

Reply to RC1 by Anonymous Referee #1

We thank the anonymous Referee for reviewing the manuscript. Most of the comments ask for clarifications of interpretations and assumptions. These comments give us the opportunity to substantially improve the readability of the manuscript. Some comments we do not fully agree or we think they are not relevant. All points raised will be discussed below.

1a) RC1:

“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;”

Response:

a) Figure 8 was composed in a way that as little of the seismic is covered by interpretation and labelling so the reader can get an impression on the (often poor) quality of the seismic image. We followed the example given by other papers in Solid Earth using nearly un-interpreted seismic with some labelling to highlight features discussed in the text (e.g. Malehmir et al. 2018, Tavani et al., 2018, Gallastegui et al. 2016). The interpreted version of the seismic is given in Figures 12a and 14a where one can see the complete interpretation (the semi-transparent seismic in the background). We added cross-references between the figures to clarify this, thus enabling the reader to compare the final structural interpretation and original seismic. Because of the different length of the sections (Seismic in Fig 8 and the final, extended sections in Figs 12a, 14a), it seems unreasonable to combine the figures. Instead, in order to provide an unbiased documentation on the database, we added a supplementary figure that shows the seismic of Figure 8 in high resolution without any annotations at all.

Changes to the MS

Supplementary high resolution figure with blank seismic (Supplementary Figure 2).

Improved cross-references between figure captions (Figs. 12a and 14a to Fig. 8) and text (see track changes MS).

1b) RC1:

“b) provided zooms of the key features discussed in the text;”

Response:

We do not agree that one would be able to see more details in zooms of the seismic. The size of Fig. 8 is limited by the column width of the journal but it already resolves all the features visible in the seismic which are referred to in the text (as they are indicated by small letters). The PDF version of the MS for review might be lacking the quality of the figure expected. We think by providing a large scale high resolution version of the blank seismic (Supplementary Figure 2) we sufficiently provide documentation of the data our interpretation is based on. The seismic won't get better in zoom figures. We will make also sure, that the final submitted version of Fig. 8 will have as high quality as reasonably possible.

Changes to the MS

Providing a high resolution blank seismic of Figure 8 in the supplementary material (Supplementary Figure 2).

A high resolution blank seismic of Figure 9 (Supplementary Figure 3) will be provided as well.

1c) RC1:

“c) marked on the locations of the wells that seem to be key to the correct identification of some horizons”

Response:

This comment likely asks for the exact location of the wells on the seismic?

For confidentiality reasons we cannot give exact well locations. But we edited the text and figures to improve the MS accordingly.

Changes to the MS

We improved the description of the stratigraphic control and well locations in the text (Section 4). We added seismic horizon labelling to Figure 8 with indications in the stratigraphic column (Fig. 3). Better explanation of well control location in Figure 8. Please see track changes MS.

1d) RC1:

“(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.

Response:

The horizon interpretation is done based on the well control and seismic facies. As mentioned above seismic horizons as depicted from the well control have been added to Fig. 8 and are explained in Fig. 3. Additionally the seismic characteristics of the Kirthar limestone pick is now given in Section 4.

For the fault interpretation in Figure 9 we added also the non-interpreted seismic as supplementary figure. In addition, a new figure with a kinematic scheme is provided and explained, highlighting the rationale behind the interpretation (this is also part of the answer to RC2).

We think that an interpretation like Fault 1 does not require a detailed justification. Tilting and uplifting strata from a horizontal position the way it is imaged requires a fault. Such basic interpretations follow well known structural concepts, cf. AAPG Atlas of Shaw et al., 2005). The fault 2 (roof thrust) is a geometrical necessity if no further deformation occurs to the east. This rationale is now better explained by the new figure and the description of the missing deformation towards the east in the revised MS.

Changes to the MS

Additions to section 4: Stratigraphic control by wells, seismic grids and seismic characteristics of Kirthar pick.

Additions of the picked horizons to stratigraphic column (Fig. 3) and the eastern part of the seismic lines (Fig 8).

Improved Figure 9 with a kinematic scheme and rewording of the regarding text in Section 4.1.

Supplement Fig. 3: Seismic of Fig. 9 without interpretation

2) RC1:

“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).”

Response

Focal mechanisms were downloaded from ISC database (<http://www.isc.ac.uk/iscbulletin/search/fmechanisms/>). We followed the citation scheme proposed on the ISC webpage. The contributing agencies to the ISC database that we used were actually listed in Table 1 in the author column. See also Lentas et al. (2018, "The ISC Bulletin as a comprehensive source of earthquake source mechanisms")

Changes to the MS

We clarified the contribution of other agencies to the ISC database and added the reference "Lentas et al. (2018)" to the caption of Table 1

2 contd.) RC1:

"The depth of these events is not discussed (i.e. are they within the deformed sedimentary sequence, or the underlying basement?)".

