RESPONSE TO REVIEWER RC#1

Author comment: Thank you for your careful review and useful comments that will help improve the manuscript. See below our responses to all comments.

All line numbers indicated in the responses correspond to the numbering of the revised version with correction tracking

This paper attempts to quantify the topographic and erosion-rate controls on vegetation changes in a badland drainage basin. Based on the data, feedback mechanisms involving vegetation changes and geomorphic processes are proposed.

Concerns:

1) Suitability for the journal: the analysis is heavily focused on vegetation dynamics. Four of the five figures showing results (i.e., Figs. 2-5 out of Figs. 2-6) are focused on different remote-sensing methods for quantifying vegetation change. As such, the paper would be more suitable for Biogeosciences or a similar journal that focuses on vegetation. A lidar difference map is discussed but isn't presented and so cannot be assessed or reviewed.

Response : ESURF webpage mentions that the journal is dedicated to "research on the physical, chemical, and biological processes shaping Earth's surface and their interactions on all scales", which encouraged us to submitted our contribution to this journal.

Also, analyzing the feedbacks between erosion and vegetation dynamics requires studying (1) the effects of vegetation on erosion, a topic which is widely discussed in the geomorphology community, and (2) the effects of erosion on vegetation dynamics, which is the topic of this paper, and which is less commonly addressed in the geomorphology community. That's why we thought this contribution would be of interest for ESURF readers.

We have already written much of introduction in this direction, and argued for the need to understand vegetation dynamics in order to better predict erosion, see for instance lines 38-40: "This increasing vegetation cover has the potential to reduce erosion rates. Predicting erosion therefore requires understanding how the intertwined vegetation dynamics and geomorphic processes will respond to climate forcing" and lines 44-47: "Predicting future badland erosion is therefore crucial for hazard mitigation, hydropower management, drinking-water quality, biodiversity conservation and restoration, and for improving climate models. This in turn requires understanding how vegetation control over badland erosion evolves through time"

We have added the following in the introduction in the revised version (lines 56-58): "While the effects of vegetation on slope stabilization have been largely studied, the reciprocal influence of erosion processes on vegetation dynamics has received less attention".

2) The paper discusses how differencing of lidar surveys conducted between 2015 and 2021 were adjusted for the "best alignment" and then combined with measured sediment losses to obtain erosion rates. The paper states that erosion rates obtained in this way assume uniform erosion across the study area (line 174). That contradicts Figure 6, which shows that slope and erosion rates are positively correlated. The authors should explain more convincingly how their "best alignment" procedure and the assumptions inherent in that procedure may or may not be consistent with other results they present.

Response : We understand that our initial phrasing was misleading. What do we mean exactly by realigning the two LiDAR point clouds? Each point cloud was already georeferenced so the two point clouds should be already aligned, i.e. they should coincide where there has not been topographic change between 2015 and 2021 and the places where they do not coincide should reveal erosion or deposition patterns. However, the spatial referencing of each point cloud is not perfect, so if you compute directly the distance (as a measure of erosion) between the two point clouds, what you see first is the bias due to the errors in the referencing of each point cloud. Our realignment procedure is meant to correct for this bias, i.e. remove the large scale trend, so that we can analyze the details of the small scale variability in erosion. It does not remove the small scale variability in erosion. When we realign the two LiDAR point clouds, we apply the same unique translation/rotation to all the points of one point cloud. Searching for the best alignment means searching for the translation/rotation that minimizes the distance between the two point clouds in average. It does not produce a uniform distance across all the space. The only assumption that is made here is that there is no large-scale trend in erosion (we called it "macroscopic" trend in the paper), e.g. average erosion on the Northen half of the study area is not different than average erosion on the Southern half of the catchment. This is because when we search for the best alignment, we allow rotating one point cloud relatively to the other, in order to remove any large scale bias due to initial alignment errors. If there was a real large scale trend, it would be removed by this procedure.

We have explained more accurately the procedure and associated assumption in the revised version (lines 189-199): "Diachronic analysis uses the distance between the point clouds acquired at different times to quantify topographic changes over the period. Although both LiDAR point clouds were already georeferenced by their producers, we further refined their alignment using the ICP algorithm implemented in Cloudcompare (Besl and McKay, 1992). Because flat non-eroding zones are both scarce and not well spatially distributed across the catchment, we chose to use the whole point clouds to search for the best alignment. This procedure is meant to remove any large-scale bias resulting from georeferencing inaccuracies in the point clouds, in order to improve the detection of local-scale variations in erosion. Using the full point clouds for alignment implicitely assumes that erosion does not exhibit a systematic large-scale spatial trend across the study area (e;g. consistently greater erosion in one part of the catchment). Any such trend would be interpreted as a misalignment by the alignment procedure and removed along with the georeferencing bias. On the contrary, this procedure does not affect the spatial variability in erosion rates that will be used later in our analysis".

