Reviewer 2

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The study presented in this manuscript uses a combination of quantitative techniques to establish processes associated with late Quaternary geomorphic activity in the Atacama across a climatic gradient. The climate gradient is established using exposures of soil/sediment in the upper 80 cm of unconsolidated alluvial deposits. The authors principally employ and rely upon geochemical analysis and interpretations from single-grain luminescence signals to (1) categorize their study sites by active/inactive geomorphic processes, and (2) assess the palaeoclimatic implications for their observations. I review two main criticisms of this manuscript that warrant further examination prior to publication in EGUsphere.

Site Selection and Characterization

The authors develop their hypothesis that relies upon the use of a climosequence of soils formed in alluvial fan sediments along an east-west transect in a single catchment that drains to the Pacific Ocean. Several aspects of this site selection minimize the significance of the results, as currently presented.

First, the manuscript does not adequately document the relationships of the alluvial fan surfaces to one another in a way that enables systematic interpretation of results. As shown schematically in Figure 2 of the manuscript, each sampling site appears to be associated with its own set of geomorphic factors that complicate interpretations. For example, fans appear to be produced from catchments of widely varying order and stream carrying capacity, as documented in the text. This, coupled with the lithological variations in the fan sediments that were documented, can be expected to produce a host of different stratigraphic relationships in the alluvial deposits unique to each location. Although the authors acknowledge the complexity of the lithologic variability within the catchment, the potential impacts from those variations as they relate to overall preservation of alluvial fan units are not explicitly addressed in the background or in their interpretations of the results. The authors would greatly improve upon their interpretations by incorporating some characterization and subsequent discussion of these characteristics and how they influence their laboratory results.

Similarly, the geomorphic and stratigraphic context of each site is not established in the same detail across sites. The authors rely heavily upon prior work by Walk et al (2023) where careful mapping was performed to produce a geomorphic framework for P1. I expected to see a similar level of characterization for the other four sampling sites so that claims relating to deposition/erosion/soil-forming processes could be addressed using a combination of field methods as well as quantitative methods. Only a cursory level of detail is presented in the Supplementary Materials and only addresses details pertaining to the sampling site itself. One of the principal shortcomings in this regard is the lack of field observations to support the hypotheses presented in the paper.

Reply: We thank reviewer 2 for the detailed review and we appreciate his/her comments to improve our paper. We agree that aspects such as the overall preservation of alluvial fan units with regard to the individual settings were not adequately addressed in the previous version of our manuscript. However, while we have to accept the differences of our sampling sites that all have their own geomorphic and environmental circumstances, we have modified large parts of the paper, and we think that we have improved the way how we present and interpret our results in general. We are now also addressing the limitations of our approach that are related to these varying circumstances and the overall preservation of alluvial fan

units. Considering all the changes that were made to the manuscript, we hope to have now improved these aspects.

We understand that more detail is needed with respect to the geomorphic framework of sites P2-P5. However, complexity of these sites is lower compared to the coastal fan of P1, and we consider the information provided now is enough to understand the setting of the profiles. However, we now included more information on the setting of each profile (in the form of basic mapping, catchment size) in Fig. 1, and we also amended mapping in these subfigures now providing more detail. Catchments of alluvial fans at P2 and P4, however, are of comparable size (0.42 & 0.61 km^2), as are those of the ephemeral channels (~1 km^2)

Interpretations of results

The authors performed geochemical analyses on mineral sample materials to address pedogenic processes that result in net mobilization of cations through the profile. There are two fundamental concerns that I have with this approach that influence their interpretations. First, the bulk mineral matter in the fine-earth fraction, rather than soil solution extracts, are evaluated from a geochemical perspective. The authors note a diversity of lithologies that contain some mixture of volcanic, intrusive, and metasedimentary rocks, undoubtedly with variable components of the cations under evaluation. If pedogenic processes were to be targeted directly, especially in this dry environment where salt mobility drives pedogenesis, then soil solution extracts would provide more direct and reliable evidence in support of claims related to pedogenetic processes compared with the bulk mineral content. Second, and perhaps more important, is the fact that the authors assert that the geochemical differences observed among profiles limit their ability to compare between sites, yet their following PCA analysis appears to do just that.

The PCA results indicate clusters associated with variable amounts of geochemical constituents that presumable relate to lithologic variations rather than geomorphic variations. The use of soil solution extracts in this approach would have been the ideal approach to negate the lithologic influence, but there is limited discussion of this potential shortcoming in the manuscript. Furthermore, the interpretations derived from their PCA results appear to drive their overall conclusions regarding geomorphic stability, as indicated by their annotation on the horizontal axis of Figure 5b.

