



Review Report

Aabø et al., Quantitative Analysis of Natural Fracture Networks in Maastrichtian Chalk: Insights from a 7,000 m² Digital Outcrop Model of the Rørdal Quarry, Denmark, TEKTONIKA, 2025.

Table of Contents

Notes from Executive Editor (EE)	2
1st Round of Revisions	3
Comments by Reviewer 1	3
Comments by Reviewer 2	5
Authors' Reply to Reviewer 1	10
Authors' Reply to Reviewer 2	13
2nd Round of Revisions	15
Comments by Executive Editor	15
Authors' Reply to Executive Editor	16

Notes from Executive Editor (EE)

When the manuscript was initially submitted, the Associate Editor (AE) requested major formatting changes in order that the paper be reviewed. After resubmission, the AE reported that the format changes were not fully implemented. He also expressed difficulty in finding reviewers. By mutual consent, the EE took over the responsibility for finding reviewers. EE decided that the paper was in fact reviewable and secured two reviewers. The reviews were received early 2024. They were extremely thorough, and although generally positive, required substantial changes to both the length and content. AE initially dealt with the received reviews, but the EE then took over the responsibility for the paper. Revisions were eventually received October 2024. Although one of the reviewers had recommended a second review, EE decided that, given the diligence shown in the revised version, the only further review needed was formatting and a few technical changes at the EE's request. Many of these changes were handled by email, of which the key messages are included in this report.

1st Round of Revisions

Comments by Reviewer 1

Tektonika review, Aabø et al. (2024)

This is a well written manuscript that is easy to read and of great interest to many. A 50% shortening would improve the text and help get the new information across. It reads more like a condensed (high-level) master thesis than a typical journal paper. There is also some essential information that is missing, as mentioned below. I like the paper though and recommend publication after some revision.

Abstract: A good thing about the abstract is that it deals with results, and it is easy to read. It can be shortened a bit.

Sections 1-4 all focus on introductory stuff and previous work. It may be good for someone unfamiliar with fractures and reservoirs (students), but too lengthy for most of the readership, I think.

Section 5, “Previous fracture related work...” informs us that we will look at earlier work, but that is actually not the case (change that heading). Instead, most of the methodology is presented in Section 5. 5.2 promises fracture characterization, but still is mostly about methodology. The fractures are not characterized anywhere as such (just their length/height). So here is an important point:

It is essential that fracture characterization is done also by use of thin section and/or SEM images. Are the extension fractures only separated on the basis of orientation? Are they all of the same type (thickness, no mineralization)? What is the aperture of the extension fractures?

Section 6 is ok, but again, needs to be condensed. 6.2 is, like 5.2, supposed to be on fracture characterization. I think it is more about the characteristics of the fractures than the characterization, so perhaps find a better heading (Fracture characteristics may work). Again, fracture type (if there is any difference between them) should be combined with orientation data. Also see comments in the attached file.

Consider adding a few horizontal scanlines along the data shown in Fig. 18, scanlines that show intensity variation, one way or other. This is because such graphs (scanlines) are commonly presented in the literature.

Discussion

Too much repetition of stuff from the Introduction and elsewhere. This is supposed to be discussion of data and results, not repeating what has already been said and described. 7.1-7.3 can be cut by 75%, It contains a lot of repetition. 7.4 is interesting, but it is too much description with too little discussion and regional-scale interpretation. How about discussing possible

strike-slip kinematics in relation to fold orientation and fracture orientation, and perhaps more on the relation between folds and fracture density? Age relations come in here too (between fracture sets and to fold formation). There may be no relation, but it is good to state that too. Also, it is important to understand how/why the fractures formed (tectonic setting) when applying the data to other areas, including offshore reservoirs.

Here are two things that need to be discussed in the Discussion section:

Stress: If the fractures are joints, they reflect stress orientation at the time of formation. σ_3 changing between quarries. Put this on a map together with data from other quarries? Two sets of extension fractures in a single quarry also indicate two σ_3 directions. Discuss this. Did the two populations (orientations) form at different times, or are we looking at local stress perturbations, for example around faults? Which of the two sets is related to strike-slip and which to extension? Not clearly discussed. As mentioned above, also involve fold attitude in this discussion.

