- Review of manuscript entitled

"*Polyphase tectonic, thermal and burial history of the Vocontian basin revealed by U-Pb calcite dating"*

by Boschetti et al.

We thank the editors and reviewers for their time and insightful comments on our manuscript. We are particularly encouraged by the positive and stimulating assessment from Reviewer 2, an expert in U/Pb calcite dating, who confirms the validity of our approach and the consistency of our core data.

Regarding Reviewer 1's comments, we acknowledge the differing perspectives on the integration of structural data and U-Pb calcite dating. While this is now a well-established methodology in tectonic studies, we have significantly revised the manuscript to clarify our approach and its justification. Specifically, we have rewritten the introduction to more clearly frame our research questions and methodological framework. We also enhanced the abstract to better highlight the novel data and key findings from our study. We also meticulously reviewed the entire manuscript and supplementary material for clarity and consistency.

Our detailed point-by-point responses below address each of Reviewer 1's concerns, providing robust scientific justification for our interpretations. We are confident that these revisions have substantially strengthened the paper and hope it is now suitable for publication in *Solid Earth*.

General comments

The authors aim to describe the tectonic, thermal and polyphase burial history of the Vocontian Basin. However, this very ambitious undertaking is not entirely successful, and the resulting discussion is unconvincing. Although the authors present a wealth of new data, making this a potentially interesting and publishable contribution asks for more work.

We acknowledge Reviewer 1's point that constraining the thermal and polyphase burial history of the Vocontian Basin is an ambitious goal. To meet this challenge, we present an unprecedented dataset—integrating 16 novel U-Pb calcite ages with structural analysis and 20 new RSCM estimates (complementing previous data)—that provides a robust foundation for a consistent, regional-scale tectonic reconstruction. This multi-faceted approach yields a coherent model for the basin's evolution. Please find our detailed point-by-point responses below.

The main problem with the current manuscript is that the data report is largely disconnected from the discussion.

We thank the reviewer for this insightful suggestion. Our paper presents new U-Pb ages obtained on veins and faults, combined with new RSCM temperatures and a standard basin study, investigating the burial evolution of the Vocontian Basin. The interpretation of these data, obtained at basin scale, allow us to discuss their tectonic implications at regional scale, which should reflect the tectonic evolution of SE France. We have now clearly separated the description of our core results from their interpretation within the broader framework of geodynamic events. This restructuring allows for a more focused results section and a significantly strengthened discussion, where the data are now thoroughly contextualized and put into perspective.

There are numerous inconsistencies and assumptions, and the reconciliation of the different data sets remains clumsy. It would be a shame to publish the current version, as the data, the authors' experience and the subject matter easily allow for improvement, but this would require rewriting parts of the current manuscript.

We thank the reviewer for this valuable feedback. In response to the comment on the discussion section, we have thoroughly restructured it. The previous two-point framework has been expanded into a clearer, four-stage model. This new organization provides a more logical and detailed progression of our interpretation, significantly enhancing the clarity and depth of the discussion.

The main point is that the discussion and main conclusions should instead be based on the data presented. I am not sure that the data presented allow for a large-scale interpretation of the geodynamic evolution of the Vocontian (the data are too sporadic and scattered for that), but I am convinced that they can provide interesting information and insights, which are not, however, fully discussed in the report.

As stressed in previous answers, we rewrote the discussion. As a result, this paper is quite classical in his approach in presenting new and significant data in a comparable way as many recent papers with a similar methodology (Jullien-Sicre et al., 2025).

In our view, the discussion and main conclusions presented in the ms are solely based on the data. They are confronted in the discussion to existing literature and data. This paper like most articles relying on field-based tectonic analyses is intended to link local data to the geodynamic history of the region (SE France). We modified Fig. 2B by adding precision about regional geodynamics. This new Fig. 2B is now more cited in the introduction to avoid any misunderstanding. Both the number of data (fault-slip data, U-Pb ages and RSCM analyses) and the scale of the study over the Vocontian Basin are sufficient for discussion at regional scale.

