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Solid Earth Discussions

Interactive comment on "Spatial evolution of Zagros collision zone in Kurdistan – NW Iran, constraints for Arabia–Eurasia oblique convergence" *by* S. Sadeghi and A. Yassaghi

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The manuscript "Spatial evolution of Zagros collision zone in Kurdistan – NW Iran, constraints for Arabia–Eurasia oblique convergence" of S. Sadeghi and A. Yassaghi reports structural data from the Arabia-Eurasia suture zone, with the goal of deciphering the long-term kinematic history of this highly deformed region, which resulted from a combination of obduction and collisional processes. I think that this kind of studies are really needed and may help improving our understanding on the spatio-temporal evolution not only of the Zagros Orogeny, but more in general of tectonically active margins. I therefore think that the manuscript could be of interest to a broad audience but at this

stage I think that it will need some additional work. In particular I think that:

1) The concept of strain partitioning should clarified (please see my comment on Page 2736, lines 10-11), because it sounds different from that one commonly used in the literature (at least to me).

2) The relative chronology of dip and oblique slip in the Radiolarite zone with respect to the two dominant deformation processes (obduction and collision) are poorly constrained (please see my comment on Page 2746, line 14)

3) The way the balanced cross section was constructed needs to be better explained (e.g., how did you get the length of the Radiolarite Basin; pleasesee comment on Page 2744 on lines 25-26 and following page). The implications of that section should also be discussed; for example what can we learn from those numbers?

4) A broader vision of the topic is missing; including it could lead to a more solid manuscript, which may be cited by other people working in different mountain belts; in other words what can we learn from your study area that could be exported in other regions?

5) The language needs to be really improved.

Specific comments

Title: You address both spatio and temporal evolution but the title you report only "Spatial". The other point is that I will replace collision zone with suture zone, because collision would imply a larger area (basically from the Caucasus to the Zagros)

Page 2736, lines 10-11 What do you mean with partitioning? To me oblique slip on a fault is not partitioning; strain partitioning occurs when oblique shortening is accommodated through dip and strike slip either on the same fault or on adjacent faults. Please have a look at the paper of Jackson et al., (Geoph. J. Int., 2002).

Page 2736, lines 18-19 According to plate tectonic reconstructions (McQuarrie et al.,

Geoph. Res. Letter, 2003) from at least ca. 56 to 20 Ma, plate convergence vectors should have been perpendicular to the plate margin (you may also check out the recent update published in McQuarrie and Van Hinsbergen, Geology 2013). Please verify it.

Pages 2736, (line 25) and 2736 (lines 1-2) You may be more precise about the supposed timing of the continental collision (even reporting different opinions). Cenozoic is a long period (65 Myr) and I guess that the first half of it can be easily excluded; about the citations, a lot of work has been done from different authors in the last years; for example N. McQuarrie has reviewed her previous work publishing an updated version in 2013 (McQuarrie and Van Hinsbergen, Geology 2013).

Page 2738, lines 12-14 It is fine to use new terms but they should not confuse readers; for example the term collision zone is generally thought to include the entire zone from the Caucasus to the suture or the Zagros Fold and Thrust Belt; I would suggest to use a different name for the Sanandaj-Sirjan, Gaveh rud etc... zone (for example some authors used the name "crush zone", but I am not sure if it applies here).

Page 2738, lines 15-17 I suppose that with "accumulated" you imply "tectonic emplacement" Please verify it. As it is one could interpret it in terms of sedimentary processes (i.e, sediments accumulation)

Page 2738, lines 19-23 In which kind of sedimentary basin, the Gaveh Rud unit is thought to be deposited? What is the depositional age of these volcaniclastic deposits? It may be good to mention briefly for readers that are not familiar with the geology of the Zagros. I would also refer to the paper of Agard et al., (International Journal of Earth Science, 2005

Page 2738, lines 19-23 Not clear; as it is it seems that the Sanandaj-Sirjan zone accommodated significant Cenozoic collisional deformation. Is that the case? I also doubt that we could call the Sanandaj Sirjan zone the metamorphic core of the Zagros; if you look at mountain belts like the Alps where you have metamorphic rocks in the axial zone, you will see that these rocks experienced significant exhumation during orogenic

processes (most of them experienced HP or ultra HP metamorphism). Exhumation in the Sanadaj Sirjan zone does not seem to be the case, and the age of metamorphism appears to be pre Jurassic or so, except few localized area where core complexes are thought to have developed during the Cenozoic, most likely before continental collision (and in that case rock cooling and uplift will be associated with tectonic denudation with little contribution of erosional exhumation; see Verdel et al., GSA Bull 2007).

Page 2739, lines 3-6 In which unit is the basal decollement and where are the intermediate decollements; It may be worth mentioning it

Page 2740, lines 25-27 I found this sentence a bit disconnected, especially in a section entitled "Stratigrahy of the Zagros collision zone"; please explain it better or move it for this section. Same for the figue.