Fluid flow: Will be anisotropic due to preferred orientation of fractures. Worth mentioning in the discussion. Actually, fluid flow is not really discussed in the light of the nice data set that you present. Should be. Also of interest how the (subseismic extensional fractures are oriented relative to nearby faults. Focus more on these things.

Altogether, very solid work, great data that will be useful for many.

Also see text file with some additional comments and suggestions.

Comments by Reviewer 2

For author and editor

Referee's comments to T.M Aabø, S.J Oldfield, L. Stemmerik, and L. Nielsen: Fracture characterization of Upper Cretaceous chalk onshore Denmark: applicability and implications for subsurface fracture modelling

Technical comments to the manuscript

There is no line-numbering in the manuscript I that I downloaded, which hampers the review process. I therefore applied automatic line numbering-option in Word. I assume that the editors/authors easily can reproduce the line numbers that I used in the review comments below.

General comments

Although the English language is generally fair, the authors sometimes tend to use too complex (and sometimes clumsy?) phrasings (example line 193: "...the dominant flow direction was observed to be in an ESE direction". Would not "..... the main observed flow direction was ESE" be better?

I suggest that the authors get a renewed language wash with emphasis on equivocal phrasings and grammar.

The manuscript generally contains few misprints. Exceptions are frequent format errors of the type "s_1" for s_1 (line 224). These should be identified and corrected.

Conclusion and recommendations

The paper has potential to become a valuable contribution to the understanding of fractured carbonate reservoirs. In my opinion, however, I do not feel that the authors do not fully exploit the full scientific potential of their (exceptionally) large database. I think this is because they do not formulate specific scientific targets in the beginning of the paper. I think the authors knows the best what such targets should be but I can suggest topics like: comparison with similar reservoirs in comparable or contrasting settings, the influence of bed thickness and clay content and estimation of bulk liquid flow as a function of 3D fracture networks (see comments to Discussion). The authors touch on these items but leave them without deeper discussions.

I therefore suggest that the authors are given a positive recommendation to mend the shortcomings and come back to Tektonika with a revised version.

Specific comments:

Abstract

General: Abstract should be modified to include the actual results/findings of the paper. In its present format it is rather a general introduction to state the potential of the study and we don't learn very little about the results.

Lines 7-8: Statement should be modified: The analogue value is restricted to reservoirs of similar lithology, burial history and tectonic setting.

Introduction:

Line 36: micron-sized(?)

Line 39: “structural framework of the fracture network”? You mean “3D geometry and interconnectivity of fracture networks”? or what?

Lines 41-43: Please state why this is a good analogue (beyond the lithology)

Lines 47: Should this sentence be simplified?

Lines 51ff: This is generally correct but could be said more clearly by distinguishing between 1) tectonic setting and fracture intensity 2) geometry of fracture network and 3) (mechanical/lithological) characteristics of individual fractures.

Line 68ff: Comment: I do agree with the authors that dynamic geomechanical approaches to predict fracture distribution in the subsurface is much preferable to stochastic methods.

Study area

Lines 124ff: A reference to Figure 2 and/or addition of a lithostratigraphic column should appear at the beginning of this section.

Line 138ff: “Heavily fractured” does not tell much.: Some general statements on max and min fracture frequency and general distribution pattern/orientation and position relative to main faults (without going in details and perhaps with reference to later descriptions?) would tell more. (Would a generic diagram (e.g. of the kind shown in Fig.8 in Gabrielsen, R.H., Aarland, R.-K. & Alsaker, E., 1998: Distribution of tectonic and non-tectonic fractures in siliciclastic porous rocks. In: M.P.Coward, T.S.Daltaban & H.Johnson (eds): Structural Geology in Reservoir Characterization. Geological Society of London Special Publication, 127, 49-64) be of help here?)

Lines 173ff: Is it true that fractures are randomly distributed as referred to in Odling’s works? Whether yes or no: This should be stated in the description in lines 138ff.