As presented, the manuscript resembles a data report accompanied by a discussion that, while not insignificant, is largely disconnected from the data. I therefore suggest major revisions to this manuscript. If the authors were to rewrite parts of the present manuscript, the following points could be taken into consideration.

We thank the reviewer for providing us with the opportunity to explain our work. In our view, this is exactly what a scientific paper should be: a report of data and analyses or experiments that are discussed for the evaluation of implications regarding clear objectives and questions. We do discuss the data how they agree, complement or contradict previous tectonic models. We reworked the two sections in the discussion, especially the comparison between our new data and already constrained geological evolution.

- The objectives are vague and need to be better defined

The introduction has been re-written to better highlight the objectives.

- The data are overinterpreted (particularly the paleostress data)

We thank the reviewer for raising this point regarding our paleostress inversion. The analyses of striated faults, veins and fault-slip data inversion are compared to U-Pb dates to provide a

coherent succession of tectonic events from which a tectonic scenario of brittle deformation is inferred. Our paleostress analysis was based on established methodologies for deriving principal stress directions from fault-slip data. However, we are keen to ensure our approach is as robust as possible. Could the reviewer elaborate more on their concern or suggest specific alternative methods or references for interpreting such datasets? We would be very interested to explore these avenues and incorporate this perspective into our work, should it strengthen the analysis.

- The links between the different data are not established, the presentation of the data lacks clarity, and the data points are difficult to locate (especially the RSCM data)

We thank the reviewer for highlighting this point. We have ensured that all samples are clearly located and visible on the maps (see Figures 2 and 6), with their precise coordinates provided in Table 2. Perhaps, the reviewer suggests that calcite U-Pb dating and fault-slip data cannot be linked to each other ? But the same calcite samples measured in the field in faults and veins have been checked by cathodoluminescence (all the data are presented in the supp. mat.) and then dated (there is a triple characterization of each sample). A paragraph has been introduced in the introduction to explain the objectives of the RSCM approach.

- The correlations are arbitrary, and the scaling of the data and the link to large-scale events are not constrained and are often more assumed than corroborated by actual observations (a more realistic and data-driven discussion would be welcome)

We appreciate the reviewer's comment. However, it seems there may be some confusion regarding how local structural data are linked to their large-scale tectonic implications. In fact, local deformation phases are directly dated, then compared at basin scale. After verifying consistency among the different sampling sites and measurements, these correlations allow us to propose a coherent tectonic scenario.

- The final interpretation of the various data seems fragmented, and the link between the data and their integration into a broader geodynamic framework remains unconvincing.

We appreciate the reviewer's comment on the discussion. We regret that the reviewer is not convinced by our approach of assembling tectonic data, ages (and other data) into a broader geodynamic framework. The scale of the study area covered by our local data is much larger than the crustal thickness of the region (i.e. well above 30 km). They have obvious tectonic implications at large scale. We have reworked the discussion to better connect our dataset to the regional geodynamic evolution, providing a more thorough and coherent interpretation.

Reading the manuscript, one gets the impression that a set of data is attempting to solve a geological problem without really being able to define it and offer a well-defined discussion of the data. In addition, the manuscript is repetitive and, in many places, the English could be improved.

We thank the reviewer for their engagement with our manuscript. Regarding the comment on the clarity of our geological problem, we have revised the introduction to state our objectives more directly. Our goal is to reconstruct the tectonic history of the Vocontian Basin using a well-established, multi-method approach. The reviewer notes that the data 'offer a well-defined discussion of the data,' but the overall assessment of the discussion's clarity seems negative. To address this effectively, we would be grateful for more specific guidance on which aspects of

the discussion they found unclear. Concerning the English language, the manuscript has undergone a thorough review by a native English speaker to ensure it meets the journal's standards.

More general comments that may help with rewriting the manuscript (at this stage, I am not referring to detailed comments, as I consider that the current version requires significant improvement and rewriting)

Abstract:

It needs to be rewritten. I strongly recommend focusing on the data and highlighting their implications for understanding the Vocontian Basin

We have thoroughly revised the abstract to provide a more comprehensive and precise summary of our key findings. While we agree the previous version was too concise, the new abstract now effectively highlights our core data and their direct implications for the tectonic evolution of the Vocontian Basin (VB), ensuring it accurately reflects the paper's primary contributions.

