

Review Report

Zachariasse & van Hinsbergen, Is there a Cretan Supradetachment Basin? Insights From Detailed Mapping on Northwestern Crete (Greece), TEKTONIKA, 2025.

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1st Round of Revisions

Decision Letter

(27 Feb. 2025)

Dear authors,

Thank you for submitting your manuscript to the Tektonika Journal. We have now received two reviews for your manuscript, where the reviewers praise your manuscript while also providing a number of comments which, once addressed, will improve the overall quality of the manuscript. Therefore the decision regarding your manuscript is revisions required at this time.

Reviewer 1 has provided an extensive review with several suggestions which will add to the quality of the manuscript. This includes:

- Reorganisation of certain sections of text and bringing additional information from the supplementary section into the main text to better support the argument.
- Providing greater evidence to support the structural interpretation presented by the authors including adding currently un-mapped faults to the main geological map. They also challenge some aspects of the structural model presented by the authors including evidence for the shallow detachment and changing fault style from steep northward-dipping faults to shallow southward-dipping faults.
- Including a detailed comparison with the Lissos Basin in SW Crete which has been considered coeval with the basin studied in your manuscript.
- Provide regional context to some aspects of your structural interpretation, including the age of east-west extension and the timing and causes of the sedimentary hiatuses.

They also provide several smaller comments and suggestions to improve the language of the manuscript.

Reviewer 2 provides a number of smaller comments in their annotated pdf which both improves the language and also suggest several improvements to the geological maps presented in the manuscript.

Some comments which are repeated by both reviewers include:

- Reorganisation of the results section to make it more clear. This includes moving some geological background information from the results to the introductory section to provide your readers with more context, moving some

information from the supplementary data into the main text due to its importance, and moving some of the text into a table to make it clearer.

- Revising some of the figures, particularly the geological maps, by changing the colours used, amending linework so that geological contacts and faults are more clearly defined, and ensuring that more relevant geological data is included such as the location of décollement surface referenced in-text.
- Consistently using generic terms throughout the manuscript such as 'figure/plate'.

We, as editors at Tektonika, would echo the comments of the reviewers that the justifications for the structural model presented by the authors needs more support and explanation, particularly in explaining the changing nature of fault style in the region.

We look forward to receiving your revised manuscript. Together with the revised manuscript, please upload a rebuttal answering all points raised by the reviewers and editors, and also a manuscript version with changes marked. As one reviewer has mostly commented on the manuscript's PDF, please list their main points in your rebuttal letter and answer accordingly.

Given the relatively significant nature of these revisions, we are giving you a deadline within 2 months (due date 30 April). If you think it will take you longer, please let us know with adequate explanations.

Best Regards,

Conor O'Sullivan, associate editor
Robin Lacassin, executive editor

Comments by Reviewer 1 (Armel Ménant)

I read with interest the manuscript entitled “Is there a Cretan supradetachment basin? Insights from detailed mapping on western Crete (Greece)” by Zachariasse & van Hinsbergen. The authors present an interesting study in which they use field observations, geological mapping and biostratigraphic analysis to reconstruct the sedimentary history of NW Crete since the late Miocene. They propose a 2-step formation of the basin, largely controlled by E-W-oriented high-angle normal faults, which post-date the detachment activity that controlled the exhumation of the underlying metamorphic complex. This work compiles numerous data and provide valuable insights into the late Neogene tectono-sedimentary evolution of Crete. However, I have a few key concerns regarding (i) the organization of the manuscript and the Supplementary Information (SI), (ii) the lack of key structural observations to support some tectonic interpretations, (iii) the clarity and depth of discussion and (iv) the figure quality and clarity (see below). Despite these concerns, I believe that with moderate revisions, the manuscript can be improved and would be suitable for publications in Tektonika.

Armel Menant

I present, first, my main comments on the manuscript.

1/ I recommend the following changes to improve the manuscript's structure. Most of Section 3, spanning L129-202, contains important background information about the nappe stack in W Crete and would be more appropriately placed in Section 2 where it can provide the necessary context. The age of the different formations should be discussed in more detail in a new dedicated sub-section of the Discussion because many of these age interpretations are speculative (e.g., for the Topolia Fm, see my main comment #3).

Several important new data sets are included in the numerous SI items (available on Figshare only) but are not integrated into the main text. I strongly recommend incorporating some of these items into the main body of the manuscript. Thus, (i) the locality names (SI item 1B) are key for the description of the field observation and should be included on Fig. 3. (ii) SI item 4 is almost a study in itself that is critical for constraining the age of the Topolia Fm (L293-295; see also my main comment #3). It should be incorporated into the main text. (iii) SI items 5, 6 and 8 also contain important data regarding the age of formations and should be presented in the main text, including a description of the methods used and the main findings.

2/ I have concerns regarding several structural interpretations presented the manuscript (including the geological cross-sections; Fig. 5) that are not well supported by field data.

In section 5.9 (“The main fault systems”), the authors propose the existence of multiple S-dipping high-angle normal faults affecting the Topolia Fm and underlying units (Fig. 5). However, these faults are not mapped (Figs. 3, 6) although they seem to be exposed at the surface in many places, which raises questions about the validity of this interpretation. Only the E-W-trending Roka Gorge Fault appears to match the proposed fault pattern. I suggest the author provide more field data (ex. fault measurements, kinematic criteria) to support this architecture.

A S-dipping décollement is inferred based on the tilted-block geometry (e.g., L641), but no field evidence is provided. The most robust evidence for normal fault rooting into a décollement would be the presence of roll-over structures. Did the authors observe such structure in the field? In addition, the décollement is said to crosscut the Phyllite-Quartzite unit (Fig. 5a) but the weak layer responsible for the décollement is not identified. The authors also suggest the presence of an E-dipping décollement (L808-810), but again no supporting evidence is provided. This point should be addressed in the discussion (section 6.1.2).

