Response to Geoffrey Rawling (Reviewer #1) Comments

Ref: se-2020-81

Title: Structural control on fluid flow and shallow diagenesis: Insights from calcite cementation along deformation bands in porous sandstones

Journal: Solid Earth

Major comments	
Comment	**Response author**
This is a well –written paper that summarizes incredibly detailed field observations of the relationship between deformation bands (DBs) and cementation. The figures are very well done. There are only a few comments on language and grammar.	We appreciate very much the comments of the reviewer and the help to improve our paper. Suggested edits have been implemented and they are tracked in the revised manuscript. Please, consider that the track-changes Word tool created some problems with line numbering and its jumping on the annotated manuscript. Line numbering of the "revised manuscript version with changes tracked" and "manuscript without tracked changes" may not coincide. The line numbering we use in this document (Author Response to Reviewer #1) refers to the revised manuscript with tracked change file.
My main comments on this paper is that it needs restructuring for brevity and focus or change in emphasis. As it is the paper is a good discussion of two interesting field sites and a reasonable model for how the structures and cementation at these sites formed. However, as a paper in an international journal, I think it needs to be more broad.	Thank you for this comment. Following the comment of the reviewer, we moved some part of the field (former lines 180-185) and microstructural observations (former lines 315-324; 377-381; 389-392) to the Supplementary Material S1. We also moved the paragraph 7.1 to the Suppl. Mat. S1 as suggested by the Reviewer#2 and re-numbered the other paragraphs accordingly. We have created a Reference list in the Suppl. Mat. where we added the refs. that are cited only here and not in the main text.
I think there needs to be some explicit statements in the introduction and conclusions about what new insights are provided by this study and how they are relevant to outstanding questions relating to the control of fluid flow by deformation bands. Are fundamental questions being answered? Are ideas proposed elsewhere given a more robust foundation? I think a focus on how the observations presented are relevant to, or support, proposed processes for cementation as discussed in section 7.3 would be good. But prior to all of this, there needs to be some discussion of what the outstanding questions are and why the reader should care about them.	-We think that in the introduction we explain the rationale of the paper (lines 74-104), what lacks in the existing literature (lines 133-154), and what is it the aim of this paper (lines 155-164). In the conclusions, and in particular in bullet points 2, 3, 6, 8 we sum up our insights. -We have reorganized and rephrased the Introduction and, hopefully, now it is more in line with what asked by the reviewer. -The discussions about how the observations support the mechanisms proposed in Section 7.2 are made in the Discussion section (7) and in particular in Sections 7.1 and 7.2. Our discussion is also supported by field, microstructural, and petrophysical data published in past works.
I think these changes would give the paper much more focus and attract the readers interest immediately. The observations in the paper should be limited to those that are relevant to these questions and or are genuinely new. The maps and thin section figures are impressive and the	Ok, thanks for the comment. We have moved some field and microstructural observations, and Section 7.1 to the Supplementary Material S1 (see also response above in this file). We have reorganized and shortened other Sections as