Line 178: It may be obvious from the abstract and the general description, but please clarify that “As exposed in 2016 to 2013” refers to the excavation progress in the quarry. It would help with a reference to Figure 3 here. (This figure ref seems to be generally omitted (see comment to Line 240ff).

Line 184: **Folding** is a dynamic process that is rarely observed, whereas **folds** can be described in the field. **Folding hinges** is therefore a meaningless term (line 187).

Previous fracture study.... Rørdal Quarry

See comment to line 239ff below.

Danish onshore fracture populations

This is a somewhat misleading heading: I don't think the authors want to describe all Danish onshore fracture populations.

Lines 199 ff: The authors should define what they mean by "tectonic strain". I guess the mean strain imposed by far-field stresses, excluding local stress associated with surface/bathymetric or subsurface reliefs, also excluding burial, liquid pressure and thermal gradients? Please specify.

Line 292ff: It is a little alarming that bedding-parallel fractures are excluded from the fracture characterization (and modeling?), since this fracture population may play a key role in the 3D-fluid communication system. This item should be discussed and explained. E.g.: are they stress release structures associated with unroofing? (please explain effects on calculated flow (lines 387-396).

Previous fracture-related work Rørdal Quarry

Line 239: Why are headings 3 and 5 the same? Could these sections be merged. The way it comes out now make the reader suspicious that the manuscript was not planned well.

Line 240 - 249: This section jumps directly from Figure 2 to Figure 4

Line 249ff: For the reader not to lose track on the logics in this paper, the authors should present a sketch map here showing the positions of the main structural elements and particularly **the northern and southern faults** and their related structures. Perhaps the classical concept of structural subareas could be of help here?

Line 366: Does the splay node act as indicator of connectivity take into account fracture mode? Please say, since fracture mode and shear intensity in mode II-fractures reflect connectivity. Please explain and set I relation to the situation in the Rørdal Quarry.

Line 384: V_c values = crack shear force? Please explain.

Line 397ff: See comments to line 292ff.

Results

Line 406: The description comes on very abruptly and it is hard for the reader to orient himself. See comment line 249ff.

Line 406 ff: The descriptions of the faults seem a little whimsical and does not use traditional structural geological terms consistently (e.g. fault zone). This can be said in a more rational and precise way, e.g. "The northern fault has a top-SE normal displacement of xxx meters juxtaposing the Hvidskud and Sigerslev members (say which is the hanging and which is the footwall). It is enveloped by a 7 meters wide damage zone and consists of four segments." Then the characteristics of the fault as imaged by different tools can be described and additional information can be added.

Line 423: What does “additional faulting” mean? No additional (2nd order) displacement? Or no more faults?

Line 428ff: see comments to line 406ff.

Line 433: I don’t understand this. “... appears before...” in what context? Please explain properly the geometric relations here and please use Figures 8 and 9 more actively. The reader can indeed collect information from these figures, but the descriptions in the main text should still be precise and unequivocal. I think the authors should put some effort into explaining the structural relations in an established structural geological nomenclature (e.g. Riedel and Riedel’-shears, strike slip duplexes or whatever).

The folds associated with the fault should be set in the same context.

Line501ff: Please explain how fractures related to glacioeustatic processes were distinguished from those of “tectonic” origin. In addition, please comment on structures of glaciotectonic origin (short-field effects) that are common throughout Denmark (e.g. Pedersen 1993; Jakobsen 1996) from fractures of “true” tectonic origin (far-field stress). Alternatively explain why this distinction is not relevant.

Line 477 ff: Fracture orientation should be set in relation to proximity to major structure, separating between “back-ground fractures and zonation of major structures like faults (e.g. Gabrielsen, R.H. & Braathen A., 2014: Models of fracture lineaments – Joint swarms, fracture corridors and faults in crystalline rocks, and their genetic relations Tectonophysics, 628, 26-44, doi:10.1016/j.tecto.2014.04.022), again highlighting the use of concepts like structural subareas (see comment 249ff).

Line527ff: Again, fracture spacing should be related to the position relatively to larger structures like faults (Gabrielsen & Braathen 2014; see above).

