Reviewer 1:

The authors present a global dataset of landforms derived from a high-resolution DEM. They propose new ways to identify plain areas and their transition to hilly and mountainous terrain. The novel way to do this by identifying core areas and including transition areas through a cost distance analysis yields results that seem visually quite accurate when the map is overlayed onto a relief background. Plain and higher relief areas are neatly differentiated. This type of information can be quite useful for geographical and ecological macro studies. The precise workflow does miss details to be reproducible. It is a pity that proprietary software was used and the workflow described in general terms only, which makes replication more difficult. The choice for some cut off values or thresholds (slope, elevation, accumulated cost) is not always clearly explained or motivated.

Response:

Thank you for your valuable feedback. We appreciate your recognition of the dataset's potential for geographical and ecological macro studies.

We acknowledge the need for a more detailed workflow description to enhance reproducibility. In the revised version, we have provided additional details on the specific steps, including explanations of the principles guiding our selection of threshold values (slope, elevation, and accumulated cost).

To classify the hilly and mountainous areas the authors propose a new approach as an alternative to a moving windows analysis that has documented limitations. Landform relief is not calculated with reference to the nearest elevation data within a (small) window, but expressed with reference to a regional baseline calculated by creating a TIN on the basis of the elevation at the border of a mountain range (i.e. where it transitions to plain). In addition, the baseline elevation takes into account the elevation of points along water courses within the mountain to create a baseline surface to act as reference for the roughness calculations. Thresholds are applied to the elevation differences calculated by subtracting the baseline elevation from the actual surface elevation. The lowest elevation differences are labelled hills, followed by low relief mountain up to highest relief mountain. This leads to a conceptual problem. In my opinion, when one talks about a mountain or mountain range such as the Himalaya as a landform, one considers the mountain as a whole, from the foothills to the highest summits as the landform "highest relief mountain". Similarly when talking about the Jura or the Vosges mountains, one would talk about low relief mountains, but not consider only the mountain summits to be low, but the whole landform down to the foot slopes as being the low mountain.

Response:

Thank you for your valuable feedback. We carefully reviewed your concern and analyzed the potential reasons behind the differences in defining what constitutes a mountain. We think that the perception of a mountain is largely scale- and context-dependent. Below, we present two key considerations:

(1) Scale Perspective: In general geomorphometry, "landform" can refer to units at multiple scales. In our results, "mountain" in Level 1 (L1) aligns with the conventional, broader concept of a complete mountain entity. Levels 2 and 3 (L2 and L3) aim to capture local variations within that mountain by subdividing it according to specific altitude and relief thresholds. In other words, Levels 2 and 3 represent finer-scale morphological facets compared to the L1 "mountain." To avoid confusion about terms, we have renamed the L3 'mountain' classes to 'mountain slope', thereby clarifying that L3 focuses on local slope-based subdivisions rather than a single, unified mountain. This multi-scale approach allows users who only need a macroscopic view (i.e., one label for the entire mountain range) to rely on L 1. Meanwhile, researchers focusing on localized processes (e.g., slope erosion, microclimate differences, or altitudinal ecological zones) may benefit from the finer distinctions at Levels 2 and 3.

(2) Context Perspective: As you noted, viewing a mountain as a single entity is a common perspective, emphasizing its unified formation process and general independence. In our study, L1 was designed to capture this "common landform" notion of a mountain. In GBLU, we have improved the bounding precision of L1 with higher-resolution data and advanced methods. However, "mountain" can be a somewhat vague term—different definitions may be useful for different contexts [1]. Mountains often exhibit significant internal variability in altitude, relief, and slope, which in turn can influence climate, vegetation, biodiversity, and geomorphic processes. Because the GBLU dataset is intended for broad usage in geoscience, L2 and 3 highlight these internal subdivisions, which is particularly relevant for analyses of force accumulation, mountain ecosystems, and microclimatic gradients. Similar approaches are reported in the subfields of geoscience such as climate, ecology and biology [2-5]. From this perspective, subdividing what is commonly called "a single mountain" into multiple levels is necessary in many research scenarios.

In the revised manuscript, we have supplemented the text with more details on how each level's terminology is constrained to avoid ambiguity (Lines 99-106). Meanwhile, we have updated naming conventions within Level 3.

Specially, Level 3 classes initially labeled as “mountain” have been renamed to “mountain slope” to reflect their smaller-scale morphological nature. We hope these clarifications address your concerns.

[1] Evans, I.S., 2012. Geomorphometry and landform mapping: What is a landform?. Geomorphology, 137(1), pp.94-106.
[2] Antonelli, A., Kissling, W.D., Flantua, S.G., Bermúdez, M.A., Mulch, A., Muellner-Riehl, A.N., Kreft, H., Linder, H.P., Badgley, C., Fjeldså, J. and Fritz, S.A., 2018. Geological and climatic influences on mountain biodiversity. Nature geoscience, 11(10), pp.718-725.
[3] García-Ruiz, J.M., Arnáez, J., Lasanta, T., Nadal-Romero, E. and López-Moreno, J.I., 2024. The Main Features of Mountain Vegetation and Its Altitudinal Organization. The Timberline. In Mountain Environments: Changes and Impacts: Natural Landscapes and Human Adaptations to Diversity (pp. 167-202). Cham: Springer Nature Switzerland.
[4] Rahbek, C., Borregaard, M.K., Antonelli, A., Colwell, R.K., Holt, B.G., Nogues-Bravo, D., Rasmussen, C.M., Richardson, K., Rosing, M.T., Whittaker, R.J. and Fjeldså, J., 2019. Building mountain biodiversity: Geological and evolutionary processes. Science, 365(6458), pp.1114-1119.
[5] Rahbek, C., Borregaard, M.K., Colwell, R.K., Dalsgaard, B.O., Holt, B.G., Morueta-Holme, N., Nogues-Bravo, D., Whittaker, R.J. and Fjeldså, J., 2019. Humboldt’s enigma: What causes global patterns of mountain biodiversity?. Science, 365(6458), pp.1108-1113.

In some cases the transitions from different categories of mountain to hilly land is well captured in this approach, typically in ancient eroded landscapes with remnants of higher mountains. The dissected rolling hill landscape gets the label hills, while the remaining inselbergs are classified as mountain.

However, the story is very different in younger mountain areas such as the European Alps or Himalayas. If one looks at the GBLU map without legend overlaid onto a relief map, valley-like shapes appear very distinctly that follow the actual valleys of these mountains. When looking at the legend, one sees that these are actually classified as hills. The same holds for flat valley bottoms inside the mountains, these are classified plains, even if they are long, narrow and sinuous.

In my conceptualization of a mountain, the mid slopes of high mountains do not pertain to the landform class middle relief mountain. They are mid slopes of a high mountain. Similarly, mountain valleys are not hills, just because the local surface elevation is below a certain threshold.

Response:

Thank you for your detailed analysis of our results, particularly regarding ancient eroded landscapes. We appreciate your in-depth perspective, and in response to the issues you raised, our considerations are as follows:

First, to avoid semantic confusion, we have renamed all Level 3 “mountain” to “mountain slope”. This change clarifies that Level 3 targets finer-scale morphological units rather than large, unified mountain bodies.

Secondly, we fully acknowledge the importance of valleys in geomorphological research. One major challenge in classifying valleys lies in the absence of a unified definition, particularly regarding the valley extent or boundary on the mountainous slope. This ambiguity is often greater than that for mountains. Moreover, “valley” and “mountain” highlight fundamentally different concepts—one emphasizes downward incision, and the other highlights upward uplift. Thus, if we were to include both “valley” and “mountain” within a single classification system, we would need to define a conceptual interface to separate them. However, there is no broadly accepted standard for doing so. From a technical standpoint, it is also difficult to classify valleys because they typically lack a pronounced terrain break, which complicates classification in traditional approaches.

Although GBLU does not explicitly label “valleys,” it does provide a basis for valley extraction. As you noted, the polygons in GBLU—be they categorized as “hill” or “plain”—often capture the shapes of valleys. Our approach captures the cumulative characteristics of landform objects and uses slope accumulation to delineate subunits within a mountain. Thus, even though “valley” is not designated as a distinct category, the GBLU already produces polygons that effectively represent valley-like features. Once a user identifies a specific mountainous region of interest, they can extract those GBLU patches with valley-like shapes (classified as plain or hill, for instance) and reassign them as “valley,” thereby defining the valley object according to their own study’s requirements.

When I look at the methods and results of this paper, I think of the product as something like "Map of relief classes and relative (or regional) elevation zones", and I am convinced that this classification is useful for different scientific applications. Ontologically I don't think that the presented map units should be thought of as representations of landforms.

In summary, I commend the authors for what seems to be a very detailed and precise work and the product and the work that has gone into its production. Also, the results seem to be useful for certain research applications. I do not however agree with the authors that what is represented here are landforms, ontologically speaking.

The distinction I make here is further illustrated in the figure.

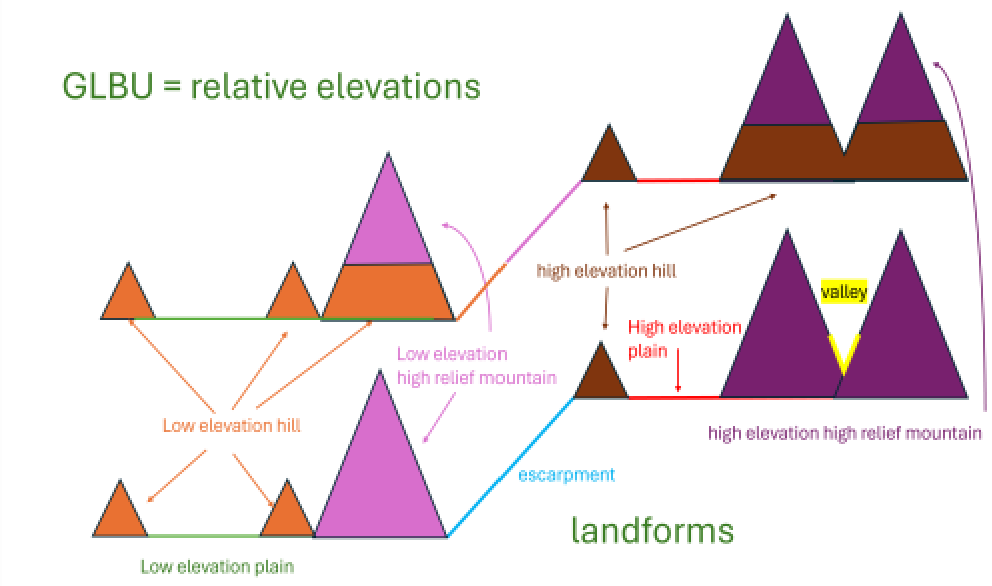


Figure: Upper transect: how I understand the current version of the GLBU. Lower transect: how I think landforms should be conceptualized in this context.