3/ The authors propose that the Topolia Fm represents a large basin that covered much of W Crete at 10-11 Ma. To validate (or refute) this hypothesis, I recommend a detailed comparison with the Lissos basin in SW Crete, which is considered an equivalent of the Topolia Fm (Seidel et al., 2007). Seidel et al. describe large olistoliths (referred to as “slab”) of the Upper Nappe system in the Lissos basin, suggesting a topographic high nearby (likely to the north). This evidence seems to contradict the idea of a single, large basin and instead point to several discontinuous and small basins with a proximal source. I encourage the authors to explore this point in greater detail in the discussion and to reconsider the 2nd point in the conclusions accordingly.

Furthermore, the proposed age of the Topolia Fm (10.9-10.4 Ma) contradicts the middle Miocene age suggested by Seidel et al. (2007). This discrepancy should be addressed in the context of the new data presented in SI Item 4.

4/The manuscript suggests that E-W extension was active after the late Pliocene (L808-810). This young age should be discussed in light of existing literature that suggests arc-parallel stretching began as early as the middle Miocene (e.g., Marsellos et al., 2010).

5/ While I am generally convinced by the authors’ argument for the tectonic control of sedimentary basins in NW-Crete by high-angle normal faults, there is a lack of discussion about the transition between detachment activity and high-angle fault (and décollement) behavior. How do the authors explain the change in kinematics from a top-to-the-N detachment to a top-to-the-S décollement associated with the tilted-block geometry? This should be considered in the discussion, and the detachment should be clearly labeled on the geological cross-sections (Fig. 5).

6/ The manuscript identifies 3 sedimentary hiatuses in NW Crete, each occurring at intervals of approximately 3-4 Myr (Fig. 4). It would be valuable to discuss the potential causes of these hiatuses and whether they could be linked to a quasi-periodic cycle of uplift and subsidence in the Cretan forearc crust. This could offer additional insights into the regional tectonic evolution.

Here is a list of minor (but still important) comments on the manuscript and the figures.

L14. "[...] between ~20 and 13 Ma [...]" (see Marsellos et al. 2010).

L38. I would personally prefer "thinning the crust" rather than "attenuating". This comment is valid for the whole manuscript.

L42. The strongest crustal thinning is below the Cretan Sea (not Crete where the crust is still thick; Snopek et al., 2007). Please, modify this sentence.

L48-51. Poor sentence. Please rewrite.

L54-56. While it is possibly questionable, Seidel et al. (2007) propose a maximum sediment age as old as 20-15 Ma for western Crete. This should be mentioned (see also my main comment #3). In addition, Zachariass et al. (2011) acknowledge that there are sediments older than 10.8 Ma in Crete, but with no evidence that they were deposited in an extensional basin (see p.695 of their paper).

L95-98. This is a complex sentence that deserves to be rewritten.

L141. According to Seidel et al. (1982), "~7 kbar" is a minimum P estimate. "≥7 kbar" would be more appropriate.

L143. Please, indicate that the Phyllite-Quartzite nappe is metamorphosed under HP-LT conditions (Seidel et al., 1982; Jolivet et al., 1996).

L160-162. The base of the Tripolitza unit is considered to be made of a Triassic volcano-sedimentary formation; i.e., the Ravdoucha beds (equivalent to the Tyros beds; e.g., Papanikolaou & Vassilakis, 2010). This sentence has to be modified accordingly.

L170-172. Poor sentence. Please, rewrite.

L183-188. It is a presentation of new data unrelated to the main topic of this work. I suggest to remove them as it is not key for this study.

L228. I suggest to use "Figure" (and not "Plate") to refer to all the illustrations/figures provided with the main text.

L242-246. If the marine sediments belonging to the Topolia Fm correspond to the "inner shelf" in Fig. 3, this should be clearly stated.

L268-270. The "décollement surface" is impossible to localize on Fig. 3. This should be labeled on the map to support this sentence.

L378. Please, make a sentence and justify this statement by providing relevant field observations. Same comment for L430 and L570-571.

L380. Poor sentence. Please, rewrite.

L427. This statement contradicts the claim made in L384-386, where the contact between the Roka and the Kissamou Fm is described as either tectonic or as a questionable stratigraphic continuity (L384-386). Please, clarify the nature of this contact and modify the discussion if needed (L730-731).

L516-517. Please, indicate the nature of the contact between (i) the Elliniko Fm and (ii) the Topolia Fm and the Phyllite-Quartzite nappe; i.e., either erosional or tectonic according to Fig. 3.

L561-563. This sentence lacks a scientific writing style. Please, rewrite it.

L620-623. Fig. 6 should be also cited to illustrate this sentence.

L636-638. It would be helpful to present your strata measurements on stereographic projections to (i) assist in visualizing the overall basin architecture and (ii) provide additional support for your geological cross-sections.

L657-658. Plate 4e is too small to identify the W-dipping faults.

L763-766. This erosional unconformity (Plate 4f) should be presented in the Result section first. Same comment for L813-820.

L824-828. U/Pb dating of HP-LT veins by Ring et al. (2022) suggests that the Phyllite-Quartzite nappe (or a part of it) remained under HP-LT conditions at 13-16 Ma. This challenges the proposed depth of 2-3 km for the metamorphic complex at 12-15 Ma. The possibility of a later and rapid exhumation of the Phyllite-Quartzite should be considered within the context of your study.

