Manuscript review “The Glacial Paleolandscapes of Southern Africa: the Legacy of the Late Paleozoic Ice Age” by Dietrich et al.

Suggestion: Major Revision

The manuscript “The Glacial Paleolandscapes of Southern Africa: the Legacy of the Late Paleozoic Ice Age” by Dietrich et al. explores the influence of Late Paleozoic glaciations on the modern landscapes of Southern Africa, using thermochronological data, geomorphological evidence, and stratigraphy to reconstruct past environments and burial/denudation processes. My field of expertise is in thermochronology, which allows me to assess the interpretations related to the thermal history and exhumation models presented in the paper. However, Sections 1, 2, and 3, which focus on the broader geomorphological context and historical geology of Southern Africa, fall outside my area of specialization, so my comments on these sections are based on general observations.

In general, I find the hypothesis presented in the paper—suggesting that glacial processes during the Late Paleozoic have left a significant and lasting imprint on Southern Africa’s landscapes—both interesting and worthy of further exploration. Nonetheless, the paper requires significant revisions, particularly in its reliance on localized data and the absence of broader, quantitative comparisons with modern glacial landscapes, which could better support its hypotheses. Some interpretations, specifically related to the thermochronological data, appear overstated without sufficient supporting evidence or acknowledgment of uncertainties inherent in the methods used. Furthermore, the manuscript would benefit from integrating alternative hypotheses and providing a more balanced discussion of the geomorphological features. Structural improvements, consistent referencing, and the inclusion of supplementary materials are also recommended to enhance clarity and strengthen the paper’s arguments.

Major comments are attached to this introduction.

Major Comments

1) Sections 1, 2, and 3 fall outside my area of expertise, so I will limit my comments to general impressions, which the editor and authors may assess for relevance. Overall, I believe these sections provide a reasonable summary of previous publications, contextualizing the current landscape as reflective of past glaciogenic processes, though not necessarily confined to the LPIA. However, the reliance on limited data (e.g., photos of outcrops and landscapes) is not particularly convincing in supporting the broader claims, especially for the Zimbabwe Highaland. I suggest incorporating a comparison with modern glacial landscapes for additional context.

    - I think that including supplementary materials that provide more detailed evidence for each locality (e.g., photos with precise locations) would strengthen the argument. In its

current form, the evidence seems quite localized, lacking a broader, more widespread perspective.

- In sections 2 and 3, I also noticed the absence of alternative hypotheses or explanations for these landscapes, which could offer a more comprehensive view. Addressing potential competing interpretations would provide a more balanced discussion of the geomorphological features.

- I also found myself lost at several points in these sections, particularly between lines 368–396 and 533–548. I recommend a revision to make these sections more concise and improve readability.

- Additionally, a more quantitative analysis of the landscape could enhance the argument significantly. This could involve calculating and representing key geomorphological parameters such as slope, relief, ... Comparing these metrics with those from present-day glacial environments would, in my view, lend greater support to the hypotheses outlined by the authors, making their conclusions more convincing.

- I noticed errors in the figure numbers and references. I have highlighted some of these issues (eg., pg. 16. L.399; pg. 18, L.431; Isabell and Cole, 2008; pg. 9 L. 266; ….), but it is the responsibility of the authors to carefully review and correct all misreferences throughout the manuscript.

2) From this point on, I will focus on the thermochronological aspects of the paper, specifically related to the thermal history of the referred crystalline basement (Section 4). In general, there seems to be an over and/or misinterpretation of the thermal paths and the modeling results. The authors need to keep in mind **the limitations of thermochronological modeling** and acknowledge these in the text. I recommend consulting Fox et al. (2020) and Ding (2023), for example, for a more balanced perspective.

Section 4

- I disagree with the statement:

> “*Finally, we would like to stress that assessing the controversial exhumation history of this region is beyond the scope of the paper and we objectively provide information we have at hand*” (L. 735).

If you are using these models for your interpretations, you need to explain why these models were chosen over others and justify your methodological decisions.

- It would enhance the paper to include a brief overview of the temperature ranges associated with each thermochronological method (AFT, ZFT, AHe, ZHe). This information would provide readers with a clearer understanding of the applicability and limitations of the various techniques discussed.

Section 4.1. The Kaoko highland

- This sentence:

> *“Margirier et al. (2019) was used since the early Cretaceous, where Raab et al. (2005) and Krob et al. (2020) were used since the LPIA, although the geological set-up in Krob et al. (2020) may be too restrictive.”* (L.741)

Is unclear and lacks justification for your choices. I suggest revising it to: “*The thermal modeling of Margirier et al. (2019) for samples from **[insert location]** was selected to represent the thermal history of the region since the early Cretaceous due to **[insert reasons, such as the methodologies employed, quality of the analysis, and presence of reliable constraints]**. Additionally, the thermal modeling paths from Raab et al. (2005) and Krob et al. (2020) were utilized to represent the thermal history of the region since the LPIA (ca. 300 Ma), as they provide **[insert reasons, such as methodological robustness, data quality, and relevant constraints]**.*”.

- In:

> “*From the demise of the LPIA until Early Jurassic (190 Ma), i.e. for 110 Ma, thermochronological data indicate a warming of ca. 35°C (Krob et al., 2020), i.e. a burial of 1.4 km considering the thermic gradient described before.*” (L. 747)

it’s important to acknowledge the **uncertainty** in the warming estimation. You should note that Krob et al. (2020) used a constraint of surface temperatures between 325-305 Ma, which is hypothetical. This warming of approximately 35°C **was necessary to fit the AFT data** due to this constraint. It would be helpful to briefly explain why these surface temperature constraints are considered reasonable. Without this constraint, the modeled warming would not have been necessary to explain the AFT data.

