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Comment

## ***Interactive comment on “Tectonic and climatic controls on the Chuquibamba landslide (western Andes, southern Peru)” by A. Margirier et al.***

### **Anonymous Referee #1**

Received and published: 22 January 2015

Overall:

The TCN based study of the age of the Chuquibamba landslide unit in southern Peru provides some new insights into the potential climatic triggering of a giant landslide (or set of landslides) in a region that appears to have been preconditioned for failure as a result of tectonic activity. The data are straight-forward and provide a reasonable basis for this general conclusion, but as I note below, there are several additional statements the authors make that seem to go beyond what they can reasonably conclude from currently available data (although if more references would be available to back up those statements, then they may be fine). Given the growing interest in this topic, and in particular the difficulty of deconvolving the tectonic versus climatic influences on landscape evolution in a region subject to megathrust earthquakes like the western

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Central Andes, I think this topic is well suited for ESurf. While the manuscript in general is fairly polished, below I include several line-specific comments where I believe the referencing and organization of the manuscript can be improved, and I point out a few places where the authors should clarify their statements or provide better support.

Line-specific comments:

P. 1130, Line 24: McPhillips et al. (2014, Nature Geoscience) seems to be an appropriate reference to add to this list (or alternatively to line 25) , particularly as it relates to the possible seismic triggering of landslides.

Nice, concise introduction.

P. 1132, Line 3: This unit is quite well dated by Thouret et al. (2007), so it wouldn't hurt to mention the specific age range that they report.

P. 1132, Line 19: This sentence should be restructured, because there are many studies that have now documented climatic fluctuations on the Altiplano over those timescales; is there a reason you mention Placzek specifically? If so, please state why, otherwise provide at least a representative list of the paleoclimate studies that have been done close to the field area.

P. 1132, Lines 20-22: To be fair, the study by McPhillips et al. (2014, Nature Geoscience) should also be mentioned, because they reported a very different interpretation in one of the areas where Steffen was working.

P. 1132, Section 2.2: The results of the study by Roperch et al. (2006, Tectonics) could be useful to add to this description.

P.1133, Lines 1-2: To be more precise, Schildgen et al. (2009) stated that the uplift that occurred between 14 and 2.2 Ma was accommodated by normal faulting and monoclinal warping, but that doesn't necessarily mean that normal faulting did not continue afterwards (or did not start earlier). Rewriting could be as simple as changing "the Tertiary episode" to "a Tertiary episode", the latter of which doesn't exclude the possibility

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of later faulting.

P. 1133, Lines 2-3: After reading the whole paragraph, I understand that the authors are talking about a strike-slip system, but calling them “rather vertical faults acting as thrust with punctual normal apparent movements” here is confusing as a technical description. . . this implies that the kinematics have changed over time, but I don’t think that is what the authors are trying to say (?). Perhaps there are near-vertical faults with both apparent thrust and normal offsets, which vary laterally according to the fault orientation (transpression versus transtension)?

P. 1134, 9-12: This material would be better placed at the end of the previous section, because it describes results from another study, not the “Sampling strategy and methods” of this paper.

P. 1134, Lines 21-24: I guess that an explanation for how a quartz pebble can be used to estimate the erosion rate on rhyolite blocks that are higher in elevation and subject to higher rainfall will come later, but for now, I’m skeptical.

P. 1135, Lines 11-24: Why not include this basic description of the morphology in section 2.3, where there is already some description of the geomorphology? This way, the results section could move directly into the cosmogenic data, instead of first reviewing more aspects of the morphology.

P. 1136, Lines 2-7: There should at least be a caveat here that one would expect a quartz pebble to yield a lower erosion rate compared to rhyolite blocks. I think the assumption of low erosion is reasonable, but the authors should be a bit more careful in this argument.

P. 1137, Line 1: It’s a reduction of the normal stress (or a reduction in shear strength, but certainly not a reduction in shear stress) due to hydrostatic pressure that typically leads to landsliding; Strasser and Schlunegger referred to decreased friction along a basal shear plane. To quote Ritter, Kochel, and Miller: “A body of material on a slope

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will remain in equilibrium (stable) as long as the sum of the applied shear stresses does not exceed the sum of the shear strength of the slope materials.”

P. 1137, Lines 7-9: Sounds reasonable, but then why would the later wet events not have triggered additional landslides?

P. 1137, Line 15: Can this generalization really be applied to all landslides? More evidence is needed to back that up, although it seems to be reasonably demonstrated in this case.

