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).

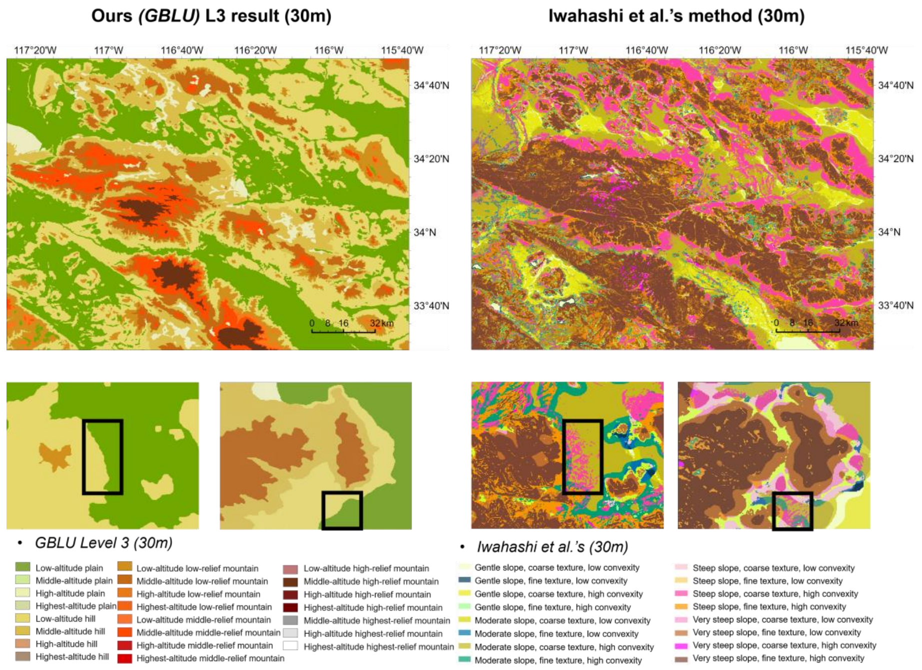
We appreciate your valuable comments. We acknowledge that our previous use of the term "noise" might have caused confusion. In fact, we intended to emphasize both data noise (errors) and abrupt terrain changes in our original text, as both significantly affect the classification process and results. 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. 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. 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. Under these considerations, appropriate handling or aggregation of these variations becomes necessary.

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, <u>the types of landforms used for classification are context-dependent</u>. 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.

