Reviewer: Christof Sager, Museum für Naturkunde - Leibniz Institute for Evolution and Biodiversity Science; 10115 Berlin, Germany.

1. An initial paragraph or section evaluating the overall quality of the preprint ("general comments")

Zinelabedin *et al.* (manuscript No.: egusphere-2024-592) present a compelling study on polygonal patterned ground featuring sulphate-cemented wedges overlain by a sulphate crust in the Atacama Desert. This research employs a robust multi-method approach to investigate these unique features, which stand out from previously studied polygon-wedge systems in the region.
The authors attribute the formation of these wedges primarily to so-called haloturbation processes and the expansion-contraction dynamics (swell-shrinking) of calcium sulphate under hyper-arid conditions with sporadic rainfall events. The study is relevant for the readers of Earth Surface Dynamics as it makes a significant contribution to the research field of arid environments, while particularly enriching our understanding of patterned ground. Furthermore, its implications extend to extraterrestrial studies, particularly in understanding patterned grounds on Mars. Overall, the study is of good scientific quality and is well written, with the data presented appropriately.
However, there are several aspects, including some major concerns, that need to be clarified before the manuscript may be published. These concerns do not relate to the presented methods or acquired data, but rather to the interpretation of the results and the terminology used for salt-related processes and wedge formation. Addressing these issues should not pose significant challenges and will enhance the manuscript's clarity.

Major comments:

A major concern is the use of the term "haloturbation" as the dominant process for polygon-wedge formation, which I believe is not appropriate. First, this process is not explained in sufficient detail and is not clearly differentiated from other salt-related processes forming polygons/wedges, potentially leading to confusion for the reader. Second, the authors attribute the wedge formation in lines 413-415 mainly to shrinking processes due to phase transitions of sulfates, rather than directly to haloturbation, "which dominates in the polygon body, causing salt heave.". In my opinion, shrinking-swelling should not be seen equivalent to or be summarized under the term haloturbation (as in line 396), as shrinking-swelling is rather a term for volumetric changes in sediment or soil. Thus, shrinking process (or more general contraction) as the authors state in lines 413-415 is more appropriate than haloturbation as the main driving force for repeated soil cracking and thus, wedge formation. The authors already present very strong indication of hydration and dehydration processes by the presence of different calcium sulfate hydration forms with the XRD data. In contrast, haloturbation refers to the deformation of the original soil or sediment texture by the precipitation or dissolution of salts, but it remains too vague how it leads to meter-deep soil cracks. Therefore, the argument in line 381 that soil cracks are formed by subsurface pressure is questionable without further elaboration and contradicts the statement in lines 413-415 where contraction (shrinking) rather than expansion (which leads to subsurface pressure) is attributed to soil cracking.

For easier understanding, I suggest the following terms be clearly defined and differentiated in the introduction section: salt heave, clast/salt shattering as a form of salt weathering, contraction and expansion in the context of shrinking-swelling, thermal contraction, dehydration and hydration of minerals, and desiccation (as a crack formation mechanism). The term haloturbation is not necessarily needed when considering the above-mentioned concerns. It seems that these terms are not consistently defined throughout the literature, and their use and definitions vary across disciplines and studies which can lead to confusion for readers not familiar with these processes.

My understanding of the terms: contraction and expansion are more general terms used for volumetric changes in sediments and soils. Shrinking and swelling are commonly used for volume changes due to changes in water content in clay minerals with swelling potential, occurring in e.g., in playa environments where rain events lead to initial swelling of clays and subsequent drying/shrinking, resulting in desiccation crack polygons. For phase transitions between gypsum, bassanite, and anhydrite, the terms hydration and dehydration (as a form of desiccation) would be more appropriate, distinguishing them from the shrinking-swelling of clay minerals. Therefore, I suggest the dominant wedge formation processes be termed contraction due to dehydration of calcium sulfates rather than using the term haloturbation.

