

Interactive comment on “Extreme extension across Seram and Ambon, eastern Indonesia: Evidence for Banda slab rollback” by J. M. Pownall et al.

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The authors are very grateful for the reviews by Torgeir Andersen and an anonymous referee, and for the comments posted by Martin Norvick. We are delighted that both reviewers have recommended the manuscript be published without additional changes. We have identified a small number of minor revisions that we wish to make (outlined below).

First, here is our response to the general comments and questions raised by the reviewers...

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Reply to Interactive Comment by T. B. Andersen

“...their model and documentation still lack some critical data, particular modern age constraints.”

We fully agree that additional ‘modern’ geochronological data will help to quantify our working model for extreme extension and HT-UHT metamorphism on Seram. To this end, we are currently engaged in obtaining U–Pb zircon ages and Ar–Ar ages for several of the rocks described in the manuscript from the Kaibobo and Hoamoal peninsulas, the Kobipoto Mountains, and from Ambon. Our objective in writing this paper was to document the new geological mapping, newly-discovered field relations, and to present a working tectonic model for the islands in order to provide a platform for future studies. We could not more fully agree with Dr. Andersen’s comment; however we feel it is beyond the scope of this current paper to report such results at this stage.

“There are some obvious points of possible additions and improvement concerning various aspects of the UHT metamorphism that could have been added in the discussion.”

We also have identified that additional work is required in this area, and have since have been working on determining the P–T conditions of the high-temperature residual granulites of the Kobipoto Complex by means of THERMOCALC phase modelling. However, this work is ongoing and planned to be the subject of a future publication. Although we fully agree with Dr. Andersen’s comment, we feel it is beyond the scope of this current paper to provide a more comprehensive account of these rocks.

Reply to Interactive comment by Anonymous Referee #1

The referee raised no concerns over the manuscript.

Reply to Interactive comment by M. Norvick

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“The first comment concerns the Wai Tuh section. . . there were several large blocks of pillow basalt”

We thank Martin Norvick for bringing the basalts to our attention. Unfortunately, we were unable to travel downstream far enough to have encountered this section. A few boulders of basaltic float were observed by us in the same river, but never in situ examples of the rocks. Voluminous basalts might be expected if the peridotites had experienced high degrees of partial melting, but their dominantly lherzolitic composition demonstrates this was unlikely to have been the case. Therefore, we do not anticipate the pillow basalts observed during the 1975 University of London expedition to be widespread or a major component of the Kobipoto Complex. However, we do know of several locations in west Seram where there are small exposures of basalts, some pillowed, and those from Kelang have yielded K–Ar ages of between 8 and 1 Ma (Beckinsale and Nakapandungrat, 1978; Honthaas et al., 1999). Thus, it seems most likely the Wai Tuh basalts are Neogene.

“The second issue concerns the structure of the Kobipoto Mountains... could the Kobipoto Mountains represent a pop-up of basement from the Australian plate in a major strike-slip zone, without the need to invoke significant crustal thinning?”

There is certainly evidence that crustal thinning was required for the peridotites to have been juxtaposed against the lower/mid crust in order for HT-UHT conditions to have been attained. Furthermore, in western Seram, hot peridotites and Kobipoto Complex cordierite diatexites were exhumed by detachment faulting, which requires further crustal extension and therefore also further crustal thinning. In the instance of the Kobipoto Mountains, we are proposing only that the pop-up structure facilitated the final stages of exhumation, so even if the strike-slip faulting does not itself necessarily require crustal thinning, the granulite-facies migmatite complex exhumed by the strike-slip faulting does. A further point to mention is, as evidenced in the Kawa Shear Zone where lenses of serpentinised peridotites have been incorporated into the fault

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gouges, the strike-slip faults may themselves be reactivated lithospheric detachment faults. Lastly, it must be emphasised that the Kobipoto Complex cannot be considered proper ‘basement’ as demonstrated by Mio-Pliocene ages for cordierite granites (strictly cordierite diatexites) in western Seram (Priem et al., 1978; Honthaas et al., 1999; Linthout et al., 1996) which belong to the same migmatite complex that has been exhumed in the Kobipoto Mountains.

“The third comment is of a more philosophical nature and concerns the comparison of extremely extended continental margins. . . I would like to invite the authors to comment on why these other extended terranes are so different.”

As outlined by Martin Norvick, it is certainly noteworthy that there are major differences between ‘cold’ magma-poor hyperextended passive continental margins (cf. Lavier and Manatschal, 2006) and the ‘hot’ model we propose for rollback-driven extreme extension on Seram. These two contrasting extensional mechanisms evidently occur under vastly different geothermal gradients, which may to a large extent be controlled by (a) extension rate; and (b) additional heat input from the asthenosphere. A major difference is the tectonic setting. In the hyperextended passive continental margins a large plate is splitting, perhaps due to plume activity or some other mechanism leading to lithospheric thinning. In the Banda region the extension is driven by subduction. In the instance of the rollback-induced extensional margin on Seram, there is evidence for rapid extension, and it may be possible that asthenospheric upwelling controlled by the rolling-back slab could have contributed to the elevated heat-flow. The questions raised by Martin Norvick are very interesting and may well provide the basis for future work on rollback-induced extended margins. Modelling is now required to investigate further, but it is difficult to speculate beyond these points with the currently available data.

Minor revisions requested by the authors

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1. We have made two amendments to our original field interpretations on Ambon in light of new thin section evidence: (a) pebbles described as 'schist' (pg. 557; line 13) from southern Hitu, Ambon, are fine-grained granite ultramylonites and therefore there is no evidence for Tehoru Formation 'basement' rocks on Ambon; (b) The 'hornblende granodiorite' described from Tanjung Seri (pg. 558; line 23) is a leucogranite containing chlorite pseudomorphs after biotite.
2. Our interpretation of the relationship between members of the Kobipoto Complex requires clarification. It should be more clearly stated that the 'cordierite granites' and dry, aluminous granulites are best explained as representing different components of an extensive migmatite complex: the granulites representing the dry residual component, and the cordierite granites representing advanced diatexites. The 'cordierite granites' were not produced by melting of the granulites but both lithologies were instead created contemporaneously by the same partial melting event. In addition, migmatite terminology has been improved: our use of the terms 'restite' and 'clots' has been modified, and the terms schlieren or schollen (Sawyer, 2008) have been used instead for those aluminous mineral assemblages. Minor modification of the manuscript pertaining to the Kobipoto Complex (especially section 3.5) has been made.
3. 'Kaibobo Complex' has been amended to 'Kobipoto Complex' on pg. 532 (line 12) and pg. 555 (line 5).

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