Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-106-AC1, 2020

Solid Earth
Discussions

Interactive comment on "Birth and closure of the Kallipetra Basin: Late Cretaceous reworking of the Jurassic Pelagonian – Axios-Vardar contact (Northern Greece)" *by* Lydia R. Bailey et al.

Lydia R. Bailey et al.

lydiabailey@email.arizona.edu

Received and published: 11 September 2020

We are grateful for the positive response and constructive comments provided by the anonymous referee. They raise 3 main points: (1) the focus of the study needs to be addressed more clearly in the introduction and in the discussion/conclusions; (2) our discussion and reasoning on the tilted thrust fault should be developed further; (3) the extensive discussion on the origin of fluids in the fault zone is not the topic of this study. These are addressed below, and specific comments to individual points of the manuscript are provided. We will upload a revised manuscript and edited/new figures shortly.

Anonymous Referee #1 Dear Editor, I have read with great interest the work of Bailey and coauthors regarding the Kallipetra Basin in N. Greece. The manuscript is well written and it presents new data and interpretations in connection to the geodynamics and the problems of ophiolite obduction in the Hellenides. The writing is clear and easy to follow. The authors have put a great effort to document the data and the field evidence related to this study and I have to admit that it is rare to see papers with such a level of detail when it comes to the primary data. However, I have a few comments related to the overall presentation and some of the conclusions of the study. The most important points that I can mention here are: 1) The focus of the study with respect to the general problematics of tectonic scenarios in the Hellenides must be addressed more clearly. This is because the study may look a bit "too regional" from the perspective of a researcher who is not familiar with Eastern Mediterranean geology.

We will alter the introduction of our manuscript to more clearly address the focus of our study with respect to the 'controversies' of the Hellenides, please see our more detailed response on this issue below, in the response to a comment on l.41 by the referee.

2) The authors present their view of the main contact being a tilted thrust towards the NNE. This is a very important point given the current discussion in the literature related to the ophiolite obduction problem (Pindos vs Vardar etc). I would thus suggest that the authors develop the discussion and explain their reasoning a bit in more detail.

We thank the reviewer for this suggestion. We will add a sketch for more clear presentation of our tilted thrust zone and discuss our reasoning in more detail in the manuscript.

3) The authors get into an extensive discussion about the importance of fluids vs viscous heating while at the same time they also admit that the evidence is not so clear. Since the discrimination of the additional source of heat which is required is not the topic of this study (and there has been no effort inquantify the arguments), I would suggest that the authors mention the possibilities andnot go in a specific discussion on the importance of a particular mechanism.

Our data document an inverted metamorphism below a shear zone. The heating that produced this metamorphism occurred during deformation and reset the FT ages, permitting us to date deformation. We acknowledge that physical models are needed to reproduce and quantify length and timing of the observed metamorphism. However, our field observations help to rule out some processes related to the different possible heat sources. We therefore think that the critical issue of heat source should remain in this study. However, as asked by the reviewer, will reduce the extensive discussion so the reader can simply recognize the presence of heat along the thrust zone without getting deterred from the main conclusions of our study.

The Specific comments follow below: l. 27: Please add “e.g.” in the reference list. There are numerous works to be cited here.

Done.

l. 28: Please define what is meant by “Internal” Hellenides, either by definition or by citation to the map.

We will refer to the map and edit the map (Fig. 1) accordingly.

l. 34: Please be more specific about the “Cretaceous Basin”. Does it have a name? Is there in a particular location that you refer to?

Here we refer to the Kallipetra Basin. The Cretaceous basins that formed at the eastern Pelagonian margin and over the Axios/Vardar zone were first mapped at the large scale by Kossmat 1924. Many workers have found sparse Cretaceous sediments since then (e.g. Mercier and Vergely 1994, Sharp and Robertson 2006). Schenker et al (2015) brought clear evidence that the gneissic detritus in one of these basins (named in this contribution Kallipetra basin) is of Pelagonian and not of Rhodopian origin (e.g. Ricou and Godfriaux 1995). We will rephrase this part of the text: “...by the deposition of metamorphic Pelagonian detritus in a Late Cretaceous basin (Schenker et al. 2015) subsequently referred to as the Kallipetra Basin in this study.”

l. 37: I would suggest that “pulse” is not the right word here. It is known that the extension and basin formation in the Hellenides is diachronous and migrating southwards (see also Papanikolaou & Royden, 2007) for more details and the relevant literature.

