

Reviewer 1

Overview comments:

The manuscript entitled 'Geomorphological activity and stability of surfaces and soils formed in hyperarid alluvial deposits (Atacama Desert, Chile)' submitted to ESD (EGU) by Linda Andrea Elisabeth Maßon, Simon Matthias May, Svenja Riedesel, Willem Marijn van der Meij, Stephan Opitz, Andreas Pefeköver, and Tony Reimann whether landform stability and depositional/reworking processes in landforms within a hyperarid setting (alluvial fans and ephemeral channels) can be studied by means of single-grain feldspar luminescence, geochemistry, and granulometry. The authors also claim that field observations are key to this aim; however, there is a need for a much sharper linking of basic sedimentological observations with the proxies used, which I find to be the major shortcoming of the manuscript in its present form.

This manuscript contains a tremendous amount of laboratory work on luminescence analysis; however, most of the final interpretations (processes and potential triggers) are still addressed only vaguely. This likely arises from the lack of basic stratigraphic descriptions (¿classic? sedimentological descriptions), which are presented to some extent in the Supplementary Material but are not convincingly connected to the proxies presented. As a consequence, the discussion is structured on a mix of ideas/concepts based on profile chronology and paleoclimatic interpretations that are not sufficiently justified by basic stratigraphic work. This issue alone should not, in my view, be sufficient to reject a manuscript; however, it is a major flaw that must be addressed. Unless greater attention and detail are devoted to the sedimentary characteristics of the selected profiles, any interpretation can be refuted and remains vague.

The authors have also chosen to frame all their working hypotheses under the assumption of a climatic gradient within a catchment located in the Atacama Desert Coastal Cordillera. Without additional evidence (including references) and without basic information on, for example, catchment lithology, fault activity, and Late Quaternary vegetation history, most of the claims made in the manuscript remain too speculative.

The selection of study sites (alluvial fans and ephemeral channels) needs to be clearly explained from the outset. Rather than searching for abiotic, biotic, and climatic gradients, I suggest simplifying the hypothesis towards evaluating long-term stability of alluvial fan surfaces in a landform subject to different weathering histories, i.e., coastal fan) against tributary-junction fans located >1000 m a.s.l. without influence of i.e. marine aerosols. Additionally, when considering ephemeral channels (and therefore active – see references for events impacting the Atacama Desert in modern times: [Wilcox et al. \(2016\)](#), [Scott et al. \(2017\)](#), [Cabr   et al. \(2022\)](#), among others I strongly recommend the lecture of basic literature on how such landforms form and respond to runoff events (see, for example, the books by Bull & Kirkby, 2000, and Graf (1988) on dryland rivers).

There is also a substantial body of literature on fog in the Atacama Desert and on biotic and atmospheric factors (such as fog and ENSO) feedbacks with landforms, including research by del R  o et al. (2024, and references therein). Thus, I recommend a thorough review of this literature (maybe authors can point out others) to identify appropriate references relevant to the regional climatic setting in this Coastal region of the Atacama Desert. Although this region lies within the Atacama Desert, it exhibits very particular atmospheric conditions that need to be addressed accordingly.

I also recommend that the authors clearly identify the “paleoclimatic” proxies discussed (cited) in the paleoclimatic interpretation section by plotting their geographical locations. Doing so will show that most of the archives considered do not belong to the Coastal Cordillera physiographic domain, and therefore their applicability/relevance/influence should, at the very least, be explicitly discussed.

The conceptual understanding of alluvial fan formation presented in the manuscript lacks adequate referencing and is largely (almost exclusively) based on Blair and McPherson (2009) work. Given that the authors are working on a coastal alluvial fan, fan formation in relation to sea level, coastal uplift, and associated boundary conditions should be explicitly addressed by outlining the basic controls on fan

development in such environments. Relevant works by Adrian Harvey (Harvey, [1999](#), [2002](#), [2018](#)) or others ([Hartley et al., 2005](#); [Viseras et al., 2003](#); among others) could be consulted to strengthen this section. In this regard, Figure 2 also requires refinement and updating using recent literature focused on the same region. Authors can check for details in the line-by-line section of comments.

The discussion of previous work done by Walk et al. (2020, 2023) on the same(similar) alluvial fan(s) is very limited and mainly restricted to surface dating results. I would have expected a more detailed comparison of the geomorphic processes identified by both Walk and the present authors in order to provide a more comprehensive understanding of what geomorphic stability means in hyperarid environments and in particular in coastal alluvial fans. The authors could consider recent work ([Walk, 2025](#)), which offers fresh insights into coastal Atacama Desert alluvial fan systems.