L870. "Triplitza" (not "Triplitza").

L873. Please, modify: "[...] extreme thinning by the buoyant rise of the Phyllite-Quartzite and deeper nappes [...]".

L874-876. This sentence is confusing. What do you mean by "[...] remained uncoupled to the upper plate until late in the exhumation"? By considering geochronological and thermochronological ages for the HP/LT complex, the metamorphic nappes must have exhumed through the (thick) Cretan crust by the end of the middle Miocene (~30-40 km thick today; Snopek et al., 2007). So, the Phyllite-Quartzite must belong (be coupled?) to the upper plate (i.e., the Cretan forearc crust) much earlier. Please clarify this part of the discussion.

Fig. 2. There is a confusion between the Tripali and Plattenkalk units in the figure caption.

Plates 1-4. Additional legends should be included on the field photographs to help readers quickly identify key geological features. When legends are provided, they should be more legible, as the images are quite small. The abbreviations (e.g., "d" and "s" on Plates 3d, 3e) have to be defined in figure captions. Furthermore, the orientation of the field photographs should be indicated more frequently, particularly for Plates 2c, 2d, 3c, 3d and all panels of Plate 4.

Plate 4c. A detailed description of this outcrop, where a well-exposed tectonic structure is present, is needed to support the interpretation of a high-angle normal fault (and not a detachment) at the contact between the Phyllite-Quartzite unit and the Upper Nappe system.

Fig. 7 (upper panel). The detachment should be highlighted on this figure.

Furthermore, it is unlikely that this structure was horizontal as it is depicted here (e.g., Fassoulas et al., 1994; Jolivet et al., 1996). Please, modify the figure.

References used in this review.

- Fassoulas, C., Kiliyas, A., & Mountrakis, D. (1994). Postnappe stacking extension and exhumation of high-pressure/low-temperature rocks in the island of Crete, Greece. *Tectonics*, 13(1), 127-138.
- Jolivet, L., Goffé, B., Monié, P., Truffert-Luxey, C., Patriat, M., & Bonneau, M. (1996). Miocene detachment in Crete and exhumation P-T-t paths of high-pressure metamorphic rocks. *Tectonics*, 15(6), 1129-1153.
- Marsellos, A. E., Kidd, W. S. F., & Garver, J. I. (2010). Extension and exhumation of the HP/LT rocks in the Hellenic forearc ridge. *American Journal of Science*, 310(1), 1-36.
- Papanikolaou, D., & Vassilakis, E. (2010). Thrust faults and extensional detachment faults in Cretan tectono-stratigraphy: Implications for Middle Miocene extension. *Tectonophysics*, 488(1-4), 233-247.
- Ring, U., Fassoulas, C., Uysal, I. T., Bolhar, R., Tong, K., & Todd, A. (2022). Nappe imbrication within the Phyllite-Quartzite Unit of West Crete: Implications for sustained high-pressure metamorphism in the Hellenide subduction orogen, Greece. *Tectonics*, 41(11), e2022TC007430.
- Seidel, E., Kreuzer, H., & Harre, W. (1982). A late Oligocene/early Miocene high pressure belt in the external Hellenides. *Geologisches Jahrbuch. Reihe E, Geophysik*, (23), 165-206.
- Seidel, M., Seidel, E., & Stöckhert, B. (2007). Tectono-sedimentary evolution of lower to middle Miocene half-graben basins related to an extensional detachment fault (western Crete, Greece). *Terra Nova*, 19(1), 39-47.
- Snopek, K., Meier, T., Endrun, B., Bohnhoff, M., & Casten, U. (2007). Comparison of gravimetric and seismic constraints on the structure of the Aegean lithosphere in the forearc of the Hellenic subduction zone in the area of Crete. *Journal of Geodynamics*, 44(3-5), 173-185.
- Zachariasse, W. J., van Hinsbergen, D. J., & Fortuin, A. R. (2011). Formation and fragmentation of a late Miocene supradetachment basin in central Crete: Implications for exhumation mechanisms of high-pressure rocks in the Aegean forearc. *Basin Research*, 23(6), 678-701.

Comments by Reviewer 2 (Alessandro Petroccia)

Reviewer 2 has also commented on the PDF of the manuscript

Dear authors and editor,

I appreciate the proposed paper by Ilem Jan Zachariasse and Douwe J.J. van Hinsbergen, “Is there a Cretan supradetachment basin? Insights from detailed mapping on northwestern Crete (Greece)”

The study provides a detailed geological map of northwestern Crete and a detailed description of the stratigraphy coupled with the tectonic reorganizations, making this paper potentially suitable for *Tektonika*.

The figures are good and well made, but I suggest paying more attention to the provided geological map in Figure 3. It represents the core of this article, and it should be clear and readable in all its parts (see the comments on the PDF, e.g., the font size, the used colors, etc.). Maybe it can be very useful to upload it as supplementary material in high resolution separately. In this way, all can use it in the field.

Pay attention to how authors refer in the main text to the figures; they change from figure to plate. Homogenize it.

Finally, I suggest reorganizing the description of the results using a synoptic table instead of a list of “descriptions” as a report. A synoptic table or a summary image can strongly increase the clarity of the discussion paragraph, which is well sustained by the obtained and presented data.

See the annotated PDF for both general and specific comments.