Also, the paper presents contradictory statements about who filtered vegetation from the lidar. On lines 164-165 it states that the producer did this filtering while line 170 states that the authors did this.

Response : We thank the reviewer for pointing this out. Actually, the LiDAR datasets were both classified by the producers into several vegetation and soil surface classes (Sintegra company for the 2015 dataset and IGN for the 2021 dataset). The filtering, in the sense of selecting only the points that had been classified as "soil surface" from the full dataset, is only a technical step, and was done by the authors. We have revised the text to make this clearer.

3) Mismatch in time scale between vegetation changes and measured erosion rates: Vegetation changes occurred almost entirely as a transition of grassland to forest between 1994 and 2012 (Fig. 2c). The erosion rates were measured later (2015-2021). So, there is no overlap in time between the vegetation changes and the erosion rates.

Response : Regarding the temporality of vegetation changes, we would like to point out that they occur all over the study period (1982-2021), and not only during the period 1994-2012 as stated by the reviewer. Figure 2c was not plotted with a linear horizontal time axis, but as a stack of datasets separated by time intervals of different durations. Vegetation changes appear bigger, in absolute values, between the second and third column (1994 to 2012) because this is the largest time interval (18 years). However when looking at rates of change, i.e. normalizing the absolute change by the number of years, we can see that most changes occur over the whole study period. It is true that the reduction of grassland is mostly concentrated in the period 1994-2012, but on the contrary, we can see that the reduction of bare surface (i.e. colonization) occurs regularly over the whole period, with a slightly faster rate during the first time interval (1982 to 1994).

To make it clearer for the reader, we have updated figure 2c by scaling the horizontal axis with time. And we have modified the text in the revised version to explain that colonization occurs regularly all over the study period: "Comparing these maps indicates a general colonization trend, with a total vegetation cover that increases from 38.87% to 46.12% over the study period. This increase happens regularly over the period, at a rate of +0.2% per year in average. Bare ground diminishes from 62.23% to

53.98% as it transitions towards grassland and pine forest. G, and grasslands is reduced from 14.76% to 7.57% and is mostly replaced by pine forest, with most of this transition happening. in the 1994-2012 interval. In total, the largest change is the pine forest that increases all over the period from 17.318.4% to 34.14%, at a rate of +0.4% per year in average. The amount of deciduous forest stays stable at around 4-5%" . (lines234-239)

Next, erosion data is only available over the period 2015-2021. One option was to compare it with vegetation changes over the same period, which would include more noise because the vegetation changes are smaller than changes over the whole period. Second option, that we chose, was to compare is with vegetation changes over the whole period, based on the observation that the colonization trend was regular over the period. We have actually run the analysis for the two options, and the results are similar. We have added this information in the revised version of the paper (lines 305-308) and have added a plot in supplementary materials (figure S2) that show that the trend in figure 6e is the same, whether transitions are computed over the period 2015-2021 or over the whole study period 1982-2021.

4) Comments regarding Figure 6: C1: The results presented in the figure lead the authors to conclude that slope (line 311) and drainage area (line 314) are the main controls on spatial trends in vegetation change. Figure 6d (there are no letters labeling the panels of this figure, so I am just guessing that this is supposed to be panel d) shows that the transition probability to extinction approaches 1 and colonization approaches 0 as drainage area approaches 10^5-10^6 m^2. Figure 5 does not seem to show this. Areas of extinction tend to occur throughout the study area – they are not clustered in valley bottoms with drainage areas 10^5-10^6 m^2 as far as I can tell. If they are, please visualize this clearly.

Response: First we thank the reviewer for noting that labels were missing and we have modified this in the revised version.

We would like to underline that the transition probabilities that are represented in Figure 6 are computed as the ratio of the number of pixels that were colonized (respectively extincted) divided by the number of pixels that were initially bare (resp. vegetalized), as explained in the method section (lines 225-226): "for each potential explanatory factor, we binned the data into classes and computed empirical colonization probabilities as the observed proportion of pixels that become vegetated among the pixels that were initially baren in each class") . Areas of high drainage are infrequent compared to the whole surface of the catchment, and among these, and most of them were bare at the beginning of the study period. Therefore the extinction probability close to 1 in this class does not represent a large absolute number of pixels, that's why it does not appear as a cluster of extinction in valley bottoms. In order to make this clearer, we have added a plot in the supplementary material that show the initial distribution of bare and vegetalized areas across the different variables (Figure S1), and we have added a mention in the text that the transition probabilities plotted in Figure 6 are relative and not absolute values. “These probabilities do not provide an absolute surface change in the catchment because they are related to the initial distribution of vegetated and bare surfaces according to each factor, which is provided in the supplementary material (Fig. S1)”.