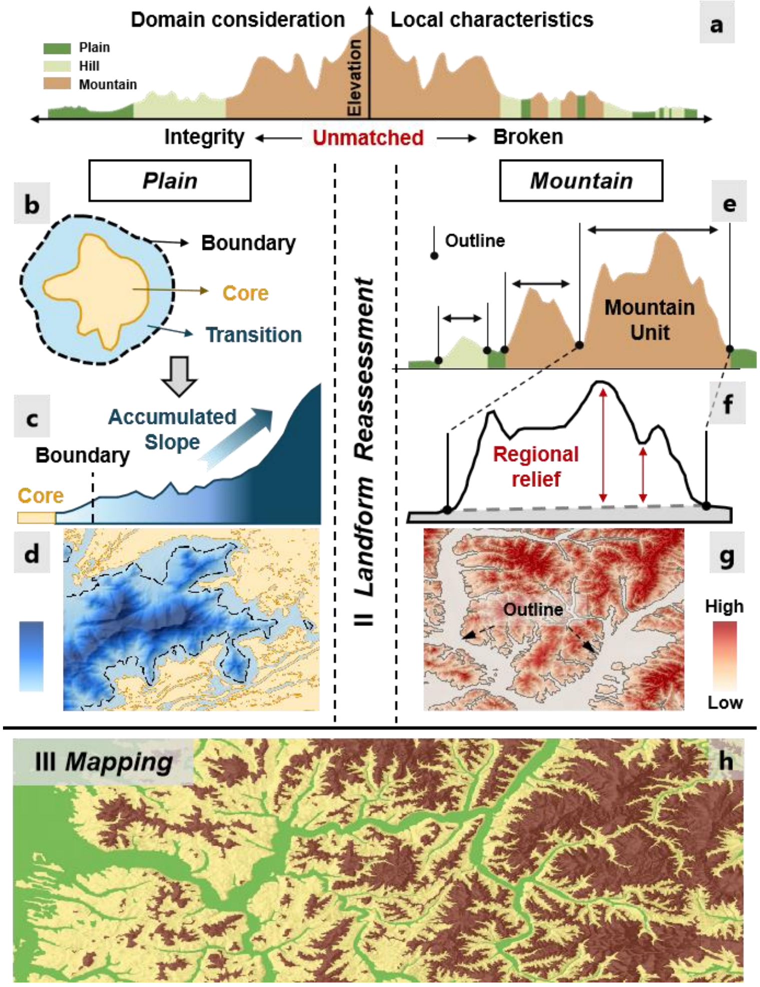


Figure 1

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.

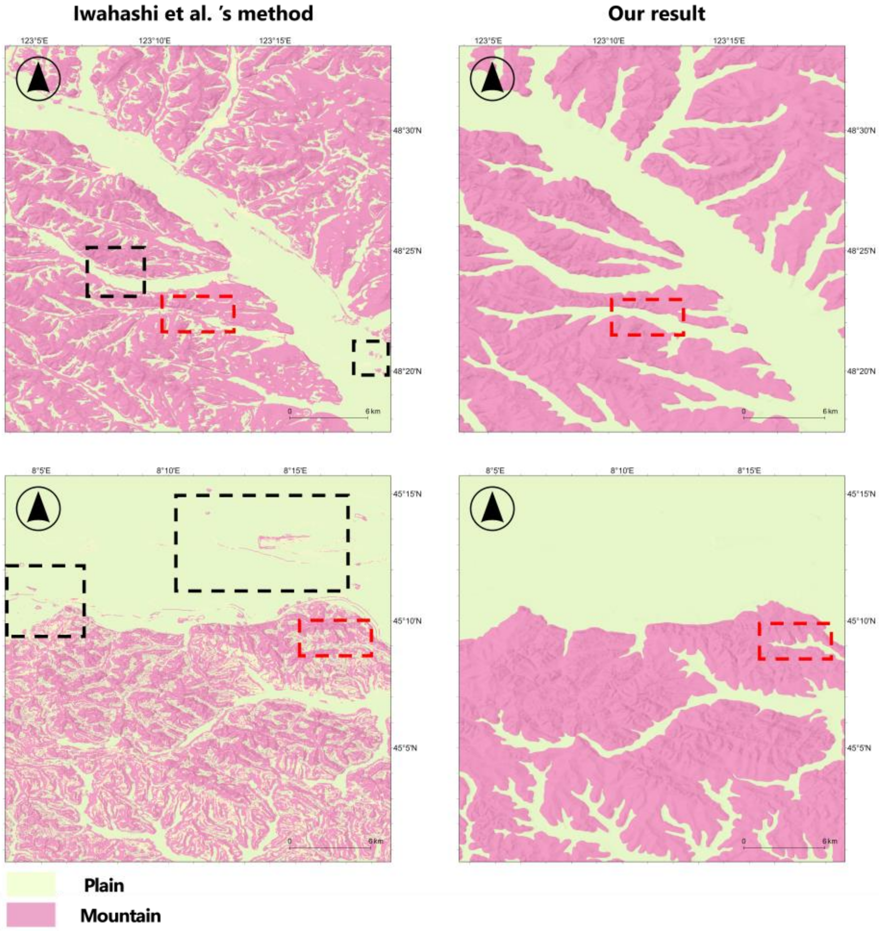


Figure 2

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.

