Figure 17). See track-changes MS.

3) RC2
Page C2 Paragraph 3 "I am not entirely convinced by the uniqueness of the sequence of thrusting derived in Fig. 9. The advance of a thrust wedge between thrust 1 and backthrust 2 would result in kink band migration and not "progressive limb rotation" as described by the authors (l. 22 in text) and actually suggested by the growth strata geometries. It is also interesting that the kink axis shown to be associated with the tip of the wedge at deeper levels appears offset to the west in the growth strata, but also in the syncline suggested below thrust 1. I could imagine a scenario with no bedding-parallel backthrust and thrust 1 as a late subhorizontal structure displacing the syncline axis towards the east. The implication would be that there must be another thin-skinned thrust further east.

Response:
Indeed, thrust wedge advance of thrust 2 would lead to kink band migration. The observed strata indicates rather limb rotation (although small internal unconformities in the growth strata could mask a higher complexity. The suggested order of deformation by the Referee can be excluded, as there is no additional thin-skinned deformation east of the tip of the wedge (not in the confidential seismic data and there are no indications on surface geology etc.). However, the referee accurately observed the presence of another, deeper kink, offset in respect to the kink in our original Figure 9. We include this to reinvestigate the potential kinematics. Figure 9 has been extended to show a possible evolution of the frontal system. The forelimb of the anticline apparently stays relative stationary due to the stacking of two wedges. Therefore, the growth strata would build two small stacked wedges with kink-band migration that appear similar to limb rotation in the image. This is not necessarily the only explanation and kinematical order, but one in-line with the observations.

Changes on the MS:
Figure 9 has been extended.

In section 4.1 the new Figure 9 and the suggested evolution is described. The caption has been adapted accordingly (see track-changes MS).

4) RC2
Page C3 Paragraph 2 upper part "One thing I am deeply skeptical about is the landslide interpreted in Fig. 6 b. The way this feature is described in the caption I gather that it is supposed to have formed by draping over the topography of the steep forelimb (or did I get that wrong?). I find it hard to believe that you could form the orderly anticline depicted in the satellite image from a rock mass sliding over an irregular land surface."

Response:
The original explanation in the text of Section 3.1 for Fig. 6b is probably too short to reasonably explain this feature. The explanation has been extended. A model is proposed based on similar features known from the Zagros.

Changes on the MS:
We adapted the text and added the reference to similar features in Iran (Harrison and Falcon, 1934, 1936); see track-changes MS. Furthermore, we added a supplementary figure to illustrate a potential evolution of the slide (into supplement, because we consider this not of key importance for the manuscript and the conclusions).

5) RC2

Page C3 Paragraph 2 lower part: "I think that the paper would strongly benefit from a few additional figures. First, it would help the imagination to have a regional cross-section reaching west to the strike-slip system. Secondly, I strongly recommend to prepare a synthetic figure that combines the new cross-sections with those from published studies whose locations are shown in Fig. 1, preferrably redrawn such that comparison is made easy. Nobody wants to look up four other papers to see what the paper they are presently reading is talking about"

Response

Agreed. We prepared an additional figure (Figure 16, note figure numbers will be resorted in the final revised MS), which shows redrafted sections studies mentioned in Section 1. Additionally we added a tentative regional sketch section (Fig. 16g) which includes in the frontal part the simplified version of Fig. 14a, the final section of the northern sector. The original sections (Fig. 12 and Fig. 14) are in a smaller scale, thus showing more details and therefore should remain as they are. The simplified version in Figure 16g should serve the requested purpose to be able to compare scale and style of our results to the published sections.

Changes on the MS:

Added Figure 16 which includes both sections from published studies and a regional section including study results. The background in Fig. 2 has been replaced to show the geological map the regional section is based on. The description of the regional section is now part of the extended Section 5 (as already mentioned above), see track changes MS.

Comments on figure pages in se-2018-137-RC2-supplement.pdf

Referee 2 (Jonas Kley) left some comments and text correction suggestions in two supplementary PDFs. As stated in se-2018-137-RC3.pdf the first supplementary PDF (se-2018-137-RC2-supplement.pdf) contains uncommented mark-ups which are removed or commented in the file se-2018-137-RC3-supplement.pdf. Comments to the figures are only in the file se-2018-137-RC2-supplement.pdf, which will be answered below.

An additional reply will be given for RC3 containing the responses to the comments in the to the manuscript text in file se-2018-137-RC3-supplement.pdf

se-2018-137-RC2-supplement.pdf Page 24, line 4.

"What is actually visible is a strip of emergent land, presumably the Kohistan-Ladakh arc. It would be nice to add to this figure the locations and kinematics of plate boundaries"

Response and Change on MS:

We changed the text in the caption accordingly. Adding locations and kinematics to this figure would indeed be a nice addition but we do not have the database. Researching this seems out-of scope for the purpose of this paper.

se-2018-137-RC2-supplement.pdf Page 25

Explain Kirthar Escarpment

Response and Change on MS:

Added to the caption

se-2018-137-RC2-supplement.pdf Page 31

Several small suggestion to improve Figure 11

Response and Change on MS:

The suggestions have been accepted and are implemented in the reworked Figure 11

se-2018-137-RC2-supplement.pdf Page 32

"Why have you chosen to ignore seismics here?"

Response

The seismic in the background is a combination of a 2D line (the western line) and a section from a 3D seismic (as described in caption to Fig. 8). The 2D line shows on both ends acquisition and processing artefacts especially at greater depth with reflectors curving up. The 3D data shows sub-horizontal reflectors at depth. Therefore, the deepest (dipping) reflectors on the eastern end of the 2D seismic line have been considered unreliable and have been ignored.

se-2018-137-RC2-supplement.pdf Page 34

Mismatch between seismic and dip measurements (Fig. 14)

Response and Change on MS:

The dip measurements in the northern sector of the study area were collected by different campaigns before the acquisition of the seismic. Thus, they are not located on the seismic trace and needed projection between 2.5 and 4 km. Additionally, some locations of the older measurements in the northern sector might not be exact (without GPS).

The projection distance has been added to the caption of Fig. 14

References not listen in the revised MS:

Sibson, R. H., Selective fault reactivation during basin inversion: potential for fluid redistribution through fault-valve action, Geol. Soc. Spec. Publ., 88, 3-19, 1995