C2: Please help me understand how the transition probability for colonization of areas with 25% slope could be 0.5 (meaning non-colonized areas with 25% slope have a 50% probability of becoming colonized, and, as Fig. 6a shows, with all areas of slopes between 10% and 60% having transition probabilities of at least 0.3) when colonization is, in fact, extremely rare (i.e., the dark green pixels in Fig. 5 seem to make up less than 1% of the study area). This is another example in which the results presented in Figure 6 just do not seem to align with the trends (or lack thereof) in Figure 5

Response : We would like to underline that the colonization probability is computed based on the surfaces that were initially bare in 1982 (beginning of the study period). In this catchment, it is important to note that (1) slopes are very high, mean slope is 58% as stated line 87, and (2) most of the lowest slopes were already vegetalized in 1982. Therefore surfaces of slope <60% that were bare in 1982 represented only a small fraction of the total catchment in 1982. What we observe in Figure 6a is that among these surfaces, the colonization probability is 0.3 to 0.5. Even if it represents a large proportion of these initially bare surfaces of slope <60%, it does not necessarily mean a large surface relatively to the whole catchment (as seen in Figure5).

Also, note the dark green pixels in Figure 5 represent 7.5% of the study area (increase from 38.7 to 46.2% as stated in lines 206)

As an attempt to avoid such misunderstandings, we have provided a supplementary plot that shows the initial distribution of vegetated and bare surfaces across all factors, i.e. the surface over which the probability is computed for each range of values (Fig S1). We hope this will help the reader realize that a large transition probability in Figure 6 does not necessarily mean a large surface change in Figure 5.

5) Variability in the data about mean trends: please present box plots (min, max, median, and quartiles) for each bin, rather than just the average) in Fig. 6 so that we can see how much variability is present.

Response: The values that are represented in Figure 6 are not mean values computed within a distribution of values. Instead, they correspond to the empirical probability of transition computed over each range, as the number of observed transitions divided by the number of initially bare pixels (see methods line 220-222). For this reason, it is not possible to estimate a min, max or quartiles for this value.

6) Lines 289-290 suggest that increased veg cover and/or greening is happening worldwide. In North America, the primary temporal trend is increasing tree mortality due

to an increase in wildfire size/frequency/intensity and pest infestations, both driven in part by greenhouse warming (doi.org/10.1016/j.foreco.2009.09.001

Response: we totally agree that greening is not happening worldwide, but is rather concentrated in cool places (where vegetation growth is temperature limited), i.e. mountains and high-latitude region. This is why we wrote in our text (line 320) “greening trends reported in other mountainous and high latitude region worldwide”.

We have added some text in the discussion to account for this (line 330-332): “On the contrary, the greening trend observed here also indicates that wildfire and pest infestation, which are increasing tree mortality in other parts of the world, are not playing a role in our site”

7) Why focus on greening? I can understand how changes in vegetation type (Fig. 5) may be influenced by, and in turn influence, erosion rates. But I don’t see how greening (which comprises a large portion of this study) is related to erosion rates (which are primarily related to vegetation through vegetation type or percent bare ground) because greening can occur without any change in root cohesion or other aspect of vegetation that tends to be more directly related to erosion. This concern dovetails with point 1 on how this is primarily a vegetation study with limited connection to geomorphology or landscape evolution.

Response: Greening is a macroscopic measure easily obtained from remote sensing, and widely used to track large scale vegetation change. For this reason, we think it is useful to study how this measure can be interpreted in relation with erosion studies. Greening is an integrated measure that results from different mechanisms. Here we show that combining greening and aerial images (at a finer spatial scale) allow disentangling what is in this greening measure: an effect of vegetation growth, an effect of the initial type of vegetation, and an effect of spatial increase in the surface covered by vegetation (ecological succession). From a geomorphological perspective, disentangling these different drivers of greening is essential, because each of them may interact in a different way with erosion. For instance, the transition from bare soil to grassland is likely to affect erosion rates, and be controlled by erosion rates, more than the growth of an existing stand of trees. We have added some discussion in the revised version to explain how this greening analysis could be of interest for geomorphological studies. (lines 492-494)

RESPONSE TO REVIEWER RC#2

Author comment: Thank you for your careful review and useful comments that will help improve the manuscript. See below our responses to all comments.

