

Dear Editor,

This is my review of the paper **Fault and fracture networks as long-lived conduits for lithium transport**, by Cawood and coauthors.

The manuscript presents a multi-disciplinary dataset (field observations, U–Pb calcite geochronology, clumped isotopes, and lithium concentrations) to investigate the role of fault and fracture networks in lithium transport in Clayton Valley, Nevada. The study addresses an important question in the context of lithium brine systems and provides valuable new geochemical constraints on structurally controlled fluid flow in continental settings.

The dataset is novel and potentially significant, particularly the integration of absolute timing of vein formation with lithium geochemistry. The text is clear, terminology is correct, and figures are adequate, self-explicative and very beautiful. The comparison with literature is sound, and the overall dataset is certainly worth to be published.

However, in my view (structural geology perspective) there are three main aspects that require clarification or additional support. These concern with overall structural characterization of faults, petrographic and microstructural control on U-Pb ages, and source of lithium. Below more detailed comments.

In summary, this manuscript presents a valuable and potentially publishable dataset, but the interpretations would be substantially strengthened by a more rigorous integration of petrography, structural geology, and fluid-source discussion. I believe the paper has strong potential after revision and I therefore recommend **moderate revision**.

Best regards

Fabrizio Balsamo

Major points to be addressed:

1. Petrographic characterization and vein microstructures

One of the main weaknesses of the manuscript is the absence of a general petrographic and microstructural characterization of the sampled veins. The paper repeatedly emphasizes the role of fault and fracture networks as long-lived conduits for lithium-bearing fluids, yet the evidence presented is almost entirely based on field observations and bulk geochemical analyses. In a contribution centered on structural controls on fluid circulation, this leaves an important gap in the overall argument.

At present, the manuscript does not provide sufficient information regarding the internal textures of the calcite veins, the relationships between different generations of calcite, or the deformation mechanisms associated with vein formation (opening-mode, shear veins?). There is little discussion of whether the veins formed during active deformation, whether they record repeated opening and sealing events, or whether multiple mineralization phases are present

within individual structures. Likewise, the manuscript does not address the occurrence of crack-seal textures, recrystallization fabrics, brecciation, possible cross-cutting relationships, or evidence for vein reactivation. These observations are essential because they provide the direct structural context needed to interpret the veins as long-lived pathways for episodic fluid circulation through time (even at the scale of individual veins).

This issue becomes particularly important given that the manuscript interprets the calcite mineralization as recording repeated lithium-bearing fluid flow over multi-million-year timescales. Without petrographic evidence demonstrating multiple generations of vein growth or reactivation, it remains difficult to evaluate whether the analyzed calcites represent discrete mineralization events or composite vein histories that potentially integrate multiple fluid-flow episodes.

I strongly encourage the authors to include a dedicated section describing the petrography and microstructures of the sampled veins. Representative thin-section images, accompanied by descriptions of vein fabrics and paragenetic relationships, would substantially strengthen the manuscript. Such observations would also help clarify the temporal and structural relationships between deformation, vein formation, and fluid circulation, thereby reinforcing the broader interpretations developed in the Discussion.

2. Structural characterization/description of the fault systems

A second major limitation of the manuscript is the lack of quantitative structural characterization of the studied faults, particularly the basin-bounding structures that form the basis for several of the paper's conceptual interpretations. Although the manuscript frequently refers to "faults," "fracture networks," and "basin-bounding faults," the actual geometry, scale, and internal architecture of these structures remain poorly constrained throughout the paper.

For example, the manuscript does not provide sufficient information regarding the displacement of the major faults, the thickness of the fault cores, the width of associated damage zones, or the intensity and distribution of fracturing adjacent to the principal slip surfaces. These details (even qualitative description) are fundamental in any study addressing structurally controlled permeability because the hydraulic behavior of fault systems varies significantly depending on structural attributes (in broad sense) and position within the fault architecture (i.e. wall damage zone, tip damage zone, intersecting damage zone, following recent classifications...).

At present, it is unclear where the analyzed samples were collected relative to the structural architecture of the faults. The manuscript should clarify whether the calcite samples derive from the principal slip surface, brecciated fault-core material, subsidiary fractures within the damage zone, or fractures external to the fault zone altogether. Similarly, there is little quantitative description of the sampled veins themselves, including their thickness, continuity, orientation.

These structural details are critical because the manuscript ultimately argues that faults acted as long-lived pathways organizing lithium transport through the basin. Without a more rigorous structural framework, it is difficult to assess the extent to which the analyzed veins genuinely record basin-scale fault-controlled flow as opposed to more localized fracture-related circulation.

I therefore recommend that the authors expand the structural description substantially. The paper would benefit greatly from a more detailed description of the principal faults and fracture systems, including quantitative parameters where possible, together with clearer documentation of sampling positions relative to fault zone structure and map view architecture.

3. Source of lithium and implications for basin evolution

The discussion regarding the origin of lithium is one of the most interesting aspects of the manuscript, but it is currently underdeveloped relative to the importance of the conclusions being proposed. A central claim of the paper is that several lithium-bearing calcite veins predate emplacement of the late Miocene silicic volcanic units commonly invoked as the principal lithium source in Clayton Valley. This observation is potentially significant because it suggests that lithium-bearing fluids were already circulating within the evolving basin prior to the emplacement of the volcanic units typically considered critical to lithium enrichment.

However, while the manuscript questions the conventional volcanogenic source model, it does not sufficiently develop alternative explanations for the origin of the lithium. The current discussion largely demonstrates that lithium transport occurred before the emplacement of certain volcanic units, but it does not clearly establish where the lithium was sourced from during these earlier stages of basin evolution. As a result, the interpretation remains somewhat incomplete and, at times, speculative.

The manuscript would benefit from a broader and more balanced discussion of possible lithium reservoirs and mobilization mechanisms. For example, the potential role of basement lithologies, earlier volcanic sequences, sedimentary recycling, long-lived regional groundwater circulation, or inherited basin fluids deserves more careful consideration. Likewise, the extent to which the presented geochemical and isotopic data can actually discriminate among these possible sources should be discussed more explicitly.

This point is particularly important because the manuscript presently risks overextending the implications of the geochronological dataset. Demonstrating that lithium-bearing fluids predate a specific volcanic event is not necessarily equivalent to demonstrating that volcanism was unimportant in the overall lithium budget of the basin. The authors should therefore clarify the distinction between evidence for early lithium circulation and evidence for the ultimate lithium source itself.

A more nuanced treatment of lithium sourcing would considerably strengthen the manuscript and place the results in a broader basin-evolution and fluid-flow context.