REVIEWER 2 : JANA EICHEL

Thank you Dr. Jana Eichel for your precious and constructive comments on our work. We provided discussion on your general comments and made changes in the manuscript accordingly. These changes clearly improved our manuscript.

GENERAL COMMENT

REVIEWER: The authors present an impressive and novel study quantifying the role of environmental heterogeneity on vegetation succession in glacier forelands. The study very nicely combines classical fields with novel remote sensing approaches, making it possible to compare vegetation succession across eight glacier forelands in the Alps. From the technical side, the authors dealt in my opinion very well with many difficulties arising in the study's context, such as assigning a continuous deglaciation age, problems with Landsat data quality etc. The manuscript is very well written and was a pleasure to read.

AUTHORS: Thank you for your kind words.

REVIEWER: I only have a few general comments and some specific comments (see below):

REVIEWER: When comparing your real-time remote sensing approach with a chronosequence approach, your time scale is limited to <40 years by the availability of Landsat data. So maybe the time since deglaciation becomes more meaningful on longer timescales (centuries) than you investigated and the chronosequence approach, despite all its large limitations, is still needed to investigate the complete vegetation succession since the end of the Little Ice Age. This needs to be mentioned and discussed.

AUTHORS: We agree with your comment. Accordingly, we made changes to the text regarding the interpretation of time since deglaciation in the NMDS, also in response Thomas Wagner's comments. We removed the TDS variable from the NMDS framework and changed the manuscript accordingly. The most important result obtained from the NMDS analysis is to be found in Figure 9, which shows the relation between NMDS1 and time since deglaciation. We show that the trend in NMDS1 score is almost always insufficient to surpass the starting point of the succession. We changed Figure 8 and Table 1 accordingly. In Figure 9, it is indeed possible that time since deglaciation will result in overstepping the determinism of the initial starting point over a longer time period (since LIA for example). We added "early" in the beginning of the discussion before "plant succession" to state more clearly that our results are only relevant for the beginning of the plant succession and that other mechanisms might be more important than local environmental context in later plant succession stages (as shown by Wojcik et al. 2021).

REVIEWER: In addition, I see the use of snow free-growing degree days somewhat critical, as you could only determine them for one season (2019). Over a period of nearly 40 years in a changing climate, I would expect that snow cover and growing degree days changed over and between the years, which would have affected vegetation succession. Thus, the reliability of this key indicator needs to be discussed. Looking at your NMDS and pairwise correlation results, it appears that SF-GDD is closely related to elevation anyway, so elevation of the glacier forelands might be a similarly important and more reliable factor for vegetation succession onset and dynamics.

AUTHORS: In this work, we used the snow free-growing degree days (SF-GDD) variable as a proxy of energy availability for plant growth by integrating the length of the growing period (informed by snow melt-out date from Sentinel-2) with modeled air temperature. In addition to elevation and aspect, it also integrates biogeographical elements which is key in our analysis

as our study site spans 1° in latitude with difference in continentality. Accordingly, we consider SF-GDD to be a more meaningful ecological predictor compared to elevation. We agree that relying on only one year in a changing climate might appear limited but the SF-GDD gradient should be interpreted relatively rather than absolutely. Here, the absolute value of SF-GDD is not interpreted but only the relative position of pixels along the SF-GDD gradient.

As you mentioned, it also should be interpreted, in part, as elevation (which explains the strong negative correlation between these two variables). Elevation was kept in the NMDS for interpretation but Elevation, SF-GDD and Neighboring vegetation cover are three necessarily correlated variables.

SPECIFIC COMMENTS

l. 57: Space-for-time approaches: This method does not necessarily rely on the position of plant surveys to estimate time since deglaciation, but can also be done using known terrain ages from glacier stages, maps, dating etc. (cf. Matthews, 1992, "The ecology of recently deglaciated terrain"). Please revise.

We adapted the text to be more generalist and accurate.

l. 213: “Thus, we applied the same method as in Bayle et al. (2022)”. Please add details which method exactly and for which purpose.

We clarified the method used.

l. 222: Add “The” before “complete workflow”

Done

l. 237: add “-“ after intra

Done

l. 315 ff: If I understand your interpretation of Figure 4 correctly, you are looking at when the mean NDVImax per terrain age class crosses the NDVI threshold? Because the error bars seem to imply that some pixels already crossed that threshold earlier than stated here in the text? If this is the case, please clarify in the text that you are talking about mean NDVImax.

You are right. The time specified in the text corresponds to the number of years it takes for the average NDVImax to reach the threshold. We clarified this in the text.

l. 336 “Floristic plots are representative of glacier forefields vegetation dynamics“. Wouldn’t it be the other way around that your Landsat detection matches what is happening at the ground in the plots?

Yes, in a way. But here our thinking is the following: floristic plots on the ground were done with limitation due to field sampling, which could have hindered the representativity of the whole forefield (because more limited or disturbed vegetation tends to be less represented in field sampling because it might be less accessible). With Landsat, we consider that we have information on the entire forefield (even if it is obviously degraded information compared to field sampling) and thus that we are able to capture the “true” heterogeneity of vegetation dynamics within study areas. We showed that for our two remote sensing indicators (TL and GR), variance was similar when considering the entire margin or only pixels overlapping field sampling, meaning that our field sampling captured the overall heterogeneity of the margin’s vegetation. We acknowledge however that if we considered our field sampling to be representative, and that remote sensing is not necessarily spatially representative, we could

argue that we are indeed demonstrating that Landsat detection matches what is happening at the ground in the plots. We modified the section title to more clear.

l. 395: “dynamic” in which sense? Vegetation colonization? Please specify.

We specified “(in term of vegetation colonization rate)”

l. 451: “periglacial”

Done

l. 460: “that” instead of “the”

Done

l. 470: I would not term natural disturbances such as geomorphic processes in glacier forelands “erratic”. They do follow a certain pattern in time during paraglacial adjustment (e.g. Ballantyne et al., 2002; Eichel et al. 2018) and geomorphic processes can have a certain magnitude-frequency distribution. So those geomorphic disturbances are not erratic but can follow certain patterns.

We changed “erratic” by “varying”.

Figure 3: Which data is exactly shown ? Changes of all pixels in one glacier foreland? Changes of one pixel at the site of the coordinates given? Please provide more information in the figure caption.

We corrected the Figure 3 caption which had several wrong pieces of information. We clarified that panel A was for only 1 pixel in Glacier Blanc (corresponding to the coordinates). Panel B and C represent data for the 8 margins.

Fig. 10: Missing reference to Fig. 10 in results section.

Corrected

Table 1: Coarse debris is given as an indicator for instability – is this correct or would it more be an indicator for stability?

Terminology: different terms are used to refer to the interactions between ecologic and geomorphic processes, e.g. bio-geomorphic and eco-geomorphological. I would suggest you choose one term to use throughout the manuscript, most commonly used is in my opinion biogeomorphic (ecogeomorphology was first defined for fluvial systems).

Done