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Interactive Comment

Interactive comment on "Exposure dating of Late Glacial and pre-LGM moraines in the Cordonde Doña Rosa, Northern/Central Chile ($\sim$31° S)" *by* R. Zech et al.

R. Zech et al.

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We highly appreciate the comments by the Anonymous Referees 1 and 2. We are especially pleased that both Referees acknowledge the importance of our contribution to the reconstruction of the glacier and climate history in the Central Andes and - due to the specific location of the research area at the transition between the tropical precipitation regime and the mid-latitude westerlies - to the understanding of past changes in the atmospheric circulation systems.

Referee 2 generally seems to agree with both our methodological approach and with the paleoclimatic conclusions that we draw. This shall especially be emphasized, because we did not use the widely used standard procedure to calculate surface expo-

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sure ages (following Stone, 2000). Instead, we critically compared various available scaling systems/ calculation schemes and preferred the recently published calculation procedure proposed by Desilets and Zreda (2003). We should, however, stress in the final manuscript that although the age offsets are quite large, the paleoclimatic interpretation of our results would not differ a lot. The minor 'specific comments' will be considered when preparing the manuscript for submission to Climate of the Past.

Referee 1 appears to have more serious concerns about the exposure age uncertainties and our paleoclimatic conclusions. We appreciate the critical comments and are happy to address them here, following their order in the Interactive comment (quotations refer directly to the wording of Referee 1):

1. Referee 1 is worried about the 'difficulties to precisely estimate the ages of individual moraine boulders', which may 'limit the paleoclimatic inferences of the current results'. We hope that our detailed discussion of the systematic exposure age uncertainties makes the reader aware of the current limitations of the method. Apparently we have to emphasize in the revised version that even taking all the uncertainties into account, our general paleoclimatic conclusions would hardly be affected. This is due to the fact that we do not focus on interpreting millennial scale events (e.g. Younger Dryas YD or Antarctic Cold Reversal ACR), as this would be beyond the method's current potential (which we explicitly highlight in the discussions part). Instead, we focus our interpretation on the dated glacial periods, i.e. the pre-LGM and Late Glacial. We therefore neither share the concern of Referee 1 about the 'stratigraphic discrepancies' nor his/her concern that 'all the assumptions made here for the LGM are quite inconclusive' ... as Referee 1 suggests, our assumptions are not focussed on the LGM.

2. Referee 1 claims that 'the punctual nature of moraine depositional events makes [it] almost impracticable to explain the glacial advances as a direct result of changes in insolation, as [they have been] presented in Fig. 6'. Besides, (s)he is worried that 'although some coincidence apparently exists [between them], the discussion of the climate mechanisms responsible for the glacial advances has not been appropriately

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addressed.'

We argue that insolation (displayed in Fig. 6b) at the time of glacial advances needs to be discussed, because solar forcing undoubtly sets boundary conditions more or less favourable for positive glacial mass balances (directly influencing temperature and, indirectly, influencing the atmospheric circulation pattern, i.e. moisture advection). Apart from insolation, we included two other paleoclimate aspects in our discussion thus trying to decipher the underlying climate mechanisms that might explain the observed glacial advances: (i) the intensity and/or latitudinal position of the ITCZ, which plays a crucial role in controlling the tropical monsoonal circulation (speleothem data may serve as innovative new proxies and are displayed in Fig. 6c) and (ii) high-latitude temperatures as recorded in the polar ice-cores (commonly used isotope proxies are displayed in Fig. 6a), which can be discussed with regards to their impact on temperatures at lower latitudes.

2.1. Referee 1 takes the Late Glacial advances as first example to illustrate his/her concerns: (S)he argues that the Late Glacial advances occurred despite of low SH (= southern hemispheric) summer insolation and thus at a time when the 'South American Summer Monsoon is unlikely to be intensified'. There is plenty of evidence - cited in the manuscript and acknowledged by Referee 1 him/herself - that the southern tropical circulation was very intensive during the Late Glacial despite of low SH summer insolation (Fig. 6b). The speleothem data for example (Fig. 6c) document the southward position and/or intensification of the ITCZ and we explained that climate modelling results show that not only insolation but also high-latitude boundary conditions control the latitudinal position of the ITCZ (Clement et al., 2004). Other references for further similar modelling results would be e.g. Chiang and Bitz (2005) or Zhang and Delworth (2005). Accordingly, low NH (= northern hemispheric) winter temperatures during the YD and Heinrich event 1 (when the Thermo-Haline Circulation THC was weakened or shut off) provide a possible explanation for the Late Glacial wet phase in the Central Andes. Additionally, we emphasized that the glacier-climate model applied for the Late

