

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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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.

The interpretation of the data with respect to tectonics and mineralization 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.

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.

In the end this debate is much less important than the geochronological aspects of the

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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

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