All line numbers indicated in the responses correspond to the numbering of the revised version with correction tracking

General:

This paper explores the combination of high-resolution, low-frequency data with low-resolution, high-frequency data on vegetation class and NDVI to explore vegetation growth/die-off patterns in a badland complex and their relation to erosion. The research is exciting and timely and its rationale is made clear for a general ESurf readership; the discussion of the results is thoughtful and well-contextualised. Nonetheless, I felt the methods and results need clarification and the figures need polishing; in particular, there were frequent discrepancies between the text and figures. I have made numerous comments below in this vein and therefore suggest minor revisions before publication.

Throughout:

There are grammatical / typographical errors. A few examples (not all): superscripts appear to have been omitted; some references need formatting; units for distance switch between m and cm; figure numbering in text doesn't match captions.

Author response: This has been adjusted in the revised manuscript

Intro:

Anderson et al. 2020 is not in the reference list.

Author response: the reference has been added

51-55: There may be some relevant examples from the New Zealand literature here concerning vegetation controls on badland (I believe this is equivalent to what they'd call "gully complex") erosion. Mike Marden's work is probably the place to start, e.g. https://doi.org/10.33494/nzjfs522022x226x, https://doi.org/10.1002/rra.882.

Author response: Thank you for these references, we have extended to introduction to account for this previous work.

Materials and methods

82 "Highest recorded worldwide" – do the authors have a reference for this statement?

Author response: references have been added to support this statement (Chen et al, 2022, Vanmaercke et al, 2011)

93 “An extended study area…” – what assumptions allow this larger area to act as a proxy for the study catchment? I feel more detail is needed to justify this decision.

Author response: We have indicated in the revised version that the distributions of slope, elevation, aspect, were similar between the study catchment and the extended area.

Figure 1: What is the underlying image in the top-left panel? How does the 8 km^2 Landsat analysis area extend around this catchment – is it marked on this panel? I note that vegetation outside of the catchment (particularly east and north) is more dense and complete – how does this affect its ability to act as a proxy for the Laval catchment? Please add subplot labels. Please remove the spell-check underlines from the bottom-left panel. The caption requires some elaboration – e.g. why were these two photos selected? What are their locations and viewfields? These could be marked in the bottom-right map to orient the reader.

Author response: Thank you for this feedback. We have adjusted the figure as suggested. The location of the 8km2 landsat analysis area appears in Figure 4. The extended area is not supposed to be an exact proxy of the Laval catchment in terms of the relative distribution of each vegetation type, but rather to provide more examples of the same vegetation types in a similar setting (topography, lithology, climate). For instance, the east and north sides of the Laval catchment with denser vegetation are similar in topography with the deciduous forest in the center of the catchment, making it possible to increase the number of Landsat pixel representative of this vegetation type. We have detailed the explanation in the revised version (lines 99-100)

The photos are taken from within the catchment, and are meant to illustrate the type of landscape, and young pine colonization on bare surface. The specific viewpoint from which they were taken in the catchment was not recorded.

85 “Two areas”: the first is well defined, but I was expecting a second catchment to be defined after. Is the “second” area in fact the “extended study area” included in the Landsat analysis? I recommend clarifying this point.

Author response: Yes the “second area” is actually the “extended area” mentioned later, we have clarified this point in the revised manuscript

102: Could the two rain gauges be named on Figure 1, or identified somehow with annotations? This would help the reader know if “Laval rain gauge” refers to the one in the headwaters or outlet.

Author response: we have added in the text that the Laval raingauge was located near the catchment outlet.

103-105: Is the Plateau station the same as the Draix station? I recommend clarifying and/or annotating the map.

Author response: This has been clarified in the text

111: I suggest distinguishing between “high frequency” and “high resolution” throughout. Readers might assume that, given the Landsat data / other rasters used here, “high resolution” refers to spatial rather than temporal resolution.

Author response: we thank the reviewer for this suggestion that was incorporated in the revised version

112 “complemented” how? A quick note on any gap-filling procedure is required.

Author response: missing periods at Laval raingauge are simply directly replaced by Pompe raingauge data, based on the fact that these raingauges are less than 1km apart and have been found to measure very similar daily precipitations (R2 = 0.96) and yearly precipitations (difference <1%). This has been explained in the revised paper.

116: I realize the extended details are in Klotz et al., but a quick note on the meaning of “automatic samplers” (what are they sampling – bedload??) would help here.