descriptions complete and accurate but I found myself saying "What is new here? Is all this detail needed?" Many papers have contained such detailed observations of deformation bands and cements. What is essential to make the point?	well (see tracked changes). -We hope the paper is more to the point now.
As explained below some of the conclusions are vague. What data could help strengthen them? Recommendations are made that other workers should use the types of data presented here. How? A discussion of these two points would also strengthen the paper in my opinion and give it a wider audience.	Do you mean the discussion section? ...in the Conclusions no recommendations were made. We think you refer to comments on lines 733 and 740 (below in this file). Please, refer to the responses below. See also edit in the text (lines 1215-1217 and 1222-1224)
As an overall recommendation I suggest accept with moderate to major revision. Perhaps resubmittal and another review is necessary if there are major changes.	
Specific Comments	
50. "Fluid flow mechanisms...." I don't agree. The next 20 plus lines describe numerous studies addressing the effects of dbs on fluid flow, so it seems fairly well understood to me and heavily studied. And see the discussion and number of references listed in section 7.3. To justify the sentence in quotes I recommend stating clearly and explicitly exactly what is not currently understood and how the present study addresses and clarifies these problems.	Thanks for the comment. Now the introduction has been rephrased. We explicitly state what is known and which are the open questions this paper is trying to address (lines 92-164).
Figures 1 and 2. Clarify what is meant by DB azimuth. Is this strike or dip direction? The azimuths range from +- 90 relative to what geographic direction?	Ok, we have done that (lines 291 and 306). The azimuth data refers to the DBs strike and the ± 90° is relative to the north direction.
171. I would find it much clearer if you would use the average strike or dip direction to refer to the DBs rather than EESSWWNNWWE. Sorry, that's what it looks like on the page! You have calculated the mean orientations of the distributions in Figs 1d and 2d, so you could use them in the text.	We have added the mean orientations as suggested by the reviewer (lines 354-355). We did the same for Bollène (see response to comment just below). However, we would like to keep the cardinal directions.
244. Same comment as line 171. Reference to Figure 11b here should probably be 6b?	We have added the mean orientations as suggested by the reviewer (lines 445-447). See also the response just above. The reviewer is right about the fig. reference. We changed it accordingly.
305. The microstructural observations that follow here are exhaustive. To my mind all of these features have been described elsewhere in studies of DBs. What is new here? What is relevant and essential to the main points and arguments of the paper? If it is not relevant then it can go in a supplement and or briefly summarized. It seems to me that many of the microstructural observations could be replaced by permeability data and other hydrogeologic data by these authors (from their other papers?) or summarized from the literature. Such data are much more relevant to the hydrogeological model proposed in Sections 7.3 and 7.4.	Thank you for this comment. -Following the comment of the reviewer, we moved some part of the field (former lines 180-185) and microstructural observations (former lines 315-324; 377-381; 389-392) to the Supplementary Material S1. -We have added a table in the Supplementary Material S3 where we summarize the petrophysical data for two field sites along with their references We reference to it in the text (lines 867, 921, 940). We have preferred to not add these data in the main text, because those data have been already published, and so that the paper does not become longer than it already is.
591. Do you mean to say"...decrease of hydraulic	Ok corrected (lines 944-945).

conductivity dominates over the flow velocity increase caused by porosity reduction.....”	
624. I don’t think hypotheses is the right word here. The field observations tend to confirm the theoretical flow simulations and experiments etc. I think this should be emphasized more in the paper overall and mentioned in the introduction. The three mechanisms described in this section have been invoked in other field studies and/or examined in the laboratory, and field observations in this study suggests that they are all relevant and or viable as possible explanations for the cement distributions.	We thank the reviewer for this comment. We rephrased the text (lines 984-986), however we would like to stress the following points: -As far as we know, the flow “slow down” has never been invoked as a possible cause for cement precipitation in DBs. -Regarding the role of “reactive microfractures and fine-grained comminution products” on cement precipitation, we refer to several papers that propose or mention this model as relevant to the precipitation of cements around DBs (lines 952-964). -We do not say that we are hypothesizing a cement precipitation induced by solute-sieving. We suggest the applicability of this model to cement precipitation associated with DBs, whereas this model was proposed via experiment with an analog for fault gauge (Whitworth et al., 1999). Why should this be emphasized in the introduction? We think that this is a sort of conclusive statement. In the introduction we state our aims (lines 155-164) and the 3 mechanisms are widely discussed after.
634. it would be good to add a line or two somewhere about basic interpretation of cathodoluminescence colors for those who aren’t familiar.	We have added a few lines in the methods where the controlling factors of CL characteristics (visual colors and intensity of emission) are summarized (lines 338-342).
676. Add a reference to the measurements of DB hydraulic properties, or better , add the data to the paper as suggested above.	We have added the references as suggested by the reviewer (line 1098). -We have added a table in the Supplementary Material S3 where we summarize the petrophysical data for two field sites along with their references. We have preferred to not add these data in the main text, because those data have been already published, and so that the paper does not become longer than it already is.
700. Can you expand on this? Are there any microstructural characteristics of the cement that would allow interpretation of its growth direction?	No, unfortunately we do not have enough microstructural evidences to assess the “net” growth direction of the cement.
685 – 715. In general this discussion about the Bollene site seems reasonable but not definitive and rather underwhelming. E.g., Statements like “ most likely” and “probably”. Can you rework it to say what is known about the flow patterns and diagenesis definitively versus not. What about the three mechanisms described in section 7.3. Which do you think are applicable here (and at the Italy site), or would that just be total speculation? What additional info would be necessary to better understand the flow patterns and how they are controlled by the DBs and the resulting cement diagenesis. Ie, what could be a future research direction here?	-Ok, we rephrased the discussion about Bollène in Section 7.3 (lines 1106-1181) to make the discussion more solid. However, we would like to point out that these are discussions and, here, we make (working) hypothesis and propose the most robust (from our point of view) mechanisms/model considering all the data available (our and from literature). “In geology there are rarely any absolute models” (cit. Charrach, 2020 – JSG). -We discuss these mechanisms, as also their applicability to both field sites, in Section 7.2 (former Sect. 7.3) (lines 937-980), and Section 7.3 (former Sect. 7.4) (lines 1031-1032; 1083-1085; 1094-1095; 1164-1166). Certainly, in Bollène, the second and third mechanisms are not applicable since there is no