Before proceeding to line-by-line comments, I would like to draw the authors' attention to the misuse or incorrect use of the term lithological unit when referring to stratigraphic layers, beds, or horizons in their profiles. The term lithological unit should be restricted to [appropriate stratigraphic usage], and its current application throughout the manuscript is misleading. You can check (as an example) the definitions provided by the international commission on stratigraphy ([link](#)).

It is also unclear how the XRD-derived geochemical data are ultimately integrated into the discussion of the results (apart from the PCA plot). It is concerning that the authors state in the discussion that the site-specific geochemical results are not conclusive and do not discuss potential limitations (at least). This raises the question of whether there was a clear rationale for applying XRD analyses in the first place.

Reply: We thank reviewer 1 for this thorough and detailed review, and we very much appreciate the numerous comments and suggestions on how to improve our paper. R1 suggests to add a considerable amount of information to the manuscript, and we are willing to do so. We understand that for a proper interpretation, a full documentation of our findings is needed, and this includes stratigraphy and facies interpretation that is needed for an appropriate understanding and integration of the luminescence results. Our intention was to reduce the length of the paper, why we provided some of the information - now proposed to be moved to the main manuscript - in the supplements. However, some information was simply considered of minor importance, so we had removed it from a previous version of the manuscript. Against this background, we will shift large parts of the supplements (stratigraphical description, section 4.1) and add a facies interpretation (sections 5.2 and 5.3) to the main paper, so that the context is clear to the reader.

We also understand that more information is needed regarding the lithological characteristics and the climatic situation in the study area (sections 2.1 and 2.2), and we added both to the main text. We now think that there is enough justification for calling our transect a climatic transect/gradient; we also provide a theoretical basis for the following facies interpretation, as we now introduce to the typical alluvial processes and facies in such hyperarid environments, as e.g. described in Cabre et al (2020). This represents the basis for the facies interpretation in the discussion section.

We also agree that it is difficult to directly compare alluvial fans and ephemeral channels. We therefore modified the manuscript now focusing on the alluvial fan surfaces that are reflecting a climatic gradient already. We interpret the results from the ephemeral channels separately. In this regard, we modified the hypothesis and aims of the study.

R1 also refers to Fig. 6S, i.e., schematic profile sections presented in the supplements. Since presenting one figure for each of the profiles will take a lot of space, we will present the profiles in 2 new combined figures (new Fig. 3 and 4), showing the alluvial fan profiles and the ephemeral channel profiles separately. We think that, although it requires 2 pages, these figures shall be part of the main manuscript, as stratigraphical description and facies interpretation have also been included.

Use of the term "Units": Indeed, we now excluded "lithological unit" as it is incorrect in this context. However, for stratigraphical description, it makes sense to separate the profiles into depositional units: depositional units are the smallest mappable package of sediment that represents a single, coherent

phase of deposition. It is a fundamental concept in sedimentology and stratigraphy; our units therefore refer to this meaning (lines 239).

As to the connection of proxies and results, we have modified large parts of the manuscript and think that now all results are sufficiently integrated into the discussion.

In the revised version with tracked changes you can see all the changes that were made compared to the first draft; due to the number of changes, most of the comments cannot be directly addressed at the respective manuscript locations (line numbers completely changed), as most of the text was rewritten/restructured/modified/shifted. A number of specific comments turned redundant due to these changes as well. Line numbers in the reply to R1 and R2 refer to the revised manuscript without tracked changes.

Specific comments & Minor comments:

L9: “Hyperarid earth surface processes” as a keyword is not reflected in the manuscript content. This is not a review paper; therefore, I recommend restricting the keywords to processes that can actually be addressed and constrained by the methods applied in this study.

We agree and excluded “Hyperarid earth surface processes” from the keyword list.

L12: Please clarify what is meant by “exceptionally high geomorphic stability” and specify which parameters or metrics this assessment is based on.

Ok, we rephrased the sentence. However, hyperaridity leads to 1) very low frequency and spatial variability of geomorphic activity, e.g. in the form of sheet flow/precipitation induced runoff, plus 2) surface and subsurface crusts, plus 3) the presence or even dominance of liquid water-independent processes such as seismic shaking or haloturbation.

L14: This statement is not entirely accurate. [Muñoz-Farías et al. \(2023\)](#) and also [Binnie et al. \(2018\)](#) demonstrate that surfaces commonly referred to as “stable” can record markedly different sedimentary and sediment-capture histories.

We agree. We referred to the “old landscape, surfaces and landforms” that had been revealed by cosmogenic nuclides, but indeed this does not mean there is no geomorphic change, as other and especially more recent studies show. We rephrased the sentence and included the suggested references.

L16: Please explain how the proposed “wetter” Quaternary episodes are reconciled with the episodic sedimentation events recorded by debris flows in the same coastal alluvial fans studied here (see [Bartz et al., 2020](#); [Walk et al., 2020](#)).