Discussion

General comments: The discussion could easily be more focused: In its present form the first 2-3 pages is more a summary of what is previously said in the paper.

1. 22-25: Much of this could be transferred to the Introduction and erased in the cases where this is already stated there.

Section 7.2: The discussion would gain much if one (or several) topics of discussion are formulated and **expanded upon**, e.g.: Comparison with fracture patterns of carbonate reservoirs in other study areas **in light of similar or contrasting structural parameters** (such as burial history, p,T, stress field etc.). In such context the absolute orientation of the fracture population would perhaps be less important than their relative orientations.

Lines 781ff: I am not sure that fracture lengths are of paramount interest *per se*, unless this is linked to the study of mechanics of fracture growth mechanism -

which is beyond the scope of the present study). However, the mechanical stratification and the relation between bed thickness, clay content and fracture frequency/fracture length would be interesting. Such studies in siliciclastic sediments are abundant, but less common for carbonates (as far as I know).

In the evaluation of the fluid communication, it would be interesting to know to what extent the bed-parallel fractures (that I understand was omitted in the study?) would influence the result.

Line 727: I suggest that the use of the term “lineament” should be avoided in this context.

According to O’Leary (1976) a lineament is “a linear or curvilinear feature, which is **identified by remote sensing methods**, and which is believed to represent the trace of intersection between a planar or subplanar structural inhomogeneity (such as a fault) and the surface of the Earth». If a lineament is confirmed to be a fault the nomenclature should reflect this (Gabrielsen & Baaathen ;2014).

Figures

Figures are generally of good quality, but care should be taken for Figs. 5,11, 12, 18 and 20, where necessary details easily can be lost in the graphical reproduction/printing.

Figure 8: Possible model? Are there alternative interpretations? Which?

Figure 10: The identification of the fault in these data is less than convincing. Was the fault trace picked automatically by Petrel? Which were the criteria for identification of the fault reflection(s)?

Figure 11: Purple arrow identifies normal fault. Please say so.

Wouldn’t one of these lines magnified, produce a better representation of the structures in question?

Line 1144: Misprint

Figure 12: Folds on different levels have contrasting amplitudes and wavelengths and top of folds are placed differently. Please comment. Not highlighted in text (line 464ff).

Figure 14: See comments to main text.

Figure 20: From this graphics I could advocate different interpretation of the northern fault zone, but I assume that the authors have support from additional data?

References

Not controlled by me, neither when Tektonika-format nor when actual references are concerned.

Oslo February 5, 2024,

Authors' Reply to Reviewer 1

We highly appreciate the thoughtful and constructive feedback on our manuscript. The following responses detail the revisions and clarifications made in light of the comments provided, which we believe have significantly strengthened the quality and clarity of the work.

A 50% shortening would improve the text and help get the new information across.

The manuscript has been significantly reduced, according to the remarks from the reviewers. We have removed segments focused at the structural framework and rather kept the focus towards fracture characteristics and the scale of the applied dataset.

Abstract: A good thing about the abstract is that it deals with results, and it is easy to read. It can be shortened a bit.

The abstract has been updated according to the new context of the paper.

Sections 1-4 all focus on introductory stuff and previous work. It may be good for someone unfamiliar with fractures and reservoirs (students), but too lengthy for most of the readership, I think.

Sections 1-4 have been condensed and streamlined, according to the new focus of the paper.

Section 5, "Previous fracture related work..." informs us that we will look at earlier work, but that is actually not the case (change that heading). Instead, most of the methodology is presented in Section 5. 5.2 promises fracture characterization, but still is mostly about methodology. The fractures are not characterized anywhere as such (just their length/height). So here is an important point:

It is essential that fracture characterization is done also by use of thin section and/or SEM images. Are the extension fractures only separated on the basis of orientation? Are they all of the same type (thickness, no mineralization)? What is the aperture of the extension fractures?