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Glacial moraines in the Encierro Valley (Kull et al., 2002) shows that higher (summer) precipitation and lower temperatures prevailed during moraine deposition. The cited climate model of Clement et al. (2004) again helps to understand how high-latitude cold-temperature events, like the Heinrich 1 event, the YD and the ACR (see Fig. 6a), could influence the tropics, where they may trigger glacial advances via temperature reductions. We concluded that conditions for glacial advances in our research area were favourable during the Late Glacial, namely humid and cold, and we discussed several mechanisms that might have played together, especially: 1. the southward shift and/or intensification of the ITCZ (Fig. 6c) -> intensified SH tropical circulation (despite of low summer insolation, Fig. 6b!) and 3. the cold high-latitude events (Fig. 6a) -> cold events in the research area.

2.2. Referee 1 also seems to be worried about our paleoclimatic interpretation of the pre-LGM event. As we have explained in the manuscript, increased tropical circulation can almost certainly be excluded for that time period: (i) Recent shoreline studies on the Altiplano show that the (pre-LGM) 'Minchin' phase is actually several ten thousand years older than previously thought (Placzek et al., 2006). (ii) The speleothem data in SE-Brazil show no intensification of the SH tropical circulation at that time (Cruz et al., 2006: see Fig. 6c). (iii) In case that Referee 1 had interpreted the recent exposure age study by Smith et al. (2005a; 2005b) in Bolivia and Peru as evidence for increased monsoonal precipitation during the pre-LGM, we note that those results have to be discussed critically in light of the exposure age calculations: Recalculation of Smith's data using more recent scaling systems makes the 'Early Local Last Glacial Maximum' much younger ... synchronous with the global LGM! (discussed in detail in Zech et al., submitted and ; Zech et al., submitted). As a consequence, we concluded that the westerlies must have provided more moisture, either through intensification and/or a northward shift. Searching for a possible climate mechanism, we proposed that reduced high-latitude austral summer insolation (90°S, Dec, Fig. 6b) might exert a hitherto unrecognised strong control over the sea ice extent, which in turn pushes the atmospheric circulation, in particular the westerlies, northward. We cited Mosola

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and Anderson (in press) and Anderson et al. (2002) in order to corroborate this idea; they found that at least parts of both the East and West Antarctic ice sheets reached their maximum extent before the global LGM. We admit that our discussion about the ‘role of direct insolation and temperature’ (4.3.3) is indeed speculative and that ‘there is no evidence of climate forcing by winter insolation at 30°S and also by summer insolation at 90°S’. We note, however, that modelling studies do show the influence of the sea ice extent on the atmospheric circulation as well as the influence of the insolation on the ocean-terrestrial pressure gradient and thus on the resulting moisture advection (Clemens et al., 1991; Clement et al., 2004). We therefore disagree with Referee 1’s statement: ‘Southern Hemisphere summer insolation doesn’t influence directly changes in (sub)tropical climate but through the impact it produces on atmospheric circulation patterns’. Even if Referee 1 considered the ocean-terrestrial pressure gradient and the resulting moisture advection not as ‘direct’ influence, we are not aware of any evidence against insolation directly affecting surface temperature and glacier mass balance. In fact, isn’t it a widely accepted theory that low NH insolation causes snow/ ice accumulation and eventually, due to the albedo feedback, even global glaciations? It therefore seems justifiable to assume that similar mechanisms - although without the same large terrestrial albedo feedback - did at least favour more extensive glaciations in the SH at times of low austral summer insolation. There is actually growing evidence for an early local LGM at 30 ka BP from several sites in the SH (Shulmeister et al., 2006, and own unpublished exposure data from the Andes at 39°S. Remember, however, that the exposure ages from Bolivia/Peru may require recalculation). We do not, of course, dispute the global temperature minimum at 20 ka BP, but we conclude that the paleoclimatic conditions were favourable for an extensive glaciation at our research area at 30 ka BP: 1. low temperatures (at high latitudes and probably also in the tropics, Fig. 6a), possibly partly due to low austral summer insolation (Fig. 6b), and 2. increased precipitation (likely due to an intensification and/or northward shift of the westerlies with low high-latitude insolation and more extensive sea ice as underlying mechanism (Fig. 6b).