Author response: they sample water including suspended sediment, and the samples are used to measure suspended sediment concentration. This was added in the revised paper.

137-145: Could have slightly less detail here. I feel e.g. that it is sufficient to say “Landsat 5-8” and “June-September” etc.

Author response: this was modified as suggested

147: “NDVImax” in which context: annual? Monthly? The reader needs more context here (I don’t know that NDVImax was defined until much later in the paper). In general, I think this paragraph needs more clear explanation for a geomorphological audience; at present I don’t think the description favours reproducibility. For instance, how exactly is phenology modelled on a pixelwise basis? How exactly are NDVImax estimates “adjusted”? L150 says this method “partially” corrects for sampling bias resulting from increasing Landsat frequency – so how did the authors resolve (or even measure) any remaining bias? Hence I feel more detail would help readers understand the method here.

Author response: NDVImax is defined in the text as the maximal value from June to September. We clarified that. For the phenology modelling to correct for the sampling bias, we refer to Choler et al. (2025) that explain in detail the method and demonstrate its validity for correcting sampling bias

157: Which “high resolution vegetation maps”? The 0.5 m ones described earlier, in their pre-binarization state? A little more clarity/consistent terminology is warranted.

Author response: clarified in the revised version

176: “C2M algorithm” - citation?

Author response: was added in the revised version

182 : “the distance associated with the point cloud” – does this refer to the vertical distance between the two point clouds after their offset was applied? Please clarify.

Author response: clarified in the revised version

183 “Annual erosion” – suggest “mean annual erosion”.

Author response: added as suggested in the revised version

188: “local erosion rate” – from the DoD? If so, this could be made clearer.

Author response: done

189: Ellipsis feels inappropriate here.

Author response: removed

198: “depend on” – suggest “vary with” to avoid conflating correlation and causality.

Response: changed as suggested

Results:

Figure 2: I suggest scaling the distance between columns in (c) to approximate the time interval between years – e.g. the final 3 columns should be closer together. Suggest annotating (a) and (b) with the year.

Response: Yes we agree that this would make the figure clearer ans we have modified it accordingly.

I note that L 208 says pine forest was 17.3% in 1982 but Fig 2 c shows 18.4%. The paper needs a careful check to ensure details are consistent across text and figures.

Response: we thank the reviewer for noting this discrepancy that we have corrected

214: “Fig. 6” – does this refer to Fig. 3? There are numerous other mismatches of this type; again, a careful check for consistency between text and figures is needed.

Response: corrected and all references to figures in the text have been carefully checked

Figure 3: What is the red heptagon in panel (b)? Panels a/b need labelling. Areas that “have been removed” need delineating – e.g. is there a minimum red patch size that was removed? Were all red patches removed, or only those outside the catchment boundary? Could the caption explain why both panels are needed, not just a larger view of panel (a)? It might be a good idea to remind the reader in the caption (or the paragraph above) of the duration over which the trends were measured.

Response: The red heptagon in panel b represents the extended area that was used to identify pure pixels (Landsat data, 30x30m) for NDVI analysis. This extended area was used to ensure a sufficient number of pure pixels for significance. Pure pixels were defined as those where more than 90% of the surface is the same vegetation type over the whole study period. The red zones in figure 3 corresponds to clear cuts, therefore they did not exhibit the same vegetation type over the study period and had no chance to be defined as pure pixels. They were not removed using a specific delineation, but simply no pure pixels were identified in these zones, so there were not used for the NDVI analysis. We have explained this better in the methodology section, by writing: "When more than 90% of the Landsat pixel is of the same vegetation type over the whole study period, this pixel is defined as a "pure" Landsat pixel, which means that it is representative of one vegetation type. This definition automatically excludes from the analysis the few clear-cut zones where initial vegetation type differs from the final one".

We have also removed panel (b) as suggested. And the duration of the greening measurement was added in the caption.

Figure 4: Remember units for the y-axis label. Remember to label panels a/b. Could the legend for (b) have more detail about the direction of the transition (e.g. bare-veg vs veg-bare)? I know this is mentioned in the text, but adding it to the legend will help time-scarce readers. I think the legend of (a) needs more detail: e.g. "Density" might be updated to "% of pixels purely in this class" or similar – if I am interpreting this wrong, you'll hopefully see why more precision is needed! Also, what are the black vs grey markers in (a)? Aside from these details, this figure is informative and eye-catching, and I commend the authors on the great data visualisation.