We have rephrased the text and wording throughout the manuscript and think that this comment is addressed.

L18: Are the authors truly determining the drivers of geomorphic activity? The manuscript does not present a clear end-member or comparative framework that would support this claim.

We now rephrased this section, as we understand that data at hand does not allow to unambiguously determine the drivers of the recorded activity.

L20: Do the authors intend to apply the 2 cm bleaching-depth rule uniformly across the entire Atacama Desert (see statement at L385: “we can infer that the bleaching depth ...”)?

Comment not relevant anymore, abstract was modified.

L21: Profiles—not landforms—are grouped as active or stable based on the multi-method approach. This leads to a more general concern: although the manuscript repeatedly refers to a multi-method approach

(and this is evident in the Methods section), I still miss a clear linkage between the different methods and how they jointly support or constrain the interpretations presented. There are clues -presented in figure S6 by the authors- to how to proceed.

As mentioned above, we have modified large parts of the manuscript and think that now all results are sufficiently integrated into the discussion.

L23: Please clarify which vertical grain-transport mechanism is being considered.

Comment not relevant anymore, abstract modified.

L24: Aren't desert pavements problematic for OSL dating? Please check and cite relevant references. Recent work of Fuchs et al. (2025) might give the literature search but is not exclusive. See: [Carr et al. \(2024\)](#), [Fuchs et al. \(2015\)](#), among others.

Comment not relevant anymore, abstract modified. However, indeed, we have addressed this problem in the discussion section of P4 in more detail (lines 730ff).

L24: This issue may arise because the selected catchment does not exhibit a clear climatic gradient.

See comments below. We have clarified this in the text (e.g. lines 110ff, and 221ff).

L25–28: There are not enough arguments provided to support this statement. I will return to this point in the Discussion section.

We agree. We have modified relevant sections, now being more conservative in the interpretation of (climatic) drivers of the detected activity, but putting it into the context of other findings.

L31–32: The study area does not fall within the extremely hyperarid core that is claimed. Please check and revise this statement using appropriate references.

We agree, see comments below.

L35–44: This paragraph needs to be better structured and should provide a concise framework for the studied region, rather than a broad introduction to paleoclimatic studies in the Atacama Desert that may have little or no relevance to this study. Please also check the spatial extent of areas influenced by increased moisture; the Altiplano should probably not be included here.

We agree and modified the introduction.

L45–53: The Introduction reads as mixed and unfocused. Again, the climatic transect concept does not work convincingly. Please clarify that the study focuses on alluvial deposits from different landforms with well-established depositional and reworking processes.

We agree and have modified the introduction.

L58: The references by Zinelabedin et al. (2022, 2025) focus on dating calcium sulphate wedges in distal parts of large alluvial fans in the Central or Intermediate Depression. Apart from the use of single-grain luminescence dating, their relevance to the present study is unclear. References such as del Río et al. (2018) and Bartz et al. (2020) may be more appropriate for the stated aims. The second aim of the study also remains questionable. Is there a clear and unambiguous application of the combined techniques that directly addresses this aim? The stated aims need to be revisited.

Ok, we have modified the introduction and clarified the study aims. These references, however, were used to show the added value of single grain luminescence data in hyperarid settings.

L60–62: This objective cannot be achieved without detailed stratigraphic field descriptions.

We agree and added stratigraphic and field descriptions to the manuscript (lines 340ff).

L64: Figure 1: On what criteria are the biotic–abiotic domains defined? Are there references that support this subdivision?

We have now excluded the terms biotic–abiotic and provide more information on why our transect reflects a climatic gradient, i.e., the characteristics of or the climatic differences within the climatic transect of this study.

L70: Providing general regional insights into hyperaridity is only necessary if the study aims to investigate processes affecting landforms in hyperarid settings. This does not appear to be the case, as the study area is not located within the hyperarid core of the Atacama Desert. Please support this with references, including maps based i.e., on pluviometry records.

We agree. We modified the section about climate and now provide more information on local conditions of fog, rain and vegetation, now also providing arguments for the coastal–inland subdivision within the catchment. We also included “del Rio” references. (lines 110ff)

L74–80: I strongly recommend providing more detailed information on fog influence in the Coastal Cordillera and its effects on landforms. The proposed coastal–inland subdivision within the catchment could be strengthened by incorporating references from del Río et al. provided above.

We agree, see comments above.

L80: Please explain or specify what is meant by “on a local level”.

We modified the section and deleted this wording.

L86: The climatic data are acceptable, but how relevant is seasonal moisture variability in the context you consider? How does this relate to actual precipitation events? A more in-depth understanding of the atmospheric processes operating in the selected study area is required.