A second concern is the interpretation of wedge cementation. Did the authors consider alternative explanations? The authors correctly state that in other studies, the wedges were largely free of salts or salt-poor. In Sager et al. (2023) (https://doi.org/10.1029/2022JG007328), a rain experiment was conducted showing that salt precipitation occurs on the surface of the wedges after wetting, likely due to upward movement of saline water along the wedges. Additionally, Sager et al. (2021) observed that the outer parts of the wedges had higher salt content than the inner parts. It was suggested that, over time and with sufficient rain events, salts migrate from salt-rich polygons towards the initially salt-poor wedges, eventually cementing them. Therefore, it is very interesting that Zinelabedin *et al.* observed so intensely cemented wedges. In Figure S.1 it appears that the wedge center shows a lower sulfate content (brownish colors) compared to the periphery (whitish color). The processes proposed by Sager *et al.* could also be relevant for the cementation of the Aroma fan wedges. Can the authors discuss this possibility, or can it be excluded that the calcium sulfates in the wedges originated from the polygon? Addressing these alternative explanations would provide a more comprehensive understanding of the wedge cementation process and strengthen the study's conclusions.

2. Section addressing individual scientific questions/issues ("specific comments") and technical corrections

Title & Abstract: The title and abstract should be changed regarding the major concern that haloturbation might not be the appropriate term. The summary would benefit from more information, e.g., what is the composition of the polygons (the sediment/soil) between the wedges, how deep are the wedges and how many wedges were examined? The authors were able to determine an age for the crust. Since such ages are very important and rare, I would add them to the abstract.

Lines 11-12

Consider rephrasing “post sedimentary features”, since it remains questionable if wedges are completely post sedimentary features since they are formed by sediment deposition in the cracks and are accompanied by ongoing sedimentary processes as dry dust and salt deposition migration by percolating rainwater. Or rephrase, if with post sedimentary is meant after the deposition of the host material.

14-16

From my point, these are very interesting polygon wedges, due to the high anhydrite content. However, wording suggests that the high anhydrite content lacking in other locations implies different formation process. However, it could be also post-formational processes that led to these type of wedges. As stated above in the major concerns, I would add the possibility (maybe better in the discussion) that overprinting of sand wedges by calcium sulfate from the polygon could occur as a secondary process after initial wedge formation.

16-17

Please rephrase under consideration of major concerns. Also, it is unclear who assume the process, the authors or the general scientific community. Better: “We assume contraction/shrinking due to dehydration of calcium sulfates to be the main driver wedge formation at the Aroma fan site.”

23-24

Which are the other processes, do you include thermal contraction and desiccation as possible processes?

Introduction

35-38
I would disagree that cryoturbation is the main mechanism for (non-sorted) polygon formation. Cracking and thus wedge formation is rather caused by stresses from rapid cooling or low temperature (thermal contraction), see Lachenbruch (1962) (https://doi.org/10.1130/SPE70). After initial cracking, seasonal freeze-thaw cycles allow water to enter the cracks and subsequently freeze, which forms wedges over time. However, sorted patterned ground, such as sorted stone circles (which do not have classical wedges as non-sorted patterns) are assumed to be a result of repeated frost heave processes (e.g., Kessler 2001, https://doi.org/10.1029/2001JB000279). Thus cryoturbation is a process often occurring in the periglacial soils, but do not necessarily contribute directly to the pattern formation.

38-39
This is correct, maybe the authors could elaborate when wedges are expected and when not. The differentiation between sorted and non-sorted patterns could help, as all non-sorted polygonal ground is defined by the presence of wedges (filled cracks) or unfilled cracks, that separate the polygons.

43-44
Please rephrase the sentence, as "strongly differing" relates probably to the periglacial environments, but it reads as if this comparison is drawn to Mars, since it was discussed above.

46
"wedge structures can also be found in the "Atacama Desert" instead of "here".

52
Buck et al. 2006 is very interesting but unfortunately only a conference abstract. Do the authors have found the appropriate manuscript to this study? Also, Buck et al. write:
"These features are interpreted to have formed through salt heave, which occurs when salt minerals cement soil grains creating the cohesion necessary for tensional stresses (caused by desiccation, and/or thermal contraction of salt minerals) to form contraction cracks. The contraction cracks are filled with eolian dust (salt/sediment), preventing their closure during periods of expansion caused by salt mineral precipitation and/or thermal expansion." Which means that wedges and cracks are formed by contraction rather than the salt heave itself.
In Ewing et al. 2006 it is said:
"The presence of sulfate polygonal prisms in multiple horizons (Fig. 10; Table 5c) suggests cyclical hydration and dehydration of gypsum/anhydrite (Chatterji and Jeffrey, 1963)" please consider putting this reference behind "dry environments" rather than "haloturbation". Or exchange haloturbation with contraction/dehydration.