Response:

Thank you for acknowledging the potential applications of our dataset. As noted in our earlier reply, the term "landform" carries multiple meanings, and its specific interpretation depends closely on the chosen scale and disciplinary context. In terms of scale, Levels 2 and 3 in our classification indeed differ from the broader notion of a mountain (i.e., Level 1), yet they all reflect the underlying morphology of the Earth's surface. To avoid semantic confusion, we have renamed all Level 3 "mountain" categories to "mountain slope", thus distinguishing them from the more general, higher-level concept of a mountain. From an application standpoint, given that geomorphology intersects with subfields of geoscience such as climate and ecology, we provide a conventional categorization (i.e., the plain and mountain) at Level 1 while also offering finer distinctions at Levels 2 and 3 to meet more specialized research needs. For further details, please refer to above response.

Regarding the data availability, the authors have presented the resources they developed on Zenodo. The files are easily accessible and useable in open source software. Files are presented in folders by 10 degree latitudinal bands, and it is quite easy to find a region of interest. All terrestrial areas of the world seem to be included in the data. There is a possible issue for global level use of the data in that it consists of many different tiles that need to be mosaiced, but this can be coded.

Response:

Thank you for your suggestion. In this revised version, we have improved the file organization by mosaicking the tiles into 10° × 10° regions. These mosaicked files are now grouped into folders based on their latitude for easier access and use.

The validation is done against a number of similar products where one of the main differences is the resolution of the source layers (DEM), this product being based on very high resolution sources (~30 m at the equator.) The identification of plains seems to be more accurate than in any of the products with which it is compared.

Response:

Thank you for your comment. As you mentioned, more accurate plain boundaries are an important improvement of our dataset, thanks to the innovative approach employed in our methodology.

Overall the manuscript is sufficiently concise, the language clear, although it could benefit from some minor edits here and there (see below). On several points the methods section should be developed a bit further to allow full replication of the work flow.

Overall the language is clear and very understandable, but some suggestions for minor improvements are given.

Response:

We appreciate your feedback and have carefully revised the manuscript. We have expanded the methods section to provide more details for the replication (Lines 159-186 and Lines 214-235), and build a code repository to publish our workflow. More details can be found in https://github.com/nnu-dta/GBLU-code.

As said, in my opinion the layers presented in this work do not represent landforms. However I think that the classification of relief in plains and mountains with different values of elevation and relief intensity (roughness) can be quite useful for a series of environmental applications. My recommendation would therefore to revise the title and some sections of the text where the product is labelled as a map of landforms and replace this with formulations that more accurately reflect what is shown, that is, not to speak of landforms but about a map of relief (roughness) and elevation classes (or something similar) instead. This would require rather limited changes to the text and figures.

Response:

Thank you for your suggestion. As noted in our previous responses, landforms have inherent scale and context dependencies. To avoid misunderstanding, we have clearly defined the concepts and scale limitations of the landform types discussed in this study within the manuscript. We have substituted the term "landform" with "relief" or "elevation". Additionally, we have revised some category names in our classification system; specifically, we changed "mountain" at L3 to "mountain slope" to better reflect its terrain-related implications. To ensure comprehensibility in both general geomorphometry and related specific fields, we have retained the term "landform" in the title and in certain sections of the text.

Specific comments

35-36: I would add evolution or genesis to this list of research subfields of geomorphology

Response: Thank you for your suggestion. We have added "genesis" to this list (Line 35).

43: I would add that field work is an essential component of landform mapping (geomorphology)

Response: Thank you for your suggestion. We have added "the survey based on the field work" in this sentence. (Line 43)

46-47: there is a more recent product produced by Amatulli et al. that might be useful to refer to here: Amatulli, G., McInerney, D., Sethi, T., Strobl, P., & Domisch, S. (2020). Geomorpho90m, empirical evaluation and accuracy assessment of global high-resolution geomorphometric layers. Scientific Data, 7(1), 162. https://doi.org/10.1038/s41597-020-0479-6

Response: We have added this reference as your suggestion. (Line 47)

56-58: However, as the authors stated, unsupervised classification based methods to perform higher-resolution global landform classification require an international team with knowledge of geomorphological development in a variety of climatic and physiographic settings. > do you address this?

Response: Thank you for your question. In fact, we cannot fully resolve this issue. We included this statement because, as Iwahashi (Iwahashi and Yamazaki, 2022) described, unsupervised methods (such as clustering) require considerable effort to determine the geomorphological meaning of each category. This is challenging since these derived classes may differ significantly from conventional landform perception. In our study, we hope to optimize this process by pre-defining the landform classification system before applying our technical methods, and we based this system on a comprehensive review of existing work.

69-70: not clear if this paper only object is to classify the shape or also something about the material (lithology) and / or genesis, / evolution. Methods and final product seem to be focusing on shape irrespective of material / genesis.

Response: This study focuses primarily on the fundamental morphology of landforms. We have changed them to “maintaining the morphological integrity of the identified landforms” and “diverse and complex environmental factors have shaped different types of increase the complexity landform morphology”. (Lines 70-71)

80: objective: "to construct a global classification system for landforms that integrates geomorphological knowledge," : not clear where the geomorphological knowledge comes in in the method

Response: After careful consideration, we think that the term "knowledge" could potentially cause misunderstandings. Therefore, in the revised manuscript, we replaced it with "a global classification system for landforms that integrates domain consideration of landform-related studies". (Line 82)

82: typo: "high-resolutiojn" > high-resolution

Response: Thank you for your comment. Based on your suggestions and those of the other reviewers, to avoid misunderstanding regarding “high-resolution,” we have revised the sentence to:“(4) to make available a comprehensive global dataset of landform units.” (Line 84)

99-100 "The first-level (L1) types are defined as ‘plain’ and ‘mountain’, reflecting the most fundamental morphological characteristics of landforms." If I understand it well, the first level distinguishes between plain and non-plain (i.e. hills and mountains), as all that is not plain is later subdivided into several classes of hills and mountains, not mountains alone.

Response: Thank you for your comment. We have given considerable thought to this naming. In some cases, as you mentioned, hills formed by the erosion of ancient mountains can be regarded as a subclass of mountains. Therefore, to better capture the general concept of landforms at Level 1, we have retained both category names in L1.

102: "This classification perspective aids researchers in conducting macro-scale studies" This is indeed a valuable distinction

Response: Thank you for your recognition.

113: "the area the missing from FABDEM" > the area missing from FABDEM

Response: Thank you for pointing that out. We have corrected it to "the area missing from FABDEM."

120: "The following sections provide details that should allow users to reproduce our results." : some more details would be needed to achieve this I think

Response: Thank you for your comment. We have supplemented the manuscript with detailed computational information: (1) we have added the rationale for constructing the accumulated cost (AS) and provided a detailed computational process (Lines 159-186); (2) we have included the detailed calculation process for the new relief metric (Line 214-235).

123: Fig 1: "accumulate slope " > accumulated slope?
"Interecting with flat landforms" > Intersecting with flat landforms
"Eliminating fragement blocks" > Eliminating fragment blocks

Response: We have changed this figure as your suggestion.

125: data preprocessing or data pre-processing (see figure, perhaps harmonize?)

Response: We have removed the hyphen ("-") in the section title to ensure consistency throughout the manuscript.

130: "data from latitudes below 70° are transposed onto the Behrmann projection, and the remaining data are transported onto the Lambert azimuth equal-area projection. " : suggested edit: Tiles between 70° N/S are reprojected to the equal area Behrmann projection, and the tiles polewards of 70° N/S to Lambert azimuthal equal-area.

Response: Thank you for your suggestion. We have replaced the original text with the revised version as you suggested. (Lines 136-141)

132-133: this first sentence is more of a statement that would perhaps be better in the introduction. Starting this section with the second sentence works quite well.

Response: We have removed the first sentence and now begin the section with the second sentence for improved flow and clarity.

140: Fig 2b typo: "varient" > variant

Response: We have changed this figure as your suggestion.

147: how large must the continuous area of plain be to be considered a core area? I.e. how many contiguous pixels constitute a plain core area? Do you also apply a shape criterion, or can a very long area of contiguous plain pixels also constitute a core plain area?

Response: Thank you for your question. An area must be greater than 0.1 km^2, and we do not apply any shape criterion. In practice, due to slope limitations—especially in mountainous regions—it is rare to include plain core areas with an extremely elongated shape. We have added explanation in the revised manuscript. (Lines 159-161)

148-150: it is not clear to me what the cost layer is in this calculation: elevation, slope, or something else? Same holds for 'cost' in Fig 2a.

Response: In our calculation, the cost layer represents the slope layer. We have clarified this in the manuscript Line 181. Additionally, we have updated the description in Figure 2a to explicitly state that "cost" refers to slope, improving clarity for readers.

149: "The AS is calculated as the minimum cumulative cost of each position to the nearest landform core along a specific path" Would it not be more precise to say: The AS is calculated as the minimum cumulative cost of each position to the nearest plain core along a specific path.