Sincerely,
Petroccia Alessandro

Authors' Reply to Reviewers

Reviewer 1 (Armel Menant):

I read with interest the manuscript entitled “Is there a Cretan supradetachment basin? Insights from detailed mapping on western Crete (Greece)” by Zachariasse & van Hinsbergen. The authors present an interesting study in which they use field observations, geological mapping and biostratigraphic analysis to reconstruct the sedimentary history of NW Crete since the late Miocene. They propose a 2-step formation of the basin, largely controlled by E-W-oriented high-angle normal faults, which post-date the detachment activity that controlled the exhumation of the underlying metamorphic complex. This work compiles numerous data and provide valuable insights into the late Neogene tectono-sedimentary evolution of Crete. However, I have a few key concerns regarding (i) the organization of the manuscript and the Supplementary Information (SI), (ii) the lack of key structural observations to support some tectonic interpretations, (iii) the clarity and depth of discussion and (iv) the figure quality and clarity (see below). Despite these concerns, I believe that with moderate revisions, the manuscript can be improved and would be suitable for publications in Tektonika.

Armel Menant

I present, first, my main comments on the manuscript.

1/ I recommend the following changes to improve the manuscript's structure.

Most of Section 3, spanning L129-202, contains important background information about the nappe stack in W Crete and would be more appropriately placed in Section 2 where it can provide the necessary context.

Confusion: Should have been section 4. Changed numbering

The age of the different formations should be discussed in more detail in a new dedicated sub-section of the Discussion because many of these age interpretations are speculative (e.g., for the Topolia Fm, see my main comment #3).

We feel that taking the age assignments out of the formation descriptions and making a new section would introduce a lot of repetition.

What we have done, however, is scrutinizing the age discussions once more. This holds specifically for the discussion about the age of the Topolia Fm which was – in view of your questions – incomplete.

See changes in main text.

Several important new data sets are included in the numerous SI items (available on Figshare only) but are not integrated into the main text. I strongly recommend incorporating some of these items into the main body of the manuscript. Thus, (i) the locality names (SI item 1B) are key for the description of the field observation and should be included on Fig. 3.

It is a dilemma (see also above). That we have omitted place names (not the places themselves) and dip angles in text Figure 3 is because of readability. Figure 3 must, after all, fit into page layout which means that the original map is reduced 6x. This means that text/lines will be magnified by the same factor making the map unreadable. The map is simply too detailed.

For all the details, the reader is referred to SI Item 1B.

(ii) SI item 4 is almost a study in itself that is critical for constraining the age of the Topolia Fm (L293-295; see also my main comment #3). It should be incorporated into the main text. (iii) SI items 5, 6 and 8 also contain important data regarding the age of formations and should be presented in the main text, including a description of the methods used and the main findings.

See changes in main text

2/ I have concerns regarding several structural interpretations presented the manuscript (including the geological cross-sections; Fig. 5) that are not well supported by field data.

In section 5.9 ("The main fault systems"), the authors propose the existence of multiple S-dipping high-angle normal faults affecting the Topolia Fm and underlying units (Fig. 5). However, these faults are not mapped (Figs. 3, 6) although they seem to be exposed at the surface in many places, which raises questions about the validity of this interpretation. Only the E-W-trending Roka Gorge Fault appears to match the proposed fault pattern. I suggest the author provide more field data (ex. fault measurements, kinematic criteria) to support this architecture.

Thanks for raising this point!

The reason for not drawing S-dipping faults in the geological maps in Figures 3,6 is that faults are actually invisible in the field because of the uniform lithology, strong karstification and travertine coated gorge walls. Nevertheless, we infer the presence of these listric faults because persistent N-dipping without such faults leads to unrealistic thicknesses of 1000,1200 and 700m in cross sections Figures 9A, B and C, respectively. The inferred faults are drawn for visualization in Figures 9 and 11 where number and size of the blocks in Figure 9 is determined by thickness Topolia Fm, dips, and the fact that the Roka and Topolia Gorges (Figure 9A and B) are floored in Topolia Fm.

We better explained that in text.

A S-dipping décollement is inferred based on the tilted-block geometry (e.g., L641), but no field evidence is provided. The most robust evidence for normal fault rooting into a décollement would be the presence of roll-over structures. Did the authors observe such structure in the field?

See above. The train of thought is: 1) the Topolia Fm is systematically tilted northward; 2) It is bounded in the field by a south-dipping normal fault demonstrating that extension is at play and is a likely cause for the tilting and 3) if otherwise unbroken, the

Topolia Fm would have an unrealistically thick stratigraphic thickness of well over 1 km. So multiple listric fault-bounded blocks are inferred. Conceptually, this is easiest to explain if those faults root in a common décollement. Indeed, roll-over structures would provide the smoking gun. All exposures of Topolia in the exposed remnants of the Topolia basin are tilted, and a hypothetical roll-over must have been present in the now-eroded portions that once were overlying the PQ in the center of the island.

However, we realize that our use of the term 'décollement' may be confusing in the literature of the Aegean region, which is rich in metamorphic core complexes, as well as fold-thrust belts. In those settings, 'décollement' is often used for large-displacement detachments along which metamorphic core complexes exhume, or weakness zones along which nappes decouple. In our case, the displacements may be more modest, and therefore, we stressed in the text that we infer listric normal faulting to explain the tilts, and that we speculate that these root in a common décollement as depicted in Figure 7.

In addition, the décollement is said to crosscut the Phyllite-Quartzite unit (Fig. 5a) but the weak layer responsible for the décollement is not identified.

See comment above on the décollement discussion.

The authors also suggest the presence of an E-dipping décollement (L808-810), but again no supporting evidence is provided. This point should be addressed in the discussion (section 6.1.2).

Also here, we now explain that we infer listric normal faulting to explain the tilts, and removed the inference of a common décollement.