60
What is was/where the other process or processes in addition to gypsification?

66-67
Unclear wording, please rephrase. Do the authors want to say the dehydration /hydration processes were observed or potentially can be identified?

2 Regional setting

126-128
Regarding wedge depth, consider writing "between 1-2 m", since in Fig. 1 you state that cracks are all >1 m

128-131
Consider adding these sentences to the results section rather than the regional setting section.

Figure 1
General:
Please homogenize the size of frames and labels, e.g., frame of panel C is smaller than E frame.

Panel A: Please refer to the map of South America and the red dot in the lower left corner. Consider using a frame.
Please give numbers for all isohyets, as it is missing for the one above 200 mm/yr.
Consider combining the symbols for wedge and polygon structures as except for dessication crack polygons, wedges and polygons are one system and it would simplify the map and enhance the position of published structures.
There may be an unintended black line at the lower end of the 100mm/yr line and below “Gonzalez” further to the left.
Is there a reason for the non-continuous coloring style of the DEM (orange vs gray, 2000m vs 2500 m)? If appropriate, consider using a continuous shading.
PANEL C:
“Displaying a subsurface network of large soil cracks (>1 m depth) and vertically laminated wedge structures”
It could appear to the reader that soil cracks and wedges at the outcrop are two distinct features appearing next to each other, but in this case the belong together (I admit that in general cracks can be present without wedges and vice versa). Consider rewriting the sentence. e.g., “vertically laminated wedges with vertical soil cracks along their centers”.
Panel D:
If D is a close-up can the authors show the positions in C?
Panel E:
This is a nicely drawn sketch. I wonder is there a reason for the more whitish colors in the upper part, and the more brownish colors in the lower part of the polygonbody, left of the '5'. The same for the surface crust on the right side of the sketch, which seems to be underlain by a darker surface crust? Please clarify.
142 „Detailed description of the wedge network is given in the result section” → I think this is not necessary and can be deleted.

3 Material and methods

148
Consider using just 'trench wall' as in Fig. 1 instead of trench sidewall

148-151
It is unclear how many samples were collected and at which depths. E.g., What were the dimensions, weight and depth of the surface crust block? How many samples were taken below the crust, e.g., from wedges or polygon?

152
What kind of foil was used?

154
The multi-methodological approach was only applied to the crust and wedges but not to the polygon? Was there a reason for this?

159

Considering the high sulfate cementation, can the authors elaborate how was sampled, using a spoon, hammer, jackhammer?

164
Please add the depth of the base of the crust.

165
Can the authors elaborate how the powder samples were generated? E.g.; hand grinded, grinding mill, grinding duration, temperature during grinding, drying of samples etc., also to ensure for the reader that a phase transition occurred accidentally during sample preparation.

167-169
If whole powder pattern fitting was used for ‘Quantitative phase analysis’, I recommend adding this information to the sentence.

176
EDX stands for energy dispersive X-ray spectroscopy.

179-181
How did the authors verify that all the $CaSO_4$ was removed from the sediment? In Figure 4, largest grain size is coarse sand, what happened with the fraction larger 2 mm, was it excluded? If so, I would add this information.

183
“Etched” suggests that not all carbonates were removed. I suggest to use another word for HCl treatment. Delete space between ‘10’ and ‘%’.

185
It seems that ICP-OES is used here for the first time. When used for the first it should be spelled in full.

187
Please summarize the procedure by Voigt et al. shortly.

200
How was sieved, wet or dry, by hand or sieving machine?

209-211
Please rephrase to “we sampled dust/sediment from a cavity” instead of “we sampled a cavity”.

4 Results

231-233

I would assume that the sulphate-cemented sediment contains also clasts smaller than pebbles. If the authors want to highlight that pebbles to boulder sized clasts are visible in a more fine-grained matrix, the sentence should be rephrased. If referring to ‘Figure3 panel c’, panel a and b must be mentioned first, or at least the whole figure 3.

233-234
From figure s3 it seems that ~30% of crack filling are other salts than anhydrite, can the others state which are these salts or add here the word "mainly" or "dominantly"? It is not clear if the authors refer to table S3 or Figure S3 → ‘(see S.3 in the supplementary material).’

