

## Response to 2<sup>nd</sup> reviewers comments

*Dear Dr. Laurich, thank you very much for your committed work towards our re-uploaded manuscript, which is highly appreciated. We have revised the manuscript according to your suggestions. Below we answer your individual comments. Our initial (first round) answers are highlighted in green, the reviewers answers are purple, and our latest response are blue. The applied changes are marked in the re-uploaded manuscript with track changes on. In addition the document is uploaded with all changes accepted.*

### **Review on the response to the referee #1 (Dr. Minisini, apparently openly non-anonymous)**

#### **Comment #1 - Better define “homogeneous” and terms the like**

The authors did well in simply deleting previously imprecise occurrences of the term “homogenous”, leaving it only in three occasions where it now refers to a uniform contribution of mineralogical phases “at the fm. scale”. To me, this is fine, as it conveys the important interpretation that sample differences are not -at least not foremost- caused by mineralogical variability but by maturity. However, if the authors fancy, they could even completely avoid the term and thus not give rise to any need of a complex (or incomplete) definition.

[With many publications the term “homogeneous” is used in an imprecise manner. That is unfortunate, as most of the time the term can be simply changed with what is actually meant, like “rock volumes of similar mechanical properties” or, as with this manuscript, “...samples are uniform in mineralogical composition, with clay minerals being the consistent dominant phase throughout the entire sampled formation”.]

*Thank you very much for this comment. We agree and have further adapted the manuscript in this regard. The term is now entirely avoided and replaced with more accurate wording.*

#### **Comment #4 – Selection of site**

The changed manuscript part (now line 204ff) is not concise. It can be summarized, maybe like:

*“To study the explicit influence of maturity on physical rock properties, investigated samples should ideally show a high variability in maturity while other controlling factors being of as little effect as possible – in particular the mineralogical variability. Fortunately for this purpose, previous studies (Bruns, Koch, Littke, Mackenzie,...) attest notable maturity variations in close distances for the here investigated ... site.”*

*We followed your valuable suggestion and adapted the respective section to make it more concise. However, we considered it important to retain a brief statement on the Opalinuston-Fm/Jurensismergel-Fm, as both units represent key lithostratigraphic sub-areas in the German siting process. Omitting this information would likely raise the question why these formations were not included in the present study. The revised section therefore concisely explains the rationale for focusing on the Amaltheenton-Fm while explicitly acknowledging the relevance of the other units.*

*“To study the explicit influence of burial history on physical claystone properties, the investigated rock sequences should ideally exhibit a high variability in thermal maturity while other controlling factors, particularly mineralogical composition, remain similar. This ensures that variability in the observed rock properties is predominantly controlled by burial-related alteration processes. Previous studies (see i.e. Koch & Arnemann, 1975; Littke et al., 1988; Mackenzie et al., 1988; Bruns et al., 2016) document*

*pronounced thermal maturity variations over short distances within parts of the Lower Saxony Basin (LSB), Germany, making this region particularly suitable for the MATURITY project.*

*Although several lithostratigraphic units in the area, including the Amaltheenton-Fm and the Opalinuston-Fm/Jurensismergel-Fm, are evaluated as potential sub-areas in the German siting process (Fig. 3), the Amaltheenton-Fm was selected as the primary target formation of this study. The Opalinuston-Fm/Jurensismergel-Fm locally exhibits deformation related to degassing of the underlying Posidonienschiefer-Fm and occurs in a hanging-wall position closer to the surface, increasing the likelihood of weathering-related alteration.”*

#### **Comment #inline 53ff – chemical lithification over mechanical consolidation**

The authors did well in their response here, maybe they want to add Corkum et al. (2007), who also described for OPA at Mont Terri, that pores are larger than anticipated by pure depth relation – also due to chemical lithification that preserved pore structures (diagenetic bonds).