The rather widespread references throughout the manuscript to post-depositional mixing and the mechanisms attributed to these interpretations is somewhat problematic in the sense that the geomorphic and stratigraphic observations are not utilized to their full extent to evaluate this hypothesis. For example, the authors reference proximity to a fault that has not been documented in this manuscript in terms of its activity and recency or the direct relationship that such faulting would have in terms of plausible geomorphic processes. Despite this, they use this logic to invoke soil mixing based on the observation of heterogenous grain sizes in their profile. In my view, the system and its dynamics have not been evaluated to the degree to which such a hypothesis can be made or tested. Similarly, the authors reference relationships between heterogeneous grain size distributions and root density to arrive at the interpretation of mixing so that they can explain their luminescence results. This, to me, is somewhat reversed logic and the luminescence should be used to test the hypotheses that are based on field evidence and observation.

Overall, I believe that a significant restructure and organization is warranted for publication. I recommend that more careful geomorphic documentation of each site is required to provide the basis for stratigraphic observations. These stratigraphic observations should then be presented, again in greater detail, as evidence which is used to formulate the hypothesis of geomorphic activity/stability across the climate gradient, and which is supported by the appropriate literature. Only then, in my opinion, can the geochemical and luminescence

observations be used to test the hypotheses. The results from the luminescence analyses are quite interesting and have the potential to significantly improve our understanding of arid-land alluvial processes in relation to external forcings, so it is critical that the appropriate context be developed to arrive at meaningful and broadly applicable results. I have provided additional line-by-line comments in the attached marked-up pdf

Reply: We agree that soil solution extracts would have possibly been a better way for analyzing the soil profiles of this study. However, as said before, we have modified large parts of the manuscript including the way of discussing and interpreting our geochemical and granulometric data. This includes also a more differentiated analysis and discussion of the data at hand that considers both the differences between the different profile types (alluvial fan vs ephemeral channels) and between different lithologies. We know discuss the data with respect to lithological differences of source areas and concentrate on the alluvial fan profiles; the ephemeral channels are of lower importance here and are shown in grey in the new figure. At least for a comparison of P2 and P4 that are sourced by similar lithology, the bulk sediment geochemical characteristics are assumed to reflect soil- and/or weathering-related differences rather than being controlled by different source material. However, we also consider possible bias by intra-profile lithological variability.

We have additionally added a stratigraphic description and facies interpretation to the discussion section, and we excluded speculative sections such as on the proximity of fault lines and its potential influence on soil evolution. In our view, we have improved the structure of our manuscript by including an improved geomorphic documentation and stratigraphic description, and we now use the geochemical and luminescence data to examine the soil-related surface dynamics along the transect.

Specific/minor comments:

R2: You should define what is meant by "profile". This could be interpreted as surface topographic profile among many other possibilities. I think stratigraphic or soil profiles are more representative here. Given the lack of focus on the soils explicitly, perhaps the former terminology is most appropriate.

Reply: Ok, we now use the term “sediment profile” in the abstract, and we define what is meant by profile later (lines 233ff)

R2: in the upper 80 cm of

Reply: We agree, we included this into the text (line 19).

R2: i think the timescale is of particular relevance here. maybe indicate that a millenial timescale is used to categorize these sites.

Reply: Comment redundant, text changed.

R2: beneath

Reply: Comment redundant, text changed.

R2: I think that it's worth stating that this observation is based on luminescence properties and not field-based observations

Reply: Comment redundant, text changed.

R2: This, to me, speaks more to the site selection than the active landscape processes and is one criticism i have of the paper in general.

Reply: text modified; however, we think that we now have improved our interpretation and provide a more differentiated view of our findings; we also provide a more thorough discussion on the significance of our results, both regarding depositional activity and chronological trends as well as post-depositional soil processes, and potential climatic drivers.

R2: In general, my main criticism with the manuscript lies within the site selection, or at least in the presentation of the geomorphic and site context. I think that you would have a much more convincing argument if you could dedicate a section relating to these aspects of each site and how they correspond to one another. The significance here is a lack of any ability to relate the profiles outside of the luminescence results. My experience with exposure dating and burial dating have convinced me that neither of these tools can be adequately applied without some sort of context and framework which forms a testable hypothesis. I am not familiar with this study area, but the diversity of lithologies, fan ages, preservation potential, and subcatchment sedimentation dynamics that you've highlighted in the introduction and in Figure 2 suggest that a more systematic framework is required to enable robust interpretation of the geochemical and geochronological results. In my opinion, the geomorphic survey and site documentation aspects should be relied upon more heavily during interpretation of results than they are currently presented.

Reply: We agree and have modified large parts of the manuscript to address this main criticism; see also reply to R1. While we cannot change site selection (that was indeed not perfect), we now provide a more detailed description of setting and stratigraphy (lines 340ff, including schematic drawings of the profiles in new Figures 3 and 4), and this is now part of the discussion along with a facies interpretation.

R2: please clarify what you mean by surface profile. im not sure this conveys the intended meaning

Reply: Ok, text changed.

