

## ***Interactive comment on “LGM and Late Glacial glacier advances in the Cordillera Real and Cochabamba (Bolivia) deduced from $^{10}\text{Be}$ surface exposure dating” by R. Zech et al.***

**R. Zech et al.**

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I greatly appreciate the constructive review of Jacqueline Smith. In the following, I will try to address her specific comments:

1. In the introduction, it was the aim to highlight the importance of the glacial chronology in the Andes for the global climate system. In order to avoid confusion, the revised version will clearly point out that it is the Late Glacial moraines Schaefer et al. (2006) were referring to, not the moraines of the maximum last glaciation.
2. Following the suggestion, "recessional end moraine" will be used instead of "recessional terminal moraine".

3. My sensitivity test of the exposure ages on uplift indeed uses an extreme value in order to highlight that uplift can almost certainly be neglected. It will be stated in the revised version, that more realistic values are much smaller.

4. I agree: a tacit conclusion that the scaling and calculation problems are solved would be misleading. Rewording shall make clear that the re-calculation of the previously published exposure ages yields younger ages, consistent with our own data, but that there is still debate as to which calculation is more appropriate. Our tentative conclusion will nevertheless be that moraine ages may have been overestimated so far.

5. I will try to make more clear that although the Breque site may serve as test for the exposure age calculations, it can not be inferred that the scaling system of Stone (2000) is preferable over Lifton et al. (2005). Including atmospheric corrections in Lifton's calculations results in virtually the same ages as Stone's scaling system. No matter how calculations are done, the exposure ages seem to underestimate the "independent"  $^{14}\text{C}$  age of the moraine. We therefore doubt the correctness of the Breque site. As stated in the text, it would yield a much lower reference production rate than all other calibration sites, and therefore requires verification before it can actually be used for local calibration.

6. I will readily include in the revised version that the maximum-limiting  $^{14}\text{C}$  ages in Bolivia are highly uncertain. They should probably nevertheless be mentioned because they are the only published maximum ages.

7. I agree that the MS signal is elevated already before 25 ka and that glacial advances can therefore be inferred. On the other hand, the much higher MS values between 25 and 20 ka suggest that glaciers were then probably more extensive than before.

8. Age estimates for YD and ACR will be included.

9. The revised version will more explicitly emphasize that our paleoclimatic conclusions

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depend on the choice of the scaling system, which is subject to ongoing debate. From my point of view, however, there are no inconsistencies with radiocarbon data, lake sediment analyses, and results from glacier-climate-modelling, which would indicate that our calculations are less appropriate than others.

10. Although our own dataset is small, this does not limit our ability to reach meaningful paleoclimate interpretations, because those are explicitly based on all published datasets. It is mainly the systematic uncertainties that may limit our (and others') paleoclimate interpretation. Minor rewording shall emphasize this in the conclusions.

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The anonymous reviewer mentions several important points that partly deal with our sampling strategy, but partly may also appear to be general criticism of surface exposure dating. I am pleased to have the possibility to address those points:

1. From my point of view, it is not "inadequate to make any conclusions because of the small number of samples". A dataset of 28 boulders is undoubtedly not the smallest one, and – being aware of the limitations concerning glacial fluctuations on millennial timescales – it is stated several times in the manuscript that more exposure ages would be useful to further refine the proposed glacial chronologies. Given a limited feasible sample number due to financial and labour constraints, I consider it the best strategy to spread the sampling over several moraines. For a pilot study, there is not much sense in constraining the age of 3 moraines with 10 boulders each, when the overall glaciation history and its spatial variability remains completely unknown.

2. Because the "stratigraphic relationships [...] are unclear" only in one of the three investigated valleys (and that case is explicitly stated in section "4.1 Stratigraphy and exposure ages ..."), the stratigraphic constraints within valleys help assessing the exposure ages (as done in section 4.2). Stratigraphic relationships between valleys can also be done, but given even the limited precision of exposure dating with few samples, I would prefer comparing "dated moraines" (as illustrated in Fig. 5), rather

than comparing equilibrium line altitudes, evaluating soil development or just counting moraines.

3. Concerning the "spread of individual  $^{10}\text{Be}$  ages", it would be indeed dilettantish, if "the authors did [...] just] guess which ages are good". In section "4.2 Exposure age scatter ...", we explain in detail the effect of inheritance and degradation, and we outline both in the text and in Fig. 5 that the application of the oldest age model allows to establish a generally consistent glacial chronology for all three valleys.

4. The anonymous reviewer also questions the "validity of scaling factors and production rates" used. To my knowledge, our manuscript is the first one to discuss in detail how different scaling systems, reference productions rates, atmospheric anomalies, neotectonics and eustatic sea level changes may affect systematic uncertainties of real exposure data (section "4.3 Systematic uncertainties ..."). Our choice of the calculation scheme is based on arguments presented in the text and "is [not!] guesswork again". As the reviewer asks: "if individual  $^{10}\text{Be}$  ages show much spread, how can they be used to evaluate production parameters?", I apparently need to elucidate that a spread in ages is often due to minor AMS measurement errors and mainly due to geomorphic processes (erosion, degradation, possibly inheritance, etc., see 4.2), whereas "production parameters" (the reviewer probably refers to scaling and production rates?) affect all samples in more or less the same way (see 4.3). In any case, the exposure ages of the Breque moraine (from Farber et al., 2005) cluster close enough to test the different calculation schemes against the independent age control (as done and illustrated in Fig. 7). Strictly speaking, one could argue that also for the calibration sites, one would have to apply the oldest age model. The conclusion that the Breque site corroborates both Stone's of Lifton's ages would, however, be flawed, because reference production rates are typically calculated averages rather than oldest ages. This issue will have to be addressed elsewhere and is far beyond the scope of this manuscript.

5. I agree that "the paleoclimatic interpretations cannot be based on the presented

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dataset [alone!]”. At least it would be more challenging and speculative, if the large (re-calculated) datasets of Smith et al. (2005a, b) and Farber et al. (2005) had not been available for comparison.

In conclusion, the manuscript critically questions previous exposure age calculations and results, puts the newly proposed glacial chronology of Bolivia into a conceptual paleoclimate framework, and it enunciates the justification for ongoing research – both more detailed exposure dating in selected valleys in Bolivia, and joint international efforts to identify and work on potential calibration sites in the Central Andes. I would regret not seeing the manuscript being published in *Climate of the Past*.

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