We have added more information on the general relationships and climate situation in the study area, and this gives the context for the presented data; we think the weather station data illustrates the conditions in different parts of the climatic transect.

L90: What do you consider an “extensive drainage basin”? Is the reference to Walk et al. (2023) applied to the catchment or to the alluvial fan?

The reference is referring to the catchment size. However, we rephrased the sentence as there is no need to classify the size of the catchment as extensive.

L91: What do you mean by a “heterogeneous lithological composition”? For example, are you referring to a conglomerate with a polyphasic clast composition? This needs to be clarified, and more detailed information on catchment lithology should be provided.

We understand the benefit from a more detailed description and now extended this section. We now also provide a more detailed description of local source areas for the studied profiles (e.g., lines 180ff).

L91–92: Please clarify whether the broad, shallow valleys are filled with conglomerates or whether these valleys incise Neogene intramontane basins.

We have revised the section now providing more information; however, this explicit comment is redundant in our view.

L93: Is there an available reference for the Matancilla intrusive complex?

We think the given references (Alvarez) are sufficient, as not more info is needed.

L96: Please check for a typo in the citation.

Done.

L97: What is meant by a “fault that crosses the area”? Does this description make geological sense in this context? You also need to clarify whether you are referring to the Atacama Fault System (see relevant references) or using informal terminology. If the Atacama Fault System is considered, there is no need to describe individual minor faults, as the AFS is a composite fault system rather than a single fault trace. A more thorough work/presentation of the geology of the study area is required.

We agree. We have modified the section and hope to have clarified this comment.

L100: What do you mean by “except for P2”? How do you know that faults in this region are active? Please provide references or field evidence for recent activity or ruptures.

See comments above, we modified the section, now without stating an explicit activity of the Paposos fault segment.

L101: What controls the elevation profile of the transect? To what physiographic or geological characteristics is the reported ~110 m elevation difference related?

See comments above, we modified the section (see lines 193ff).

L105: “Geomorphological survey” is a very broad term. Please specify more precisely which aspects were investigated. What is meant by “site documentation,” and what does this include?

We modified the section now providing more details on the actual field work.

L106: Are you describing soil profiles or sedimentary logs? A combination of both seems necessary to address the main research question. Figure S6 shows particularly promising material; rather than leaving it as supplementary information, you may want to reconsider using such figures as key elements of the manuscript. You also state that profiles were excavated along a climatic transect, yet earlier you emphasize hyperarid climatic conditions. Does this imply the presence of a climatic gradient? Please justify this statement and provide supporting references.

Indeed, we agree, both the description of soil-related properties in the profile and the sedimentary logs is necessary in the context of our study. We have thoroughly revised the related sections, now including (1) a proper stratigraphical description of the profiles, and (2) the description of soil type characteristics, if present. We also included schematic figures of the sediment profiles, previously presented in the Supplements. In addition, we added justification of the climatic transect in the “study area setting” section, now presenting arguments for calling it a climatic transect.

L107: I do not understand the sentence beginning with “All profiles ...”. Please rewrite it for clarity.

Ok, rephrased.

L110: How much material was recovered from each sampled layer? Is it possible to provide an average amount? This is not a critical concern, but it would be useful for reproducibility.

We added a note about amount of material recovered (line 256).

L114: Figure 2: What is meant by an “uplifted mountain block”? Is the coastal cliff interpreted as a fault trace? If so, is there evidence supporting this observation? Why are sub-catchments delineated? Is there a specific geomorphological relevance—such as catchment piracy or other constraints—that justifies this subdivision? Could potential drainage capture have influenced the evolution of the landforms whose development you aim to reconstruct? Please refer to works dealing with that geomorphological topic for insights into this concept, which has not been considered in your investigation. Again, on what basis is the biotic–abiotic transition defined? More generally regarding Figure 2, I recommend using the original source of the figure rather than a previously modified version. Please clarify whether the figure represents the general structure of an alluvial fan or is intended to depict your specific study area. Please also clarify what is meant by “the left segment.” Do you mean the northern or southern part of the fan, or another

directional reference? Additionally, is there an explanation for the occurrence of sheetflood deposits on the left and debris-flow deposits on the right? From a sedimentological perspective, this is a pattern that requires further explanation.

We understand the criticism about this figure. We however consider it to be redundant and deleted it from the manuscript.

L120: Figure 3 caption: The statement “Profile units apply only to the respective profile” is unclear. Please rewrite it for clarity. The term “lithological units” is incorrectly used here and should be replaced with “stratigraphic layers.”

Ok, we rephrased the sentence.