Response: We have modified the sentence as your suggestion. (Line 182)

155-156: not clear to me how such an algorithm achieves the most direct integration of geomorphological knowledge and expertise

Response: Thank you for your suggestion. We have modified the sentence to: "Segmenting landforms through the determination of the thresholds for landform derivatives is one of the most common methods used in geomorphological studies and transforms geomorphological qualitative perception towards quantitative computation.” (Lines 187-188)

160: does T2 have a dimension and a unit? 1500-2000, is that length in meters, or slope in degrees or something else?

Response: Thank you for your question. T2 is measured in degree·meters (°·m), representing the accumulated cost-distance where slope (degrees) serves as the cost factor and distance (meters) accumulates along the path.

161-162: "but needs to be determined by integration with expert knowledge within different geomorphic regions". Not clear if you state that this should be done or that it has been done, and if so how?

Response: Thank you for your comment. We have revised the statement as follows: “This threshold range is provided as a reference but gentle adjustments to the thresholds may be required in some special areas, such as small islands, through human-computer interaction.” (Lines 193-194)

162: "In some cases, it may exceed the recommended threshold range." – not clear where and when

Response: Thank you for your question. The statement refers to specific cases where terrain complexity makes it challenging to apply standard threshold values. By referencing hillshade data and satellite imagery, we identified special terrain structures, including small islands, where traditional watershed and TIN-based methods struggle to perform effectively within predefined threshold ranges. We have added explanation in the revised manuscript. (Line 195)

165-167: "This novel method avoids the negative effect of local window analysis and is beneficial for maintaining the landform semantics for each block." Visual inspection of a number of tiles indeed shows a neat identification of the borders of plains and their transition to hilly or mountainous terrain.

Response: We appreciate your recognition of how our method effectively delineates the boundaries between plains and transition zones.

176-177: "a method that fails to account for geomorphological semantics, and which therefore disregards the integrity of a mountain. " I would argue that the classification of L2 landforms proposed in this paper does just that. I do not see any landform concept reflected in the classes, and even less so in the map units corresponding to these classes. See general comments above

Response: Thank you for your comment. We have provided detailed responses in the general comment section; please refer to that for further information.

192: "on basis of the plain boundary" > on the basis of the plain boundary

Response: We have corrected "on basis of the plain boundary" to "on the basis of the plain boundary."

192-193: "To refine the representation of surface relief, we also take into account linear features representing the rivers. " I suppose you do not consider all rivers and streams to construct your TIN of mountain base. Rivers and streams go up to great altitudes. Which sections of mountain rivers did you consider to construct the TIN?

Response: In this step, we employed the hydrologic analysis workflow from digital terrain analysis to extract the drainage network. We did not include all rivers or streams; instead, we retained only those of relatively higher order, such as primary or secondary channels. Specifically, we established a segmentation threshold based on flow accumulation—only river networks with values above this threshold were preserved. For reference, in an 11°×11° area, we set a threshold of 200,000, and we adjust this value in accordance with local geomorphic features. For example, in areas with more valleys, the threshold is increased. Regardless of these adjustments, as you mentioned, the final extracted river network does not extend to higher elevation areas.

206: was there any reasoning behind the selection of these elevation bands? 0-1000, 1000-3500, 3500-5000 and >5000?

Response: The selection of these elevation bands (0–1000 m, 1000–3500 m, 3500–5000 m, and >5000 m) was based on previous studies, particularly those by Zhou et al. and research on European landscapes. These elevation thresholds reflect major geomorphic and climatic transitions and were chosen to ensure a meaningful classification of landforms based on both process-based and regional geomorphic considerations. Detail information are as follows:

0–1000 m: Represents regions primarily influenced by fluvial erosion, where river dynamics play a dominant role in shaping the landscape.

1000–3500 m: Corresponds to the corrosion function line, a threshold that marks significant shifts in geomorphic processes.

3500–5000 m: Represents areas where periglacial and high-altitude processes become more dominant.

>5000 m: Aligns with the average elevation of modern glaciers, where glacial processes are the primary drivers of landform development.

207-208: idem

Response: We carefully reviewed this sentence, but we were unable to fully follow your comment.

277: Figure 7. Comparison between the GBLU and the Global Mountain Biodiversity Assessment (GMBA) projects. > Figure 7. Comparison between the GBLU and three mountain definitions presented on the Global Mountain Explorer (https://rmgsc.cr.usgs.gov/gme/)

Response: Thank you for the reminding. We have corrected the figure caption as you suggested.

278-279: this does not seem to be entirely accurate: "We conducted a more detailed comparison for mountain regions using the Global Mountain Biodiversity Assessment (GMBA) (Snethlage et al., 2022) as reference data." The three definitions are from three different institutions (WCMC, GMBA and USGS) but have conveniently been presented together on the Global Mountain Explorer (https://rmgsc.cr.usgs.gov/gme/). The latest mountain definition is the one by Snethlage et al (2002) which can be obtained from https://www.earthenv.org/mountains (scroll down to: Download the GMBA Mountain Definition v.2 here.)

Response: Thank you. We have corrected it in the revised manuscript.

337: "fundamental role in supporting the identification of landforms that incorporates complex semantics." > not clear what semantics means in this context

Response: In this section, our aim is to emphasize some background knowledge from various specific studies. We changed “complex semantics” to “domain background” in the revised manuscript. (Line 371)

344 "influencinge community structure and function," > influencing community structure and function,

Response: We have corrected it.

Reviewer 2：

The authors introduce a global landform classification dataset (GBLU) that represents a significant advancement in resolution compared to existing global geomorphological data. Their three-levels classification system with 26 distinct landform classes demonstrates an approach to categorizing Earth's surface features. The use of 1 arc-second DEMs provides unprecedented detail at the global scale, and their methodology of combining geomorphological ontologies with key derivatives appears to effectively balance noise reduction while preserving important landform characteristics.

However, a notable limitation is the lack of a fully documented methodological scripting procedure (even an example code would be helpful) to enable complete reproducibility of the results. Several Python libraries, such as rasterio, pyjeo, xarray, and numpy, along with GRASS GIS modules, offer matrix filtering procedures and cumulative cost analysis that could facilitate the replication of the methodology in a more transparent way.

The full methodology (AS, TIN, SUI) is novel; however, several issues arise during the processing phase due to the absence of a computational scripting framework that would enhance the rigor of the geocomputation procedure.

Response: Thank you for your recognition and comments. In our previous work, we implemented the workflow using ESRI ArcGIS Pro, but due to version differences, some tools may not function consistently across different systems, limiting reproducibility. To address this, we are actively adapting our workflow to open-source alternatives where feasible. We have constructed a Github repository and uploaded a part of tool incorporating Whitebox Geospatial Tools, GRASS GIS, and other open-source software and libraries to enhance accessibility and reproducibility. Due to time constraints, we have not yet provided all the tools, but we will continue to update them in the future. More details can be found in https://github.com/nnu-dta/GBLU-code.For transparency and usability, we have also provided a more detailed explanation about the workflow, including the rationale for constructing the new factor and the detailed calculation processes, in the revised manuscript. (Lines 159-186 and Lines 214-235)

Below are some geocomputation issues identified in the manuscript:

Data pre-processing

To reduce projection distortion, the authors state:

"Data from latitudes below 70° are transposed onto the Behrmann projection, while data above this threshold are projected onto the Lambert azimuthal equal-area projection."

This approach is reasonable; however, an overlap between the two projection zones is necessary to avoid border effects.

Response: Thank you for your reminder. To mitigate border effects, we have implemented an overlapping strategy in our processing. Specifically, we processed the DEMs in 11° × 11° tiles, ensuring that the main 10° × 10° area is used as the final output. This approach helps maintain consistency and minimizes distortions at the transition between projection zones. Related explanation has been added in the revised manuscript. (Lines 136-141)

Methodology

Figures 1, 2, and 3 are well designed and effectively illustrate the methods. However, they are not supported by a scripting procedure that can be followed step by step. Additionally, several thresholds (e.g., Tas, Tss) are defined in the methodology but appear to be based on empirical, subjective decisions. It would be preferable to define them using statistical or mathematical criteria.

Response: For ease of scripting, we have created a GitHub repository (https://github.com/nnu-dta/GBLU-code) and will continue to update the related tools based on open-source libraries. Additionally, we have supplemented the description of the calculation processes in the revised manuscript. Furthermore, as you mentioned, using statistical or mathematical criteria to define thresholds is an excellent approach. However, given the complex and diverse nature of surface morphology in our study, we attempted histogram-based and mathematical methods but found it challenging to establish a unified standard.

Figures 5–7 are well presented, but it would be beneficial to show the GBLU classification results alongside a transect, similar to Figure 3c, but using real relief data.

Response: Thank you for your suggestion. In the revised manuscript, we have optimized the visual appearance of these figures.

Due that the post-processing includes several aggregation/smoothing procedure do you really need to use a 1 arc-second DEM?

Response: This is an interesting question. Regarding the use of a 1 arc-second DEM, there is no contradiction between the aggregation procedure and spatial resolution. The aggregation is applied to reduce scattered noise without altering the boundaries generated in our classification. Thus, the final data resolution remains consistent with the original 1 arc-second DEM.

Would be more effective to use 3 arc-second MERIT Hydro in combination with the stream-network Hydrography90m to have a landform classification more in line with existing DEM-derived products?

Response: Thank you for your suggestion. While combining 3 arc-second MERIT Hydro with the 90m stream-network Hydrography90m could potentially improve consistency with existing DEM-derived products, the effectiveness remains uncertain due to differences in spatial resolution. Our primary goal is to develop a 1 arc-second landform classification map, and currently, there are no globally available and publicly accessible 30m (1 arc-second) stream network datasets that align with our resolution requirements.