3/ The authors propose that the Topolia Fm represents a large basin that covered much of W Crete at 10-11 Ma. To validate (or refute) this hypothesis, I recommend a detailed comparison with the Lissos basin in SW Crete, which is considered an equivalent of the Topolia Fm (Seidel et al., 2007). Seidel et al. describe large olistoliths (referred to as “slab”) of the Upper Nappe system in the Lissos basin, suggesting a topographic high nearby (likely to the north). This evidence seems to contradict the idea of a single, large basin and instead point to several discontinuous and small basins with a proximal source. I encourage the authors to explore this point in greater detail in the discussion and to reconsider the 2nd point in the conclusions accordingly.

Good point! The “olistoliths” mentioned by Seidel et al (2007) are included in a Miocene coastal alluvial fan complex that is bounded in the N by a S-dipping high-angle normal fault. The debris and olistoliths of Tripolitza carbonates are derived from the footwall. Seidel et al (2007) also mapped the time-equivalent alluvial fan complex in NW Crete, i.e. the Topolia Basin alluvial fan complex. Unlike the Topolia complex, the Lissos complex extends into the deep marine domain.

The first author has studied and mapped SW Crete as well and is now at the writing stage. What I found is that the Tripolitza olistoliths belong to a deep marine mass wasting deposit that overlies the distal part of the fluvial Topolia Fm of NW Crete. Plankton foraminiferal biostratigraphy indicates an age range of 10.3-10.5 Ma for these

mass wasting deposits. This younger basin has a basin bounding fault in the N with the Tripolitza olistoliths being derived from the footwall.

These data from SW Crete thus indicate that the Topolia Basin extended over most of western Crete. The point is that these new data are not yet published.

We have circumvented this by explaining in the text that the Topolia Basin may have extended all the way to the south coast, as argued for by Kopp & Richter (1983), or that it may have been part of a larger system of half grabens (Seidel et al., 2007).

Furthermore, the proposed age of the Topolia Fm (10.9-10.4 Ma) contradicts the middle Miocene age suggested by Seidel et al. (2007). This discrepancy should be addressed in the context of the new data presented in SI Item 4.

Explained in main text under age Topolia Fm

4/The manuscript suggests that E-W extension was active after the late Pliocene (L808-810). This young age should be discussed in light of existing literature that suggests arc-parallel stretching began as early as the middle Miocene (e.g., Marsellos et al., 2010).

Arc-parallel stretching on Crete occurs before basin formation at 10.9 Ma as follows for instance from reconstructions of oroclinal bending (van Hinsbergen & Schmid 2012), or from normal faulting in the Heraklion Basin (Zachariasse et al, 2011). Our point here is not that E-W extension only started in the Pliocene, but simply that on western Crete, it also occurred in the Pliocene based on the E-tilted blocks. We indicated in the text that E-W extension (re-) started in the Pliocene.

Changed in text.

5/ While I am generally convinced by the authors' argument for the tectonic control of sedimentary basins in NW-Crete by high-angle normal faults, there is a lack of discussion about the transition between detachment activity and high-angle fault (and décollement) behavior. How do the authors explain the change in kinematics from a top-to-the-N detachment to a top-to-the-S decollement associated with the tilted-block geometry? This should be considered in the discussion, and the detachment should be clearly labeled on the geological cross-sections (Fig. 5).

This is a fair point, and we now offered a speculation in the final part the discussion:

"We speculate that the extreme thinning of the upper nappes led to a loss of structural coherence, such that further extension could no longer be accommodated along the existing detachment. Since then, the Cretan Detachment became inactive and ongoing extension became accommodated along new fault systems that crosscut the older ones, and that we document in this paper."

6/ The manuscript identifies 3 sedimentary hiatuses in NW Crete, each occurring at intervals of approximately 3-4 Myr (Fig. 4). It would be valuable to discuss the potential causes of these hiatuses and whether they could be linked to a quasi-periodic cycle of uplift and subsidence in the Cretan forearc crust. This could offer additional insights

into the regional tectonic evolution.

Indeed, hiatuses are spaced by roughly these time intervals. However, two hiatuses are determined by regional uplift and have a regional tectonic cause: the one around 10-9.6 Ma, and the one between 3-1 Ma. The terminal Messinian hiatus is controlled by drawdown of the Mediterranean and subaerial erosion in the marginal basins due to tectonics in the Gibraltar gateway area (e.g. Krijgsman et al., 2024).

We have added a general remark on how the basin information may be used for future thinking of deeper crustal processes in the final part of the discussion:

"Ongoing rise of subducted buoyant rock at depth may have continued to play a role in the vertical motions that ensued after the onset of basin formation and could have played a role in the youngest uplift (e.g., Gallen et al., 2014), and perhaps underpin the transition from the Topolia to the Northwest Cretan Basin."

Here is a list of minor (but still important) comments on the manuscript and the figures.

L14. "[...] between ~20 and 13 Ma [...]" (see Marsellos et al. 2010).

Corrected

L38. I would personally prefer "thinning the crust" rather than "attenuating". This comment is valid for the whole manuscript.

Corrected

L42. The strongest crustal thinning is below the Cretan Sea (not Crete where the crust is still thick; Snopek et al., 2007). Please, modify this sentence.

We specified: strongly thinning the original forearc crust. The thinning of that crust is extreme: the original thickness of >20 km is now only a few hundred meters. Most of the crustal thickness of Crete is Phyllite Quartzite and deeper units that were not part of the forearc before the Miocene.

L48-51. Poor sentence. Please rewrite.

We don't see it that way and prefer to keep it as it is.

L54-56. While it is possibly questionable, Seidel et al. (2007) propose a maximum sediment age as old as 20-15 Ma for western Crete. This should be mentioned (see also my main comment #3). In addition, Zacharias et al. (2011) acknowledge that there are sediments older than 10.8 Ma in Crete, but with no evidence that they were deposited in an extensional basin (see p.695 of their paper).

