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Interactive comment on "Resolving multiple geological events using in situ Rb-Sr geochronology: implications for metallogenesis at Tropicana, Western Australia" *by* Hugo K. H. Olierook et al.

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Reviewer #1: Thomas Blenkinsop

The aim of this paper is to demonstrate that in situ Rb/Sr dating has now become sophisticated enough to unravel different geological events. The study uses samples from the Tropicana area, which by now is quite well constrained in terms of regional and deposit scale geochronology. The paper is very clearly written and illustrated, and communicates its message very well. There is no doubt that it shows the power of the

method, which is an exciting advance in geochronology and this aspect needs to be published.

We thank the reviewer for their time in reviewing this contribution.

The interpretation of the data with respect to tectonics and mineralisation is more controversial. The older ages agree with previous geochronology (a strength of the paper), but they are interpreted in a new and different way as simple cooling ages rather than relating to the D3 deformation event. This is regarded as having the younger age (1210 Ma). The major reason for this is the interpretation that a single shearing event is seen in the microstructures, unlike the reactivation scenario previously postulated. This is not consistent with the change in kinematics of shear zones from D3 to D4, D5 that is documented in Blenkinsop and Doyle (2014). In that study, D3 shear zones were identified as having only biotite as the phyllosilicate phase, whereas most of the samples in this study have some muscovite/phengite, so they would be classified as D4 or D5 according to the previous work. It could therefore be suggested that none of the samples adequately dated a true D3 shear zone.

PARTLY AGREE. We agree with the disputed aspect of reactivation. This work is not focused on detailed microstructural and petrological study, that is required to assess reactivation scenario. Therefore, it is possible that the reactivation of the greenschist facies D3/D4 shears took place during Mesoproterozoic. We have corrected it in the text, accordingly citing Blenkinsop and Doyle (2014). However, we firmly believe that a low-strain brittle-ductile microstructure we used for dating both Biotite 1 and 2 represents the D3 event as described by Blenkinsop and Doyle (2014). A more discussion to this point is in the following point.

This study has the advantage of the TIMA images which may have revealed additional aspects of shear zones not seen in the 2014 study, so it may be that the petrographic distinction claimed previously is not real. However, there is a clear morphological difference between the shear zones with biotite and pyrite and those with phengite - the

latter are generally wider, with much stronger fabrics. This can be seen for example in the differences between Fig. 7, a and b compared to c and d in Blenkinsop and Doyle (2014) There is clear structural evidence for shear zones that cross cut earlier biotite fabrics in the drill core, and there are clearly sets of shear zones with different kinematics (Blenkinsop and Doyle Fig. 14). It would be very strange if this was not the case in such a polymetamorphic setting, although that is not a strong argument. So it is a bold claim that there is no evidence for reactivation and that all deformation textures belong to a single event. To substantiate the new interpretation, it would be useful to see some more microstructural analysis with kinematics and some more detailed photomicrographs of the dated samples.

DISAGREE. We went through the paper of Blenkinsop and Doyle (2014) carefully again. Unfortunately, none of the figures shows a clear cross-cutting relationship between D3 and D4 textures. Also, it is quite intriguing that both events show the same kinematics of NE-SW shortening in their paper (Table 1 for D3 event and see text in Page 198 for the D4 event). It is necessary to emphasise that the work of Blenkinsop and Doyle (2014) is based on a structural analysis of drill holes when pits were not open. Consequently, it is likely that a spatial and temporal relationship between D3 extensional textures and D4 shears could not have been adequately assessed. We see a possibility that a variable amount of muscovite/phengite might be a function of variable plagioclase content of the host syenitic gneiss. An easy breakdown of plagioclase to micas localises strain within anastomosing and simple shear-dominated D4 ductile shears, while K-feldspar dominated domains show a more brittle response within an apparent low-strain and pure shear-dominated domain. A presence of carbonate and euhedral pyrite in both microstructures support the coincident development of the D3 and D4 microstructures. If only a single shearing event is implicated, this would have to occur during the formation of mineral assemblage 2 (i.e., 1210 Ma). Although the question of a possible reactivation is not the primary goal of this paper, it is likely that some reactivation of D3-D4 fabric might occur during localised D5 event as suggested by Blenkinsop and Doyle (2014). A presence of carbonate and euhedral pyrite in both

microstructures support the coincident development of the D3 and D4 microstructures.

In the end this debate is much less important than the geochronological aspects of the paper, which seem really solid. The tectonic interpretation could therefore be presented with a more nuanced discussion, acknowledging the points above. It would be good to see this paper published, after dealing with this point. Tom Blenkinsop

AGREE. We thank the reviewer again for his constructive comments. We appreciate his knowledgeable insights to the geology of Tropicana deposit very much during this review as well as at the time when he worked on kinematics from drill holes. We realise how difficult it is to resolve the structural story in a remote and very poorly known area without outcrops and open pit observations. We have corrected our discussion to reflect this appreciation.

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