Projection

The manuscript states: "Data from latitudes below 71° are transposed onto the Behrmann projection, while data above 69° are projected onto the Lambert azimuthal equal-area projection." However, WGS84 (World Geodetic System 1984) is a geodetic datum and can be represented using either a geographic coordinate system (latitude/longitude, expressed in degrees) or a projected coordinate system (e.g., UTM). The final tif files appear to be stored in the latter, but no specific explanation is provided in the manuscript.

Are the final tif files stored under two separate projections, or have they been homogenized into a single projection? Either approach is valid, but this should be explicitly stated in the manuscript and in the README.txt file available in the Zenodo repository.

Response: Thank you for your suggestion. In our processing workflow, we used the Behrmann projection for latitudes below 71° and the Lambert Azimuthal Equal-Area projection for latitudes above 69°. For consistency and ease of use, the final TIFF files have been reprojected into a single coordinate system (EPSG:3857). We have stated this in the manuscript and update the README.md file in the Zenodo repository accordingly.

Additionally, the processing appears to be done in 10° × 10° tiles. What happens at the tile borders? Is there an overlapping procedure in place?

Response: As the response above, we have implemented an overlapping strategy in our processing flow. We used DEMs in 11° × 11° tiles, and the main 10° × 10° area is used as the final output. For the boundary, we have manually checked and modified it.

tif files

The inclusion of tif file overviews (*.ovr) and a color table palette is appreciated, as they facilitate fast and visually informative rendering. However, it would be useful to include the code legend as metadata within the tif files themselves or at least document it in the README.txt file.

The .aux.xml files store statistical information about the tif files (e.g., mean, median). However, since the tif files contain categorical variables, this statistical information is not particularly useful.

I suggest increasing the grid tile size of the final tif files to 2° × 2° (or even 4° × 4°) to reduce the total number of files. This would simplify tile management, especially for large-scale downloads.

Response: Thank you for your reminder. In this version, we have uploaded the README.md file, which now includes explanations of the code meanings and colormap. While the .aux.xml file (which contains statistical information) is not essential for most applications, it is necessary in ArcGIS Pro for rendering data in unique value mode, which enhances usability. Additionally, we have mosaicked the data into 10° × 10° tiles and organized them into folders based on latitude for better accessibility.

Reviewer 3：

I read the paper "Global basic landform units derived from multi-source digital elevation models at 1 arc-second resolution". There are some interesting aspects, but even if it a technical/data paper there is the need o improvements. Apart from the description of the methodology that is unclear, I think that there are many drawbacks in the paper that require a full restructuring of the work. First, the landforms classification is too simple and in no way reflects the complexity of landscapes. For example, the approach of Iwahashi et al. uses much more information, for example the texture of terrain (even if with a simplified index). The comparison with other methods is debatable both for the different rational behind some methods as well as for the different resolutions. You should at least apply those methods on the same DEMs you used with your approach. Here I suggest some references, to which I refer in the following more detailed comments.

Suggested references

Guth, P.; Kane, M. Slope, Aspect, and Hillshade Algorithms for Non-Square Digital Elevation Models. Transactions in GIS 2021, 25, 2309–2332, doi:10.1111/tgis.12852.

Fisher, P.; Wood, J.; Cheng, T. Where Is Helvellyn? Fuzziness of Multi-Scale Landscape Morphometry. Transactions of the Institute of British Geographers 2004, 29, 106–128.

Trevisani, S.; Guth, P.L. Terrain Analysis According to Multiscale Surface Roughness in the Taklimakan Desert. Land 2024, 13.

Minár, J.; Drăguţ, L.; Evans, I.S.; Feciskanin, R.; Gallay, M.; Jenčo, M.; Popov, A. Physical Geomorphometry for Elementary Land Surface Segmentation and Digital Geomorphological Mapping. Earth-Science Reviews 2024, 248, doi:10.1016/j.earscirev.2023.104631.

Lindsay, J.B.; Newman, D.R.; Francioni, A. Scale-Optimized Surface Roughness for Topographic Analysis. Geosciences (Switzerland) 2019, 9, doi:10.3390/geosciences9070322.

Guth, P.L.; Trevisani, S.; Grohmann, C.H.; Lindsay, J.; Gesch, D.; Hawker, L.; Bielski, C. Ranking of 10 Global One-Arc-Second DEMs Reveals Limitations in Terrain Morphology Representation. Remote Sensing 2024, 16, doi:10.3390/rs16173273.

Response:

Thank you for your feedback and suggestions. Following your suggestions, we have reviewed relevant literature and expanded our comparisons to existing landform classification methods and indices, which has significantly enhanced the quality and originality of our paper. Below, we provide a general response to your comments, followed by detailed point-by-point replies

First, regarding the complexity of classification systems, it is important to clarify that our method and the method proposed by Iwahashi emphasize different perspectives. The term "landform" is inherently scale- and context-dependent. For example, "mountain" can represent complete geomorphological entities in general geomorphology or subdivisions emphasizing vertical zonation relevant in climatic and biodiversity research [1]. Iwahashi's classification primarily highlights local variations in terrain features, incorporating a slope level of detail at a smaller scale. This study, however, differs from ours in the classification perspective. We specifically emphasizes force accumulation, mountain ecosystems, and microclimatic gradients before constructing the classification system. GBLU dataset's Level 1 corresponds to the conventional concept of a complete landform entity, while Levels 2 and 3 provide progressively finer-scale morphological information. However, the scale of our finest level remains slightly larger compared to Iwahashi's results. Therefore, while we acknowledge the complexity and effectiveness of the methods used by Iwahashi, our approach differs in terms of the classification perspective and scale, making it suitable for different geomorphological research contexts. Related explanation has been added in the revised manuscript (Lines 99-106).

Secondly, although our approach and that of Iwahashi employ different indicators, the core geomorphological information emphasized in both methods—relief and elevation—is essentially similar. We referred to the excellent work by Iwahashi when constructing the GBLU. Regarding the indicator "texture" mentioned in your comments, Iwahashi defines it as "Texture is calculated by extracting grid cells (here, informally, "pits" and "peaks") that outline the distribution of valleys and ridges in the DEM". We think this indicator differs from the "texture" commonly used in remote sensing studies, such as the gray-level co-occurrence matrix, and is closer to terrain roughness or relief. In our research, we similarly utilized relief but introduced a novel, regional-scale method to measure terrain relief. Furthermore, we did not follow the conventional window-based analysis approach to address scale effects. Instead, we adopted an alternative cumulative perspective for calculating relief, effectively mitigating the scale effects associated with window-based methods. Although it is challenging to precisely determine which approach contains more information, our method captures a similar scope of terrain characteristics as Iwahashi's but through a different analytical strategy. We have added more details about out method and metric in Lines 159-186 and Lines 214-235.

Additionally, it is worth noting that although the segmentation method used by Iwahashi can effectively capture complex terrain characteristics at finer scales, it involves parameter selection processes that may introduce uncertainties or ambiguities. Similarly, clustering methods can effectively unravel complex relationships among terrain variables, but it has the "black-box" or "gray-box" issues. Specifically, the cluster's results do not inherently possess clear geomorphological meanings, necessitating expert interpretation, as highlighted by Iwahashi and Yamazaki (2022). We greatly appreciate the methods proposed by Iwahashi, but we also recognize that when addressing geomorphological issues, these approaches are not the only feasible solutions.

Regarding method comparisons, we appreciate your comment about using DEMs with differing resolutions. We agree that this issue needs consideration. To address this, we reproduced Iwahashi's classification approach using tools available in SAGA GIS. The results can be found in the following response. We ensured the inclusion of texture metrics emphasized by Iwahashi in our experimental replication. Overall, our results perform better in preserving the integrity of geomorphological features, effectively capturing their macroscopic characteristics and cumulative attributes. The Iwahashi method have good performance in characterizing objects ate smaller scales and but generate relatively fragmented patches in a perspective of the macro scale. In the revised manuscript, these additional analyses and comparisons further clarify our method's robustness and highlight its contributions to broader-scale geomorphological studies.

[1] Evans, I.S., 2012. Geomorphometry and landform mapping: What is a landform?. Geomorphology, 137(1), pp.94-106.

Specific comments (A: author R: reviewer)

A: Lines 67- 69 and also lines 72-74 "Nevertheless, higher DEM data resolution can be regarded as a double-edged sword, in that it at once provides the opportunity for landform mapping at a finer scale while at the same time increasing the challenge of reducing the noise effect (Jasiewicz and Stepinski, 2013) and maintaining the integrity of the identified landforms."

R: I think that the referred problem of noise related to high resolution is a false problem. Apart from the ambiguity of the term "noise" (e.g., noise because of errors in the digital representation, or because you consider noise the fine-scale morphology?), multi-resolution approaches permit to analyze the landscape having control of the “noise” (independently from the interpretation). In addition, surface texture analysis should be an important component of landscape segmentation approaches (as Iwahashi et al. or Jasiewicz and Stepinski, 2013) and can be particularly informative when computed at higher resolutions than global DEMs. Apart from the papers you cited I would consider the ones from Fisher Lindsay and Trevisani

Response:

In the original manuscript, our description of the workflow and the factors used was not entirely clear, which may have led to some misunderstandings. In the revised manuscript, we have made the following modifications:

(1) We have clarified and defined the classification objects (Lines 99-106);

(2) We have added explanations for two key factors along with detailed computational steps (Lines 159-186 and Lines 214-235);

(3) We have supplemented the results comparison with additional explanations (Lines 296-308).