Discussed in the main text under the age Topolia Fm

L95-98. This is a complex sentence that deserves to be rewritten.

Done

L141. According to Seidel et al. (1982), "~7 kbar" is a minimum P estimate. "≥7 kbar" would be more appropriate.

Corrected

L143. Please, indicate that the Phyllite-Quartzite nappe is metamorphosed under HP-LT conditions (Seidel et al., 1982; Jolivet et al., 1996).

Done

L160-162. The base of the Tripolitza unit is considered to be made of a Triassic volcano-sedimentary formation; i.e., the Ravdoucha beds (equivalent to the Tyros beds; e.g., Papanikolaou & Vassilakis, 2010). This sentence has to be modified accordingly.

This is of course correct, but those Triassic rift-related volcanics are not found on western Crete.

L170-172. Poor sentence. Please, rewrite.

Done

L183-188. It is a presentation of new data unrelated to the main topic of this work. I suggest to remove them as it is not key for this study.

Well, the observations explain why the true stratigraphic sequence of the Pindos unit is difficult to establish in the field and also why we have included any Tripolitza flysch in the Pindos unit: they're impossible to distinguish in the field. We prefer to keep it as it is.

L228. I suggest to use “Figure” (and not “Plate”) to refer to all the illustrations/figures provided with the main text.

Done

L242-246. If the marine sediments belonging to the Topolia Fm correspond to the “inner shelf” in Fig. 3, this should be clearly stated.

Figure 3 distinguishes between Topolia Fm (fluvial) and Topolia (inner shelf). See different colors in legend Figure 3.

L268-270. The “décollement surface” is impossible to localize on Fig. 3. This should be labeled on the map to support this sentence.

Correct, this is not possible to find. We removed the reference to Fig. 3, and we rephrased the sentence a bit more generically.

L378. Please, make a sentence and justify this statement by providing relevant field observations. Same comment for L430 and L570-571.

Done

L380. Poor sentence. Please, rewrite.

Done

L427. This statement contradicts the claim made in L384-386, where the contact between the Roka and the Kissamou Fm is described as either tectonic or as a questionable stratigraphic continuity (L384-386). Please, clarify the nature of this contact and modify the discussion if needed (L730-731).

Clarified in main text under age of the Roka Fm

L516-517. Please, indicate the nature of the contact between (i) the Elliniko Fm and (ii) the Topolia Fm and the Phyllite-Quartzite nappe; i.e., either erosional or tectonic according to Fig. 3.

Done

L561-563. This sentence lacks a scientific writing style. Please, rewrite it.

Done

L620-623. Fig. 6 should be also cited to illustrate this sentence.

Done

L636-638. It would be helpful to present your strata measurements on stereographic projections to (i) assist in visualizing the overall basin architecture and (ii) provide additional support for your geological cross-sections.

We added stereographic projections to the cross-section diagrams to illustrate the measured dips in the Topolia Fm that we used as input.

L657-658. Plate 4e is too small to identify the W-dipping faults.

We realized that we placed the Plates in portrait formation, but they were intended for landscape. We rotated the Plates in the manuscript to display them at the intended size.

L763-766. This erosional unconformity (Plate 4f) should be presented in the Result section first. Same comment for L813-820.

We have deleted Plate 4f and accompanying text because this location is outside of the studied area and is not per se relevant for this paper.

L813-820: describes the latest part of the evolution of the northwest Cretan Basin, i.e. how glacial sea level changes and tectonic uplift have shaped the Quaternary coastline and is illustrated by the photographs on Plate 4f-h. The Quaternary deposits described in 5.8 refer to the lithologies of three separately mapped Quaternary units. Nevertheless, we made changes in text to make this more clear.

L824-828. U/Pb dating of HP-LT veins by Ring et al. (2022) suggests that the Phyllite-Quartzite nappe (or a part of it) remained under HP-LT conditions at 13-16 Ma. This challenges the proposed depth of 2-3 km for the metamorphic complex at 12-15 Ma. The possibility of a later and rapid exhumation of the Phyllite-Quartzite should be considered within the context of your study.

Thanks for notifying: it is for the purpose of our paper not important whether the bulk of PQ exhumation occurred before or after 13 Ma: what is important is that it preceded the onset of sedimentation around 11 Ma. We have rephrased the sentence:

The question that initiated this study was whether the formation of the oldest sedimentary basins on Crete was governed by the same processes and structures that drove the long-term exhumation of HP/LT metamorphic rocks on Crete from depths of

tens of kilometers to the near-surface, as often inferred, or reflect an unrelated and younger tectonic event. In any case, it is clear that when sedimentation started, the Phyllite-Quartzite unit must have already been exhumed to close to the surface, demonstrating that the main exhumation mechanism did not generate topographic depressions prone to sedimentation.

L870. “Tripolitza” (not “Triplitza”).

Done

L873. Please, modify: “[...] extreme thinning by the buoyant rise of the Phyllite-Quartzite and deeper nappes [...]”.

Corrected

L874-876. This sentence is confusing. What do you mean by “[...] remained uncoupled to the upper plate until late in the exhumation”? By considering geochronological and thermochronological ages for the HP/LT complex, the metamorphic nappes must have exhumed through the (thick) Cretan crust by the end of the middle Miocene (~30-40 km thick today; Snopek et al., 2007). So, the Phyllite-Quartzite must belong (be coupled?) to the upper plate (i.e., the Cretan forearc crust) much earlier. Please clarify this part of the discussion.