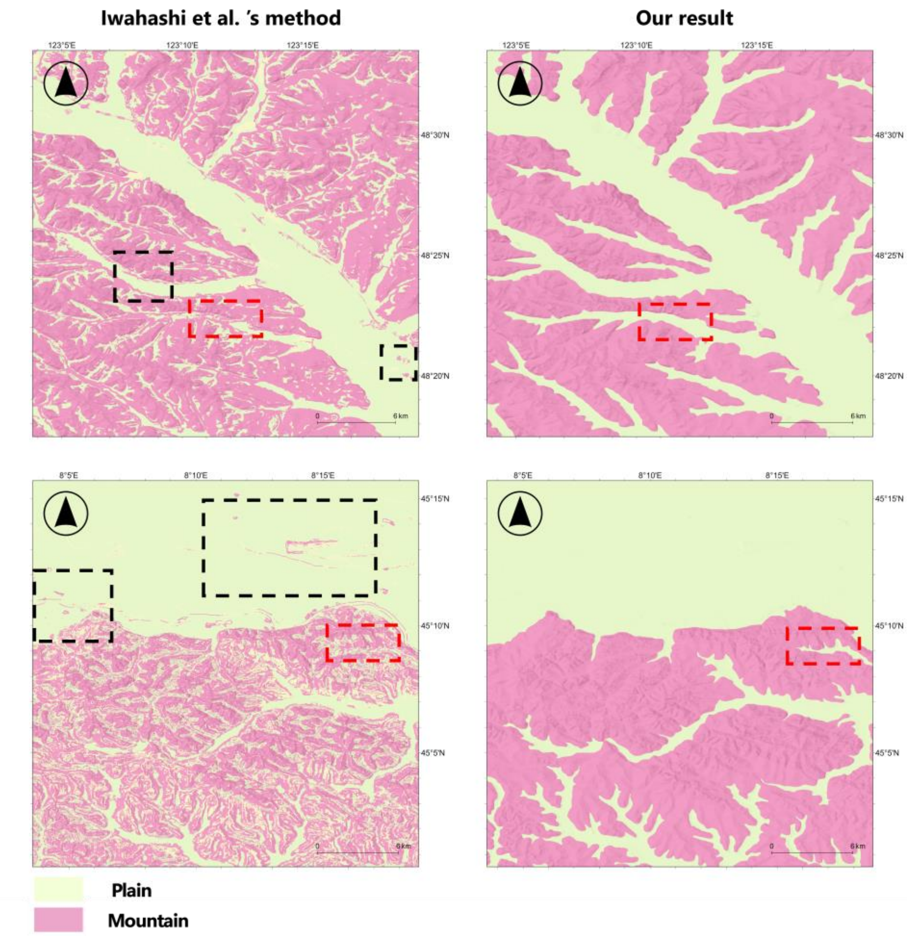
Response:

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 scale effects inherent in window-based analyses. As noted earlier, our methodology seeks to “jump out” the window analysis process entirely—in other words, our approach modifies the treatment of ambiguity before any window analysis is performed. 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:

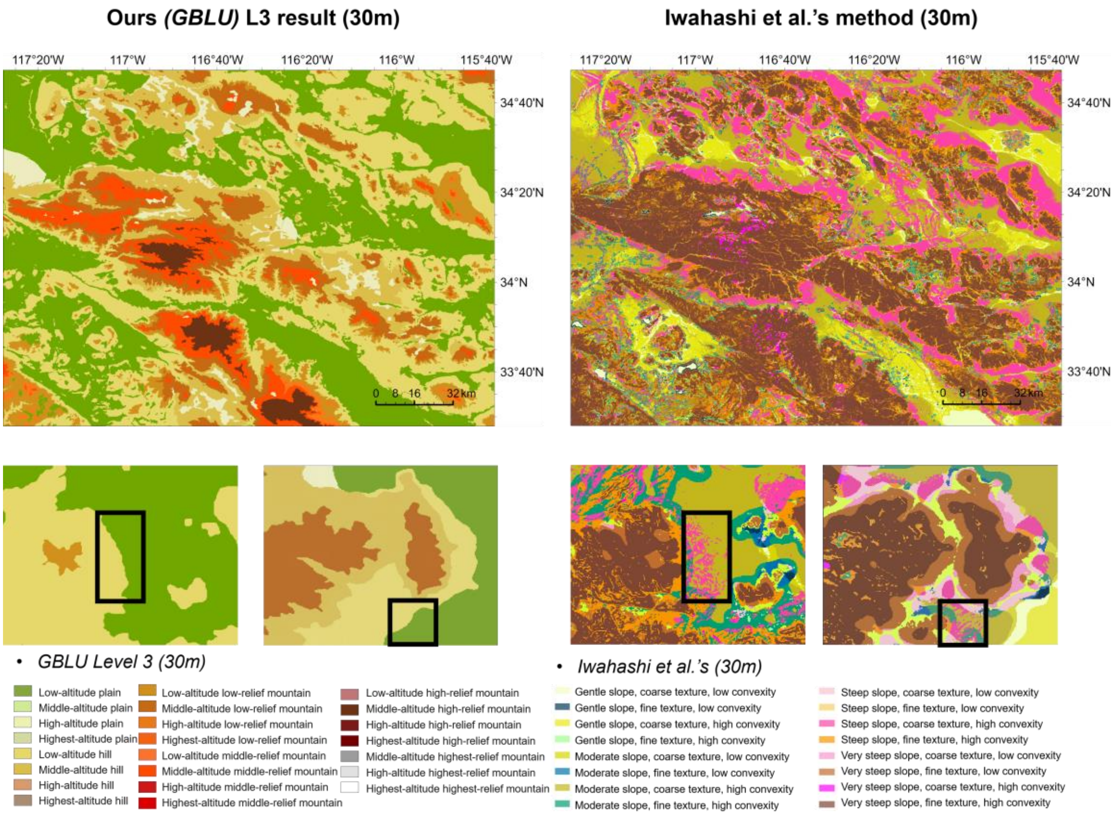
In the revised manuscript, we have revised Figure 2 and the corresponding text with a detailed explanation of why we use cost for AS calculation and how it is calculated. The specific explanations can be found in Lines 159-186.