L126: How do the geochemical data help in deciphering depositional processes? Please clarify. This also raises a more general concern regarding the use of geochemistry: what is the underlying purpose of these analyses? Why were sedimentary facies not linked to end-member definitions using the applied proxies? Stating that geochemistry is indicative of depositional processes is either incomplete or, as currently presented, incorrect.

We agree and modified the section. While we did not link sedimentary facies to end-member definitions, we do provide reasonable arguments for defining different facies in the discussion sections; we also provide an overview of typical facies as e.g. described in Cabré et al. 2020 (lines 204ff).

L128: When you refer to “insufficient material,” is this something that could have been anticipated, or are there specific indicators of how much sediment would have been required? Fieldwork limitations are understandable, but simply stating “insufficient material” does not clearly convey whether this is a sampling limitation or a methodological constraint.

Sampling was done in usual amounts of 200 g, which equals to standard OSL cylinders and is sufficient in most studies according to our experience. Also, we needed to consider the export of sample materials from Chile to Germany. For some samples, however, it turned out that very (!) few grains suitable for luminescence analyses were present, so that all material was used for processing. We modified the section and hope to have clarified this comment.

L139: The wording “attention was ...” is inappropriate here. Please rewrite the sentence for clarity.

We agree, modified.

L140: Is there any indication of how representative the selected grain-size fraction is for the different deposits studied (for luminescence purposes, of course)?

The selection of this grain-size range is mainly motivated/justified through the suitability of this grain-size for single-grain luminescence analyses, which provide the necessary analytical resolution for disentangling post-depositional soil-type mixing and depositional processes. However, most of the samples and sediments are indicated by a sand-dominated matrix, with a considerable portion of the selected grain size fraction. Except for those samples from the assumed desert pavement (more details regarding the significance of the selected grain size fraction for dating the desert pavement layer in the discussion chapter!), most of the samples are indicated by 60%+ sand in the matrix, suggesting the entrainment of sand during the debris or hyper-concentrated flows.

L156: Given that you rely heavily on De distribution values, please consider expanding on how these distributions relate to the sedimentary facies present in the studied profiles. Again, your figure S6 is very promising in that regard.

We agree and modified several text sections. In addition, we now provide a modified version of Figure 7 (now Fig 8), now including the relationship of theoretical De distributions and their common interpretation with De distributions and interpretations of this study.

L172: Question: Do you consider these samples to be “modern analogue” samples or not? Please clarify.

This depends on the process you look at and how one defines modern analog. The uppermost surficial samples are no modern analogue for the deposition of the fans; however, they instead provide indication whether any post-depositional turbation is able to reset the feldspar luminescence signals of the grains. We have modified the section and hope to have clarified this comment.

L176: Were De values close to zero or negative excluded from the analysis?

No, these grains are of course not excluded. This is the reason why unlogged data was used as input for all age models (see method text section).

L178: What is meant by “upward transport”? Please clarify this term.

Ok, modified/clarified.

L182: Could this observation be related to the development of desert varnish on desert pavement surfaces? If so, you may want to provide a reference or explore this as a possible explanation for incomplete bleaching.

That would be very (in our view too) speculative and we think there are more likely explanations for these observations. Please also note that the study of Bateman et al. (2007) works on quartz single-grains which provide different bleaching behaviors compared to feldspar grains. Our most likely explanation is: recent studies suggest that the intrinsic bleaching properties of feldspar grains are very heterogenous in terms of their bleachability and unbleachable/hard-to-bleach components (Biswas et al. 2026). To clarify this aspect a bit more, we rephrased to:

“Because our surface samples retain residual doses mainly from intrinsic properties such as heterogenous bleachability of the grains or different contributions of hard-to-bleach components, which are common for feldspar grains (e.g. Biswas et al. 2025, <https://doi.org/10.5194/egusphere-2025-4809>), we use this value instead...”

L192: What do you mean by an “exemplary grain-size distribution”? Please clarify.

Rephrased, not relevant anymore.

L195: Is the “mean dust frequency distribution” an indirect way of suggesting that the fine sediments accumulated in the profiles are allochthonous? The relevance of dust-trap sediments from previous studies is not clear to me in the context of this manuscript. Perhaps you could expand on potential extra-catchment sediment sources and their relevance to your interpretations? Although the grain-size fraction used for luminescence differs from that of the dust samples, dust input may still strongly influence the geochemical results. I have not been able to determine whether the geochemical analyses considered bulk sediment samples or a particular grain-size fraction. If so, this should be stated more explicitly in the methods section.

We agree; we modified numerous text sections now providing more information on the interpretation of the dust deposits and their potential influence on the sediments studied here (lines 475ff, 492ff, 524).

L196: The manuscript then shifts to weathering indices without clearly linking them to the stated research questions. Immediately afterward, it is stated that these indices are not well suited for desert environments, which makes their inclusion appear contradictory and confusing. Chemical weathering topics require a clearer framework or justification in this context.