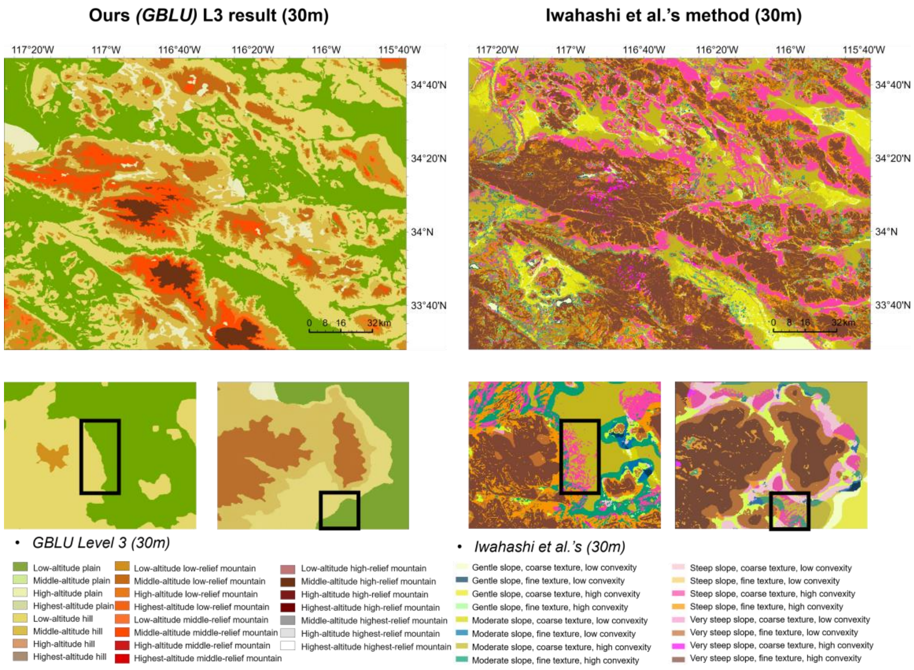
We appreciate your valuable comments. We acknowledge that our previous use of the term "noise" might have caused confusion. In fact, we intended to emphasize both data noise (errors) and abrupt terrain changes in our original text, as both significantly affect the classification process and results. To avoid potential misunderstandings, we have revised the original text to “the negative effects of data noise and abrupt terrain variations”. (Lines 69-70)

These fine-scale morphological variations have significant value for detailed landform classification, especially at slope or finer scales. The texture employed by Iwahashi is essentially a typical metric emphasize fine-scale morphology which is calculated based on local terrain variability derived from DEMs. <u>However, for geomorphological studies beyond detailed slope-scale analyses, such as vertical mountain zonation, leaving these variations unprocessed would hinder the generation of meaningful classification results.</u> Specifically, the unprocessed fine-scale variability will lead to fragment landscape units and incorrect topographic structures. Therefore, whether such "noise" is beneficial or detrimental depends not solely on data resolution but fundamentally on the specific research context. As we emphasized previously, the GBLU dataset is intended for broader applications in geoscience, particularly in studies focusing on force accumulation, mountain ecosystems, and microclimatic gradients. <u>Under these considerations, appropriate handling or aggregation of these variations becomes necessary.</u>

In practical implementation, the multi-resolution approaches you mentioned indeed provide a feasible solution. By synthesizing terrain characteristics across multiple scales, these approaches can effectively mitigate scale-dependent limitations. However, these methods still inherently face challenges associated with determining appropriate scales ranges in algorithms. How to select the optimal scale range and properly combine multi-scale terrain features remains a persistent issue. These methods, while widely adopted, are not the only possible solution, and we suggest an alternative approach. Our strategy begins with a step back. Specifically, we consider whether decreasing the reliance on window-based analysis, and then design the novel accumulated slope and relief index.

Regarding texture analysis, we agree that it plays a crucial role in terrain quantification, particularly in multi-scale segmentation approaches. As you noted, a key challenge lies in selecting an appropriate analysis radius or window size. Jasiewicz and Stepinski (2013) also highlighted the difficulty of achieving a universally optimal result using a multi-window approach. After reviewing the terrain texture approach you mentioned, we found that its underlying concept is similar to our relief-related index (previously referred to as the "surface uplift index"). As described by Iwahashi et al (2007), texture is derived by extracting “pits” and “peaks” from a DEM based on elevation differences between the original and a median-filtered DEM. This approach effectively removes high-frequency variations while highlighting terrain features at a local scale. However, it still relies on a predefined window size, which may limit its ability to capture broader topographic patterns. In other words, our methodology and texture-based approaches share a common foundation, as both aim to emphasize topographic relief. Specifically, our approach, which emphasizes regional topographic variations, and texture-based methods, which highlight local terrain variability, represent two complementary strategies aimed at reducing scale-dependent uncertainties in digital terrain analysis.

To more clearly illustrate the differences, we conducted an additional comparative analysis using the Iwahashi classification method implemented in SAGA GIS at the same data foundation (FABDEM) as GBLU. Results based on Iwahashi’s method emphasizes local terrain variability, resulting in numerous small-scale geomorphological units. But many of them consist of isolated and fragmented patches—even at the single-pixel scale. While this method effectively captures fine-scale terrain variability, such fragmented landform units pose substantial challenges for macroscopic geomorphological studies, as well as related climate and ecological analyses. Specifically, small and isolated landform units, such as those shown within the highlighted box (black marked in the following figure), cannot support the exploration of macroscale geomorphic patterns due to their limited scale and unclear geomorphological meanings. Additionally, the spatial continuity and relationships among these fragmented units has been broken and cannot be effectively restored through post-processing techniques, such as filtering.



Addition response for literature noted by the review：

Regarding the other papers you mentioned, we have analyzed them as well:

Fisher's study: Similar to the geomorphon approach, it focuses on terrain feature extraction, employing an membership function to resolve classification ambiguities.

Lindsay's study: Introduces a Locally Adaptive Scale-Optimized Surface Roughness Measurement, which applies Gaussian blur to suppress terrain complexity at scales smaller than the filter size.

Trevisani's study: Investigates landform classification in desert regions using multiscale terrain roughness, employing a simplified Multiscale Geostatistical approach to address multiscale effects.

These studies share a common methodology of synthesizing multiscale features by integrating results from multiple window sizes or radius, primarily emphasizing local topographic attributes such as roughness. As we previously discussed, while our approach differs in methodology, it does not conflict with these techniques but rather offers an alternative solution.

Additionally, based on our findings, our dataset demonstrates an improved representation of individual dune features in desert regions compared to Trevisani's approach. While Trevisani's unsupervised classification method provides a more classes, it remains uncertain whether these additional classes hold strong geomorphological relevance or have meaningful applications in fields such as ecology and environmental studies.

A: Lines 77- 79 “We focus on the classification of basic landforms that emphasizes morphological differences and, in so doing, we present the practical expression of landform ontology at the global scale that offers valuable insights into the Earth’smsurface structure comprising the constellation of landform types and their boundaries.”
Lines 80-82. “The objectives of this research are: (1) to construct a global classification system for landforms that integrates geomorphological knowledge, (2) to design a novel framework for global basic landform classification, (3) to develop an automated classification and mapping model for global landforms, and (4) to make available a comprehensive high-resolutiojn dataset of global landform units”
R: I have the feeling that the stated objectives of the research are only partially covered. In regard to 1, I don’t see big integration with geomorphological knowledge. In regard to point 3, you are just mapping very simple aggregates of landforms (mountain, hill, plain) that do not represent the complexity of landforms. I think that the work of Iwahashi should be considered the starting point for new approaches, maybe considering additional geomorphometric derivatives. But just working with elevation, even if the algorithm could be interesting, does not seem a step forward and very useful practically. Finally, in regard to (4) I don’t think that term “high resolution” can be used with something derived from global DEM at 1 arcsecond resolution.

Response:

After careful consideration, we think that the term "knowledge" could potentially cause misunderstandings. Therefore, in the revised manuscript, we replaced it with "domain consideration of landform-related studies". (Line *)

To address your comments, we still begin with a discussion of the classification objects. Specifically, the types of landforms used for classification are context-dependent. For example, in subfields of geoscience such as climate and ecological studies, the accumulated effects of energy and materials require a certain continuity of landform objects. This is because accumulated environmental effects typically occur within continuous and coherent units. Additionally, in practical scenarios, an area with slopes slightly steeper than the moderate slope threshold but generally exhibiting gentle trends is not commonly perceived as a "steep slope" by observers. Hence, emphasizing continuity and coherence in landforms aligns better with perceptions and practical applications as shown in the Figure 1(a). Through this perspective, although "plain", "hill", and "mountain" are commonly used terms, their precise classification at a global scale introduces considerable complexity due to variations in local context and field-dependent definitions. During this process, we need to accept minor local variations to ensure the integrity of geomorphological units. This domain consideration is precisely the original intention behind our earlier emphasis.

