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Solid Earth Discussions

SED

Interactive comment

Interactive comment on "Spatiotemporal history of fluid-fault interaction in the Hurricane fault zone, western USA" *by* Jace M. Koger and Dennis L. Newell

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In the following we detail how we have addressed each of the comments and suggestions from Matthew Steele-MacInnis. In summary, we greatly appreciate the constructive and insightful comments and suggestions that were provided on our manuscript. We have addressed all of the major comments and line-by-line comments.

Review by Matthew Steele-MacInnis I read the paper by Koger and Newell with interest. In my opinion, the motivation is clearly articulated, the data appear robust, and the interpretations seem sound. I recommend publication with only minor revisions. Main comment: My only real "main" comment is related to the origin of the saline brine.

Around lines 320-326, the authors suggest that the brine originated as meteoric water, which circulated deep and acquired a high solute load. Perhaps. But there seem to be other possibilities, and I'm not sure why they are not discussed. If the source of the salinity is thought to be marine sediments, then why shouldn't we consider paleo-seawater- derived brine as a possibility? In many cases, halogen compositions of basinal brines show evidence of salinity acquired by partial evaporation of seawater. I'm not saying this is the case here; just that it could be permissible, as far as I can tell. The authors may wish to check the papers by Bruce Yardley on this subject, and may also wish to expand the discussion of where these brines may have originated. Or, if other lines of evidence argue against something like this, then please explain that here?

Author response: We agree that other sources of salinity are a possibility and we only provide our preferred interpretation. Certainly, the ïĄď18O and moderate salinity of our most saline endmember could represent some fraction of a paleo-seawater derived brine. Unfortunately, we do not have halogen data (e.g., Cl/Br) available from our samples, and these data are not published for the thermal springs along the Hurricane fault (Pah Tempe, Travertine Grotto) that could help with fingerprinting the source. Thus with the available data we cannot distinguish between meteoric water mixing with evolved paleo-seawater or meteoric-water-rock interaction. As the reviewer points out in comment #1, the relatively low salinity (11 wt %), although within the range observed for basinal brines, is on the low end, and thus our dataset may not capture the saline endmember. We agree that our mixing trend in Figure 6 could extend to higher salinity, higher ïĄď18O values. We have updated our discussion to address the other possible interpretations.

And related to this previous point, a couple smaller comments: 1) The salinity of 11 wt% NaCl is on the low end for basinal brines. This might actually be an (equivocal) argument in favor of the brine representing original meteoric water that has picked up some solutes, though I would be wary of over-interpreting this. Basinal brines generally show salinities from 5 to >30 wt%, and our large dataset from MVT deposits shows a prominent mode around 20 wt% (Bodnar et al., 2014, TOG). 11 wt% is certainly permissible for a basinal brine, just it might be worth noting that such brines can be more saline, and this may even suggest that the true basinal "end member" has not been sampled here.

Author response: See our response to the above "main comment".

2) Basinal brines are commonly enriched in Ca, which gives rise to first-melting temperatures around -50âŮęC. Was first melting truly never observed in this study? That is a bit unfortunate, though I guess "it is what it is." Still, I would ask you to revisit your notebooks and have a look for any notes you may have made about first melting, even if only for a few inclusions. Also, calcic brines commonly show a characteristic "orange peel" texture when frozen (Schlegel et al., 2012). Was anything like this observed?

Author response:

During the fluid inclusion measurements, we looked very carefully for first melting, and it was not observed. Also, we were not aware of the "orange peel" texture at the time of analysis; however, we did not note any unusual textures during freezing. Perhaps future work on these samples could utilize other methods to address the actual composition of the fluid inclusions (such as Raman work).

These two latter comments are obviously little things, not crucial, but might help bolster your arguments about the brine and fluid mixing. Detailed comments:

L11: constrains

Author response: corrected

Around L170: I suggest adding a sentence or two explaining that stretching of the fluid inclusions should have no effect on the measured Tm,ice, because stretching does not modify the composition. BUT, if the inclusions underwent any degree of leakage, then this would render the observed melting T's uninterpretable (owing to unknown degrees

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of H2O loss, which previous studies have shown to occur preferentially when inclusions partially leak). Hence, I assume that you did careful petrographic examination to confirm that there was no evidence of partial leakage. This should probably be stated. Author response: This is a good point, and the text has been revised to include this discussion.

L220: A very minor comment, but it is awkward phrasing to say that “Secondary minerals include primarily...” I suggest to re-word Author response: We reworded this sentence.

L261: I do not understand this sentence: “Where present, single-phase fluid inclusion aperture is <15 μm.” Please rephrase and clarify.

Author response: This sentence was rewritten for clarity.

L265: “are generally inferred as <50 âŮęC” – This is a bit misleading. Nucleation of vapor bubbles in high-density inclusions definitely depends on inclusion size (smaller inclusions are more likely to be monophase), and even inclusions with nominal Th as high as 150âŮęC sometimes fail to nucleate bubbles. The relationship with inclusion size should be noted here, and I would shy away from setting a rigid threshold at 50âŮęC.

Author response: Thank you for pointing this out; the text has been modified to include this information.

I would delete Eq2 and the sentence that precedes it. Just say that salinity was calculated using the equation of Bodnar ’93.

Author response: We have changed the sentence as suggested and removed the equation.

L305: are used to estimate (not “are used to estimates of”) Author response: corrected Around line 315: This is nice – the crux of the paper. Author response: We agree, thank you.

Around line 325: See my “main” comment above.

Author response: See above discussion. L327: Personally, I would use the term “meteoric water,” instead of “meteoric groundwa- ter.” Simply because the term “groundwater” is sometimes used interchangeably with basinal or connate water. I’m not advocating for that (I find that even more confusing but just for clarity and to avoid confusion, why not “meteoric water?”

Author response:Where appropriate, we have changed meteoric groundwater to meteoric water.

Around line 425: Don’t your fluid inclusion observations provide an additional argument against a role of CO2 degassing? Because of course, if CO2 degassing was occurring, you ought to find vapor-rich inclusions dominated by CO2. From what I can tell, there is no evidence of free CO2 in your dataset, right? Maybe worth mentioning.

Author response: This is a good point. We do not observe any vapor dominated fluid inclusions. We have added this detail to the discussion.

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