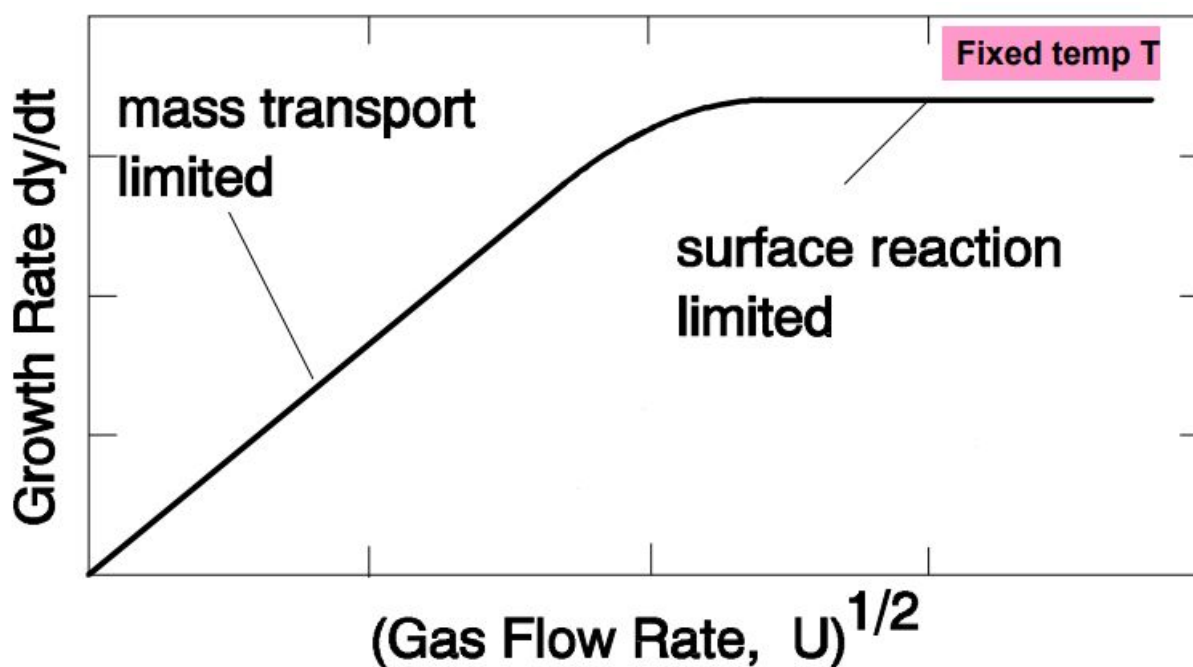


Dear Dr Del Sole,

Thank you for diligently addressing my previous comments on your manuscript. I find that this manuscript is much improved from the previous, particularly in the areas of readability/grammar and in providing a more nuanced discussion of data interpretation. There are, however, a few outstanding issues that need to be addressed. I would like to reconsider this manuscript following minor revisions. My hope is that this will be the last round and afterwards we can proceed to publication.

Below you will find several comments related to grammar and presentation. These are extremely minor, as your most recent revision made major advances in this area. The manuscript in its current form reads very well. More major concerns are present with respect to two areas of the presented science.

“Slow Down” Effect: The first, is that in your new discussion of the “slow down” effect, you mention that your inference is that the rate of the surface reaction is the rate-limiting step in cement accumulation. You then posited based on this inference that if flow is “fast”, then the reaction “may not have time to occur”. This is entirely counter to the classical geochemical view of rate-limiting reactions. Specifically, if the rate of the surface reaction is the rate-limiting step, then the overall rate of calcite growth is *invariant to transport rates*. See this image that I admittedly just pulled off of google, but it illustrates the point well.



If calcite growth is transport limited, then growth rates are proportional to the rates of transport. If it is surface reaction limited, then the rate of transport makes no difference. Thus, if you wish to continue to argue for the “slow down” effect as differentiated from solute sieving then that is no problem, but your explanation of it must be consistent with the broader body of knowledge

considering reactive transport. In short, I think your proposed mechanism needs a bit more thought.

Oxygen Isotope Interpretations: I appreciate your doing the calculation to estimate fluid d18O values, but in this version you simply mention that you did that and that they are consistent with your interpretations. The values need to be: 1) stated explicitly in the manuscript (rather than pointing the reader toward the Supplement); and 2) discussed relative to the *expected* values for meteoric fluids in the study area. The values you list in the supplement are a bit heavy for meteoric fluids - is this consistent with what has been observed for modern precipitation/ground water in the study area, or better yet, any values obtained from paleoclimatic studies?

See: *Giustini, Francesca, Mauro Brilli, and Antonio Patera. "Mapping oxygen stable isotopes of precipitation in Italy." Journal of Hydrology: Regional Studies 8 (2016): 162-181.*

Line Referenced Comments

89: As this sentence is currently constructed, an additional comma after "between" would be preferable.

92: Suggest rephrasing for english grammar clarity: "Our study also allows evaluation of the impact of..."

252: Thank you for adopting the more standard azimuth notation. Something has gone wrong here though - N334E?? That does not make sense. It should just be left at "334°". The N and E information are redundant. Please address here and throughout.

544-546: There is a problem with the logic here, in that a classical view of surface reaction limited growth implies that the overall growth rate is invariant with respect to the rate of transport.

555-560: The point I was making in my previous comments was that we very frequently see preferential calcite cementation of deformation bands that do not record significant cataclasis or fracturing. This implies that there must be something else (or at least something in addition to the role of fracturing) in helping to localize calcite cements in those structures. This makes them different from quartz cements, where the main thing seems to be fracturing. You do not need to change anything here. I bring this up only as an interesting point of discussion and potential impetus for future work.

585-590: I appreciate your adding this paragraph in response to my comments, but I do not think you actually need it. The other modifications you have made to this section make it a bit superfluous. You are welcome to keep it if you chose, but its elimination may make for more concise reading.

611-621: This still needs work. The opening sentence should more reasonably stated that "Oxygen isotope data also support a meteoric environment" or similar, and then go on to state the supporting data. The negative nature of the data say nothing on their own, and I also find the notion of any particular $\delta^{13}\text{C}$ value being indicative of meteoric environments preferentially questionable. You can certainly discuss aspects of the $\delta^{13}\text{C}$ signal that are *consistent with* a meteoric origin, but I do not think they can be an independent indicator. Oxygen, however, can. Toward that end, a bigger problem is that you mention calculating the fluid $\delta^{18}\text{O}$, but do not discuss the inferred values here? Its a fundamental data type you argue to support your interpretation - it cannot be relegated to the supplemental information and not stated. The calculation can reasonably reside in the supplement, but you need to actually state the inferred fluid $\delta^{18}\text{O}$ values.

I see in the supplement that the values you calculate are between about -7 and -3. These actually strike me as slightly heavy for meteoric fluids, but those do vary by quite a lot over the surface of the earth. You will also need some brief discussion as to whether these values are consistent with meteoric fluids in the study areas, either now or from a paleoclimatic perspective around the inferred time of cement formation.

633: It is your inference that calcite precipitates where fluids slow down in the vicinity of DBs.

650-655: Same problem mentioned above.

731: Please plan to address issues with isotope interpretations here in the conclusions as well.