Corkum, A.G., Martin, C.D., 2007. The mechanical behaviour of weak mudstone (Opalinus Clay) at low stresses. International Journal of Rock Mechanics and Mining Sciences 44, 196–209. <https://doi.org/10.1016/j.ijrmms.2006.06.004>

(Note: I have made this suggestion earlier in my 1st review)

*Indeed, Corkum et al. (2007) provide a relevant and complementary reference on diagenetic cementation and its effect on pore structures in Opalinus Clay. The reference was added to the manuscript. Thank you for the suggestion.*

#### **Review on the response to the referee #2 (myself, openly non-anonymous)**

##### **Comment #1 – Risk of misinterpretation**

For the conclusion part that seems to be addressed in line 880ff:

“It is important to emphasize that these deviations illustrate the site-specific nature of burial-related property trends. Local processes such as overpressure generation, hydrochemical evolution, or changes in effective stress can modify or even invert depth-dependent trends. Consequently, the observed relationships should not be interpreted as universally beneficial or detrimental for repository performance.”

I like to suggest an improvement here to better match my previous concern on misinterpretation (sorry for not being clearer earlier). What I suggest to emphasize is that knowing material properties alone is not sufficient to speculate on material behaviour (or suitability for a repository in this case). Instead, it is the interplay of material properties and environmental controls (see first review for example cases and also in the now rewritten introduction). I acknowledge that this interplay is somewhat trivial, yet find it often overlooked. I do not want to encourage the start of a deep discussion here, just a one sentence disclaimer to avoid that burial trends are thought of as a straight-forward proxy for ranking clays on their suitability for a radioactive waste repository, like: “the deeper it has been, the higher in strength, the better it is”. For the same reason I earlier wondered about the term “barrier properties”, which the authors now have omitted. Anyhow, the now emphasized limitation - to be careful in assuming a universality for the here presented site-specific trend - is also valuable yet not quite what I meant.

Moreover, I suggest to avoid the term “performance” (here and in other occasions) as it leaves ambiguous what is meant. Similar to “homogenous”, the term “performance” can be replaced by a precise wording in almost all cases that I am aware of.

*Thank you very much for this clarification. We fully agree with the underlying concern. While our previous revision emphasized the site-specific nature of burial-related property trends, we now recognize that this alone does not sufficiently convey that material properties cannot be interpreted independently of their environmental context. To address this, we have revised the Conclusions to explicitly state that burial trends in individual properties do not provide a direct proxy for host-rock suitability, and that repository-relevant behaviour arises from the interaction of material properties with site-specific boundary conditions such as stress state, hydrochemistry, and structural overprint.*

*In addition, we have replaced the term “performance” with more precise wording to avoid ambiguity. These changes are intended as a concise conceptual disclaimer to prevent oversimplified interpretations, without expanding the discussion beyond the scope of this contribution.*

*We modified the section as follows: “Consequently, burial-related trends in individual material properties should not be interpreted as a direct proxy for host-rock suitability, as repository-relevant material behaviour emerges from the interaction of these properties with site-specific boundary conditions such as stress state, hydrochemistry, and structural overprint. In this sense, the presented burial trends provide process-based constraints on property evolution that can support data transfer to sites with limited property information, but they do not constitute a standalone criterion for evaluating or comparing repository sites.”*

## **Comment #2 – Improving Conciseness**

### **TITLE:**

The revised title remains unclear with respect to what is meant by “different scales.” It is not specified whether this refers to spatial scale, methodological scale (e.g. laboratory vs. borehole), or another dimension. Even after reading the manuscript, I still don’t know what is meant, too. I frankly suggest a slightly changed, more specific alternative, for example:

“The influence of burial history on physical rock properties: A case study of Lower Jurassic claystones from the Hils and Sack Synclines (Germany).”

*Thank you for the helpful suggestion. We agree that the previous title was ambiguous regarding “different scales” and have revised it to the proposed formulation, which more clearly conveys the study’s scope and focus.*

### **INTRODUCTION:**

The introduction contains all the necessary elements, but it remains rather long (~7 pages) and would still benefit from a more streamlined formulation. In its current form, it provides limited guidance on what is most relevant for which of the now listed specific aims (good!).

I perceive the introduction to be currently structured as:

1. relevance of claystones in radwaste disposal
2. relevance that claystone properties are dependent on multiple controls
3. relevance of the control burial history
4. relevance of fracture permeability
5. relevance of sealing capacity
6. relevance of mechanical behaviour (and its controls, including burial history) (?)