Section 5.1 has been removed. Section 5.2 is now termed methodology. The term fracture characterization is no longer applied in the paper, the title has therefore also been modified. From: Fracture characterization of Upper Cretaceous chalk onshore Denmark: applicability and implications for subsurface fracture modelling to "Quantitative Analysis of Natural Fracture Networks in Maastrichtian Chalk: Insights from a 7,000 m² Digital Outcrop Model of the Rørdal Quarry, Denmark"

Section 6 is ok, but again, needs to be condensed. 6.2 is, like 5.2, supposed to be on fracture characterization. I think it is more about the characteristics of the fractures than the characterization, so perhaps find a better heading (Fracture characteristics may work). Again, fracture type

(if there is any difference between them) should be combined with orientation data. Also see comments in the attached file.

The reference to fracture characterization has been removed. Fracture type and orientations are now discussed in the updated manuscript.

Consider adding a few horizontal scanlines along the data shown in Fig. 18, scanlines that show intensity variation, one way or other. This is because such graphs (scanlines) are commonly presented in the literature.

We have not included scanlines, however we emphasize the benefit of aerial sampling compared to scan line sampling and highlight the differences in results at the Rørdal Quarry (aerial sampling) compared to the Joule 2 program, in which scan lines were used.

Discussion

Too much repetition of stuff from the Introduction and elsewhere. This is supposed to be discussion of data and results, not repeating what has already been said and described. 7.1-7.3 can be cut by 75%, It contains a lot of repetition. 7.4 is interesting, but it is too much description with too little discussion and regional-scale interpretation. How about discussing possible strike-slip kinematics in relation to fold orientation and fracture orientation, and perhaps more on the relation between folds and fracture density? Age relations come in here too (between fracture sets and to fold formation). There may be no relation, but it is good to state that too. Also, it is important to understand how/why the fractures formed (tectonic setting) when applying the data to other areas, including offshore reservoirs.

Here are two things that need to be discussed in the Discussion section:

Stress: If the fractures are joints, they reflect stress orientation at the time of formation. σ_3 changing between quarries. Put this on a map together with data from other quarries? Two sets of extension fractures in a single quarry also indicate two σ_3 directions. Discuss this. Did the two populations (orientations) form at different times, or are we looking at local stress perturbations, for example around faults? Which of the two sets is related to strike-slip and which to extension? Not clearly discussed. As mentioned above, also involve fold attitude in this discussion.

The discussion of different fracture sets with relation to timing, stress field evolution and connection to the two local fault zones has been added to the manuscript.

Fluid flow: Will be anisotropic due to preferred orientation of fractures. Worth mentioning in the discussion. Actually, fluid flow is not really discussed in the light of the nice data set that you present. Should be. Also of interest how the

(subseismic extensional fractures are oriented relative to nearby faults. Focus more on these things.

As mentioned, the focus has been changed and implications for fluid flow are now part of the discussion.

Authors' Reply to Reviewer 2

Response to reviewer 2

Thank you for the thorough feedback provided on our manuscript. We appreciate your comments and suggestions, which have significantly contributed to the refinement of our work. In response to your feedback, we have made substantial revisions to the manuscript, reducing its length by almost 50%. This has been done in accordance with the detailed recommendations from Reviewer 1. Given the extensive changes, we have focused our responses on addressing the general themes and overarching concerns raised in your feedback, rather than responding to each comment line by line. We hope that these revisions address your concerns and enhance the clarity and impact of the manuscript.

The following scientific objectives have now been established for the paper: a) analyze the evolution of the local stress field by examining distinct fracture populations; b) compare findings from our extensive aerial dataset with earlier fracture descriptions from the site, which were limited by sampling methods, and assess the implications for subsurface fluid flow; and c) compare our findings with public records of fracture attributes from other Danish chalk outcrops to evaluate similarities and differences in fracture orientations between onshore exposures outside (Rørdal and Sigerslev) and within (Nye Kløv and Hillerslev) salt-impacted areas. Consequently, the title has been updated from "Fracture Characterization of Upper Cretaceous Chalk Onshore Denmark: Applicability and Implications for Subsurface Fracture Modelling" to "Quantitative Analysis of Natural Fracture Networks in Maastrichtian Chalk: Insights from a 7,000 m² Digital Outcrop Model of the Rørdal Quarry, Denmark."