Response: units have been added. The transition in figure 4b is the transition from bare to veg, this was added in the figure. In the legend, "Density" does not correspond to the percentage of pixels that are purely in this class. It indicates with a level of transparency an area of the plot in which xx% of the pure pixel points are located. We have updated the caption to make this clearer.

240: "temporal dynamic of NDVImax, averaged across all pure pixels of each type" – I am confused here: does it mean the trend is extracted pixel-wise for each pixel of e.g. class "Pine", and then these trends are averaged, and then the trend is compared to the trend in e.g. temperature? Or does it mean that, for each year, the pixelwise NDVImax is averaged across all "Pine" pixels, and this value is then compared to e.g. mean annual temperature for the same year? Clarification will prevent readers having the same confusion; moreover, I suggest moving such details to the methods.

Response: yes we have clarified this point and moved it to the methods (line 176-177) What we actually do is that we take all the pine pixels, and then for each year, we average NDVImax over all these pixels. From this we obtain a time serie of "pine NDVImax", which is compared to the time serie of temperature.

We have added this text in the revised version: “For each year, we averaged NDVImax across all pure pixels within each vegetation type to obtain time series of average NDVImax for each vegetation type”

245: The legend of Table 1 goes some way to clarifying my confusion above, but introduces a new confusion: “annual maximum NDVI for each vegetation type” doesn’t mention any averaging. What about “annual maximum NDVI, averaged across each vegetation type”?

Response: yes that is a much better option that we have adopted in the revised version

Figure 5: Legend text hard to read. Suggest updating caption to remove mention of vegetation “types”, given that this figure is based on the binary data.

Response: agree, we have replaced by “evolution of the distribution of vegetated and bare states over the study period”

257: Slopes were expressed as proportions earlier; suggest using consistent units.

Response: we have adjusted slope units into % across the paper

260: How can a slope be higher than 300%? Also, looking at the graph, I think this should say “more than 0.25” instead of “more than 0.5”? As with the earlier figures, please ensure the text matches the values in the figure.

Response: There were a few typing errors and inaccuracy in this paragraph that we have corrected. Revised version now says: “On the contrary, colonization is infrequent at slopes close to zero, and at slopes higher than 80%. Extinction shows the opposite trend, with extinction probability less than 0.1 for slopes lower than 70% and more than 0.25 for slopes higher than 120%”.

261: Earlier in the paragraph, the first tick-mark value on the y-axis was rounded down to 0.1. Here, 0.12 is used. Please be consistent with rounding across all sub-plots and the text describing them. Similarly, I was confused by “then reaches 0.5 when 4% of the neighborhood…” because, as far as I can tell from Figure 6b, colonisation probability reaches 0.5 when about 20% of the neighborhood is vegetated. Again, please ensure text accurately describes the figures.

Response: thank you for noting these inaccuracies. We have adjusted the rounding to 0.01 in all the result section to be consistent as suggested and have corrected the error about figure 6b

274-278 (correlation tests): It would be good to have table or plot in the supplement to show what the authors mean by e.g. “strongly associated” in L 277.

Response: added as a supplement as suggested, Figure S3

Figure 6: Remember to label the subpanels.

Discussion :

In figure 2 and the relevant results text, it is clear that pine forest has almost doubled in relative presence across the study period. What do the authors feel is the mechanism for this increase? Is this to do with the specific physical traits of the pine species that allows them to outcompete the deciduous forest when there is grassland/bare soil to be colonized? These questions arose when I read the results and I felt like they could be explored in more detail in the discussion.

Response: Pine trees are a pioneer species, meaning that they are able to colonized quickly new habitats. That's why we observe a lot of spontaneous colonization by young pines (see figure 1). On the contrary, the deciduous forest (here mainly oaks and beeches) comes later in the ecological succession, because these specie grow more slowly, requires a more stable environment.

This was added in the revised version: "Pine trees account for the largest changes observed, both in spatial extent and growth. As pioneer species, the pines present at this site can rapidly colonize new habitats, as shown by the spontaneous establishment of young individuals on bare slopes (Fig. 1d). In contrast, oak and beech are characteristic of later successional stages, since they establish more slowly and require more stable environmental conditions" (lines 340-343)

I felt that the negative greening trends in Figure 3 and the pattern of erosion they reveal (or not) could have been better contextualised against the literature from badlands in other humid and/or rapidly eroding landscapes, e.g. Kumar et al. 2026 (https://doi.org/10.1029/2025JF008720), Taylor et al. 2018 (https://doi.org/10.1016/j.geomorph.2017.10.007), Fuller et al. 2020 (https://doi.org/10.1002/esp.5010), Leenman and Tunnicliffe, 2018 (https://doi.org/10.1130/B31849.1).