7. relevance of unloading effects due to uplift (kind of belonging to burial history again)
8. relevance of burial history (again) / maturity (sensu property, not the project name)
9. outline of the project MATURITY (single subchapter)
10. reasoning for site-selection
11. aims of the paper
12. material / fm description

A rigorous condensation of the arguments would help the authors sharpen the motivation of the study and more clearly articulate the rationale underlying their work.

Moreover,

- i) I feel that the now added subchapter on outlining the MATURITY project gives redundant argumentation on the relevant aspects listed. Plus, a single subchapter is unnecessary if it is not contrasted with other subchapters (I guess that “1.1...” ends at line 204: “A claystone formation exhibiting...”, right?).
- ii) I made a suggestion in shortening the “reasoning of site-selection”, line 204ff, see reviewer 1, comment 4 on first page.

*Thank you very much for this thoughtful and detailed assessment of the introduction. We appreciate the suggestion to further streamline the argumentation and sharpen the link between the general background and the specific aims of the study.*

*While we agree that condensation improves clarity, we would like to clarify that the introduction is intentionally structured as a stepwise argument rather than a collection of partly redundant themes. In our view, the structure is as follows:*

- 1. The relevance of claystones as potential host rocks for radioactive waste disposal.*
- 2. The recognition that important claystone properties are governed by multiple controls, with burial history (including maximum burial and subsequent uplift) representing a key factor.*
- 3. A property-specific discussion of how burial affects porosity, permeability, self-sealing behaviour, and mechanical behaviour.*
- 4. The role of uplift and unloading as an integral component of the burial history.*
- 5. The implications of burial-history-induced alterations for site selection and data transferability, motivating the need for systematic quantification.*

*Our intention was to guide the reader from general relevance toward the specific knowledge gap addressed by the MATURITY project, namely the need to better constrain burial-controlled property evolution for application in the siting process. In this context, the detailed overview of processes affecting claystone properties is meant to provide the conceptual framework for the study. These burial-induced changes are additionally summarized and visualized in Figure 1, which integrates the discussed alterations along the burial history into a coherent process-based scheme. For these reasons, we believe that maintaining this structured and process-oriented introduction is important to properly contextualize the study and its objectives within the broader framework of the MATURITY project.*

*Regarding your additional comments i) and ii):*

- i) *We agree with your structural concern. To address this, we have introduced a second subchapter for the study objectives (1.2) following the site-selection discussion (1.1), which improves readability and clarifies the rationale for focusing on the Amaltheenton-Fm.*

- ii) *Additionally, the site-selection section has been revised for conciseness while retaining essential information about the relevance of other lithostratigraphic units (see answer to comment #4 – Selection of site). In this way, we aim to balance comprehensive scientific background with clear motivation and focus for the present study.*

#### **STUDY AREA:**

Commonly, chapters as “study area” or “sampling material” and the like are listed under methods, following the usually strict procedure in scientific publications of abstract, intro, methods, results, discussion, summary & outlook, acknowledgements.

*Thank you very much for the suggestions regarding the organization of the “Study Area” chapter. We respectfully retain a separate chapter for the study area context, as we consider that providing a dedicated overview of the regional geology and previous work on the thermal maturity and burial evolution is essential to guide the reader through the complex spatial and burial-history setting of the study area.*

A brief introductory sentence under each main heading might better guide the reader in this many, many, multiple investigations presented (moreover, a direct transition from a section heading to a subheading (e.g. “2” to “2.1”) is redundant). This holds true for other headings, e.g. “3 Methods”.

*We agree that short introductory sentences under major section headings improve readability and reader guidance, particularly given the multidisciplinary nature of the investigations. Accordingly, we added such short paragraphs where needed.*

The heading 2.2 “Previous Studies” should be defined by what kind of studies is meant. (“Previous studies on the maturity gradient in the ... region”, right? Previous studies on geology have been given just before)

*Indeed, we have refined the title to make the focus explicit. The revised title now reads: “2.2 Previous studies on the burial and maturity gradients in the Hills and Sack Syncline region”. This clearly defines the scope of the section and avoids potential confusion with general geological studies.*

#### **OUTLOOK:**

There is a well-written and concise outlook at the end of the summary chapter, too:

“Currently, individual studies carried out in the framework of the MATURITY project aim to further investigate the complex dependencies and interactions between burial history and mineralogical (CEC and reactive surface area), petrophysical (porosity, permeability), mechanical (rock strength and elasticity), and hydrogeological (hydraulic conductivity, storativity) claystone properties. These detailed studies will complement the previous analyses to deliver an important data set for the further site selection process in Germany. Ultimately, this effort will (a) enhance the understanding of the processes altering claystone properties throughout their burial and uplift and (b) facilitate robust site and scale data transferability by establishing transfer functions for the investigated parameters.”