Introduction:

The large-scale geodynamic context and the small-scale/sample scale should be better linked. In the current version, it is not clear how the point data on veins, palaeostresses and thermal state can be linked to large-scale interpretations. It is clear that the authors see the link, but the reader should be able to follow their reasoning. An attempt should be made to define the actual objective of this study more precisely and to remain focused on that objective.

We have used complementary data obtained locally but distributed over a large domain (> 50 km x 50 kmin our case). When integrated at basin scale this dataset form the basis for discussion at a larger scale. We agree anyway that the introduction needed to be improved. It has been rewritten.

Geological context:

It is not clear how stratigraphic logs can be defined from maps alone. Why did the authors not rely on existing logs and/or drilling data (both of which exist and are available)? In addition, the geological context would benefit from being better structured. Why not start by describing the location/geodynamic context and its evolution (some parts of which are included in the introduction)? A second section could present the tectono-stratigraphy (which constitutes the bulk of the current geological context). A third section could summarise the existing data sets and interpretations, a section that is missing in the current geological context.

We thank the reviewer for their comments on the geological context and presentation. We have provided clarifications below to address their specific points.

-Each BRGM map is published with an explanatory note providing the average thickness of a given stratigraphic layer (we followed strictly these informations, available to everyone). These data are based on field survey and existing drilling data summarized in the note. The BRGM's synthesized average values are specifically designed to offer the most reliable and representative framework for a basin-scale reconstruction like ours.

-On the scope of the geological context provided, our introduction aims to succinctly present the key stratigraphic and tectonic history necessary to frame our process-oriented study. A highly detailed synthesis of local geology, which is comprehensively documented in the available BRGM literature, would fall outside the intended scope of this paper, which focuses on presenting and interpreting new geochronological and structural data.

-On the structure and narrative of the manuscript, we have carefully considered the reviewer's suggestion for restructuring the text. After reflection, we believe that our current narrative approach, which directly links sedimentary events to their tectonic implications, provides a more integrated and fluid discussion. We are concerned that a more segmented structure might introduce repetition and disrupt the narrative flow. We are confident that the current structure with the introduction (general framework and questions) and geological context (focus on data) allows for a clear and compelling presentation of our findings, but we remain open to specific suggestions to enhance clarity within this framework.

Sampling strategy:

The analysis of palaeostresses is based on the assumption that structures record the large-scale tectonic stress field that controls large-scale deformation. However, in a salt basin, the sedimentary sequences covering the salt may be decoupled and the deformation structures may not necessarily record the larger-scale stress field. The authors do not address this point, which could call into question the validity of the data.

We thank the reviewer for this comment about paleostress analysis. We would like to clarify several fundamental points to address the concerns raised.

The analysis of paleostress is not assuming a direct relationship between local structures and large-scale tectonic stress field. The team, F. Mouthereau especially, is very familiar with fault-slip analysis and paleostress inversion. The local state of stress is calculated from fault-slip data (a minimum of four is necessary to invert the tensor) using the approach of Angelier (1990). The calculation is based on the minimization (angle, or magnitude and angle) of the difference between the observed fault slip and the one predicted. The large-scale logic comes when similar fault patterns, stress regimes and states of stress have a similar chronology. This is constrained by crosscutting relationships, stress axes attitude relative to bedding dips and of course direct absolute U-Pb dating.

Regarding the comment on the state of stress in basin decoupled above a salt layer, it is an interesting point of view, which contradict decades of research studies in sedimentary basins and the external parts of orogens. In fact, theory and observations show that in salt-based thrust wedges (measured in sedimentary cover detached above salt layer like the Vocontian Basin), the stress field remains parallel to the regional stress field. Although deviations may occur near the décollement level, this effect is reduced when the basal layer is weaker, which is the case for fold-and-thrust best decoupled above the Triassic salt. This behavior is well documented in many fold-and-thrust belts across western Europe. In summary, we expect that the bulk of stress orientation in the Vocontian Basin remains oriented parallel to the regional stress axes. Our results support the theory and experiments, hence indicating the reliability of using local stress data to infer regional tectonic regimes. Of course local discrepancies may occur due to diapirism or large crustal-scale/lithosphere-scale faults but such structures were not the focus of this study.