R2: i am confused by what you mean here. what depositional processes are you concerned with and how do pits in alluvial profiles minimize those effects?

Reply: Ok, text changed: "*Thereby, alluvial fan surfaces were chosen as their evolution is related to well-known depositional processes, allowing for comparability of the different profiles"* (line 245)

R2: it would be worthwhile earlier in the introduction or elsewhere to indicate the expected range of transport mechanisms and how they could be identified so your results have more significance to the reader.

Reply: We agree. We added a section on expected transport processes and resulting facies (line 201ff)

R2: can you define "upward transport"? this would really only apply to hillslope soils where soil production occurs from regolith formation, correct? i cannot see how you would get upward transport unless there is a significant erosional component to the location in question.

Reply: regolith formation on hillslope soils indeed, but salt-related processes/haloturbation may provoke upward transport of particles as well.

R2: I think this argument nullifies the results from the PCA analysis. The entire utility of your PCA is to demonstrate differences in dominant geochemical signatures between profiles, but if this is largely driven by differences in lithology, then it seems that the PCA principally reflects these lithological differences and not post-depositional information.

Reply: We have modified the entire section and hope to provide better interpretation of our geochemical results.

R2: This is an interesting approach, but would the results have been any different if the mobile elemental concentrations were taken from soil solution extracts instead of the bulk mineral material? is it possible that the ratios reflect some additional component of vertical variation in parent material lithology in addition to pedogenic process?

Reply: Well, vertical variations of parent material lithology may be possible, but the general characteristics of the lithological source (of P2 and P4) are assumed to be comparable. We thus consider the observed variations relating to soil processes; we however now provide a more differentiated view of the dataset and mention a possible bias by source changes (line 524ff).

R2: is there any reason that this could not also record dilution of the mobile element concentration of the parent material due to dust influx? In other words, could dust deposition and translocation be contributing Al or Ti in an additive way in the near surface as opposed to, or in addition to downward mobilization and relative depletion of the other cations analyzed?

Reply: Indeed, it cannot be excluded. We mention a possible dust contribution here as well (line 508ff, 673).

R2: unless salt shattering is a contributing mechanism here, i would be cautious of making the assumption that in-situ weathering produces significant fines. it seems more likely that dust influx is contributing to this signal

Reply: ok, we agree, see comment above.

R2: do you have direct field (stratigraphic or geomorphic) evidence to support this hypothesis. if intense mixing did occur, one should be able to recognize it in the field. invocation of adjacent faulting is suggestive only of a potential mechanism to perturb the system and is not very strong evidence in my view. the presumptive increase in bioactivity should be easily verified by field observations as well.

Reply: We agree and added a note on stratigraphical characteristics that lack lamination and suggest turbation processes e.g., by roots. The influence of adjacent faulting was deleted, too speculative.

R2: have you observed such features in your field area that you can document as a reference? if this mechanism is widespread as indicated, not only should you see it preserved in your sampling sites, but in areas of active veg. as well.

Reply: Yes, it is now described in the profile description in the main text.

R2: I am not very convinced by this.

Reply: Well, this is what we observed; see also profile description.

R2: i personally do not think that this was incorporated in a convincing way to support the arguments being made.

Reply: We hope to have improved our paper so that incorporation of the different datasets is more convincing.

R2: do you see evidence in the form of krotovina that would indicate prior vegetal activity? i think this is a weak argument given all of the variability between sites.

Reply: No evidence of prior vegetal activity was observed.

R2: how significant is this from only five soil profiles over an entire watershed that has not been completely mapped?

Reply: We understand that there is only a low significance of the 5 profiles with regard to their paleoclimatic interpretation. We have modified this section as well, and we feel that our interpretation is now more conservative and does not overinterpret the data at hand.

R2: do you think that base-level change due to sea-level fluctuation could have also influenced the fan dynamics? i don't think this was discussed.

Reply: We have added a note on possible base-level changes in line 198ff and 651. There is indeed toe cutting visible in the coastal part of the fan, possibly 2 generations.

R2: i wonder if this is more representative of the methods used for site selection, rather than a fundamental lack of patterns in geomorphic activity. If climate conditions did, indeed play a role, as was argued, then this pattern should be evident with careful site selection. additionally, careful mapping and analysis of fan distribution throughout the entire watershed should be helpful for distinguishing significant alluvial phases that were not buried or destroyed. This latter point was not mentioned in the paper and i think is a relevant one, given the wide range of site conditions that were considered.

Reply: we agree, but cannot change site selection anymore; however, we modified most parts of the interpretation; see comments above.

R2: what do you consider to be "wet" in this context? my experience indicates that desert pavements can form in regions where MAP is ~150-200 mm/yr and dust flux is adequate.

Reply: Agreed; however, comment not relevant anymore as section was changed.