Response to the reviewers

We thank both reviewers for their positive and constructive remarks. Below we address each of the points of reviewer 2 and show how we adapted the manuscript accordingly. These and other small changes to the manuscript are also indicated in red in the updated version of the manuscript.

Reviewer 2 - Tim Jones

Overall that paper provides valuable insights into the dynamics of clastic-dominated base metal deposits. The authors cover a lot of ground to bring together geodynamics and a mineral systems analysis, providing an approach itself that will be valuable reading to multiple disciplines.

The modeling results provide some testable conjectures, such as “[the] conditions for deposit formation can briefly occur in both narrow and wide rifts for at most 3 My”, and that narrow, asymmetric rifts are the most fertile settings for these deposits. The paper contains a thorough discussion of the results that includes comparisons to real geological cases. It lists some assumptions of the model but lacks a discussion of how those assumptions may limit the specific model results and applicability of the conclusions drawn. Great value from the models could be gained by the reader if the temperature field was included in the result images.

Some minor comments to address below.

“In sedimentary basins, key components of the mineral systems model are the metal source, flow conduits for metal-bearing fluids, and a trap for concentrating metals at the deposit site.”
It would be helpful to see some references here, and explicitly state that if these are the ‘key’ processes, what are the secondary processes deemed to be less important to focus on and why.

We have added two references, the first describes the genetic model, the second the mineral system components of CD-type deposits (line 54-55):

In sedimentary basins, key components of the mineral systems model are the metal source, flow conduits for metal-bearing fluids, and a trap for concentrating metals at the deposit site ***(e.g., Wilkinson, 2014; Lawley et al., 2022)****.*

Lawley et al. (2022) for example also list preservation and direct detection as components of the CD-type deposit mineral system. These components are not key to the formation, but to the survival and discovery of the deposits, and are not considered in this paper. We described the key components mentioned – metal source, fluids, conduits and trap – in more detail in the paragraph that follows the quoted sentence. That said, every deposit experiences site-specific circumstances that can be considered secondary components and cannot be addressed by the general nature of our simulations.

In the Discussion (line 579), we now point to one future avenue to include one of these secondary components, the preservation of deposits along cratonic edges:

Future modelling should also investigate the role of thick, cold cratonic lithosphere (e.g. Raghuram et al., 2023; Gouiza and Naliboff, 2021) in promoting the formation and/or preservation of CD-type deposits, as Hoggard et al. **(2020)** ***(and Groves and Santosh, 2021; Lawley et al., 2022; Huston et al., 2022) found a correlation between deposit location and the edges of thick and/or cratonic lithosphere.***

"Eq (9) dz = 20 km is the assumed in-plane extent of the basins to create a source rock volume"
Assumed based on what? This is somewhat important when you are comparing model results to estimates of deposit endowment to validate your results since I don't see why this value couldn't be arbitrarily set much higher or lower. Unless it is calibrated using realistic volume estimates? In which case it can't be used to justify the resulting endowments from the models.

We did not calibrate the assumed in-plane extent to known endowment, and agree that the arbitrary value of 20 km could be smaller. For example, the complex subbasin scale in the southern McArthur Basin (Fig. 11d of Blaikie and Kunzmann 2020), varies from ~1.5 to 15 km. Manning and Emsbo (2018) assumed a fault trace length of 10 km to estimate the total mass of Zn and Pb in their simulations of the brine reflux system. For comparability, we have therefore decided to adopt the value of 10 km and refer to the above papers in the text. However, note that we have to make assumptions about the other parameters that feed into the computation of the endowment (Eq. 9) as well. For example, we assume that 65% of the metals is leached from the source rock, as described in the Methods section 2.4.

We have adapted the following sentence in the Discussion to emphasize the assumptions we make in the endowment calculation (line 571):

Using Eq. (9), which includes a conversion of source rock area to volume ***assuming an in-plane extent of the basins of 10 km****, the potential endowment for the average of all nine narrow asymmetric simulations can be* ***estimated*** *(Figure 6d).*
…
These mechanisms were active between 13.5 to 21.0 My in Figure 4; this time frame corresponds to a possible endowment of up to ***35*** *Mt zinc and* ***8*** *Mt lead. This is of the same order of magnitude as the maximum currently estimated zinc tonnage of 27.4 Mt (Red Dog, Alaska) and* ***similar to*** *the maximum lead tonnage of 28.0 Mt (Broken Hill, Australia) reported in the database of Hoggard et al. (2020).*
Endowment estimates would improve from resolving the relevant processes in 3D and from the inclusion of fluid flow.

We have also replotted the endowment figure for an in-plane extent of 10 km, and added the above cited references to the text in Section 2.4 as follows (line 253):

where ρ is the rock density, $dz = 10km$ is the assumed in-plane extent of the basins to create a source rock volume ***(based on basin extent in Blaikie and Kunzmann (2020) and on the assumptions of Manning and Emsbo (2018))****,*

One limitation not discussed is the modelling of near surface deformation, which is a brittle process, using equations that treat the Earth as a viscous fluid. I understand that this is common practice but strictly speaking it should not apply to the upper crust, and is even debatable in some situations at greater depths. Since a portion of the results depend on the model's ability to simulate near-surface faulting, I suggest adding a discussion around this to the limitations section.