The abstract has been updated and focused towards the results, according to the scientific targets described above. We have aimed to sharpen and condense the language to the best of our capabilities.

Analogue potential: The degree to which the Rørdal Quarry serves as a suitable analogue has been expanded upon in the text with additional references. The suggested sentence, "The analogue value is restricted to reservoirs of similar lithology, burial history, and tectonic setting," has also been incorporated into the discussion section.

Heavily fractured: In response to the comment regarding the term "heavily fractured," we have removed this term from the manuscript to provide a more precise and detailed description of the fracture network in the Rørdal Quarry. The paper now focuses on quantifying fracture orientations and their relationships to both far-field and local stress fields. We provide a detailed analysis of the fracture frequency, distribution patterns, and orientations in relation to the main faults. This approach offers a clearer understanding of the structural complexity within the quarry. While we did not include a generic diagram as suggested, the detailed descriptions and quantitative analysis

throughout the manuscript serve to convey the fracture characteristics effectively.

Spacing distribution: In response to the comment on fracture distribution, the fractures in the Rørdal Quarry are reported by Odling (1999) as being randomly distributed. However, our analysis reveals a smaller variation coefficient range compared to Odling's work, indicating a more consistent spacing pattern and supporting a log-normal distribution. This distinction is further elaborated in the updated discussion section.

Aerial extent of quarry: We appreciate the reviewer's suggestion to clarify the reference to excavation progress in the quarry. We have revised the text to explicitly mention that the phrase 'As exposed in 2016 to 2023' refers to the eastward progression of excavation. We have also made this clearer in the Results and Discussion sections, ensuring that the reader can easily follow the context. Additionally, we have included references to Figure 3 in the relevant sections to better illustrate the excavation progress and its impact on the fracture exposures.

Exclusion of horizontal fractures: The fractures at Rørdal Quarry were differentiated based on orientation, morphology, and cross-cutting relationships. Horizontal, near-bedding parallel fractures are likely associated with glacioeustatic processes, such as stress release during post-glacial uplift. In contrast, the steeply dipping vertical fractures, which sometimes cross-cut the horizontal ones, are interpreted as being of tectonic origin, driven by regional stress fields from the Sorgenfrei-Tornquist Zone. While glaciotectionic structures, as documented in previous studies, are common in Denmark, the fracture patterns at Rørdal Quarry, especially the steeply dipping fractures, are more consistent with regional far-field tectonic stresses. The presence of iron oxide coatings on some fractures supports their formation due to Neogene uplift rather than purely glaciotectionic processes. Although the distinction between glaciotectionic and tectonic fractures is acknowledged, the primary focus of this study is on the tectonic fractures, which are more significant for subsurface fluid flow modeling.

Splay nodes: The manuscript has been updated to say that the splay node acts as an indicator of connectivity, although it does not separate between fracture modes.

2nd Round of Revisions

Comments by Executive Editor

I have a couple of questions/concerns that I'd like to ask you about.

1) In shortening the paper according to the feedback, you've halved the number of figures and removed all figures from the Appendix. Is this a good idea? The reason I ask is that I feel the comments on shortening mainly refer to the text. I don't think either reviewer actually suggested removing figures. My own impression is that you could re-introduce a few, at least. I'm not sure why you took all the seismic examples and block diagrams out (Figs 7-13 in the original submission). So, I'm letting you know a bit of re-introduction would be OK with me. A picture is worth a thousand words, etc.

2) One thing that still doesn't show on any map is the palaeo-stress directions, as suggested by reviewer 1 ("**If the fractures are joints, they reflect stress orientation at the time of formation. sigma3 changing between quarries. Put this on a map together with data from other quarries? Two sets of extension fractures in a single quarry also indicate two Sigma3 directions**"). Is it possible to do this? I would also find it helpful, even if just a rough sketch on an outline map.

Sorry this one is taking so long, but I think we are closing in on the finish.

Best, Tony

Authors' Reply to Executive Editor

Dear Tony,

Here is an updated version of the manuscript according to the specifications in your letter of suggestions. Please let me know if the manuscript needs further edits.

Best regards,

Tala Maria Aabø