We agree; we now rewrote the entire paragraph/section now providing some useful data for interpretation.

L203: Figure 5b: I would avoid comparing the evolution of profiles from alluvial fans and ephemeral channels together. P1 clearly reflects the effects of a stable surface with negligible surface erosion;

however, what needs to be explained is how P4 compares to this profile. This comparison could then be used to strengthen the hypothesis of a persistent long-term climatic gradient. You also state at L200 that comparisons between profiles are not possible due to differences in parent material, presumably reflecting lithological variability within the catchment. This again highlights the need to better characterize catchment lithology, as mentioned in earlier comments. If lithology exerts a strong control on the observed differences, the climatic interpretation becomes less robust, and this issue therefore needs to be addressed more thoroughly.

We thank the reviewer for the comments and modified this part of the manuscript; we now discuss the data with respect to lithological differences of source areas and concentrate on alluvial fan profiles; the ephemeral channels are of lower importance here and are shown in grey in the new figure.

L209: Again, is the granulometry entirely a product of weathering or differences in regolith from different rock units, or is there a component derived from external sources? This possibility should at least be considered, even if there are no data to support one explanation over another.

We agree and modified the text section; generally, P1 and P4 are older surfaces indicated by clay and silt, rather than sand; there might be a contribution of aeolian dust here as well, especially at P4. While P1 has a different lithology, P2 and P4 may have similar source areas, but different environments; the differences may therefore be due to climate.

L218: When you refer to “sufficiently bleached,” is this your own assessment, or are you using a reference value? If it is the latter, please provide appropriate references. The same concern applies to the term “effectively bleached” at L223.

This refers to grains with a dose (incl assigned uncertainty) in agreement with the residual dose level, which is defined in section 3.3. We however rephrased this sentence since “sufficiently” is indeed subjective, we now use an alternative phrasing.

L235: You report negative D_e values within the figure. Is there any explanation for this? What is meant by the “dust-trapping phase”? Please clarify. Why are dashed lines drawn between points in the figure? Is there any evidence of continuous sedimentation in these landforms that could be discussed?

If the sample D_e distribution of a sample approaches zero, a significant part of the D_e distribution will be below zero, which basically means that for those grains the net signal of the background is higher than the net signal of the initial signal. We have also added information about the dashed lines in the figure. As to the continuous sedimentation, I do not fully understand this comment: based on our interpretation, we only infer a sort of continuous sedimentation related to fine sediment trapping on the abandoned surface at P4 (desert pavement).

L241: When you refer to the “oldest” material, relative to what is this defined? Is there a potential preservation bias that should be considered?

Text changes, comment not relevant any more.

L248: Instead of stating “Unit I is the only one extensively penetrated...,” I recommend rewriting to something like “Unit I shows evidence of bioturbation by roots” or a similar description that specifies the observation.

Ok, rephrased.

L249: Are the features you describe modern roots, or are them root moulds, tubules? Please clarify.

Clarified.

L251: Can the material exchange you mention be attributed to a particular feature identified in the field? For example, is there evidence of desert pavement formation?

No, material exchange cannot be attributed to a particular feature identified in the field.

L255: To define depositional phases, it is necessary to clearly identify the sedimentary processes present. Without this, the interpretation risks being inconsistent.

We agree, modified.

L259: Is a deeper discussion warranted regarding the selection of one model over another for your interpretations?

We think have addressed this and provided all necessary information in section 3.3.

L260: The definition of aggradation phases remains complex without additional stratigraphic information.

We agree, modified.

L261: Can you list the initial soil formation processes observed in your profiles?

We refer to this now in several text sections.

L264: This comparison with Haug's work is not valid because their study focuses on fans in the Intermediate Depression, which are not comparable to coastal fans. You also state that the oldest surface might have been active—does this imply it is no longer the oldest surface? Please clarify.

We agree and deleted the reference/modified text sections.

L266: When discussing inherited ages, could you also provide insight into the potential reworking of Neogene conglomerate units and how this may influence your interpretations?

We have modified the section about inheritance and now think to addressed this question sufficiently (lines 635ff).

General comment on the last paragraphs: I think a much more detailed study of the observed sedimentary facies and/or soil formation is needed in order to interpret the ages in the most 'honest' and meaningful way. The depositional phases you mention, based solely on ages without stratigraphic context, are somewhat arbitrary and are not clearly supported by the chosen proxies. Perhaps the available data could have been used to develop a sedimentary end-member model that would allow you to discriminate between different sedimentary processes responsible for the ages. In other words, do you understand what the ages you present actually represent? Do you know if they correspond to a sedimentation event followed by reworking, or if they represent a single deposit that has been preserved without reworking to the present day? Much more work and description regarding the sedimentology is required. While the OSL laboratory work is impressive, its significance is best appreciated when supported by thorough sedimentological context.

