

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.

Anonymous Referee #1

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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

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the study may look a bit "too regional" from the perspective of a researcher who is not familiar with Eastern Mediterranean geology. 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. 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 in quantify the arguments), I would suggest that the authors mention the possibilities and not go in a specific discussion on the importance of a particular mechanism.

The Specific comments follow below:

- I. 27: Please add "e.g." in the reference list. There are numerous works to be cited here.
- I. 28: Please define what is meant by "Internal" Hellenides, either by definition or by citation to the map.
- I. 34: Please be more specific about the "Cretaceous Basin". Does it have a name? Is there in a particular location that you refer to?
- I. 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.
- I. 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

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problem/hypothesis and then explain why you chose to focus on this area to solve it/ test it.

I. 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)

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

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

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

I. 95: leucogneiss -> leucogneisses

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

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

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

I. 127: “package” -> “pile”?

I. 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 . . .

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

I. 168: As before, please remove the word “hydraulically”.

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

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I. 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)

I. 460: “With a $<2\text{cm/a}$ 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.

I. 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.

I. 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.

I. 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.

I. 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. I. 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.

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

I. 496-498: From Turonian to Campanian is more than 10Myr. For a crust $\sim 10\text{km}$ thick and standard thermal parameters, the conductive thermal relaxation timescale is ca 1Myr. 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

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defend or reject this conclusion.

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

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

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