Response:

This is a good point that is missing in the manuscript. We were aware of the depth and considered the depth of the events when interpreting the data (as "Z" is listed in Table 1).

Changes to the MS

We added a consideration of the depth of the events to Section 5.2 (former 5.1). See track changes document.

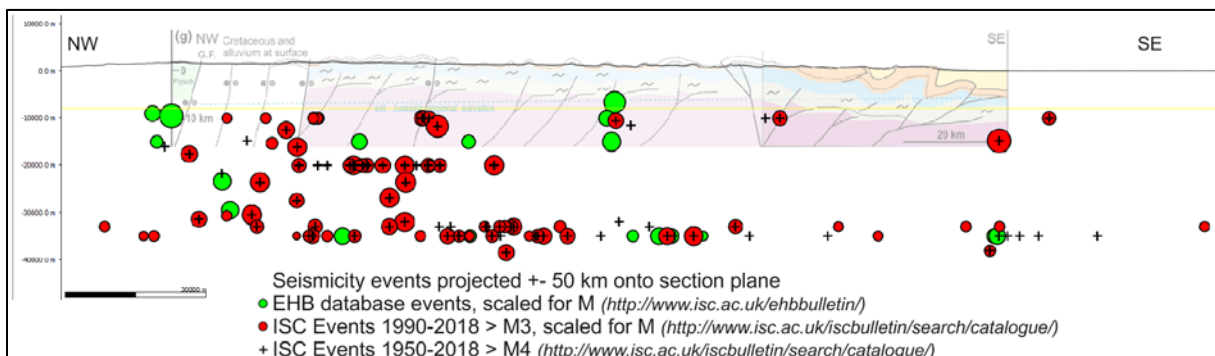
2 contd.) RC1:

"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"

Response

We struggle to find significant other sources of focal mechanisms relevant to our study area. We consider the ISC Bulletin database with approx. 150 contributing agencies to be the most relevant data source for this study. A dedicated seismological study for the study area would certainly help, but is beyond the scope of this study.

We are of the opinion, that the distribution of seismic events (i.e. the depth distribution) does not add value to the discussion about fault geometries and kinematics. The figure below shows recorded seismic events projected (± 50 km perpendicular to the section plane) on the extended section of the new regional section (Fig. 16g). Data from the ISC bulletin partly line up in certain depth which could be related to poor depth location/artefacts. Improved locations of the EHB database (using algorithms after Engdahl et al. 1998) do not improve the picture, but actually show that the data in general has a large scatter.



The paper by Ambraseys and Bilham (2003) does not provide focal mechanisms but, as suggested by the reviewer, a discussion on fault kinematics on the 1931 Mach Event (based mainly on levelling data). Using the same levelling data the potential fault shape and kinematic is evaluated also in Szeliga et al. (2009). Those authors show focal mechanisms on their Fig. 1 after Harvard MCT, a source that is included in the ISC Bulletin database that we used for our Fig. 2.

We follow the suggestion by the referee and introduce the work done on the Mach 1931 Earthquake in Section 5.2. The geological section of Szeliga et al. (2009) with the approx. fault shape of the Mach 1931 event has been added as Figure (Fig. 16d). The fault shape considered by Szeliga et al. 2009 is similar to the frontal fault system in our study area. The consequences are also now discussed in Section 6.2.

Changes to the MS

We added the citations and a description of the results of Ambraseys and Bilham (2003) and Szeliga et al. (2009) to Section 5.2 (former 5.1). The geological section given in Szeliga et al. 2009 has been added to a new Figure 16 (Figure numbers will be resorted in final revision), which shows a compilation of sections (in response to RC2). The fault responsible for the 1931 Mach Event as suggested by Szeliga et al. 2009 is also shown in Figure 16d. The consequences /relationship of the deformation is additionally discussed in Section 6.2. See track changes document.

2 contd.) RC1:

“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),”.