Response: Negative greening values in Figure 3 (red zones) indicates vegetation removal over the period, which can indeed be related to erosion processes.

We have added the following in the revised paper (lines 374-379): "Greening map (Fig. 4a) also exhibits localized areas of negative values (red zones) indicating vegetation loss over the study period. On the left bank, in the southwestern part of the catchment, these negative values correspond to a large landslide that occurred in 1998 (Traveletti et al, 2011). Similar negative greening is observed in the eastern headwaters, corresponding to the steepest slopes where shallow landslides and gullying are frequent. Negative greening is therefore a signature of intense, but spatially heterogeneous erosion processes, that tend to be concentrated in the steepest headwaters, as observed in other badland landscapes (Fuller et al, 2020, Taylor et al, 2018, Kumar et al, 2026)"

On reading Figure 4, I became curious about the fact that "pure" pine pixels show stronger greening trends than other vegetation. To what extent is this related to the age cycle of these pines – do these data record the maturing of trees that were dispersed/self-sown shortly before the satellite era? Or is this greening the result of increasing (a) density or (b) health of trees with no change in mean age? I feel the authors could discuss the potential mechanism driving this "greening" in more detail.

Response: First the greening of pine pure pixels is higher because it is a pioneer specie that grows faster than deciduous. The definition of a pure pixel implies that the vegetation type has not change over the study period, therefore these pines were already present at the beginning of the study period, but it is possible that some of these stands were young at that time and grew towards mature tree during the study period.

Secondly, greening is computed based on NDVI, which is related to green above-ground biomass. All the data points of greening vs initial NDVI (figure 4a) form a bell curve. This is consistent with the classical logistic growth model in ecology (Begon et al, 2006) according to which biomass growth first increases with initial biomass, because more biomass is available to support reproduction, and due to facilitation effects (e.g. increased soil water storage and soil stabilization), then decreases for high values of initial biomass due to resource limitation and competition effects (access to water, nutrient and light). The pine pure pixels correspond to ecosystems of intermediate initial biomass therefore have the higher biomass growth rate.

We have detailed this point in the revised version (lines 343-363): "Furthermore, the hump-shaped relationship between greening and initial NDVI (Fig. 4a) suggests that aboveground biomass dynamics follow the logistic growth pattern predicted by population ecology (Begon et al., 2006), with maximum growth occurring at intermediate biomass levels. Young pine stands fall within this high-growth phase, whereas mature stands and sparsely vegetated areas exhibit lower rates of biomass increase"

Begon, M., Townsend, C. R., & Harper, J. L. (2006). *Ecology: From Individuals to Ecosystems* (4th ed.). Blackwell Publishing.

Pettorelli, N., Vik, J. O., Mysterud, A., Gaillard, J.-M., Tucker, C. J., & Stenseth, N. C. (2005). Using the satellite-derived NDVI to assess ecological responses to environmental change. *Trends in Ecology & Evolution*, **20**, 503–510.

I had similar questions on reading L285-297 : are the authors claiming the greening in "pure" pixels results from increasing plant density, or healthier plants? I'm not sure what "enhanced growth" means in physical terms. Moreover, it's not clear whether the hypothesised driving mechanism of shorter frost periods relates to new colonisation, "enhanced growth" in "pure" vegetation pixels, or both – I think the authors could clarify this point in this paragraph.

Response: we have removed "enhanced".

Mountain plant growth tend to be limited by temperature. During the freezing period, plants and tree can not grow. For existing plants, a shorter freezing period means that the growth period can start earlier in spring and last longer. For new colonization, a shorter freezing period means that germination can occur earlier, and that the new seedlings can grow stronger and root deeper before the most erosive events of the summer. We have explained this better in the revised version (lines 336-339)

I was fascinated to read in line 265 that "all bare surface were colonized" at high elevations. I felt that this point was not explored thoroughly enough in the discussion – the authors mention lower gradients on the ridgelines, but is there also reduced competition for sunlight? Or, is there some neighborhood effect here given that the other side of the ridge is generally forested, so that pixels along this ridgeline are in the sweet spot of high vegetated neighbourhood and reasonably low slope? I would suggest a little more discussion of this finding.

Response : We agree that the high elevation points on the East side of the catchment are characterized both by low slopes and highly vegetated neighborhood, which are both favorable to colonization. We have added this in the discussion (line 429-431). However we don't have enough information to separate the elevation effect, or analyze the suggested competition effect.

297: "higher success of recent colonisation" – I couldn't see any data to back this up.