Hence, I suggest to delete the OUTLOOK chapter itself entirely.

*Thank you very much for pointing this out. We fully agree that the individual outlook chapter is redundant and was therefore deleted entirely.*

#### **DISCUSSION AND SUMMARY:**

While the four general aims are now explicitly listed in the introduction, the discussion and conclusions would benefit from a structure that more directly and sequentially addresses these aims. Instead, new

headings (e.g. “5.2 Scale- and depth-dependent hydraulic behavior”) are introduced without being clearly linked to the stated aims, although they appear to correspond to them (e.g. Section 5.2 to Aim 4). Aligning the structure more closely with the stated aims would improve focus and readability. Maybe reformulate one paragraph explicitly for each aim in the conclusion section.

*Thank you very much for pointing this out. We agree that a clearer alignment between the discussion structure and the stated study aims improves readability and focus. Subsection headings were revised where appropriate to more clearly reflect their relationship with the stated objectives and prerequisites formulated in 1.1 and 1.2.*

### **Some “left-over” In-line comments**

L6 & L7 “Chair” – I guess “Institute for / Department of” as the authors do not hold the chair positions themselves, correct? (in German, “Lehrstuhl für” would be correct).

Maybe this was overlooked?

Indeed, the German “Lehrstuhl” can be misleading, especially if transferred to English. We adapted to “Institute”.

L 77 “control larger scale behaviour” – Counterargument: Fractures in clays have impermeable side walls. Calcite veins prove to have isotope signatures unrelated to close-by Ca-fossils that microstructurally often seem intact. Moreover, tracer profiles across faults (e.g. Main Fault in Mont Terri) show now deviation from an diffusion profile.

*Initial answer: We generally agree, that fractures at a large scale do not necessarily need to enhance fluid flow, especially in low to moderately indurated formations such as OPA or COx, where self-sealing especially due to swelling of clay minerals is efficient. However, strongly indurated formations tend to lose the attribute of effective and rapid self-sealing due to swelling as result of clay mineral conversion with increasing temperatures during burial. This will also result in increased brittleness, facilitating fractures to form while the loss of swellable clay mineral phases prevents effective self-sealing. In such cases induced fractures might remain open and accessible for fluid flow, ultimately leading to pronounced scale effects between the low permeable matrix and the higher permeable rock mass (see i.e. Mazurek et al, 2009: Natural Tracer Profiles Across Argillaceous Formations: The CLAYTRAC Project). However, we adapted the wording to account for those differentiations. A dedicated study that investigates these kind of phenomena is currently on the way.*

I am good with the simple addition of the word “can” here (line ).

Note:

Yet, swelling of clay minerals is likely not a relevant self-sealing mechanism in tectonic, fully saturated settings. In EDZ and bentonite, this mechanism is frequently studied and I sense that it is often assumed to be valid in general. Or what do the authors think: Why are there frequently veins in OPA (and other shales)? Shouldn’t the relatively slow vein precipitation be overruled by the much more rapid process of smectite swelling then? There are good arguments for smectite swelling to be even unimportant in tectonic settings (don’t get me wrong: in EDZ and Bentonite, I do consider it the dominant mechanism). Hence, my argument might still hold true: undeformed claystone might retain its porewater just very next to fracture. The fracture might be hydraulically active though, maybe conducting fluids from below through the claystone fm. to the top of it, but not necessarily from it. The formations overall integrity is not in immediate question if there are localized factures – they do not necessarily control the “larger scale behaviour”.