Ok, as previously mentioned, we have modified the structure of the text and included stratigraphic information and interpretation.

L273: Can you expand on the different De peaks and the sedimentary characteristics of the sampled layers?

By modifying large parts of the manuscript, we think we have addressed this comment as well.

L276: Can you provide references supporting recent activity of this fault? Also, which sedimentary processes would be responsible for this mixing? Please clarify.

Not relevant anymore, we deleted this sentence and modified the text.

L277: Does this "marine boundary" remain stable throughout the Pleistocene and Holocene? Can you provide references to support this?

Done.

L283: Please clarify and explain exactly what aligns with the lithology.

Not relevant, deleted.

L287: A general comment on the previous lines: once you have defined sedimentary facies for the locations sampled for luminescence, you can relate how De peaks correspond to each sedimentary end-member (facies). This could itself be a meaningful result and would provide stronger support for your interpretations (check your figure S6 as an example to do this).

We agree, modified as described. See e.g. new Figure 8 and facies interpretation in discussion sections (lines 570ff).

L291: Incorrect use of the term “paleosurface.” Are you referring to a hiatus surface in sedimentation? Also, can you clarify which roots you are referring to—actual roots? Do they influence all layers of the profile? Please clarify.

We modified the text and hope to have clarified this comment.

L308: Older relative to what?

Moradi et al. 2020 determined two different fan generations, this is the older abandoned generation. There is more information during the following lines.

L330: That is interesting. Perhaps you could frame some of your De results together with the sedimentary facies to which they correspond. Again, check Fig. S6 example.

We now try to do so in new Fig 8.

General comment for the last lines: Are the mechanisms of trapping variable? Is there a potential increase in fines due to breakage on upper surfaces, exposing previously formed vuggy horizons characteristic of desert pavements? Are you recording increased erosion or deposition as a result? This could be relevant depending on the landform. Both mechanisms are known to operate in alluvial fans under the same climatic and tectonic regimes, so what is your explanation?

We expanded the discussion on desert pavement formation and hope to have thereby addressed this comment (lines 730ff)

L340: Is this a profile of an ephemeral channel? There is a clear need to provide a much more detailed sedimentary description. For example, in P5, Figure 3 shows open-framework gravel or couplets, which are known to be deposited by a single runo event; this needs to be carefully checked.

We agree and modified, see other comments.

I somehow already said that in the overview comment but: now in L340–354 that applies to the entire manuscript: Whenever you use the term “unit,” this is simply a sedimentary layer, and I do not see the relevance of defining it as a unit. A “unit” of what? Early in the manuscript, you refer to lithological units, but this is clearly incorrect. By definition, a lithological unit should be mappable under lithostratigraphic criteria; your “unit” definitions do not meet this standard.

We refer to depositional units, which is a fundamental concept in sedimentology and stratigraphy.

L355: As I mentioned in the Methods section and earlier comments, I think each profile first needs a sedimentary interpretation (e.g., alluvial fan, channel, etc.). Then, comparing them solely based on biotic–abiotic domains introduce a bias toward expected outcomes from your dataset. This manuscript should start from the basics of stratigraphy before moving into complex interpretations of De distributions for each sampled layer. Without a more detailed sedimentary description, even the most impressive laboratory work cannot effectively address the scientific question of how landforms form, evolve, and are

present today. By establishing the sedimentological framework first, you could more effectively integrate your De analysis and geochemistry with the sedimentary end-members or facies you define. A combination of the “theoretical” Figure 7 and/or your abanico plots (figures available within your supplementary materials), together with defined sedimentary facies, would be extremely valuable.

We agree, modified, as described in other comments; see new Fig. 8 and discussion.

L385: This statement reads somewhat too ambitious for a local study in a particular catchment of the Coastal Cordillera with specific lithologies. My impression is that the 2 cm bleaching-depth value is unlikely to be universally applicable across the entire Atacama Desert.

Ok, indeed it may be too ambitious. We have modified the sentence accordingly.

L388: Regarding post-depositional mixing processes, and considering your earlier mention of seismic activity, would it make sense to integrate the effects of seismic shaking here? There may be references that could support this consideration.

Indeed, it cannot be excluded. We added a sentence but did not find any suitable references.

L390: I am missing -again- more detailed explanations of the rock types present in your catchment.

We agree, done.

L392: It reads odd, despite detailed fieldwork on these profiles, the conclusion is essentially “not possible.” This is not meant as a harsh criticism, but I believe your observations could be explored in more depth to provide a more elaborate interpretation.