Response: germination occurs in spring, and when frost events occurs after germination, it kills the young plantule that has just germinated. A reduction of spring frost event therefore represents a higher chance for the plantule to survive, and for colonization to succeed. We have rephrased this in the revised version: "A decrease in spring frost events occurring after germination may also enhance the survival of young plantules"

314: "Consistent with the trends seen in areas with the highest drainage areas" – Suggest to be more specific here about the nature of the trend/pattern.

Response : added as suggested

320-1: "slope threshold therefore corresponds to a stability threshold" – this could very easily be tested by comparing the slope to the erosion/deposition depth at each pixel.

Response: Yes this has been tested, and we have added the plot in a supplementary material (Figure S4).

"unconsolidated material" – is the badlands comprised only of unconsolidated material? The introduction describes them as developing on "weak sedimentary rocks" which I think of as "consolidated". The authors might want to specify here if they are only referring to talus downslope of an initial failure that loosened material.

Response: the badlands are developed on weak sedimentary rocks (shale) which are indeed “consolidated”, but due to frost cracking, the first 10 to 50 centimeters below the surface are weathered and unconsolidated. We have mentioned the weathered rock layer in the revised version

334-335: “both studies analysed existing plant cover rather than dynamic pattern[s]” – could the authors explain why existing plant cover would have a higher slope threshold? The logic here is not clear to me.

Response: Existing plant cover might not be at equilibrium with its environment, it may be responding to a previous perturbation. That’s why it is not obvious that the threshold should be the same for existing plant cover and dynamic effect. However we agree that there is no reason why it would be specifically higher. So we have removed this sentence from the revised version

337-8: “slightly lower critical slopes on the SE aspect” – do the authors have a plot to back up this statement?

Response : yes, a plot has been added in the supplementary material (Figure S5).

I found the discussion in 383-393 very insightful. Perhaps the authors might highlight on Figure 7 where they feel their study catchment currently is in the cycle, and comment on how this autogenic cycling may be a third driver of the observed greening trend described in L 298-309?

Response : In our badland site, erosion rates currently exceed uplift, meaning that in the long-term, slopes are decreasing, which makes it easier for vegetation to recolonize. This is already discussed lines xx. However, this is a long-term trend and I am not sure that we can attribute the greening observed over the last 40 years to this effect.

412-416: This paragraph feels a bit out of place, and might be best reduced to a sentence for the conclusion?

Response: we agree that it feels out of place. But it does not feel better in the conclusion. Yet, being myself in charge of long-term observation, I find it important to underline how essential this effort is for our understanding. So we have displaced this paragraph in the next section about methodological contributions, hoping that it would be a better place.

436-460: I think this section could be condensed to be more concise; the level of detail is occasionally superfluous.

Response: we have condensed this as suggested

One thing that I had hoped the authors would reflect on in the discussion are the downstream implications of their results. For instance, knowing what we know now about patterns of vegetation establishment in the badlands in space and time, can we infer anything about the resultant control of vegetation on erosion and ultimately for sediment flux into the main outlet channel?

Response : Thank you for this useful comment. We think this point was already partly addressed in the paragraph about the future of badlands (lines 458-472), but we have extended the discussion in the revised paper (line 459-462). Also, note that our main point is that it is actually challenging to infer how erosion will evolve in the future because vegetation is not the only controlling factor that will change in these badlands (see line 469).

Conclusions:

465: Can we say "in total biomass"? I don't remember the authors commenting on biomass at all; suggest rewording to fit with the rest of the paper.

Reponse: we have replaced it by "aboveground biomass" and explain in the methodology that NDVI was a proxy for aboveground biomass

467: What are "slowly eroding, even depositing environments"? I struggled to imagine the physical processes described here and feel this phrase needs clarifying.

Response: what we mean is that the slopes below the threshold are characterized by either slowly eroding or depositing environments. Whereas the slope above the threshold are characterized by a fast erosion. This has been clarified in the revised version

A small number of statements (e.g. 468-469) need careful checking for whether they are a true factual statement or an interpretation of the results, in which case the language needs toning down with phrases like "appear to".

Response: this has been modified as suggested

469-470: "depend on vegetation type and ecological succession" – can the authors be more specific here?

Response: we have added in the revised version that the greatest greening rates were obtained for one type of vegetation (pine trees) and for surfaces undergoing ecological succession from bare to pine stands. (lines 543-544)

Misc: I could not review the vegetation maps, erosion map, and greening map.

Response: these were already made available to the editor, and next they will be made available with a DOI as soon as the paper is accepted.