Indeed we use a highly-viscous fluid description of the Earth, which is warranted by the large timescales we are interested in compared to the Maxwell relaxation time. We modify the viscosity in the governing equations into an effective viscosity that takes into account plasticity (approximation of brittle faulting). As the reviewer states, this is common practice. Comparison of these viscous-fluid approximation methods to analog models, analytical solutions of failure and to geomechanic codes shows good agreement, also for ASPECT (Buiter et al., 2006; Kaus, 2010; Buiter et al., 2016; Glerum et al., 2018; Duretz et al., 2019). A large body of thermomechanical modelling work uses this approach to look at upper crust deformation, and we have therefore limited the discussion of this common approach to the following sentence in Section 2.5 Model assumptions (line 283):

*For one****, the governing equations approximate the Earth as a highly-viscous fluid on geological timescales, modifying the viscosity to allow for non-viscous behaviour such as brittle deformation. Second****, our simulations are 2D*

Is there a missing section 3.4 or just a typo?

This was a typo, thank you, we have corrected the numbering of the sections.

Would be great to see thermal evolution here in the results, alongside composition and strain. Can you say why you thought it was not important to include that? It would help provide some insight into where melting might be focused even if not explicitly predicted.

As can be seen in the figures, we did include two crustal isotherms, those of 150 and 250 °C, as these delineate the upper boundary of the host rock temperature window and the lower boundary of the source rock temperature window, respectively. Before submission we tested the inclusion of more crustal isotherms, but this made the figures very crowded. As the majority of CD-type deposits have sedimentary rocks as metal source instead of volcanics, and we do not include melting and melt transport, we did not focus on possible melt regions.

That said, we have now reprocessed figures 3a-f to include isotherms in the lithosphere and asthenosphere. We also postprocessed one narrow asymmetric rift simulation to compute the melt fraction based on a linear interpolation between a solidus and liquidus of Katz (2003), see Figure R1 and the response to reviewer 1.

Potential melting would be focused underneath the oceanward side of the narrow margin, i.e., in the right location for intrusion into the basin where the most favourable ore formation mechanisms occur.

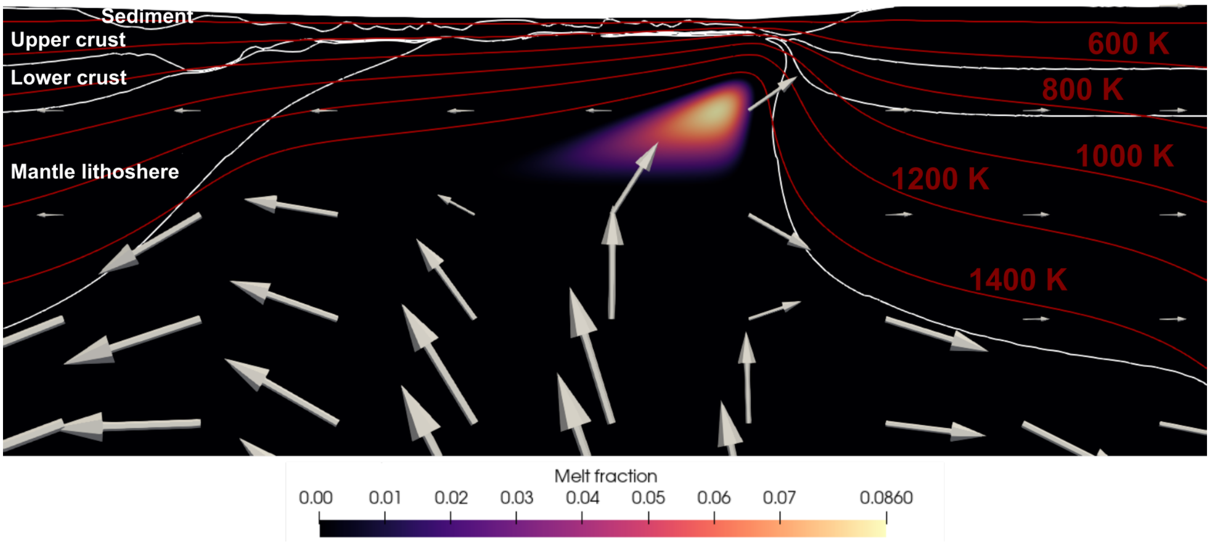

Figure R1 Melt fraction at the time of ore formation mechanism 1 (20 My) for whole model domain (bottom) and zoom-in (top) for narrow-asymmetric rifting simulation NA-4.

The sedimentation rates and volumes are discussed and compared with observations but not the resulting sedimentary structure of the basins. This seems like a key prediction of the models also. Do you have any insight into how, in a broad sense, does the predicted sequence of lithologies and their distribution compare to observations?

A direct comparison of our model lithologies and observed lithostratigraphies of known large deposits is hindered by the limited number of sediment types in the simulations and the difference in resolution. For example, a description of the Barney Creek Formation (host to the Teena and McArthur River deposits) in Hayward et al. (2021) has a stratigraphic resolution of several 10s of meters, while the maximum resolution of our simulations is 313 m. Our simulations resolve three types of sediments, predominantly coarse continental (sandstone), predominantly fine continental (siltstone), and predominantly marine sediments. The stratigraphy of the Umbolooga Subgroup, to which the Barney Creek Formation belongs, is drawn by Blaikie and Kunzmann (2020, Fig. 2) as an alternating sequence of siliciclastics, mixed siliciclastics/carbonates, and carbonates. This agrees with the sequence of predominantly sandstone and marine sediments we predict in the fertile basins.

We have added the following statement to the manuscript (line 546):

In the McArthur Basin, Blaikie and Kunzmann (2020) describe the Umbolooga Subgroup as an alternating stratigraphic sequence of siliciclastics, mixed siliciclastics/carbonates, and carbonates, which agrees with the level of stratigraphic detail we can resolve in our simulations.

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