Indeed, we agree that swelling of clay minerals is not necessarily the dominant or universal self-sealing mechanism in fully saturated tectonic settings. In this sense, localized fractures do not automatically imply a loss of formation-scale integrity. Our intention in the original wording was not to imply that fractures generally dominate large-scale flow, but rather to acknowledge that, depending on burial history, degree of induration, and mechanical state, fractures can introduce scale-dependent permeability contrasts between matrix and rock mass. We therefore appreciate the suggestion to qualify the statement accordingly and have added the word “can” to avoid overgeneralization. A comprehensive discussion of fracture–matrix interaction, vein formation, and sealing mechanisms would indeed be highly valuable, but would exceed the scope of the present contribution. We therefore restrict ourselves here to a cautious and conditional formulation. As indicated, a dedicated study addressing this topic in more detail is currently on its way.

L 83 “self-sealing” this is an often missed opportunity to state what the term actually means. With ductility, as stated here, the fm. is “self-sealing” in the sense that it hinders larger brittle fractures to form by easily giving in to stress in a viscous manner. This is strictly different to “self-sealing” of fractures, where clay swelling CAN play a crucial role if the fm is not already saturated and/or if a pore fluid change to lower-ionic strength is at play.

I thank the authors for reformulating the section, which I do not request to be further changed. The authors may double-check if “...clay swelling CAN play a crucial role if the fm is not already saturated and/or if a pore fluid change to lower-ionic strength is at play” should be addressed, similar to comment above.

*Thank you very much for this comment; we added the sentence as requested.*

L 89ff See Rutter et al. 2001 for a good differentiation on what controls the mechanical behaviour, to become more precise here. Applies to L 93ff, too. (Evirn. controls vs. material intrinsic ones)

*Initial answer: Thank you very much for this suggestion. We included the paper as important reference but do not see the need for further discussion as this study does not predominantly focusses on mechanical properties.*

Sorry, this was meant more as an interesting hint rather than a request for a change. I did not mean to address the fault zone weakening subject or citing the paper. I just wanted to highlight that there is a good approach in justifying the complex controls of mechanical behaviour (which is likely used elsewhere, too) expanding more comprehensively what is stated in (a), (b) and (c) of this section. Consider deleting Rutter again, topic does indeed not fit well.

*Thank you very much for the clarification. We see the study as valuable reference and prefer to keep it.*

L 347 Isit also Sr reduction

*Initial answer: Could you specify what Sr stands for? Do you mean saturation ratio?*

Yes. The decompacted sample has a lower Sr than the in-situ one. In particular relevant to unconfined lab tests (next to the mentioned pore water loss due to exposure to air).

*Thank you very much for the clarification. We agree and added Sr reduction to the listed alterations.*

Table2: Can Pyrite be listed, too? That is crucial in weathering

*Initial answer: We agree, that Pyrite is crucial in weathering. A former study by Littke et al. (1993) was exactly about the strong effect of weathering on pyrite, while organic matter is less affected. However, at depth below 5-10 m pyrite is fresh in the Hils Syncline and not affected by weathering. See also the Burnaz et al. and Wisinghe et al. papers, where sulphur data are presented and discussed for two of our five locations. A dedicated publication is currently prepared that will target detailed mineralogical composition of the Amaltheenton-Fm.*

Just optional: Maybe it is still helpful as an estimate for what weathering severeness could be expected when exposed to air, like within a future repository?

*This would be speculative or at least not quantitative without further research.*

L 656: 'properties particular for site selection' – This phrasing suggests a hierarchy of importance. Which property is considered more important than the others, and by whom was this order established? Perhaps the authors mean to say that they 'address those parameters which, in their view, would most severely compromise the repository's suitability if they fall outside a certain range.' If so, that would still require argumentation.

*Initial answer: "[...] particular emphasis will be placed on rock properties that directly affect barrier performance of the formation and that might undergo alteration processes along gradual burial and uplift."*

This change does not provide a list of what properties are specifically meant and does not provide argumentation why these are "directly affecting". It still seems vague. I suggest to delete it. Otherwise, replace "performance". Do the authors mean "integrity"?

*We agree and have deleted the section along with the remaining part of the outlook chapter. What we meant was indeed "integrity" and we replaced "performance" in each occurrence across the document with more accurate wording.*