Another weakness of the study is that the history of burial, as presented in the manuscript, is not really well defined. How, for example, is palaeobathymetry determined and how are uplift events (e.g. the Barremian/Aptian event) treated in the approach?

We thank the reviewer for raising this point regarding the burial history reconstruction. We would like to clarify our methodology. The calculation of total subsidence is a very standard approach in sedimentary basin analysis, provided that lithology and thicknesses are known. Standard compaction laws were applied to determine the decompacted thickness over time. This is explained in the text. We do think that the reader does not need an extended explanation of such a classical approach, which is far less technologically challenging in regard to other analyses carried out in this is study (U-Pb ages, RSCM). The aim of this study was to illustrate the first-order evolution of the basin subsidence in order to estimate maximum burial depths that could then be compared with the maximum temperature data determined using RSCM analyses. Bathymetry is usually difficult to determine, and uncertainties (max. 200 m) are small compared to the total thickness of buried sediments, which often reach several kilometers, up to up 5-6 km in the Vocontian Basin. Therefore, this uncertainty has little impact on a total subsidence. The most significant uncertainties are from the stratigraphic and sedimentary data themselves (ages, thickness, lithology) and the distribution of these errors across the area. Bathymetry however has a much greater importance for the calculation of the tectonic subsidence, as the sediment load needs to be removed. This is critical when one needs to evaluate which of the tectonic activity or sea-level variations explain the stratigraphic data. We explain this better in the revised version of the text. Regarding the 'uplift events' supposed to have occur during the Barremian/Aptian event, our data do not resolve such an uplift. This is shown by the deposition of marine deposits.

Discussion:

There is a significant disconnect between the discussion and the data set presented. It is difficult to understand how the new age provided actually constrains the geodynamic evolution of the Vocontian and how the few point data can allow for a discussion of the geodynamic evolution of the Vocontian basin.

We thank the reviewer for this comment. We wish to clarify the fundamental contribution of our new data. While previous understanding of the tectonic evolution relied on relative chronologies from cross-cutting relationships, our study provides the first direct, radiometric ages for calcite mineralization directly linked to deformation structures. This unprecedented dataset allows us to move from relative timing constraints to an absolute geochronological framework. This directly quantifies the timing of tectonic events, thereby offering a more robust and calibrated foundation for reconstructing the evolution of the Vocontian Basin. We believe this represents a significant advance in constraining the regional tectonic history. In addition, it should be reminded that we date local phase of deformation, but this is the consistencies of ages, fault types, stress regimes, between the different sampling sites that lead to propose a tectonic scenario. All materials are presented and discussed.

Indeed, if the aim of the article is to discuss the geodynamic evolution, a figure similar to Figure 12 should be inserted in the geological setting and questions should be formulated that in turn can be answered with the here presented more accurate data.

We thank the reviewer for this suggestion. After careful consideration, we have decided to retain the current structure, Adding a figure similar to Fig. 12 would be redundant. Since our

goal is to place our results within the context of existing plate reconstructions, presented Fig. 12 at the end appears appropriate.

The evolution of the burial is difficult to understand solely on the basis of the manuscript presented in this article.

This comment appears unclear especially the following sentence 'manuscript presented in this article,'. It is not indicated how the result on the analyses of sedimentary burial is difficult to understand. To address the reviewer's concern more effectively, we would need to know which aspects of the burial history reconstruction is found difficult to follow? Temperatures inferred from burial depths are compared and discussed in section 5.1. Perhaps this was not clear enough in the previous version. This part has been reworked.

The authors conclude that rifting is not limited by the U-Pb geochronology of calcite: if I understand correctly (but I am not sure), U-Pb dating of calcite was only performed on lithologies from the Upper Jurassic to the Lower Cretaceous. If this is true, it is not surprising that the veins do not record Jurassic rifting, as the rocks containing the veins are post-rift and therefore the veins cannot record Jurassic rifting events.