We will rephrase to: ”Finally, from the Oligocene-Miocene the western Pelagonia was dissected by diachronous normal faults (Schermer et al., 1990; Lacassin et al., 2007; Coutand et al., 2014; Schenker et al., 2014) within a southward extensional deformation front that affected most of the Hellenides (e.g. Papanikolaou & Royden, 2007).”

l. 41 (MAIN POINT): It is not clear what are the main features that you would like to address in all these contrasting interpretations. In terms of the sketches that are presented in Fig. 2 the focus of this work can be i) the position of Pelagonia, ii) the number of subduction zones etc. Therefore, I suggest you develop on the specific features that you want to address in more detail. In other words, please identify the problem/hypothesis and then explain why you chose to focus on this area to solve it/test it.

The many and contrasting geodynamics models present in the literature source from the difficulties of connecting the Rhodope and the Pelagonian zone. This is a long-standing debate on the number and dimension of oceans in the Mesozoic Pindos-Vardar realm between researchers proposing a single unifying Early Jurassic Vardaric ocean that has been partly subducted, partly obducted and dismembered during successive tectonic events and researchers that embraced a multi-ocean early Jurassic scenario that led to several subduction zones. In these scenarios, the Cretaceous sediments were deposited on the eastern Pelagonian zone either within a Jurassic-Cretaceous passive margin or during a subsequent Cretaceous tectonic event (compressional or extensional depending on the authors). These geodynamic interpretations are presented in Fig.2 and display the different positions of the Pelagonia-Vardar margin relative to the Alpine orogenic wedge after the Jurassic convergence. By studying the small Upper Cretaceous Kallipetra Basin that lies on the Pelagonia-Vardar ‘suture zone’, we can begin to address questions on if and how the Pelagonian-Vardar

margin was deforming. Our study will ultimately provide constraints on the position of the eastern Pelagonian margin relative to the Alpine orogenic wedge, hence ruling out some of the geodynamic models so far proposed. Thanks to the comments of both reviewers, we will adjust the manuscript so as to better identify the problem we want to address, and to show the importance of the birth and the closure of the Kallipetra basin in the context of the Hellenides.

l. 68: Please add e.g. in the citation list since the development of these basins were known already from the time of Brunn and Aubouin (1950-60s)

Done.

l. 77: “metamorphic ages of migmatites” should change to “zircon ages from the leucosomes from the migmatites”.

OK, we agree: the term proposed is more descriptive.

l. 80: “of the wedge” Please rephrase so that you can be more specific on the kind of the wedge (e.g. accretionary, orogenic etc).

We mean orogenic wedge and will rephrase accordingly.

l. 93: Please add Brun & Sokoutis as well as Dinter & Royden for the Rhodope core-complexes.

This can be done.

l. 95: leucogneiss -> leucogneisses

Done.

l. 105: Please avoid terms that refer to processes which you cannot show (i.e. “hydraulically”).

The unit “hydraulically brecciated serpentinite” will be re-named to “Dark massive fractured to brecciated serpentinites”.

l. 117: Please add reference to show who did this interpretation (after “basin”).

OK, it is the interpretation of Schenker et al 2015.

l. 118: As above, please add reference at the end.

Schenker et al 2015

l. 127: “package” -> “pile”?

We will replace “sedimentary package” with “sedimentary sequence”.

l. 131: Please be specific because there are also other kinds of grade (i.e. ore grade). I suggest rewording as: “to determine grade...” -> to determine the metamorphic grade in low-grade metapelitic

We agree with this suggestion, and the phrase “to determine grade” will be replaced with “to determine diagenetic grade”.

l. 141: What exactly do you mean by the “determination of metapelitic zones”. I think you refer to the “metamorphic” zones. Right?

Yes, we refer to low-grade metamorphic zones, so we will replace ‘metapelitic zones’ with ‘low grade metamorphic zones’ in the manuscript.

l. 168: As before, please remote the word “hydraulically”.

We will remove the term ‘hydraulically’ and use “‘Dark massive fractured to brecciated serpentinites”, as mentioned in an above comment.

l. 449: "dramatic" has been struck through. Please check the sentence.