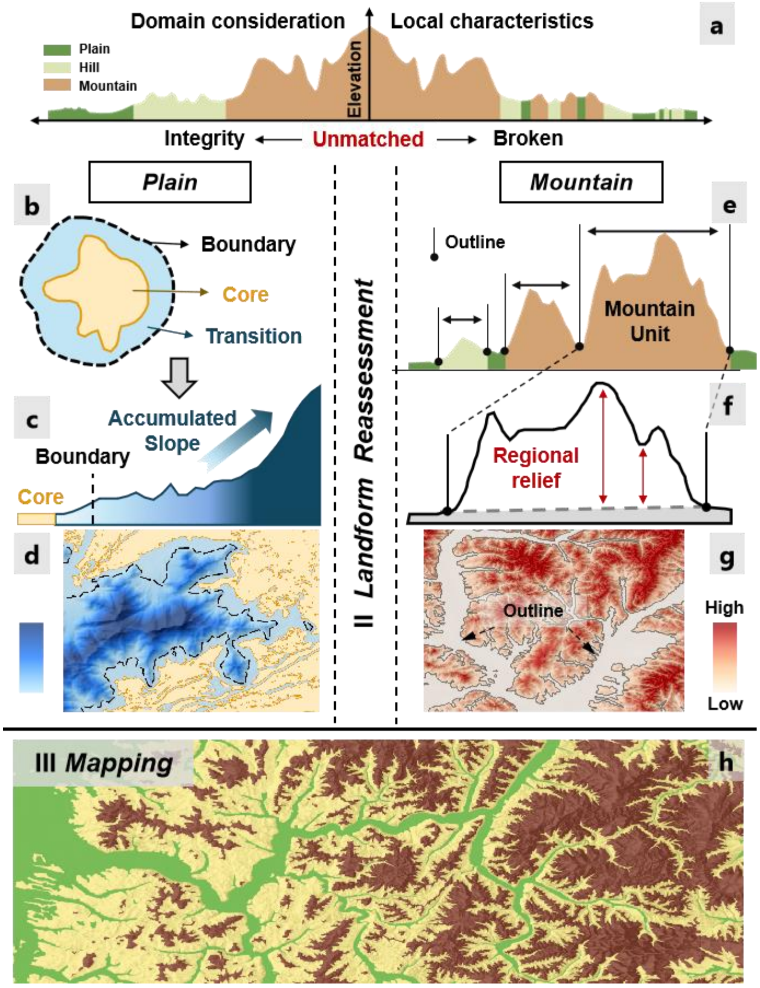


Figure 1

Furthermore, it is essential to analyze, from both methodological and result-oriented perspectives, why the classification of “plain hill mountain” poses a complex challenge. Fundamentally, the study can be approached from two scales: the micro-scale and the macro-scale. In geomorphometry, the micro-scale or slope-scale approach emphasizes the capture of detailed terrain variations, as demonstrated in Iwahashi’s work. However, a careful examination of Iwahashi’s results—whether considering the released dataset or the reproduction of their method on a 30 m DEM (based on your comment)—reveals numerous fragmented geomorphic types, some of which occupy only a single pixel. Even when we synthesize the categories (by converting "gentle" and "moderate" slopes in Iwahashi's results into "plain" and "steep" and "very steep" slopes into "mountain"), the results still contain a large number of fragmented units (marked by black dot square). From a surveying or terrain measurement standpoint, this may be regarded as an indication of high precision. Nevertheless, for macro-scale landform studies, as well as climate and ecological research related to geomorphology, such fragmented units cannot adequately support the exploration of landform or Earth system patterns. Specifically, these units lack representativeness; analyses based on such units, particularly statistical analyses, are prone to substantial deviations or “outliers” and can significantly impact the performance of subsequent simulation models. More importantly, the structural information of these fragmented patches is difficult to recover (e.g., the connectivity of valley), as indicated by the areas highlighted with red square in the figure.

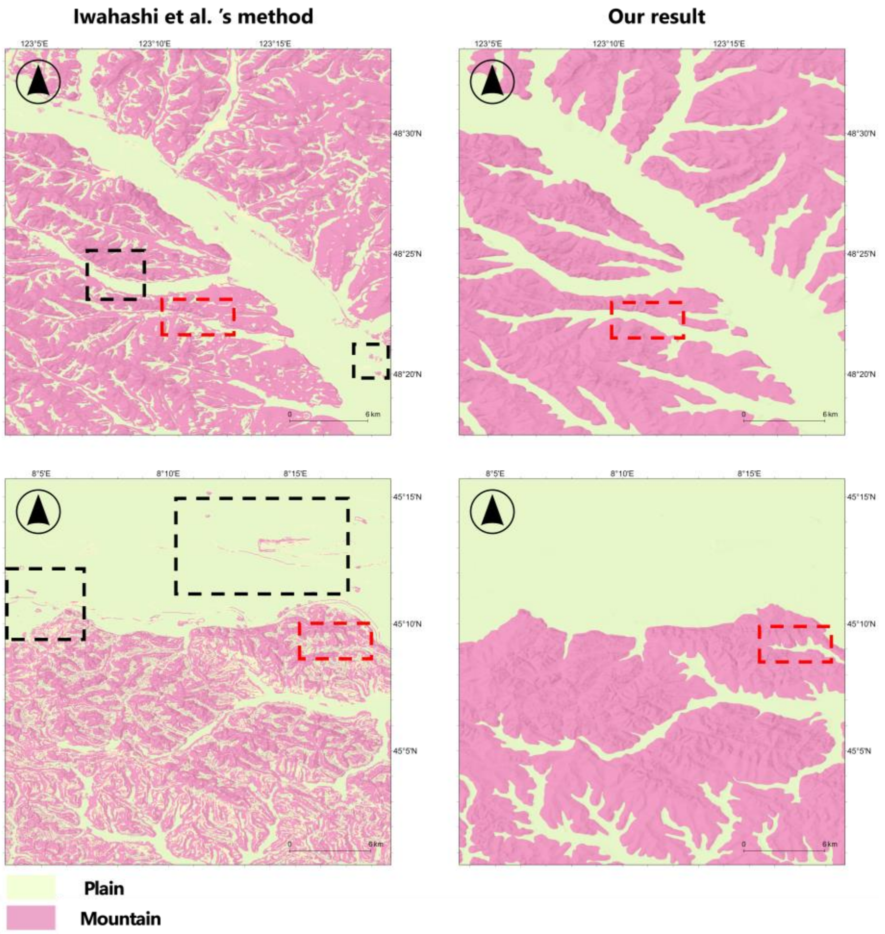


Figure 2

On this basis, when re-examining “plain hill mountain,” what is truly required in our methodology is an increased tolerance for discontinuities or non-typical variations, thereby reducing the occurrence of units with abrupt changes in the results. Consequently, although “plain hill mountain” might sound like a common term, its extraction remains highly complex and need novel method (Figure 1b-g). Our comparison with the objects and methods in Iwahashi's study is not intended as a competition to determine which approach is more complex; rather, it is aimed at achieving a synergistic enhancement tailored to different research needs.

Finally, we removed the "high resolution" descriptor and revised it to "(4) to make available a comprehensive global dataset of landform units."

A: Lines 91-100

R: The motivations behind the derivation of the simple classification scheme are unclear and someway highly debatable. I don't feel that it is a big deal to just subdivide between mountains, hills and plains. In addition, on the fuzziness of landforms perception and classification I surely would consider the work of Fisher et al.

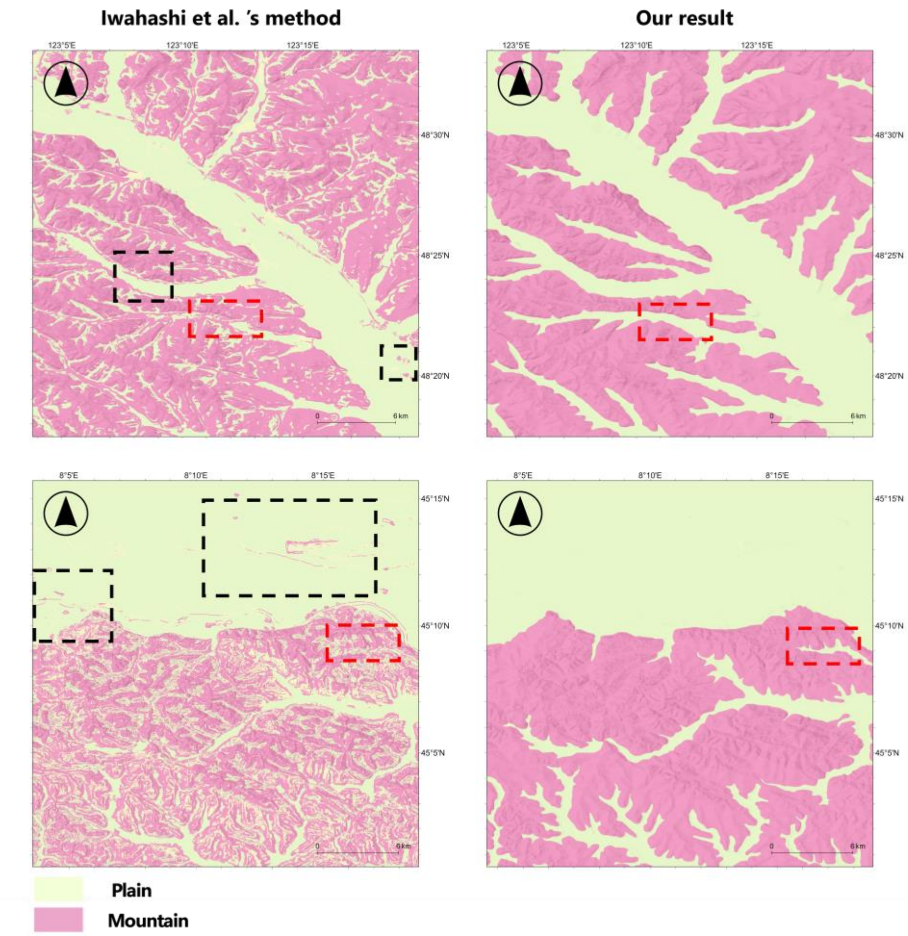
Response:

Conceptual and perceptive perspective:

As previously stated, the landform objects in our classification system hold significant importance for ecological and climate research, especially in mountainous regions. More details can also be found in the previous response.

Technical Perspective:

In practice, distinguishing between mountains, hills, and plains is not a simple task. For example, plains are not uniformly flat; they can exhibit areas that do not possess typical plain characteristics due to abrupt topographic changes or data errors. When using basic and typical terrain metrics—even with multi-scale approaches—fragmented patches persist, which in turn affect subsequent analyses and the performance of related geographic process simulation models as we noted in the previous response. This phenomenon is particularly pronounced at the interface between mountainous and plain areas (black dot squares in the following figure). Moreover, these fragmented units significantly impact the overall geomorphic structure. While isolated, meaningless pixels can be mitigated through filtering techniques, once structural aspects (such as connectivity) are disrupted, it becomes exceedingly difficult to reconstruct these relationships (red squares in the following figure).



We have carefully reviewed Fisher's work you mentioned, which presents an effective method for reducing ambiguity. However, fundamentally, his approach addresses issues arising from <u>scale effects inherent in window-based analyses.</u> As noted earlier, <u>our methodology seeks to "jump out" the window analysis process</u> entirely—in other words, <u>our approach modifies the treatment of ambiguity before any window analysis is performed</u>. We view these as two parallel routes; given the distinct underlying logics, it is challenging to definitively assess which approach is superior. Under our current objectives, we believe our method offers distinct advantages.

