Review of "Fault-related dolomitization in the Montagna dei Fiori Anticline (Central Apennines, Italy): Record of a dominantly pre-orogenic fluid migration. Se-2018-136

This manuscript presents field, petrographic and geochemical data from non-stratabound dolomites in a complex tectonic setting, and interprets their geofluid origin (parent fluids, timing) in the context of the tectonostratigraphic history.

It is coherent and logically organised. Aspects of the written English need minor improvement (punctuation, plurals, word order, etc); it will benefit from a final revision by a native English speaker.

The data are generally of good quality, and the interpretations are mostly justified from the results presented. In any study such as this, with limitations imposed by the ability to sample all the phases, there is necessarily some latitude or flexibility in the deductions that can be made. However, the authors do a good job of considering alternative possibilities for the fluid sources and timings.

My only issue with the paper is that the authors have not really considered whether there are wider implications or generic advances that can be made from the research. It presents itself as a case study, albeit one with a good integration of structural and diagenetic data. But what is the wider impact that will attract a non-specialist readership? Within the paper the authors all but admit that their findings are only modestly advanced from those of Ronchi and co-workers fifteen years ago (lines 638-642). I had hoped to see more progression in the science, and maybe the authors need to more thoroughly and critically evaluate the Ronchi model in the light of their new data. They could also work the structural data more – rather than just considering fault orientations and timings, what about the character and extent of the damage zones associated with different fault types / generations, and their relationship to the size and shape of dolomite bodies? What determines the lateral extent of dolomites? Is it other faults / fractures, or a gradual reaction front?

One generic aspect that the authors could address is implications for reservoir potential in analogues for this setting. The preponderance of planar dolomite is significant because planar dolomite is usually very beneficial for porperm (unlike many examples of hydrothermal dolomitization that feature tight nonplanar dolomites). Are there dolomitised plays in the Middle East that this study could be compared to (Zagros Mountains for example?), or maybe in Mexico?

Another factor of interest, largely by-passed in the text, is what drove the fluid circulation necessary to cause massive dolomitization when the low temperatures argue against a hydrothermal syn-rift system. Can the authors attempt a mass balance to estimate the order of magnitude fluid volumes needed? Maybe the dolomitization occurred in the down-flowing (cool) limb of a convection cell established on syn-rift faults that breached contemporary sea bed? Or does the dolomite zoning imply a pulsed fluid flow associated with strain cycling or seismic valving? I recommend the recent papers by Hollis and others on the Hammam Faraun fault and related syn-rift dolomitization. Are the D1-2 dolomites formed in a similar manner to this geologically younger example? Likewise, with the later dolomitization, which structures would have been open during compressional tectonics and able to serve as conduits for substantial fluid volumes?

If the authors address these issues their paper, which is already technically good, it will have much greater impact and interest across the sedimentary and structural geoscience community.

I have some more **specific comments** – these are tagged by line number or Figure number:

Line 43: The paper describes the role of evaporite-sourced fluids in the dolomitization process, but I am not sure that it illustrates a controlling role of evaporitic detachments. These may have influenced the tectonic development, but if it is believed that they directly controlled the dolomitization this needs to be specifically discussed later in the paper. Emphesized more in the text

Note the abstract is quite long-winded. It would be good to make it more succinct and punchier. Addressed

Line 69: The Castel Manfrino Dolostones are not labelled in Fig. 1 or Fig. 2b. Added in Fig.1

Line 73, 76: Did Ronchi (2003) base her study on the mapping of Mattei (1987)? Maybe there needs to be a couple of sentences describing Ronchi's findings so that it can be more clearly shown that the understanding has moved on. Added to text

Line 79-83: This is very long-winded and vague. It either needs to be shortened or to include specific details. The sentence is shortened.

Line 129: Given that the early dolomitization (D1-2) is later ascribed to the syn-rift stage, it would be useful to briefly describe the facies and architectural character of the syn-rift carbonates. For example, were they preferentially developed on footwall highs, in which case there was likely a juxtaposition of permeable high energy facies against faults that later hosted fluid flow? Addressed

Line 135-137: It may be a matter of debate, but the authors need to either provide the conflicting evidence or at least express a view and justify it. The sentence is deleted as further discussion is not the focus of this research

Line 152: Can the Montagna dei Fiori fault be indicated / labelled on Fig. 1d? Addressed

Line 164: Ground truthing implies that the features had previously been mapped out using remote data. If so, this should be included in the methods. Addressed

Line 167: So far as I can see, the Sibley and Gregg (1987) terminology (planar-e, planar-s, nonplanar) is <not> used anywhere in the paper, so either it needs to be incorporated or this sentence should be removed. The sentence and related references are removed

Line 186: Can the reproducibility (±0.1‰) be smaller than the precision (±0.2‰)? The inter-lab reproducability is ±0.1‰ means the measurement for the same sample in two different labs has a difference of ±0.1‰

Line 224: Bugarone Formation is not shown on Fig. 1 or Fig 2. Addressed
Line 231: Is the wider distribution of dolomitized intervals related to the topography of the valley and the exposures? If not, what is the relationship? Addressed

Line 238: I suggest not using "overprinted", which implies the original fabric / lithology is lost. Why not just use "cross-cut"? (or even just "cut") Addressed

Line 258 and elsewhere: "Dull" is not a colour! Addressed

Line 261 onwards: There could be a bit more detail on the dolomite distribution and fabric with respect to host rock facies. Is it all texturally destructive, is there any textural or mineralogical selectivity, were grainy or muddy facies preferentially dolomitized (controls by permeability versus reactive surface area…….?) Addressed

Line 272: There is an issue because CV1, CV2 etc. are introduced before they have been defined and described. I suggest starting section 4.2 with a paragenetic summary to alleviate this problem. Addressed, paragenesis is presented in Fig. 14.