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.

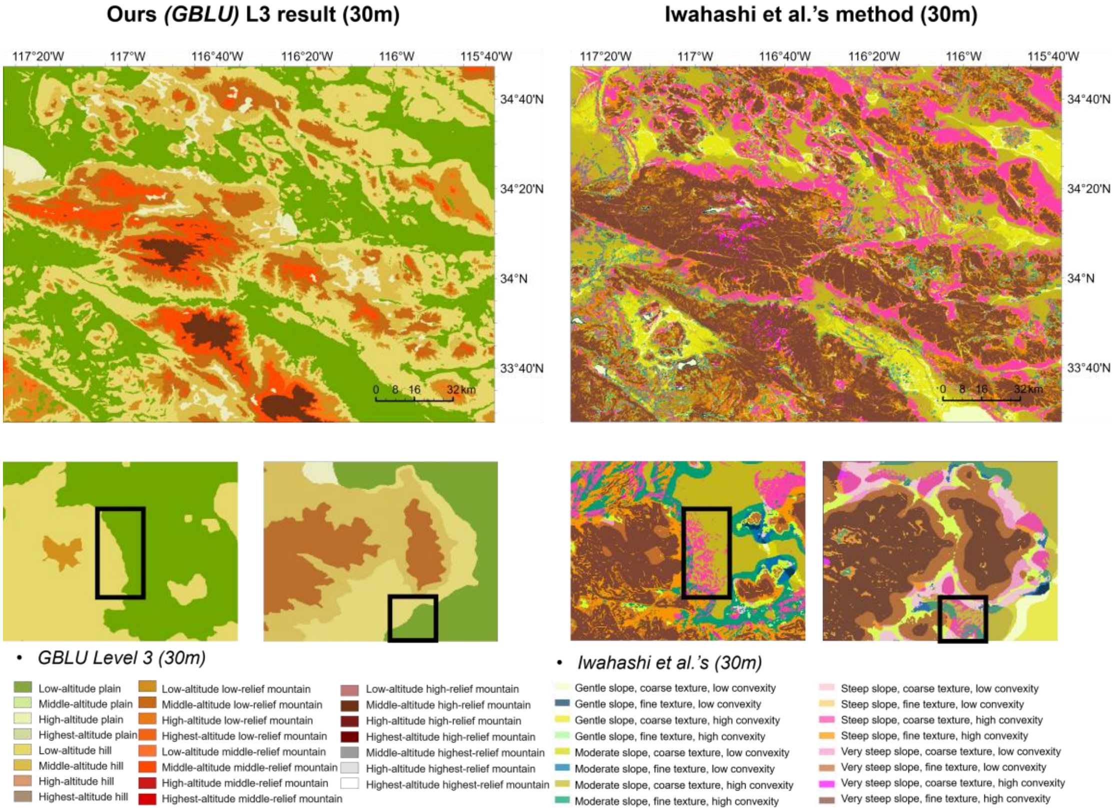
Response:

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.