We thank the reviewer for this comment. We don’t see where in the ms we say that rifting is not ‘limited’ by U-Pb dating ? and what does this mean ? It is obvious that veins can’t be older than their host rocks. As indicated in the original manuscript we targeted normal faults and veins for U-Pb geochronology that were described as syn-rift faults by Homberg et al. 2013 which formed shortly after deposition. We conclude from the U-Pb dates obtained on most of the fault with Ca-rich fluids occurred after deposition and are post-rift. This allows us to clearly differentiate between the syn-rift Jurassic-Lower Cretaceous series and the subsequent post-rift deformation, providing a critical new constraint on the tectonic evolution of the basin. These new data are discussed to help guide the reader toward possible interpretations.

The discussion of Ca-rich fluid circulation is somewhat superficial: either it is retained, but then needs to be substantiated, or it is omitted.

We seek clarification on this point. The preliminary discussion is based on the documented absence of syn-rift ages in our dataset, which contrasts with some previous models in which most veins result from intense syn-rift fluid circulation. Could the reviewer please specify what additional data or substantiation they believe is required, or clarify why this observation should be omitted?

The short discussion on Ca—rich fluids is an attempt to explain why we did not find fluids of Cretaceous ages in faults and veins formed during the Vocontian rifting, while it is independently known that deep mineralizing fluids, mainly brines, have circulated during rifting. We suggest different hypotheses which could be useful to investigate in the future. This discussion is entirely related to the topic of the paper on temporal evolution of the Vocontian Basin, and motivated by our new data.

Age correlations: it appears that the analytical ages correlate with large scale tectonic events, but do these ages actually record and result from a large scale tectonic event or is it just locale deformation? The U-Pb ages of calcite, particularly if the veins are polyphase, as seems to be the case for some of them, are probably more complex, and the presentation here seems to be

an oversimplification of the ages, which are in reality more difficult to interpret. A more detailed discussion of the ages would be desirable.

We thank Reviewer 1 for acknowledging the correlation between our analytical ages and large-scale tectonic events. We agree that this is a key finding of our study. This correlation provides a robust justification for interpreting our local paleostress data within the regional tectonic context. As established in structural geology, local deformation features, when they exhibit consistent stress regimes and synchronous ages across a wide area, are recognized as valid proxies for regional-scale deformation.

We reiterate that our paper like most field-based tectonic analyses is intended to link local data to the regional tectonic history. The veins and faults indeed show a polyphase evolution. These data have carefully separated based on geometrical/mechanical arguments, then grouped according to our new U-Pb ages. CL analyses demonstrate we are dated a fluid event that can be attributed to a given tectonic event.

Citation: https://doi.org/10.5194/egusphere-2025-3332-RC1

- **RC2**: 'Comment on egusphere-2025-3332', Anonymous Referee #2, 11 Sep 2025

I can only comment on the calcite U-Pb technique and its contribution to the paper, I do not know much about the area nor its sedimentological record, stratigraphy or tectonics. From the calcite U-Pb point of view there is no issue whatsoever with the presented data and I really like its "novel" approach using the isotopic mapping by LA ICPMS referencing the Hoareau et al (2021) paper. It shown nicely the potential of the technique and serves as a good practice for other labs worldwide. The data are convincing, well presented with proper statistics and uncertainty quotation. That does not say anything about the correct interpretation of the presented data, only it lays good analytical grounds for it.

Citation: https://doi.org/10.5194/egusphere-2025-3332-RC2

We thank Rev. 2 for this positive feedback on our work and data! We agree that the method is developed is powerful.

Refs

Angelier, J.: Inversion of field data in fault tectonics to obtain the regional stress—III. A new rapid direct inversion method by analytical means. Geophysical Journal International, 103(2), 363-376.https://doi.org/10.1111/j.1365-246X.1990.tb01777.x, 1990.

Jullien-Sicre, A., Missenard, Y., Blaise, T., Augier, R., Parizot, O., & Haurine, F. (2025). Timing of contractional stress propagation, from the Pyrenean orogen to the intraplate domain, evidenced by U-Pb dating of syn-kinematic calcite. Tectonics, 44(2), e2024TC008634.