We agree and rephrased the text section. We also think that we now provide a more elaborate interpretation.

L396: This is puzzling because you provide a PCA where there is room to improve interpretation, yet you now state it does not convey much.

We agree and modified this text section.

L399–402: After examining the landscape and sampling both active ephemeral channels and abandoned fan surfaces (i.e., no drainage developed on top), this alone could have led to the conclusions you present using a dense dataset. The question here is: should one rely on classical geomorphological mapping criteria, or conduct extensive analyses that do not appear fully conclusive, yet lead to a similar conclusion? Your approach and its benefits are not entirely clear.

We agree. We have however thoroughly modified the introduction and main text, now better clarifying the aims and idea of the study.

L403–405: Proper stratigraphic descriptions are necessary to understand the sedimentary processes operating in these profiles. For example, recent events controlled by current base level (e.g., sea level) may not be recorded or preserved, meaning much of the Holocene could be missing. Consequently, any paleoclimatic interpretations would be incomplete. What are your considerations in this regard?

We modified the text, as mentioned above, now providing stratigraphic descriptions and also referring to the limitations of the alluvial records and paleoclimatic implications.

L407: Could your observations be simply explained by the fact that the landforms are simultaneously experiencing both erosion and deposition, reflecting a dynamic system?

Ok, text modified.

L413: By “alluvial deposition,” do you mean sediment transport?

We modified the text, not relevant anymore.

L414–415: By “incision,” do you mean channel incision?

Yes, we modified the text, not relevant anymore

L417: What “topographic barrier” are you referring to?

We modified the text, not relevant anymore.

L418–419: Can you provide estimates from historical datasets? (longer than the ones you show in figure S1?)

We think this comment is not relevant anymore due to the thorough revision of our manuscript.

L422: Perhaps, but your conclusions are not strongly supported by field observations. You may want to expand on this.

We have expanded the discussion and now provide a more critical/realistic view of our interpretations.

L427: Be cautious: a single runoff event can remove >7 m of channel infilling sediments (see examples in El Salado river (Wilcox et al., 2016; [Lazo et al., 2025](#); [Contreras et al. 2024](#)) and close-by ephemeral channels close to your study area by [Cabre et al., 2022](#)). Therefore, the deposits you observe may not present the full picture because of the removal of large volumes of sediments.

We added a section on the limitation of our sediment record and also included suggested references.

L468: There is the publication by Walk (2025) on the same fans, which you may want to consider in your resubmission.

Considered.

L477: How close are these features? Would it be useful to include them in a map?

We have added a note on the locations, but we consider it not necessary to provide a map.

L495: General comment from previous lines: from five profiles, for which you do not provide detailed information about the responsible sedimentary processes, you proceed to regional climate interpretations. This seems inappropriate.

Ok, discussion on paleoclimatic significance modified.

L508: Previously, you stated that the area represents a climatic gradient. I have not found any supporting evidence for this yet.

We now provided enough arguments for stating that our transect represents a climatic gradient.

L511: [Aguilar et al. \(2020\)](#) shown this for the Atacama Desert so you can use local references.

Ok done.

L513–514: Do you mean that the characteristics of rainfall are heterogeneous and, therefore, sediment transport is also variable? Consider checking recent events (Cabre et al., 2022, Atacama) or classic studies from Nagev Desert for reference of spatially stochastic distribution of rainfall in drylands.

We modified the discussion and hope to have addressed this comment.

L517: Has this boundary been formally defined? Desert pavements are observed in coastal fans (e.g., Salar Grande; Hartley et al., 1990).

Text modified, not relevant anymore.

L520: The writing here is unclear. Please explain.

Not relevant anymore.

L521: When you mention post-deposition, isn't reworking the simplest explanation? Small-magnitude events might not be recorded in stratigraphy but could still produce significant erosion of deposits.

Indeed, but text changed, not relevant anymore.

L524: Desert pavements have indeed been described in the Coastal Atacama (Hartley et al., 1990).

Text modified, thereby addressed or not relevant anymore.

L527–529: So, what is the interpretation? If there is bypassing, there may have been activity, but in your sampled locations deposition is not recorded. A better sampling strategy -or limiting the applicability of your results to paleoclimate interpretations- seems more reasonable.

We agree and have modified the discussion accordingly.

L531–536: I would suggest revisiting this section in the final version. Try to ground your conclusions more firmly in your observations and be honest about the methodological limitations, which are already clearly presented.

We agree and think we now base our conclusion on our findings.

Conclusions: The conclusions need to be revised after editing the manuscript to reflect the points above.

Done.

L579: I'm very sorry to read that.

Haha...no need to be sorry. We hope to have amended our manuscript and thank R1 for all the detailed comments.