- The section on the exhumation history of the Kaoko Highlands needs to be reformulated for clarity. Please specify the thermochronological methods used for each assumption regarding the exhumation path. The models from different authors are not contradictory; they are based on various methods and study areas. For example, Margirier et al. (2019) suggest a cooling of approximately 290 °C due to the initial conditions imposed by the model: “Initial conditions for the model are fixed at t = 120 ± 10 Ma and T = 300 ± 50 °C based on Ar-Ar cooling dates (132–130 Ma; Schmitt et al., 2000).” **This estimation applies specifically to this Cretaceous intrusion and should not be generalized to the entire area**.

Section 4.2. The Cargonian highland

- Concerning these parts:

> *“Thermochronological data are partly contradictory. Wildman et al. (2017) indicate that a linear cooling of 60°C occurred from 350 Myr to today, which would imply 2.4 km of erosion. In line with this, Hanson et al. (2009) and Stanley et al. (2013, 2015) postulate*

> *on the basis of kimberlite pipes that ca. 1.5-2 km of Karoo sediments have been eroded from the Ghaap plateau, as indicated by the hypabyssal facies of the Makganyene kimberlite cropping out at the surface.”* L.820

And

> *“Contradictory to this model, Baughman and Flowers (2020) and Flowers & Schoene (2010) indicate an abrupt warming of 60°C between 280 and 250 Ma, followed by a quiescent period until 100 Ma.”* L. 827

I would like to reiterate that these models are not necessarily contradictory. They represent different geological formations and locations, with each model incorporating distinct constraints. It is more appropriate to discuss these models in terms of the methods and data on which each author bases their conclusions, rather than labeling them as conflicting.

Upon reviewing the cited papers, there does not appear to be conclusive evidence for the burial of these paleohighs. The data show that we have Paleozoic apatite fission track (AFT) ages (Wildman et al., 2017), partially eroded Meso-Cenozoic kimberlites, and older zircon (U-Th)/He (ZHe) ages, which Baughman and Flowers (2020) interpret in the context of Precambrian history. The burial events in their models are linked specifically to Paleozoic constraints; if these constraints are accurate, then the proposed burial is necessary.

In conclusion, I recommend focusing the discussion on the Baughman and Flowers (2020) paper and thoroughly justifying the Paleozoic constraint, as it is the primary reason to consider Mesozoic burial. Presenting this analysis will provide a clearer and more cohesive narrative, rather than framing the models as inherently contradictory.

Section 4.3. The Zimbabwe highland

- Regarding the section:

> *“Thermochronological data from Macintosh et al. (2017) indicate that a ca. 50°C warming occurred, from 300 to ca. 40-25 Ma, corresponding to a burial of 2 km. Compared to the preserved sediment thickness, thermochronological data would imply that a an almost 2 km-thick accumulation of Karoo sediments”* L.857

The interpretation of thermochronological data from Macintosh et al. (2017) suggesting a 50°C warming and corresponding burial of 2 km between 300 and 40-25 Ma may be an overstatement. The data from Macintosh et al. (2017) do not conclusively indicate burial. Rather, burial is a possibility, but it remains within the limits of uncertainty given the sensitivity of the data. To summarize, while the data allow for the possibility of slight burial, they do not provide definitive evidence for it. A more cautious interpretation would be that the data are consistent with a possible denudation of up to 2 km since the Paleozoic, and while some degree of burial is feasible, it remains uncertain without additional geological evidence. However, additional evidence is necessary to verify whether basin sediments indeed covered that area.

- Regarding the section:

> *“As for the Cargonian Highlands, the offshore stratigraphy of the margin surrounding Southern Africa can provides clues to the history of denudation on land. Thus, the sedimentary isopach map from Baby's (2017) (Figure 7.5 in Baby, 2017 and Figure 7.2 in Ponte, 2018), demonstrates the existence of a Limpopo proto-delta whose watershed may have drained the Zimbabwe region as early as the Lower Cretaceous.”* L. 866

The connection between the offshore stratigraphy, the Limpopo proto-delta, and the exhumation of the Cargonian Highlands is unclear. It is essential to explicitly explain how the proto-delta, as indicated by the offshore sedimentary records, is linked to the erosion and denudation processes of the Cargonian Highlands. Currently, the text lacks sufficient detail to establish this relationship. For instance, are the offshore deposits correlated with sediment sourced from the highlands? If so, how does this correlation support the timing and extent of exhumation?

Moreover, Be consistent and precise in citing references. For example, instead of mentioning both “Baby’s (2017)” and “Ponte (2018)” separately, ensure that the references are integrated in a cohesive manner, such as: “The sedimentary isopach map presented by Baby (2017) and further discussed by Ponte (2018) suggests…”

3) I find the hypotheses presented in sections 5 and 6 to be both interesting and worthy of discussion. The exploration of Paleozoic landscapes, paleohights, and the behavior of Gondwana's interior in relation to surrounding orogenies are important topics that remain underexplored. These discussions are crucial for understanding intraplate deformation, epeirogeny, sediment flu, craton erosion, and paleoclimate.

   While I appreciate the authors' insights, I recommend removing section 5.2.3, as it seems disconnected from the other sections and the overall purpose of the paper. **Furthermore, it is essential to moderate the tone throughout this section.** The authors should clearly state that many questions remain regarding the hypotheses about some current landscapes in southern Africa being shaped by Paleozoic glaciations, to avoid overstating conclusions that are still uncertain.

Detailed comments (in next round of revision)

I will provide detailed comments after the authors revise the first draft of the manuscript. These types of comments are time-consuming, and I prefer to address them once the broader issues in the draft have been resolved.

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