

## ***Interactive comment on “Fault-controlled fluid circulation and diagenesis along basin bounding fault systems in rifts – insights from the East Greenland rift system” by Eric Salomon et al.***

### **Anonymous Referee #1**

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This is a well written and illustrated paper that is worthy of publication. It nicely documents how early diagenetic, fault-controlled cementation can modify rock physical properties and thereby control subsequent fracture patterns, fluid flow and diagenetic overprint. I would recommend moderate revisions prior to publication in order to clarify a few points, in particular 1. To expand a little more on the conceptual model that is presented through a clearer description of the paragenesis (and inclusion of a paragenetic sequence), linked to a burial curve. Included in this summary figure and text modification should be a clear definition early in the paper as to what is meant by ‘early syn-rift’, ‘rift climax’, ‘post-rift’. I got a bit confused in parts of the paper as to the actual timing that was being discussed. 2. The discussion as to how quickly

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cementation occurred (~lines 270 – 275) needs to be drawn out a little more as the prose jumps around a bit. Establish that the most reliable dates for matrix cementation are ~150 to 140Ma, then give the age of the LB Formation. From there, build the argument of increased tensile strength controlling fracturing and use the date of G-10v1 as evidence that matrix cementation happened immediately post-deposition. This is the core of your paper, so needs to be carefully explained. 3. I was not convinced by Figure 9, which uses an interpolation of dip to calculate depth of cementation based on the present day erosional profile, because: – there is no mention of structural tilt, which must have occurred as a result of uplift and doming in the Cenozoic? – The estimation of 10 or 15o depositional dip seems steep and I couldn’t find reference earlier in the paper (e.g. from field measurements) as to where this value might have come from. Maybe I missed it. However, given that a 5o dip difference (from 10 to 15o) has an impact of ~300 m change in burial depth, should a lower dip (e.g. 6o?) also be shown? – The effect of compaction is referred to in brackets, yet this is important. Thickness is estimated from interpolation/extrapolation from ~4km from the fault, but at >1km from the fault there would have been more compaction than within the cemented zone, which presumably is undercompacted because of the effect of early cementation. What do you know of compaction from petrographical analysis of uncemented sandstones? – the absence of quartz overgrowths is not a strong argument for an absence of burial; there could have been burial but no quartz-rich fluids. 3. Consequently, there has to be a better attempt at constraining the burial history, based on what is known of the structural evolution of the basin and the overlying sediments. Linked to this, the argument for the geothermal gradient in the text is a bit muddled, as it doesn’t specifically separate the likely geothermal gradient of the basin from the heat flow along the fault and the resultant temperature of the fluid. 4. A deeper evaluation of the likely fluid source is needed, citing the 18Owater of Cretaceous seawater and assessing the likely 18Owater of meteoric water at the paleolatitude the basin was at. Then make more use of the trace element data, which I felt was really not integrated into the interpretation as well as it could have been. In

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particular, why are the Fe concentrations so high? 5. I wasn't convinced by the idea of diffusion from cement into veins to give similar trace element concentrations. How would this work? If the cement had precipitated prior to fracturing – as it has to have done to have increased tensile strength – why would trace elements diffuse into the fluids precipitating calcite in the veins? Could it not just be that calcite in the matrix and the fractures was precipitated from similar fluids? 6. More consideration needs to be given to the source of the carbonate for calcite precipitation. I agree that an organic source seems likely on the basis of  $\delta^{13}\text{C}$  but is this feasible based on what we know of the sediment source and depositional process? What does the isotope data tell us of the burial depth and redox conditions? Is cementation taking place in the zone of bacterial oxidation or sulphate reduction?

Please also note the supplement to this comment:

<https://se.copernicus.org/preprints/se-2020-72/se-2020-72-RC1-supplement.pdf>

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Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2020-72>, 2020.