240 Figure 2
I think the combination of a photograph in the background with data presentation in the foreground is

not necessary. Consider just presenting the data in figure 2, since the aroma fan surface is shown in Fig.1. alternatively add the oblique background image of the fan surface to Figure 1. If the whole photograph is directed into NE direction, I suggest to remove the NE notation in the top right corner as it is not appropriate.

245 Figure 3
Legend: 'Macroscopic' instead of 'macroskopic'
Panel notation a) and b) is both at the Aroma site, why is it only notated at panel b? I suggest to remove 'Aroma site' at panel b or to add it at panel a as well.

255
The detailed investigation of the calcium sulfate polymorphs is very interesting. Can the authors elaborate how this polymorph of anhydrite was identified or differentiated from other polymorphs, e.g.; which database was used or provide diffractograms etc.?

257
Please add the formula of halite as for the other evaporites.

264-265
Please reference to the corresponding data or figure at the end of the sentence.

265-267
I think this is very important observation, one could add, that this is visible by the color change of the wedges as well. The increasing cementation towards the periphery was also observed for the polygons in the Yungay region by Sager et al. 2021: "*Furthermore, the cementation of the marginal and thus older parts of the SW indicates that rain events have occurred since the SW formation and the soluble content is not only leached downward but also migrated in horizontal directions. Although the vesicular horizon has formed in the margin of the SW, this cementation must take place without an intense deformation because the vertical lamination is still visible (Fig. 4)".*
However, the next sentence in **lines 267-269** is confusing as it states the opposite, if I understood correctly? Please clarify.

272
Instead of 'match' use 'correlate', and state, if possible, the R^2 value.

281
Please add formula of aluminite.

286 Figure 4
Panel B: The white dots indicate XRD sampling positions. In the supplementary material in *Figure S.6, the* XRD data is given for sample *ARO18-02-001. Do I understand it correctly that this data corresponds to the most right white dote with the number 1. If not, please clarify.*
Panel B.1 it is hard to see, where the arrows for e.g., gypsum or anhydrite are pointing at. Could the authors highlight the clasts or part of the cement somehow, so that this becomes clearer?
Panel B.2: How were the wt% calculated, or is the data maybe semi-quantitative, then it should be [%].
Panel B.3 In the grain size diagram, it appears that a frame around the individual size fraction columns was used, leading to a poor recognizability (on my screen) of the small size fractions. If the fraction would be displayed without a frame, the visibility of the smaller size fractions could be increased. The authors also mention pebbles in cobbles in the sediment, why is the largest grain size fraction coarse sand? Was the other material excluded?

297
"crust surface" instead of "crust top surface" as surface should be always 'top'.

304-306

Regarding the XRD data of crust surface, please provide sampling depth and refer to the corresponding data or figure at the end of the sentence.

306-308
'Subsamples taken from a few centimetres below the surface' → please provide sampling depth

312-314
Please refer to the corresponding XRD data or figure at the end of the sentence.

316-317
Are the >15 cm the cracking width or depth?

355 Figure 5

Figure 5 is a nice figure with a lot of information. But with the current organization, it can be overwhelming for the reader. I suggest reduce the presented information and organize the figure into panels with frames and/or labels. This will also increase readability of the figure caption. In Figure 4 the individual SEM images are not labelled, while here in figure 5 each image was labeled (SEM 1, SEM 2 etc.). In Figure 4 the grain size dots are grey, while in figure 5 they are orange. The XRD data is present without [wt%] but not in figure 4. Please use the same layout for both figures.
The colored images right of SEM 3 are not explained in the figure caption, it appears that they belong to SEM 3, consider providing scale for these images as well. Also, SEM 3 appears two times, mark one as a zoom-in using frames, if this is the case.
Please explain all abbreviations also in the figure caption, e.g.; GS = grainsize
The information of the 'general crust data' should be presented in the text instead of in the figure, to further clear the Figure.

Discussion

General comments:

Please revise the discussion regarding the major comments (haloturbation vs dehydration of calcium sulfates/shrinking)

344-348
In the context of the major concerns, it remains unclear why the presence of Calcium sulfates in the wedges is an indication for haloturbation as dominate wedge formation process. As the authors nicely investigate different calcium sulfate phase and polymorphs it would be appropriate to state the mineral phases instead of the general term "calcium sulfate". As mentioned further above, cemented wedges were also observed in Yungay, but with a lower cementation degree, and a cementation of the wedges that corresponded to the cementation in the polygon, e.g., gypsum at 10 cm, anhydrite at 30 cm and halite at 70 cm depth. Consider adding a short paragraph comparing your results with wedge results from other studies in more detail, as this would be very interesting.