This point is explained in detail in van Hinsbergen and Schmid, 2012, see their figure 13, which we now refer to in the text for clarification). The Phyllite Quartzite unit was essentially moving up along the subducting plate, and it could keep rising until it became blocked by the crust of the forearc. But because the crust of the forearc became stretched by arc-parallel extension owing to oroclinal bending, the PQ could exhume to very shallow depth. In this conceptual way, the strong thinning of the original forearc crust (i.e., the upper nappes), and the exhumation of the PQ by buoyant rise along the slab are integrated.

Fig. 2. There is a confusion between the Tripali and Plattenkalk units in the figure caption.

We checked the figure, caption, and legend, but we cannot find the confusion.

Plates 1-4. Additional legends should be included on the field photographs to help readers quickly identify key geological features. When legends are provided, they should be more legible, as the images are quite small. The abbreviations (e.g., “d” and “s” on Plates 3d, 3e) have to be defined in figure captions. Furthermore, the orientation of the field photographs should be indicated more frequently, particularly for Plates 2c, 2d, 3c, 3d and all panels of Plate 4.

Thanks for the suggestions to optimize the photographs. See updated photo's. Plates are now replaced by Figures.

Plate 4c. A detailed description of this outcrop, where a well-exposed tectonic structure is present, is needed to support the interpretation of a high-angle normal fault (and not a detachment) at the contact between the Phyllite-Quartzite unit and the Upper Nappe system.

We don't make the case here for whether or not this is a high-angle normal fault or a detachment. We simply illustrate that the contact is a fault.

Fig. 7 (upper panel). The detachment should be highlighted on this figure. Furthermore, it is unlikely that this structure was horizontal as it is depicted here (e.g., Fassoulas et al., 1994; Jolivet et al., 1996). Please, modify the figure.

When a detachment forms in an undeformed block, it will indeed be dipping, and Fassoulas and Jolivet depicted this conceptually. However, as pointed out in the manuscript and elsewhere, the upper nappes in Crete are so extremely thinned by E-W extension, that they are essentially a thin, faulted veneer that lies on the remains of the detachment, which lie more or less at the surface of western Crete from the south to the north coast. The Cretan Detachment may be termed as such because it represents a fault with a tectonic omission, but the deformation history here is much more complex than in the classical interpretations that apply to e.g. the Cyclades.

Reviewer 2 (Alessandro Petroccia): Has extensively commented on the PDF of the manuscript > get this file via the Tektonika managing system under item Reviewers/Alessandro Petroccia

Dear authors and editor, I appreciate the proposed paper by Ilem Jan Zachariasse and Douwe J.J. van Hinsbergen, "Is there a Cretan supradetachment basin? Insights from detailed mapping on northwestern Crete (Greece)"

The study provides a detailed geological map of northwestern Crete and a detailed description of the stratigraphy coupled with the tectonic reorganizations, making this paper potentially suitable for *Tektonika*.

The figures are good and well made, but I suggest paying more attention to the provided geological map in Figure 3. It represents the core of this article, and it should be clear and readable in all its parts (see the comments on the PDF, e.g., the font size, the used colors, etc.). Maybe it can be very useful to upload it as supplementary material in high resolution separately. In this way, all can use it in the field.

Pay attention to how authors refer in the main text to the figures; they change from figure to plate. Homogenize it.

Finally, I suggest reorganizing the description of the results using a synoptic table instead of a list of "descriptions" as a report. A synoptic table or a summary image can strongly increase the clarity of the discussion paragraph, which is well sustained by the obtained and presented data.

See the annotated PDF for both general and specific comments.

We thank Alessandro Petroccia for the valuable general comments (specifically on the readability of the geological map in text Figure 3). Comments are addressed in the annotated text.

2nd Round of Revisions

Decision Letter

(23 June 2025)

Dear authors, dear Douwe,

We have now received a second review following the revisions you have made to your manuscript.

The reviewer, who also reviewed the initial version of your manuscript, provides a very positive review and notes the significant effort made by the authors to improve both the text and figures from the initial submission. They recommend your manuscript be accepted for publication following some minor revisions they have suggested.

These are outlined below and covered in more detail in the reviewer letter copied below too:

- Merge sections 2 and 4. The reviewer considers that section 4 presents crucial background information that would be more suitable when integrated with section 2.
- They acknowledge your choice to not include locality names on Figure 3. As an alternative, they request that you include more geographic information in the text to give the reader more clarity where individual localities are related to features illustrated on Figure 3.
- They reiterate their recommendation that some of the supplementary information should be moved to the main text given its importance to supporting your models and findings. Failing this, they request you include a small introduction to each supplement information item in the text, to ensure that readers engage with it and understand the full scope of your work.

In addition to these three main points, they include a number of minor corrections to the text, which are included in their review.

We understand it has taken some time to process your manuscript and appreciate your patience during the review process. Once these changes have been made, we look forward to accepting your manuscript for publication in the Tektonika Journal. Given the limited amount of requested revision, we look forward to receiving your revised manuscript in the few coming weeks, if possible before the end of July.

All the best,

Conor O'Sullivan, Tektonika associate editor

Robin Lacassin, Tektonika executive editor

Comments by Reviewer 1 (Armel Ménant)

This is my 2nd review of the manuscript entitled “Is there a Cretan supradetachment basin? Insights from detailed mapping on northwestern Crete (Greece)” by Zachariasse and van Hinsbergen. I appreciate the authors’ efforts in improving the manuscript and figures, particularly Figs. 9 and 10. I generally agree with their conclusions. However, a number of minor issues remain, many of which are consistent with my 1st round of review. Most notably, the SI Items still requires better integration and explanation in the main text. Based on the comments listed below, I consider the manuscript suitable for publication in Tektonika after minor revision.