A: Lines 107-111 "In this work, the 'Forest and Buildings removed Copernicus DEM' (FABDEM) (Hawker et al., 2022) is the primary data for latitudes 60°S-80°N…"

R: I would be more cautious or at least I would discuss more the selection of FABDEM instead of COPDEM, because some geomorphometric derivatives, are better represented in COP.

See for example Guth et al. In addition, another question is whether structures should be removed in urban landscapes or not.

Response:

Thank you for your insightful comment regarding the data selection. We carefully evaluated both FABDEM and COPDEM, considering studies such as Guth et al. and other related work [1]. We found (as also noted by Guth) that FABDEM performed better in digital terrain model (DTM) accuracy tests, which is crucial for accurately classifying natural landforms. In areas with extensive surface cover such as vegetation and buildings, COPDEM's performance is suboptimal. In this study, our goal is to classify natural landforms. Urban landscapes, especially buildings, tend to obscure the natural relief of the terrain. Therefore, we believe it is necessary to select data that have been stripped of building artifacts.

[1] Bielski, C.; López-Vázquez, C.; Grohmann, C.H.; Guth. P.L.; Hawker, L.; Gesch, D.; Trevisani, S.; Herrera-Cruz, V.; Riazanoff, S.; Corseaux, A.; Reuter, H.; Strobl, P., 2024. Novel approach for ranking DEMs: Copernicus DEM improves one arc second open global topography. IEEE Transactions on Geoscience & Remote Sensing. https://doi.org/10.1109/TGRS.2024.3368015

A: Line 117 "knowledge-guided framework…."

R: how? I don't see a relevant integration with expert knowledge.

Response:

Thank you for your careful review. As stated earlier, our emphasis is on the considerations for practical applications, particularly the specific needs in climate and ecological studies that are closely related to landforms. Accordingly, we have revised the text to "a new framework", and added additional explanation about the landform objects in our classification in Line 124.

A: Line 119 "calculation of the mountain uplift index (SUI)"

R: I feel that the name "uplift index" is ambiguous, it seems to imply some tectonic uplift. Moreover, see also later comment, it seems a local relief measure.

Response:

Thank you for your suggestion. We have renamed it to surface relief index. This metric quantifies the degree of relief, yet it differs significantly from traditional window-based calculations. Instead of evaluating the relative relief within a fixed analysis window, this indicator is designed to measure the relief at any given location across a regional scale. In the revised manuscript, we added detailed explanations of the computational steps. (Lines 214-235)

R: Line 121 What is "factor calculation" ?

Response:

We change it to “characteristic quantification”.

A: Figure 1, workflow and lines 128-130 “Meanwhile, due to the requirement of calculating landform derivatives, we determine the projection principles as follows: data from latitudes below 70° are transposed onto the Behrmann projection, and the remaining data are transported onto the Lambert azimuth equal-area projection. “

R: To work in a projected system is not a requirement but a choice. In every case if you project DEMs you should discuss all the related intricacies and approximations. See for example Guth and Kane.

Response:

We appreciate your comment. While working in a projected system is a choice rather than a requirement, we selected equal-area projections (Lambert Azimuthal Equal Area and Behrmann) to ensure consistency in area-based computations (e.g., using unit area to cartographic synthesis)

As noted by Guth, the Lambert Azimuthal Equal Area projection is well-suited for some regions, as it maintains consistent east-west and north-south spacing when converting arc-second DEMs to projected grids. This minimizes errors in topographic computations, including slope and aspect, which supports our methodological design.

For lower and mid-latitude regions, the Behrmann projection offers reduced scale and shape distortion compared to Lambert projections centered at 45°, providing a better balance between area fidelity and shape preservation. The Lambert projection, however, remains more suitable for mid- and high-latitude regions.

Furthermore, while slope calculation differences between arc-second DEMs and UTM projections are relatively minor (~8-9%), our cumulative slope algorithm accounts for spatial continuity, mitigating potential differences due to DEM projection error.

Finally, to mitigate border effects between the two projection zones, we have implemented an overlapping strategy in our processing. Specifically, we processed the DEMs in 11° × 11° tiles, ensuring that the main 10° × 10° area is used as the final output. This approach helps maintain consistency and minimizes distortions at the transition between projection zones.

R: Figure 2 and related caption. I think it is really difficult to understand how the AS works.

Also the description at lines 149 -160 is unclear to me: "The AS is calculated as the minimum cumulative cost of each position to the nearest landform core along a specific path…"
How is computed cost? The cost of doing what? I don't see how geomorphological knowledge enters in the method, it seems an heuristic approach.

Response:

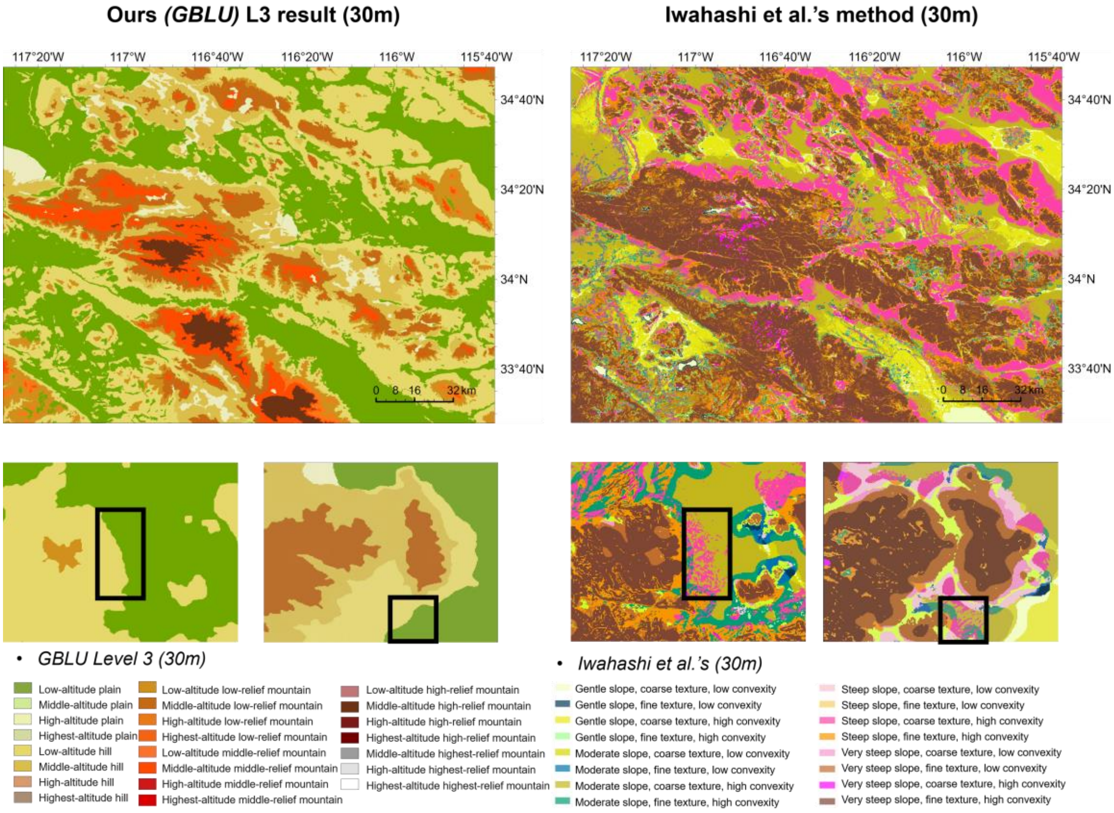
In the revised manuscript, we have revised Figure 2 and the corresponding text with a detailed explanation of why we use cost for AS calculation and how it is calculated. The specific explanations can be found in Lines 159-186.

A: Lines 176-178 "However, commonly employed indices reflecting topographic relief are achieve using a window of fixed size such as 3×3, 5×5 pixels, or larger (Maxwell and Shobe, 2022), a method that fails to account for geomorphological semantics, and which therefore disregards the integrity of a mountain. Window size has a significant impact on results of relief calculation."
R: but adopting multiscale approaches this issue can be resolved.

Response:

Multiscale approaches are an effective method that can mitigate multi-scale effects and integrate features across different scales. However, it is difficult to assert that they can fully resolve the issue. Regarding the fundamental differences between our approach and multiscale analysis, we have already addressed this in our previous responses. In summary, our method computes global features over an entire region, whereas multiscale approaches integrate local features at various scales. Although both methods aim toward similar objectives, the classification targets in our study differ from those in research that emphasizes local features. Multiscale approaches are difficult to completely resolve the issues we have identified.

Based on your suggestions, we have supplemented our manuscript with an experiment in which we reproduced landform classification using Iwahashi's tool published on SAGA with FABDEM data. The results, as shown in the figure below, indicate that even with multiscale approaches, the final outputs still exhibit a substantial number of fragmented units. We think that while such results may be advantageous for representing landform objects at the slope scale, they could have negative implications when classifying landform objects at a relatively macro scale. For macro-scale landform studies, as well as climate and ecological research related to geomorphology, such fragmented units (marked by black square in the following figure) cannot adequately support the exploration of landform or Earth system patterns. Specifically, these units lack representativeness; analyses based on such units, particularly statistical analyses, are prone to substantial deviations or "outliers" and can significantly impact the performance of subsequent simulation models. Moreover, these fragmented units significantly impact the overall geomorphic structure. While isolated, meaningless pixels can be mitigated through filtering techniques, once structural aspects (such as connectivity) are disrupted, it becomes exceedingly difficult to reconstruct these relationships.



A: Line 183 "In quantitative analysis, it is crucial to consider the underlying terrain of mountains to accurately assess changes in elevation."

R: unclear.