	spatial overlap (or it is rare) between DBs and cement. We state this also in the conclusions (bullet point 4). -About the future research direction... that is a good point. We answered to that in the conclusions (bullet point 7). Flow simulations and cement precipitation modeling could be used to reconstruct paleo-fluid flow pathways; further explore micro-scale fluid flow and diagenetic mechanisms that drove preferential cement precipitation along DBs; and constrain the reaction kinetics. These tools will also be helpful to validate the mechanisms involved in cement precipitation along DBs proposed in this work.
726. I would say enhance rather than increase porosity reduction etc....	Ok suggestion taken and implemented (line 1204).
733. How would you include this information in a fault seal analysis. Give an example...it seems some sort of upscaling would need to be involved. E,g., Spatial density or proportion of cemented rock per unit area or length of fault? Something else?	Ok, we expanded on this point, and we added some references to studies in which DBs are incorporated into reservoir models and flow simulations (lines 1215-1218).
740. Again, give an example. If someone is working with seismic data to do a fault seal analysis or reservoir engineering study how does the present study help them predict where the reservoir compartments are arranged etc. Elaborate.	Ok, we have elaborated about how this study could be helpful during reservoir characterization (lines 1222-1224). However, we would like to stress the following points: It could be difficult with standard seismic to observe the network of DBs and the associated cement nodules because in most of cases these features are below seismic resolution. That is why a field (analog) study, such as that presented in this work, is necessary during reservoir characterization since it allows to: (i) define the geometry and orientation distribution of DBs, (ii) understand which sets of DBs are associated with cement; (iii) evaluate the cement distribution and how it is arranged with respect to the DBs; (iv) evaluate how the compartments are arranged. Such information should be then integrated with other data, such as the density and clustering of DBs, the volume of cement along DBs, the petrophysical properties. This information can be then incorporated in reservoir (flow) models employed for hydrocarbon production planning. Anyway, all the attributes listed above are somewhat implicit in the reservoir characterization workflow, so we would like to not repeat them in the text. -Through the text, we point out that DBs and related cements (SDH) are subseismic features (lines 27,93 1209,1224, 1274), and that the study of an outcrop analog can improve the characterization of these features and in general be useful for the characterization of a faulted sandstone reservoir (lines 1217-1224).
756. As noted above, I would say that the field observations support three mechanisms that have	The term “proposed” was substituted with “discussed” (Line 1250).

been proposed previously as relevant to the precipitation of cements around DBs.	-However, as stated above in this file to the comment at line 624, we would like to stress the following points: -As far as we know, the flow "slow down" has never been invoked as a possible cause for cement precipitation in DBs. -Regarding the role of "reactive microfractures and fine-grained comminution products" on cement precipitation, we refer to several papers that propose or mention this model as relevant to the precipitation of cements around DBs (lines 952-964). -We do not say that we are hypothesizing a cement precipitation induced by solute-sieving. We suggest the applicability of this model to cement precipitation associated with DBs, whereas this model was proposed via experiment with an analog for fault gauge (Whitworth et al., 1999).
763. Comment at line 685 in the text, apparently I missed this discussion in the text. Perhaps that section can be rewritten to more clearly state this. But then there should be some discussion as to why the difference between the applicable mechanisms at the two sites. Something about the host rocks, db microstructures, regional geology, regional flow patterns during cementation etc	-As stated above in this file to the comment at lines 685-715, we discuss these mechanisms, as also their applicability to both field sites, in Section 7.2 (former Sect. 7.3) (lines 937-980), and Section 7.3 (former Sect. 7.4) (lines 1031-1032; 1083-1085; 1094-1095; 1164-1166). Certainly, in Bollène, the second and third mechanisms are not applicable since there is no spatial overlap (or it is rare) between DBs and cement. We state this also in the conclusions (bullet point 4). -The second and third mechanisms are not applicable to the Bollène site simply because the cement do not spatially overlap the DBs (e.g. lines 969-971). -The difference in the cementation pattern between Loiano and Bollène could be related to several factors, such as the regional vs. local flow pattern and the hydrological conditions during cementation, to the fluid conditions and the rate of the process. To discuss the reason why this difference exists would be too speculative since, in this study, we did not cover all possible controlling aspects.