Response

It has not been stated in the reviewed MS that thrusts cannot form at dips of 45°. We say, we consider the steeper faults (i.e. 45° and more) too steep to represent newly initiated faults. That is not exactly the same. The reason, why we think they are not newly initiated faults is based on a line of arguments, which includes fault and fold orientation in respect to the plate kinematic direction. Section 5 and sub-sections 5.1-5.3 have been extended to clarify the reason for our interpretation of inversion (also in respect to RC2)

We consider that the paper by Craig et al. (2014), suggested by the Referee 1, is not relevant in the discussion whether thrust faults can form at higher angles or not. In the paper Craig et al. argue that they observe normal faults forming not at an ideal angle of 60° but cluster around 45° (in oceanic crust). In order for that to happen, they imply that the rocks must have a lower than usual coefficient of friction. They speculate that the suspected low coefficient of friction is a result of hydrothermal alteration of the oceanic crust after it formed at the MOR. Craig et al. also plot histograms of nodal planes from thrust faults which show clusters above and below 45°. However, the histogram shows both nodal planes, so the cluster on the higher angles could represent the auxiliary plane – or reactivated faults. Craig et al. do not suggest that thrust faults form above a certain angle and they explicitly do not analyse the thrust faults (Craig et al. (2014): “*The population of thrust-faulting earthquakes (Fig.3C) is too small for any clear trends to emerge, and is not the subject of further analysis in this study*”). Consequently, their results are only valid for normal faults. Furthermore, the line of arguments that is used for normal faults forming a lower angles seem not to fit for thrust faults forming at steeper angles than usual. If the coefficient of friction is lower than for standard Andersonian faults (i.e. lowered by a hydrothermal processes), thrust faults would form at lower angles, not higher. For thrust/reverse faults to form at higher angles the coefficient of friction would need to be higher than the normally considered used value (i.e. 0.6 for 30° thrust fault, all needed references are given in Craig et al., 2014). Consequently,

we interpret reverse faults with dips at 45° or above indicate rather frictionally reactivated faults than newly initiated faults.

Changes to the MS:

Section 5.2 (former section 5.1): “We interpret these steep faults therefore as parts of pre-existing faults that are in a suitable angle for reactivation”.

A suggestion on partial fault reactivation and other inversion related deformation in Section 5.3. (marked as reply to RC2). See track changes document.

3) RC1:

“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 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)”.

Response

Strictly speaking Figures 13 and 15 do not show reconstructions but simplified kinematical forward models. Reconstructions are the restored sections (Fig. 12b and 14b). The techniques how Figs. 12b and 14b are restored are defined in the text (Section 5.4, last paragraph) and follow established procedures (e.g. Woodward et al., 1989). The simplified kinematical forward models in Figs. 13 and 15 are suggestions that show that the restored section also is meaningful in a kinematical sense. The necessity to link restored and present day stages are the main constraints. We clarified this relation in the revision. Some additional reasoning for the individual steps have been added as well.

The question on “how many different configurations are possible, why these models were chosen, and how alternative models would affect their conclusions” is not simple to answer. We follow in the MS the approach of using as many constraints as possible (surface geology, seismic, well data, regional setting, the nodal plane geometries, balancing constraints and regional elevation considerations etc.) and combining them in a logical way in order to shrink the amount of admissible solutions. How much change on our solution is a new solution or just an adaption in the frame of the given solution is a matter of definition and also scale dependent. We consider that the involvement of basement deformation can be considered as certain. That these are likely inverting normal faults (or part of it) is considered as very likely, based on a thread of arguments (which is now elaborated more clearly in Section 5.1. -also in respect to the comments by Referee 2). If these faults are of original Triassic or Jurassic age remains relatively uncertain. The same applies for the amount and exact shape of faults in the subsurface. Our main conclusion is based on the solution which we consider almost certain. It would require some very good ideas to combine all the constraints and come up with a solution that is different from being just a modification of our model. However, learning about potential other solutions has not only a scientific but also a business impact, so we encourage substantial alternative explanations that contradicts our main conclusions. By documenting the database as good as confidentiality allows, we hope to serve this purpose.

Changes to the MS:

The mentioning of the balanced sections and restorations (Figs. 12 and 14) moved up in the text (now in Section 5. Second paragraph). By this we can refer to the restored and balanced section as start and finite stage respectively when explaining the kinematic models (Fig. 13 and 15), improving the context/readability. Additionally some improved reasoning for the chosen steps, as suggested by the Referee1, have been added (see track changes MS).

To section 6.5 we added: “How much change on our model is a new solution or just a modification is a matter of definition and also scale dependent.”

4) RC1:

“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.”

Response:

“The second paragraph of section 5 simply states their view, without justifying it.”

Manuscript: “We suggest that the order of structural uplift (larger than 5500 m) is linked to a deeper structural level in the basement.” The order of uplift is used as an temporary justification. The sentence is part of a paragraph that is a header for the complete Section 5 (including subsections) in which the reasoning for thick-skinned contribution is elaborated and more reasoning/justification is given. We improved the wording to make this clear. Subsection 5.1 in combination with a new Figure 17 now addresses the question why a thin-skinned solution (thickening in deeper parts) is unlikely. This builds up on a newly added regional section (Fig. 16g) plus an overview geological map (new background in Fig. 2) which has been added as part of the response to RC2. The main reason is that a duplexes would cause severe balancing issues and are also not likely in the transpressional setting that does not seem to work like a classical accretionary wedge (details in the Track changes MS).