Line 278: By using “frequently” the text suggests that sometimes (infrequently) D2 post-dates bed-parallel stylolites. Is that the case? Addressed

Line 284-285: This sentence needs a figure citation. Addressed

Line 297: Repetition of “euhedral to anhedral” (cf. line 295) – note this is not Sibley and Gregg terminology. Nor is “tightly packed texture” in line 279. Addressed

Line 305: I do not think one dolomite can “recrystallize” another. Recrystallization is a solid-state process that increased mineral stability. To demonstrate it might need data on the ordering, crystallinity and stoichiometry of D1/2 versus D4 (do the authors have any XRD data?). What is more likely is that D4 has locally replaced D1 and D2 by a dissolution-precipitation mechanism. However, the text lacks a clear description of the evidence for replacement. I recall papers by Mazzullo and by Machel that discuss this – it would be good to list the criteria for this case. Addressed

Line 330: In Fig. 11C, D the dolomite does not appear to be yellow-orange, it looks more like orange-brown. Addressed

Line 332: How wide was the extensional fault master plane? Please supply the range of widths (and lengths where possible) of the different vein generations. It is addressed for vein generations.

Line 335-336: What is meant by “with no evidence of physical disruption”? Does it mean that CV3 always passively overgrows D5 in voids and never cuts it? If so, it is easier to say this. Addessed

Line 336: Translucent is not a colour. Addressed

Line 334: What colours are the zones? Addressed

Line 367-368: Rather than “the presumable Lower Jurassic marine dolomites” – which are hypothetical – it is better to say the values are lower than those expected for Lower Jurassic seawater dolomites. Addressed

Line 391: “While…..” indicates there should be a second part to the sentence. Addressed

Line 396: What is the lithology of these samples? Addressed

Line 401: Please add a sentence or two on the fluid inclusion petrography and distribution – do inclusions follow growth zones or are they randomly distributed? Are they all primary or are some pseudosecondary? What are their shapes and what are the liquid:vapour ratios in the 2-phase examples? Also, in reporting the results for different cements please give the number of inclusions the ranges are based on (n=). Addressed

Line 409-410: What is the purpose of nucleating a bubble for measurement of freezing temperatures? Addressed

Line 477-478: This sentence appears out of place or at least needs clarification. More than two values are needed to demonstrate a progression. Addressed

Line 503-507: Yes, this makes sense if the veins are filling tension gashes associated with stylolites – such as system is likely to be buffered by the dissolving carbonate. Maybe make this point more explicitly, and contrast with vein types that were more extensive and would have allowed allochthonous fluids to pass through with minimal host-rock interaction. Addressed

Line 518: Hendry et al. (2015) did not discuss ^{13}C enrichment from CO_2 outgassing due to evaporation. They made the point that negative covariance in C and O isotopes within veins could be due to precipitation during CO_2 outgassing related to pressure changes. Addressed

Line 524-551: This is good but is a very long paragraph. Can it be made more succinct? Addressed

Line 550-551: The final sentence needs rewriting; what was confined, the thrust wedge or the fluids? Addressed

Line 556: In the preceding section there is very little mention of fluid mixing. Could the poorly correlated Th, salinity and stable isotopic data reflect precipitation from allochthonous fluids as they mixed with in situ fluids (and cooled)? Degrees of mixing (and of water-rock interaction) may have different from fault to fault – is it really likely that the hydrogeological systems was as simple as is being presented here? The obtained data show no systematic variations from fault to fault. Thus, existance of different local hydrological systems cannot be addressed. The sentence is revised ad completed

Line 575-579: Please rewrite this sentence – it tries to say too many things at the same time. Addressed

Line 584: Doesn't the displacement of D1-2 on these faults indicate that the dolomite formed before faulting? What is the critical evidence that it is genuinely syn-rift? The syn-rift deposits (Corniola Formation) is affected by these dolomites. Addressed more in the text

Line 590: if D1-2 were related to basement-cutting faults, why are the Sr isotope values much less elevated than for D3 and D4? There is no other alternative for radiogenic Sr source. This is the case for all of the dolomite types. Maybe less basement deriven fluids were involved in D1.

Line 656-657: Please explain how hydrothermal fluids were able to circulate in the compressional tectonic regime – which structures were able to be in tension and therefore transmissive rather than sealing? Addressed

Line 1139: The cross-cutting relationship in Fig. 8a, b is not very evident. Addressed

Lines 1142-1148: Should this discussion be in the main text rather than in the caption? This has been made to avoid a longer discussion.

Fig. 2b: It would help if the colours and ornaments matched Fig. 8a (e.g. Salinello Fm). The text size needs to be increased for better legibility. Addressed

The stereonet data are good, but very little use is made of them in the text of the paper. Addressed in the figure caption

Fig. 5: Please increase the text size and make 5c larger – it is too small to see clearly. Addressed

Figs 6-9, 11: Some of the CL images could be a bit sharper and maybe with increased contrast to better discriminate the dolomite types. Addressed

Fig. 12: The symbols for D3 vs. CV3 and mixed dolomite vs. CV1 are too similar (especially given the small size). I am also not clear how Fig. 12b relates to Fig. 12a; maybe split the legend between the two plots according to what is in them – that might help. Addressed

Fig. 14: How were the burial temperatures in the burial history determined? What assumptions are they based on? Addressed

Fig. 15: I like this figure but I'm still not sure what the fluid flow pathway is in (b). Maybe a broader tectonic context diagram is needed showing expulsion of fluids from the foreland (if that is where they are coming from?). The fluids were migrated from hinterland (now indicated on the sketch) rather than forland.

I hope these comments are helpful, and I look forward to seeing the paper published in revised form.

Jim Hendry

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