Thank you for alerting us to this, the word dramatic should be removed.

l. 457: “and on viscosity”. I would remove the specific mention to “and on viscosity”since any irreversible deformation mechanism would also contribute to shear heating (e.g. rate-independent plasticity)

We agree with the reviewer and deleted “and on viscosity”.

l. 460: “With a <2cm/a the heat is...” This statement assumes that the movement is steady. Since this hypothesis cannot be supported by the present data, I would suggest removing this sentence.

See comment below the following point.

l. 465-467 (MAIN POINT): As before, the discussion around viscosity only, neglects the frictional part of the heat. Therefore, since this is not the main topic of this paper and there is no detailed analysis in this direction, I would remove specific conclusions related to the most-likely source and the magnitude of shear heating.

As the reviewer has helpfully pointed out, the discussion around the specific magnitudes of heating related either to shear heating or advected hot fluids is highly hypothetical, and we therefore do not have adequate evidence to support one of the two sources of heat. Rather, the goal of this particular paragraph was to draw attention to the unusual inverse geothermal gradient and explore possibilities of how/why this formed. Therefore, we will re-write, simplify, and shorten Section 5.5 ‘The inverted geothermal gradient in the Kallipetra Basin’ to address the concerns of Referee #1.

l. 470-471: As before, there is no evidence to suggest what is considered “normal” by the authors since: (i) the rheology does not have to be purely viscous, (ii) the motion does not need to be steady. Therefore, the suggestion of a particular heating mechanism is beyond the scope and the data presented in this study.

We acknowledge that we have no evidence or data that addresses the convergence rates, viscosities, or plate velocities and therefore agree that the suggestion of particular heating mechanisms goes beyond the scope and data presented in this study. Therefore, we will re-write and shorten this section so that we only relate the observed inverse geothermal gradient to the closure of the Kallipetra Basin so that it remains in the scope of our study.

l. 475: Why the direction of tectonic transport is related to the fluid flow. Assuming a fault zone as a region of high permeability is well established. However, I cannot see how the transport is related for this conclusion.

We agree that transport is not necessarily related to this conclusion, therefore we will replace “The overriding unit over the Kallipetra basin would have allowed fluid focusing and differential loading that caused any fluids to flow in the direction of tectonic transport” with “Differential loading from the overriding unit over the Kallipetra Basin could have focused fluids along the fault zone”.

l. 488-490: How did you conclude that this must be thrusting (MAIN POINT). Why not normal fault with top NE kinematics. Please explain your reasoning in more detail.

The conclusion for thrusting to the NE came from the stratigraphic evidence, predominantly from the character of the rudist mounds. We see stacking of serpentinitic breccias on south-western flanks of rudist mounds, sourced from ophiolitic debris up slope. The absence of ophiolitic detritus on the northeastern mound flanks document a ‘shadow’ effect of the mounds with respect to a south-southwestern provenance of serpentinite clasts. The highest, and therefore youngest, mound is located at Asomata Quarry which is the most northeastern mound suggesting movement of the ophiolite from SSW to NNE. Part of the reason the rudist mounds are so interesting is that they tell us something about the tectonic activity the basin is experiencing without needing to observe the fault itself. We will add a 2D sketch that documents this evolution that should make the reasoning clearer for the reader.

l.494: As before, since the authors already state in line 478 that the sources of heat are not clearly established. I would leave the interpretations out of this.

See comments above pertaining to this issue. We will remove the interpretations of heat sources from this sentence.

l. 494-496: These places are quite far from each other.

Indeed they are. We will delete this sentence.

l. 496-498: From Turonian to Campanian is more than 10 Myr. For a crust âĹij10km thick and standard thermal parameters, the conductive thermal relaxation timescale is ca1Myr. Therefore, I do not think that the advective heat was maintained long enough to cause the heating. Therefore, I would suggest that the authors revise this sentence to defend or reject this conclusion.

We will also delete this sentence as it directly follows from the previous sentence which was deleted in response to the reviewer comment above.

Legend Fig. 3: “Dark blue circles”, the samples are very small. Please use larger and more discrete symbols.

We agree, very small - we will adjust the figure.

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2020-106/se-2020-106-AC1-supplement.pdf

---

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2020-106, 2020.