Page 2744 (lines 25-26) and page 2745 (lines 1-3) How did you estimate the distance between L2 and L3, which is basically the length of the radiolarite basin? That estimate will have a profound impact on your total shortening. As you wrote above the line-length technique cannot be applied there, probably because you miss cutoff angles in footwall and hanging wall. Please provide an explanation for that estimation.

Page 2745, lines 23-25 Did we have subduction until late Miocene? I think that more recent publications pointed out that subduction should have lasted until Early Miocene (20 Ma; and possibly earlier). You may check out more recent publication of the three authors you mention here (among others) like: Allen and Armstrong, 3P 2008 Mouthereau et al., Tectonophysics 2012 McQuarrie and Von Hinsbergen Geology 2013

Page 2746, line 14 The Radiolarite zone has experienced Cretaceous (obduction related) and Tertiary (collision related) deformation. You nicely show that strike-slip faulting is younger than thrust faulting. However it is not clear to me if these thrust faults in the Radiolarite Zone accommodated only transpressional (oblique slip) or also dip slip (pure thrusting) deformation and when (Cretaceous or Tertiary)? For example, if you had only transpressional deformation in the Cretaceous I would totally agree with

the oblique obduction. On the other hand, if you had both oblique and dip-slip, how can you say that oblique motion (transpressional deformation) has occurred only in the Cretaceous while pure thrusting must reflect a more recent regime (Tertiary)? For the moment I am sure if you are reporting convincing data that could help in establishing a chronology of dip and oblique slip. Please clarify it A way to distinguish the kinematic regime associated with the obduction from that one associated with collisional deformation, could be to look at deformation processes in the Gaveh Rud; this unit is younger than Cretaceous and hence should have recorded only collisional deformation.

Page 2746, line 15 My understanding of partitioning is different. Please see comments in the Abstract: strain partitioning occurs when oblique shortening is accommodated through dip and strike slip either on the same fault or on adjacent faults. Please have a look at the paper of Jackson et al., (Geoph. J. Int., 2002).

Page 2748, line 8 Why late stage of orogeny? Do you mean early stage?

Page 2748, line 12 You should check out plate tectonic reconstructions and include them in the discussion,

Page 2748, lines 16-18 This point is not really discussed in the manuscript; it is cited but not discussed. If you consider it as one of your major conclusions it should be documented before.

Page 2748, lines 19-21 Please see my comment above on chronology of faulting in the Radiolarite zone

Page 2749, lines 1-4 Please see my comment on shortening estimates.

Figure 2: What is the color for the Qom Formation? The scale in the middle of the figure does not look nice; why do not put it on a corner (for example the NW one?)

Figure 3 I would probably add the orientation of these stratigraphic Sections (SW-NE from left to right or?) Adding a fifth column on the left showing the stratigraphy of the Sanandaj-Sirjan Zone may be also useful. What is the meaning of the colors? Please

explain in a legend or in the figure caption.

Figure 7 I suppose that the 3 black great circles are fault planes with associated sense of motion. Do they represent the major fault zone (for example in different locations), or are they different planes? Please clarify. The read great circle represents the mean direction of the High Zagros fault zone in your study area? Please write it down in the figure. On how many measurements are based the arrows showing the fault kinematic of the red plain? It may be good to know it, to have the filling on the robustness of these kinematic data.

Figure 8 I am not sure if I understood correctly what you mean with "kinematically different domains". From what I can see on figure B you considered folds as result of shortening (dip or oblique slip?) and strike slip faults as result of a wrenching tectonic (pure strike slip?). Are these conclusions just based on the pattern of structures on map view or is part of your results? What is the sense of motion of those faults that do not have strike-slip symbols but are located next to your black arrows? Please clarify it.

Figure 9 What is the meaning of the green area? Different unit?

Figure 11 I am not sure if the stereoplot of figure A shows layer parallel shearing; the fold axis is perpendicular (trend of line NNE) to the shear plane (strike of plane WNW); that means that the shear planes are perpendicular to the fold limbs. Or? Moreover, in the text you write: “plunging inclined folds (Fig. 11b) and reclined folds (Fig. 11a) in the dextrally deformed domains and horizontal inclined folds (Fig. 11c) in reversely deformed domains.” I am wondering if the fold of figure C can be defined as "horizontal inclined" considering that the axial plane is dipping to the NE and is not vertical. To me it would be plunging inclined like figure B. In case you may argue that figure B is closer to "horizontal inclined" giving that the axial plane could dip ca. 80° to the NNE.

I hope that my comments will be of help. Good luck

Paolo Ballato

Interactive comment on Solid Earth Discuss., 7, 2735, 2015.