Response:

We revised this sentence to "Therefore, we propose a new method for relief quantification method which do not rely on the traditional window-based calculation. In this paper, the surface relief index proposed in this paper is defined as the degree of relative relief to the flat areas surrounding the mountain. We regard the elevation at the foot of the mountain as the base elevation and then calculate the elevation difference between each position on the mountains and the base elevation". (Line 220)

A:Lines 185 “surface uplift index (SUI)”
R: your index seems a local relief index on which there is a huge literature (see for example Minar and cited reference therein…).

Response:

This metric quantifies the degree of relief, yet it differs significantly from traditional window-based calculations. Instead of evaluating the relative relief within a fixed analysis window, this indicator is designed to measure the relief at any given location across a regional scale. In the revised manuscript, we revised it to “we propose a new method for relief quantification method which do not rely on the traditional window-based calculation. In this paper, the relief is defined as the degree of relative relief to the flat areas surrounding the mountain. We regard the elevation at the foot of the mountain as the base elevation and then calculate the elevation difference between each position on the mountains and the base elevation. Compared to the traditional method of relief calculation (e.g., difference in elevation within a particular window size), the surface relief index proposed in this paper considers the vertical elevation differences between the surface and the mountain base, which is more suitable for the objectives in landform-related studies such as mountainous climate and biodiversity”. (Lines 217-222)

A:Lines 188-189 “SUI considers the vertical elevation differences between the surface and the mountain base, which is more consistent with the human perception of mountain morphology.”
R: The human perception is multiscale, so it just depends from the target of the analysis.

Response:

Thank you for your comment. We changed it to “the surface relief index considers the vertical elevation differences between the surface and the mountain base, which is more suitable for the objectives in landform-related studies such as mountainous climate and biodiversity”. (Lines 221-222)

R: Lines 190-203. Not able to follow.

Response:

Thank you for your suggestion. We have revised this section, adjusting the logic and incorporating detailed computational steps. (Lines 223-237)

A: Lines 241-242 “Figure 4 shows the global landform classification results based on the abovementioned framework. This hierarchical dataset provides a more comprehensive understanding of the Earth surface”

R: A more comprehensive with respect to which method? Or with respect to which reference dataset? Honestly the earth’s surface is a little bit more complex. Apart from the issues with deserts you mention, for instance big depressed areas or volcanic environments are not represented.

Response:

Regarding the specific advantages of GBLU, we have provided a more detailed explanation (Lines 296-308). (1) GBLU demonstrates exceptionally complete valley results with more accurate boundary and shape delineation. Valleys are critical landforms in geomorphology and related ecological studies, and they represent a category of depression-type features. As mentioned earlier, the other methods tend to emphasize accuracy at the slope scale, but for features that require a higher classification level with an emphasis on completeness and boundary accuracy, GBLU performs better. (2) as you pointed out, in volcanic regions, GBLU does not display certain erosional signatures that are apparent in Iwahashi’s results. Our approach captures more transitional phenomena between volcanic areas and the surrounding terrain. These revisions and the related descriptions have been incorporated into the manuscript.

A:Lines 244-245 “The selected regions contain examples of the main landforms on Earth, as well as transition areas of different landforms.”

R: Yes, in the selected regions there are interesting patterns, but your approach does not characterize/distinguish these.

Response:

Thank you for your suggestion. Regarding the results shown in Figure 5, we have added additional explanations in the manuscript. In mountainous regions, GBLU presents a more complete depiction of valleys and peaks, which together form the fundamental structure for expressing mountain. Meanwhile, in desert areas, GBLU clearly reveals the distribution of dunes and interdune regions. Based on these results, we can currently provide a foundational outcome that supports visual differentiation. However, if the focus is on quantitative indicators, unfortunately we have not introduced a metric for quantifying landform patterns in this paper. We think that such an analysis may extend beyond the core scope of the current work, and we plan to conduct more in-depth analyses of landform patterns in future studies.

A: “The abundant textural information provided by GBLU”

R: I don’t see how your approach contains textural information in the sense of Iwahashi or Trevisani.

Response:

What we intend to convey here is the basic textural information of landform composition as observed visually, rather than a specific metric as used by Iwahashi or Trevisani. To avoid any misunderstanding, we have revised the description to “the information on the landform composition”.

A: 259 “significant improvement achieved by applying GBLU is the increased detail in representing terrain features.”

R: I see a very simple representation of landforms, but any indicator of patterns/texture is totally missing.

Response:

Thank you for your suggestion. Regarding our rationale for selecting these research objects and comparing our method with texture-based method, we have provided detailed explanations in our previous responses—please refer to those for specific details. In the manuscript, we have also supplemented the discussion with additional explanations, including the complexity of landform objects and detailed steps for terrain factor calculations.

R:

Section 3.2

This section has a lot of issues. You need to describe reference data (refdata) in the text not in the captions. Most importantly, it does not make too much sense to compare classifications performed at different resolutions or with different DEMs, given the different generalization levels of the landscape. Regarding Iwahashi you could apply the method to the same data you used in the analysis (if I’m not wrong it is implemented in SAGA). In addition, the method of Iwahashi et has been designed to take into account different aspects of morphology, including texture. It is not just based on elevation and slope.

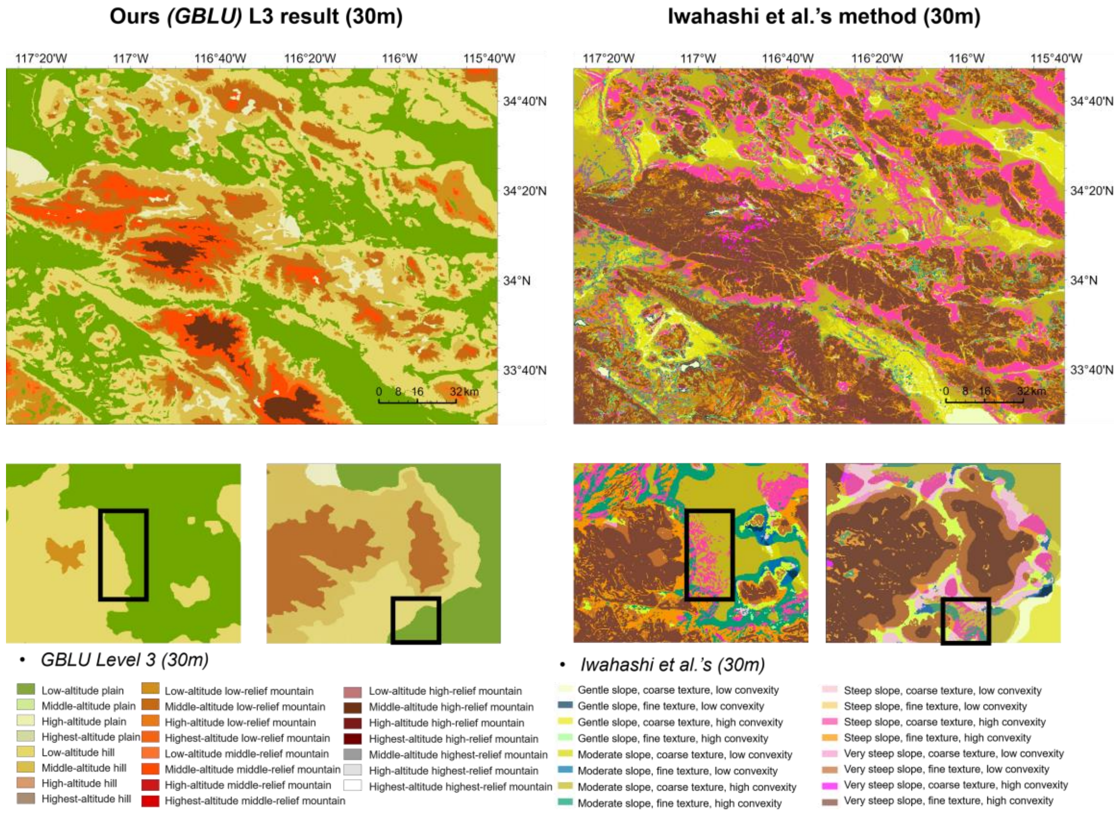
Response:

Thank you for your suggestion. First, in the revised manuscript, we have replaced “refdata” with specific citations.

Second，we have reproduced the classification results using Iwahashi’s method at 30m resolution based on the tool in SAGA, ensuring a more direct and meaningful comparison. The results, as shown in the figure below, indicate that even with Iwahashi’s approach, <u>the final outputs still exhibit a substantial</u>

number of fragmented units. We think that while such results may be advantageous for representing landform objects at the slope scale, they could have negative implications when classifying landform objects at a relatively macro scale. For macro-scale landform studies, as well as climate and ecological research related to geomorphology, such fragmented units (marked by black square in the following figure) cannot adequately support the exploration of landform or Earth system patterns. Specifically, these units lack representativeness; analyses based on such units, particularly statistical analyses, are prone to substantial deviations or “outliers” and can significantly impact the performance of subsequent simulation models. Moreover, these fragmented units significantly impact the overall geomorphic structure. While isolated, meaningless pixels can be mitigated through filtering techniques, once structural aspects (such as connectivity) are disrupted, it becomes exceedingly difficult to reconstruct these relationships.

Additionally, it is worth noting that although the segmentation method used by Iwahashi can effectively capture complex terrain characteristics at finer scales, it involves parameter selection processes that may introduce uncertainties or ambiguities. Similarly, clustering methods can effectively unravel complex relationships among terrain variables, but it have the "black-box" or "gray-box" issues. Specifically, the cluster’s results do not inherently possess clear geomorphological meanings, necessitating expert interpretation, as highlighted by Iwahashi and Yamazaki (2022). We greatly appreciate the methods proposed by Iwahashi, but we also recognize that when addressing geomorphological issues, these approaches are not the only feasible solutions.



Finally, regarding texture, we have provided detailed explanations in our previous responses. In our method, while elevation and slope serve as the foundational parameters, we have introduced innovative elements—particularly new indices—that extend the range of information used beyond just these basic variables. While we acknowledge that it is an excellent metric, it does not represent the only viable solution. For further details, please refer to our earlier responses.