352
It is unclear what is meant with 'pattern' here?

355-358
Although the formation of anhydrite polymorph is still under debate, can the authors imagine or disprefer dehydration/shrinking processes that leads directly to β-anhydrite or would gypsum dehydration inevitable lead to γ-anhydrite that needs to be dissolved in a highly saline solution and the re-precipitated as β-anhydrite? In this sense it would be also interesting what anhydrite polymorph is dominating in the polygon. If one would assume that polygonal cracks form by dehydration, the polygon should contain γ-anhydrite (as a result of dehydration) and if the wedges are cemented over time by saline solutions, one could expect β-anhydrite. Another question would be if β-anhydrite can

hydrate back to gypsum, do the author have references or hypothesis to this? β-anhydrite is often considered insoluble in the literature, do the authors know if this is also the case for geological timescales? Although it would speculative, the authors could give more insights on their thoughts regarding this topic.

358-361
How would primary gypsum be differentiated from secondary gypsum?

362
"silicon and phosphorous". Earlier in the manuscript, the authors use the element symbols (e.g., for S and Ca) instead of element names. Please homogenize.

364-367
This is very interesting point, as it could explain dehydration (shrinking) processes for both anhydrite polymorphs γ-anhydrite and β-anhydrite.

368-415

Please edit this section regarding the main concern of haloturbation vs dehydration (shrinking). Also why does the saline solution exceeds saturation at greater depth and at which depth exactly?

378-381
This is a critical sentence that should be rephrased or deleted, as there is no reference or argument for crack formation by subsurface pressure, further it contradicts the shrinking argument made by the authors in lines 413-415. The argument that expansion leads to upward deformation may be correct, but it is no explanation for soil cracking.

386-387
Consider elaborating here about cementation of wedges by repeated rain events as mentioned above.

387-391
Consider adding that swelling shrinking applies probably mainly to the polygon. It could also be added that saline water migrates also through pore space and not only along the crack.

392-394
How do the authors know that salt heave processes intensify and do not occur e.g., with the same magnitude over time? I agree that salt heave mediated microtopography can visualize a polygonal surface pattern, but it is not the only reason why a pattern can be observed. In the Atacama Desert, the patterns are also visible due to color differences, e.g., darker or brighter wedges compared to the polygon body as a result of compositional differences or by morphology, e.g., high-center and low-center polygons. Morphology differences can be a result of erosion or soil deforming processes, including thermal expansion or salt heave (as the authors state correctly) leading to elevated polygon shoulders (low-center polygons). Therefore, please elaborate on the visualization of polygon patterns in the field.

403-406
I agree that the outer parts of the wedges are likely the oldest part and I support the idea that salt-related processes may destroy initial lamination. But I disagree that wedge formation is possible without initial soil cracking. Therefore, I suggest to elaborate on this or to remove the statement.

408-410
Do the authors mean phase transition from gypsum to anhydrite at the center or periphery, please clarify?

410-415
Again, I really support the attribution of crack opening to shrinking, but it should be differentiated from haloturbation.

Figure 6

Figure 6 is a nicely drawn sketch, but considering my main concern, I suggest some re-editing.
Panel 1: looks nice, no comments.
Panel 2: I would show only the precipitation of salts and if wanted some haloturbation after the rain event (step 2). As a result of haloturbation also the original texture should change, leading to dislocation of some of the drawn clasts or some salt heave of clasts at the surface. I would move 'shrinking (dehydration)' and 'crack formation' and put it in the third panel (step 3), as this should occur after step 2. The red arrows that lead to the crack are due to contraction forces. However, this could be only visualized if the authors would show two wedges and a polygon in the center, where the arrows are directed towards the polygon center. As it is now in panel 2 shrinking and swelling seem to be directed in the same orientation.
Also in Panel 3, the subsequent filling of the cracks by aeolian-derived sediment, leading to the first vertical lamina should be visualized.
In Panel 4, I would show that the repetition of step 1-3 leads to wedges and clast shattering in the polygon over time. Then the formation of the surface crust can follow. If the authors do not recognize a microtopographic signature by salt heave I suggest to not draw it (or at least show it less intense).