Armel Menant

1/ In line with my earlier comment, I recommend merging the current Section 4 (“The nappe stack of western Crete”) into Section 2 (“Geological setting”) because it presents crucial background on the architecture of the nappe stack in western Crete, derived from literature and it is a natural continuation of the broader geodynamic context described in Section 2. As it stands, placing the Methods section between Sections 2 and 4 disrupts the logical flow.

2/ While I understand that showing all locality names on Fig. 3 would compromise readability, an alternative would be to provide clearer geographic references in the main text. For instance, in L281, the authors may consider adding a clarification, such as “[...] between Kakopetros and Sirikari (i.e., in the southern part of the study area; Figure 5a, b).”

3/ Each SI items included in the Supplementary Material should be briefly introduced in the main text. Their purpose and methodology should be clearly stated, preferably in Section 3 (“Methods”). For example, for SI Item 4, “litho- and biostratigraphic observations on XX sections from Gavdos island in order to [...]”. Regarding SI Item 5, which presents a literature-based discussion constraining the age of the Malathiros Fm, I recommend to move it to the main text at the appropriated place.

Here is a list of additional comments on the manuscript and the figures.

L48-51. The sentence remains difficult to follow. Please revise it for clarity and conciseness.

L149-151. References to “(own field data)” should not appear in the Geological Setting section. The “Geological Setting” should strictly summarize previously published work and unpublished field observations should be properly presented in the “Result” section.

L236. Please, replace with “southern Gramvousa peninsula” to match the label in SI Item 1B.

L312-313. Interpretations should be based on published data and new observations presented in this study, whereas interpretations based on unpublished data should be approached with caution. Please reformulate this part of the paragraph.

L315-319. Please, revise this sentence (by making 2 sentences maybe?).

L602. Remove the extra space before “Age:”. Please, check for similar spacing issues throughout the manuscript.

L839-840. Clarify the phrase “[...] E-W extension on northwestern Crete (re)started”. It should be indicated that an initial phase of arc-parallel extension preceded basin formation, as noted in the authors’ response to my earlier major comment #4.

L843-847. Observations on marine terraces should first be presented in the Result section (i.e., Section 5.8 “Quaternary deposits”) with corresponding references to Fig. 9f-h. These observations can then be used in the Discussion to support the latest part of the evolution of the northwest Cretan Basin.

Fig. 2. The confusion between the Pattenkalk and the Tripali units in this figure is the follow: the legend shows the Plattenkalk as light grey and Tripali as dark grey, but in the Lefka Ori region on the map, the colors are reversed (e.g., Fig. 1 in Creutzburg & Seidel, 1975). This should be corrected.

Reference used in the review

Creutzburg, N., & Seidel, E. (1975). Zum Stand der Geologie des Präneogens auf Kreta.

Authors' Reply to Reviewer 1

We have now received a second review following the revisions you have made to your manuscript.

The reviewer, who also reviewed the initial version of your manuscript, provides a very positive review and notes the significant effort made by the authors to improve both the text and figures from the initial submission. They recommend your manuscript be accepted for publication following some minor revisions they have suggested.

These are outlined below and covered in more detail in the reviewer letter copied below too:

- Merge sections 2 and 4. The reviewer considers that section 4 presents crucial background information that would be more suitable when integrated with section 2.

Completely agree! We reversed chapters 3 and 4.

- They acknowledge your choice to not include locality names on Figure 3. As an alternative, they request that you include more geographic information in the text to give the reader more clarity where individual localities are related to features illustrated on Figure 3.

While we also regret that not all geographic information can be put in Figure 3 (for readability reasons) it is impractical to provide all place names in the text with indications of where to find them. To compensate for this deficiency, we have added SI Item 1A where all geographical place names mentioned in the text can be found.

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We sympathize with the suggestion to include SI Item 5 in the main text but we think that inserting all these details does not improve the readability. Nevertheless, we have addressed this issue by adding additional information where appropriate, e.g. in Methods and paragraph preceding Ch 6.1 and end Ch 6.1.

In addition to these three main points, they include a number of minor corrections to the text, which are included in their review.

We understand it has taken some time to process your manuscript and appreciate your patience during the review process. Once these changes have been made, we look forward to accepting your manuscript for publication in the Tektonika Journal. Given the limited amount of requested revision, we look forward to receiving your revised manuscript in the few coming weeks, if possible before the end of July.

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Conor O'Sullivan, Tektonika associate editor

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Here is a list of additional comments on the manuscript and the figures.

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Done by replacing “These” by “This metamorphic complex etc

L149-151. References to “(own field data)” should not appear in the Geological Setting section. The “Geological Setting” should strictly summarize previously published work and unpublished field observations should be properly presented in the “Result” section.

Agreed! We separated our own observations from Ch 3 (the nappe stack of western Crete) and replaced these observations to the Results section in a new Chapter 5.

L236. Please, replace with “southern Gramvousa peninsula” to match the label in SI Item 1B.

Done.

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Done.

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Done.

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We missed this completely. Thanks for pointing out this error!!

We would like to add that we are very grateful to Armel Menant for his fair and fine two reviews. They greatly improved the quality of our manuscript. Again: many thanks!!

Acceptance Letter

(17 July 2025)

Dear authors

Thanks for your revised manuscript that take into account the last comments by the reviewer. Based on our evaluation we have decided to accept your manuscript today.

You will be contacted by our copy editing team in the coming weeks.

Thanks for submitting to Tektonika !

Conor O'Sullivan, Tektonika associate editor

Robin Lacassin, Tektonika executive editor