Reply to Professor Alan Collins

Alan has provided an insightful and comprehensive review and commentary on our paper and we greatly appreciate the time he has taken to do this. Among his more general comments, he points out that the timing of tectonic events across large tracts of northern Australia is strikingly similar and not confined to the Mount Isa and southern McArthur basin with which we are more familiar. As such, this would seem to reinforce our view that area covered by our seismic interpretation is more representative of northern Australia than we might have originally envisaged. We are therefore encouraged to think we may be on safe ground with our ideas on basin inversion and its links to sediment-hosted Pb-Zn mineralisation although these will need to be further tested, as does our claim that the petroleum and mineralisation overlapped and were driven by a common tectonic driver.

1. Differences raised in the timing of specific depositional and/or tectonic events are not easily addressed as, unlike in the Paleozoic, the rocks investigated by us are unfossiliferous, and tectonic interpretations and models such as ours are probably still over-reliant on detrital zircon ages or indirect dating of magmatic rocks either intruded into the sedimentary rocks or are intercalated with them. Currently available geochronological data are largely maximum depositional ages without clear evidence that the dated rocks are first cycle sediments. His suggestion that one or more tectonic events identified by us may be diachronous across the region may therefore prove correct but has yet to be properly tested and is beyond the scope of our paper. Nevertheless we are not blind to this possibility and welcome the suggestion as should it prove correct then a temporal trend of this type might help resolve long-standing questions about whether tectonism from 1800-1600 Ma in northern Australia was driven by processes along a convergent plate margin that lay to the south or east of Proterozoic eastern Australia.
2. In going through the annotated pdf, we have noted where typographic errors or further clarification of the text was deemed necessary and have made the recommended changes. This involved some re-writing of text which we have now completed.
3. It was further suggested that some of our figures (e.g. Fig. 8b) but more especially the ones showing seismic images needed to be revised to increase their readability. In reducing the size of the figures to fit the prescribed journal pdf format and size limits (all pages be in portrait format), some of their detail and resolution was lost (file sizes are inevitably large even where reproduced as jpegs or png files). In the event that our MS is accepted for publication, the offending figures will be submitted at a larger scale and size by breaking up the longer interpreted seismic images into two panels and placing them one above the other. Detail down to the level of individual reflectors should then be legible without the aid of the zoom button.

Reply to Karen Connors

As with the earlier review by Professor Alan Collins, we greatly appreciate the comments provided by Dr Connors and the thoroughness with which she reviewed our interpretations and conclusions. Some of her criticism centres on stratigraphic units we have identified in the seismic data and the extent to which these can be recognised in outcrop elsewhere across the region. In reply, we address the following points:

1. Differences of opinion about the stratigraphic affinities of the Carrara Range Group and its inferred presence in the seismic images. Dr Connors points to a recent unpublished abstract by Carson et al (2020) in which the Carrara Group is included in the Calvert as opposed to the Leichhardt Superbasin as we have it. This is the older sequence we identified at depth along line 17GA-SN1. It is important to recognise that Carson et al base their reinterpretation on detrital zircon ages but the age spectrum shown by them for the Gator Sandstone (part of the Carrara Group) is the same as for the Wire Creek Sandstone and Westmoreland Conglomerate, neither of which to our knowledge is regarded as part of the Calvert Superbasin. We also point out that the detrital zircon age signature obtained by Carson et al (2020) for the Carrara Group is the same as that obtained from rocks of undoubted Leichhardt Superbasin age by Neumann et al., 2006. Until more definitive evidence comes to light we opt to stick with our current interpretation based on previous mapping by Sweet (1984) and Rawlings et al (2008).

2. The same can be said for the 1725 Ma Top Rocky Rhyolite which according to Jackson et al (2000) is intrusive with the texture and character of a porphyry sill (very little banding or other features that might lend support to the idea that it is an ignimbrite). Again we see no reason to change our interpretation although we have introduced more detail on the Carrara Group into the relevant sections on our seismic interpretation.
3. We agree that figure 3 is not without its problems, not least of which is the age of the marine transgression that we equate with the Gun-Loretta supersquences. The age ranges are taken from Southgate et al (2000) who defined both units and they are now well entrenched. Our paper is not the place to redefine these two units and so we spelt out in the text the time that mafic magmatism ceased and marine transgression commenced. In all probability, transgression commenced at the end rather than the beginning of Gun time. The more important point for us is that the Riversleigh Tectonic Event commenced in late Gun time and continued through into Loretta time.
4. We agree that the section of the temporal as well as spatial overlap between the minerals and petroleum system needed more documentation and a better argument in support of the idea, first raised by Broadbent et al (1998). This we have done by expanding the relevant section in the discussion, drawing on the work of Golding et al (2006) and Glikson et al (2006) who suggest that such overlap probably did occur and that thermal maturation of organic carbon was likely caused by ingress of the mineralising fluid itself. So while the mineralising fluid may not have been emplaced into an existing oil and/or gas reservoir, the two systems are inextricably linked at Century and possibly also Walford Creek.
5. To bolster our thesis that the minerals and petroleum systems may have overlapped, we also point to the similarities between Century and Mississippi Valley-type Pb-Zn mineralisation where a comparable debate has been going on about mixing between a hydrocarbon and hydrothermal fluid during ore formation.
6. Yes, we could include both interpreted and uninterpreted seismic images in the one figure but this will not improve on resolution of the detail. The pdf format does not lend itself to such detail. One possibility where space is at a premium is to post the interpreted and uninterpreted images in the supplementary data so that they are readily accessible to a reader who wants to cast their eye more closely over our interpretation. Professor Collins raised the same issue and in reply we enlarged the seismic image for part of seismic line 17GA-SN1. Perhaps the Editor of the journal could advise on a possible solution here.
7. Finally, we have addressed many of the other concerns and minor points raised by Dr Connors by amending the wording or adding clarifications to our text.