437-476
The section regarding the crust formation is very interesting. Could the authors maybe elaborate on the extent of the crust. Is it only locally or covering the whole aroma fan, and is the crust also present in e.g., old gullies or river channels? How do the authors know the aroma fan surface is free of a surficial polygonal patterned ground, was there some reconnaissance by drone imaging or by satellite imaging (e.g., Google Earth)?

480-482
Please refer to the corresponding data or figure.

488
Aluminite is probably not detected in the clast, but rather in the filled fractures of the clasts right? "Aluminite is also detected in the fractured clasts in…" change to "Aluminite is also detected in the fractures of the clasts in…"

498-500
Can the authors elaborate why "presence of sulphates reflects minor dissolution and reprecipitation of salts". Why only minor, please clarify.

504
"as β-anhydrite also requires highly saline solutions to precipitate." Maybe the authors want to add "presumably" as they state earlier that formation of anhydrite polymorphs is still under debate.

550-551
This seems like a completely new model of polygon-wedge formation by fog. Is this idea presented by Cereceda et al. or by Schween et al.? If so, the reference should be at the end of the sentence. If it is a hypothesis of the authors, they should either explain it in more detail or consider removing it, as this would be an interesting topic but may not be within the scope of this study.

560
Instead of haloturbation the authors could discuss the relevance of other salt-related processes for polygon formation on Mars (salt based thermal contraction/expansion or dehydration/hydration).

Figure 7:

Figure 7 is a nice figure showing polygonal ground from different sites with different conditions, but I think the information content is too low to show it in the main manuscript, also since the comparison of polygonal ground between periglacial areas, Atacama Desert and Mars in the text is rather short, and that the polygons near Rio Loa were not investigated in this study. I suggest to remove the figure or to place it in the supplementary material.

Conclusion:
The conclusion can be shortened, as it reads more like a summary rather than providing new concluding remarks. E.g.; it could be highlighted that with this study another type of polygonal patterned ground has been identified and described in the Atacama Desert, which broadens the diversity of patterned grounds in the Atacama. Further, the analysis of the crust indicates that polygonal ground (usually a near surface feature) can be buried, thus enabling the possibility of multiple generations of polygonal patterned ground along a vertical ground profile. To make a link to Mars, the authors could mention the recent identification of buried polygonal ground underneath Utopia Planitia (https://doi.org/10.1038/s41550-023-02117-3). Given the new age constraints of the crust the authors could also highlight that polygonal patterned ground can be rather old/ancient landscape features in the study region (e.g., in contrast to recent gullies or fluvial deposits after the rain events from 2017).

845
The doi link seems not to be working for the reference.

Supplementary material

Figure S.1
Can the authors provide a scale for the photograph?

Figure S.2
Can the authors provide a rough size of the board used as scale for the photograph?

Figure S.3:
Please add more information to the figure description, e.g., that XRD data is presented, consider using panels to organize the figure. Where are the two black lines pointing to? Is the XRD quantitative or semi-quantitative please add this information too.

Figure S.4:
Do the authors maybe wanted to say “red points” instead of black points, as I don’t see black points.

Table S.4: XRD results
Can the authors state the unit for XRD analysis, e.g.; % or wt%?
For the more exotic minerals such as Aluminite or Konyaite, could the authors provide individual diffractograms as done for Figure S.6. The authors could also highlight the main peaks for these minerals.

Figure S.6:
Can the authors add the information that the sample belong to the surface crust.

Figure S.7:
The illustration description reads redundantly, please shorten it.

Figure S.9:
Could the authors leave a “white space” between the individual figures, as is the case with Figure S8?

Photogrammetry/Methology:
What is meant with: “The sample was turned upside down and pictured in four runs” where it the four

runs with similar camera orientation or was the camera angle changed for each run?
If the picture count is stated for ARO18-02, it should be done for the other samples as well (or leave it out here, as it is stated in the Agisoft reports)
Why was the scaling of the 3D model done with distinct features of the sample when scale bars were laid out, or was it the combination of both, please state?
How was the density of ARO18-02 determined using photogrammetry if it could not be turned around?
For final publication, authors should consider presenting the Agisoft processing reports as a separate file, as page numbers, figure and table comments do not match the rest of the supplementary material, or editing the reports, which is likely to be more time-consuming.