“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”-

When comparing these regions we probably need to limit the similarity of the complex fold pattern and double plunging folds to the southern/southeastern Fars Arch of the Zagros, where Hormuz salt is present as detachment (cf. Bahroudi and Koyi, 2003). The deformation style in the Simply Folded Belt along strike the Zagros is not everywhere the same (cf. Allen and Talebian 2011). Nevertheless, the main differences are: in the southern Fars area most of the folds return to regional elevation (or close to it) in the trailing synclines (as evident on sections and geological maps, e.g. Jahani et al., 2009). The folds are large scale detachment folds influenced by the halokinetic evolution (requiring diapirs) since the Paleozoic (e.g. Jahani et al. 2009, Callot et al. 2012). The double plunging fold shapes in the SE Fars are a result of complex interaction of halokinetic induced stratigraphic thickness variations and shortening on a salt detachment with a likely not planar detachment plane. The role of basement involvement in the deformation in the Fars has been proposed (e.g. Jackson 1980) and is debated since. For the SE Fars region a strong gain of structural elevation is evident only towards the hinterland in the imbricate zone (behind the High Zagros Fault cf. Fig. 2 of Mouthereau et al, 2007, or Fig. 2 of Bahroudi and Koyi, 2003). This faults thus likely marks a stepping down of the detachment into the basement.

In the Kirthar Fold belt there is no evidence of salt presence in the stratigraphy and no evidence for salt tectonics. The second most important difference is that all rocks west of the Kirthar Escarpment are significantly elevated above their regional elevation of the undeformed foreland (more than 6000 m).

Although the comparison to the Fars Arch might be interesting, we do not think that it would improve the MS.

Changes to the MS:

Added Figure 16g (regional section), Figure 16h (average topographic profile along 16g) and new background in Fig. 2 (overview geological map) also as reply to RC2.

Added Figure 17 schematic scheme for discussion of structural elevation uplift

Section 5.1 now addresses the question why a thin-skinned solution is less likely to explain the structural elevation gain towards the west.

Section 5.1 also includes a discussion why we consider a thick-skinned (not inversion related) deformation less likely also (in response to RC2). See track changes document.

5) RC1:

"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."

Response:

We do not agree that we are using un-backed up statements in the MS. Unfortunately, there is only one example given in RC1, which we think is not fully adequate: RC1: "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)". The manuscript reads in Section 4.1, page 8 Line 3-5: "*The low-reflectivity seismic facies below the Kirthar limestones are Eocene Ghazij Shales (Fig. 8a, point g). These shales thicken dramatically from wells in the East (several tens of meters) towards the West (several hundreds of meters, constrained by outcrop and seismic velocity data)*". We actually would consider this as an explanation and not an un-backed up statement. The location of the gas condensate fields on the frontal anticline has been described in Section 4. The relative reference where the location is on the section was given). The outcrop situation of the Ghazij can be checked on Figure 5 and has been described in Section 3.1. However, we admit, that this might not be easy for the reader to follow. So, for convenience we improved the description of this thickness increase and the observations where it is based on. Well control and seismic interpretation description has been improved (as described in points 1c and 1d of this reply) RC1:" This thickness change is key to their suggestion of the reactivation of normal faults." This is actually not the case. The thickness variations in the Ghazij Formation is nowhere used in the MS as argument for normal fault reactivation.

A thick Ghazij Formation, however, is considered as suitable roof thrust. The suitability as weak layer has been already demonstrated in Section 3.1/Fig. 6.

The changes in respect to the other comments from RC1 (above) and also in respect to RC2 should have significantly improved the MS and allow the reader to follow our interpretations and conclusions (without "non-back-upped statements").

Changes to the MS:

Section 2 Thickness trend description of Ghazij shales in Tectonostratigraphic evolution

Section 3.1.: These shales reach several hundred meters of thickness east of the Kirthar Escarpment.

Section 4: improved description on well control and stratigraphic interpretation (including annotations of stratigraphy in Fig. 8)

Section 4.1 Changed the description of the thickening shales to: "These shales thicken dramatically from the wells on the frontal anticline in the East (several tens of meters) towards the West (several hundreds of meters, constrained by seismic velocities and outcrop information just west of the Kirthar Escarpment, cf. Fig. 5 and Ahmad et al. 2012)".

References used in this reply not present in the reference list of the revised manuscript:

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