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3. Referee 1 notes that the 'YD and the ACR are certainly a controversial issue on South American paleoclimatology.' (S)he suggests that 'before making any assumptions the authors should discuss the deglaciation time in South America and also worldwide at high-latitude regions', because 'evidences for an abrupt change to warmer temperatures beginning at about 20-18 ka BP [show that] the importance of changes in temperature for glacial advances during the transition from LGM to Holocene might be significantly subdued.' We highly appreciate this constructive idea and are pleased to have the possibility to comment on that issue in this Author Comment, because it would make the manuscript itself probably too complex to be included: As mentioned earlier, extensive exposure dating by Smith et al. (2005a; 2005b) in Bolivia and Peru seems to suggest an early local LGM and tropical warming by 20-18 ka BP. Remember that these exposure ages may dramatically overestimate the real moraine deposition ages (Zech et al., submitted; Zech et al., submitted)! Nevertheless, these results form the main basis for the interesting hypothesis whether the tropics and the SH mid-latitudes may lead deglaciation of the high-latitudes (Shulmeister et al., 2006). Note, however, that various discrepancies with other (sub)tropical proxies would have to be explained and that no forcing mechanisms could yet be identified for this hypothesis. In another recent publication Schaefer et al. (2006) also referred to the exposure age results from Bolivia/Peru (to those from the limited 'LGM' moraines, which would be of Late Glacial age when recalculated) in order to infer a near-synchronous inter-hemispheric termination of the LGM in mid-latitudes and the tropics. Apart from the current methodological problems concerning the exposure age calculations (Zech et al., submitted), Schaefer's database is obviously very selective, particularly neglecting regions that host precipitation-sensitive glaciers. The two above literature examples demonstrate how difficult and controversial the discussion about the deglaciation still is. This holds true for both the termination of the LGM and the YD-ACR controversy in particular (see also the cited reviews of Harrison, 2004; Heine, 2004; Mark et al., 2004). We find no support for the Referee 1's hypothesis that 'the importance of changes in temperature for glacial advances [...] might be significantly subdued', and we assume that both

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NH and SH high-latitude cold temperature events may have triggered some of the observed individual glacial stages. Nevertheless, we clearly emphasize that it is currently beyond the precision of the exposure dating method to resolve millennial-scale issues like the YD or the ACR. Our discussion therefore focuses rather on the glacial 'phases', i.e. the Late Glacial and the pre-LGM.

4. Finally, we appreciate Referee 1's statement that 'changes in the dominant precipitation regime in the Cordon de Dona Rosa from extratropical to tropical is an interesting hypothesis [...]'. We agree that 'this hypothesis could be more clearly addressed in a better description of moraine deposits mapped to the south and the north of the study area [...]'. Our working group is actually working since several years north and south of the Arid Diagonal to do mapping, sedimentology, soil formation, glacier-climate modelling, and lake sediment analysis (see the cited references Amman, Grosjean, Jenny, Kull, Veit for selected examples). Only recently we have started to additionally apply surface exposure dating on moraines in the Central Andes. We are convinced of the high potential of the method to contribute to the reconstruction of the glacial and climate history and ongoing work therefore focuses on developing comparative chronologies from further research areas in the Andes from 15 to 40°S. In case that we have to extend the already long reference list and put more emphasis on 'a more detailed discussion of their [the moraine deposits] consistence with other paleoclimate records' we welcome any specific input from the reviewing community.

Thanks to the Referees' comments, it is now clear to us that we have to emphasize the high potential of surface exposure dating for the glacier and climate reconstruction in the Andes despite of all the current limitations of the method. We think that we have addressed possible climate mechanisms sufficiently, but we are aware that these issues deserve further investigation and more detailed explanations. As we have the possibility to contribute a summary paper to an upcoming special issue on 'Timing and Nature of Mountain Glaciations', we would be very grateful for any further specific comments and discussions, even if they are beyond the scope of the manuscript presented here.

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