P. 1137, Lines 19-20: Not clear what the authors are trying to say here; both subduction earthquakes and El Nino events were linked to debris flows? The “succession in time” phrase is confusing. The authors might consider referencing McPhillips et al. (2014, Nature Geoscience) here.

P. 1137, Lines 20-24: Not being able to exclude a seismic triggering (Lines 22-23) is quite far from “a likely link” (Lines 20-21). The authors should decide what interpretation they would like to make; I suggest deleting the sentence on lines 20-21, because they have not established any link between the recurrence of seismicity and landslide triggering.

P. 1138, Line 19: Not clear what is meant by “permits new destabilizations”; are more recent mass movements sourced from that region? Is there a reason for that?

P. 1138, Lines 19-24: This should be a separate paragraph, but “perfectly preserved” should be replaced with “well preserved”. It seems dangerous to assume that over the 100 kyr history of exposure, there has been no rainfall, particularly considering that the deposit is interpreted to have been mobilized during a past wet phase. Overall, I’m not convinced that these final lines add anything useful to the discussion; the impact of aridity on surface preservation has already been stated a couple times previously.

P. 1139, Lines 2-7: This is not a very effective paragraph for a conclusions section; it represents a fairly standard set of statements that can be applied to any study that

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measures TCNs in quartz from arid environments (of which there are many).

P. 1139, Lines 10-11: Text needs to be tightened: the triggering of the debris flow is favored, not the debris flow itself. Also, what part of the results presented favors the climatic triggering? If only the age (i.e., a coincidence in timing), then that should be stated directly.

P. 1139, Lines 19-23: These sentences would be more appropriate in the discussion, particularly as they provide some of the broader regional context that is currently missing from the discussion. This argument in general though would benefit from references to any studies that may have attempted to quantify the amount of material mobilized in landslides from recent large earthquakes. Are there none from southern Peru or northern Chile? Without these, it is difficult to have much confidence in the dominance of climatic triggering, as the authors themselves point out that a seismic triggering cannot be ruled out.

P. 1139, Lines 24-25: This statement needs to be supported, but I don't recall that it was brought up earlier in the discussion.

P. 1139, Lines 27-28: This sentence should be deleted, not only because grammatically it's incorrect, but also because it appears to add nothing to the conclusions.

#### Editorial comments/corrections

P. 1130, Lines 2, 18: evolution of relief

P. 1130, Line 3: Chile, several

P. 1130, Line 8: however, none

P. 1130, Line 20: individual landslides

P. 1131, Line 10: delete comma

P. 1131, Line 11: I suggest starting a new paragraph with "The Chuquibamba

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landslide. . .”

P. 1131, Line 13: piedmont, where

P. 1131, Line 17: paper, we

P. 1131, Line 19: evolution of the landslide area

P. 1131, Line 26: emplaced within a

P. 1132, Line 1: Delete “Then”

P. 1132, Line 2: “were overlain onto” rather than “caped”

P. 1132, Lines 7-8: perpendicular to the NW-SE striking Andean range

P. 1132, Line 15: the arid climatic conditions since at least the Neogene (here it also seems reasonable to cite some of the work by Jason Rech, which reconstructs aridity from soil characteristics).

P. 1132, Line 24: “structures” is not a verb, but I’m not sure what the authors are trying to say here. Maybe “comprises the primary structure of”?

P. 1133, Lines 8,15: sinistral

P. 1133, Lines 10-11: by the distribution of crustal earthquakes

P. 1133, Line 15: kinematics

P. 1134, Line 11: “places” rather than “situates”

P. 1134, Lines 18-19: rather than “minimizes” the text should state “indicates minimal post-abandonment erosion”

P. 1134, Line 20: Better to write “. . . surface to minimize the possibility of post-depositional movement and potential covering by surficial material.”

P. 1135, Line 12: The boundary of the rotational landslides

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P. 1135, Lines 14-15: the overall southwest dipping topographic slope

P. 1135, Line 16: Moreover, several

P. 1135, Line 18: smooth, sub-horizontal surfaces. . . These surfaces likely correspond to

P. 1135, Lines 26-27: Lower uncertainties are commonly measured at other AMS facilities, so I would suggest deleting this sentence rather than overstating how impressive the results might be. Those interested in the analytical uncertainties will find this information easily in Table 1.

P. 1136, Line 2: collected from the Pampa Juhuay

P. 1136, Line 6: These results support. . . of insignificant erosion

P. 1136, Lines 8, 11: I've never seen this convention of  $^{10}\text{Be}$ -ka; I would tentatively discourage the authors from using it unless there is a very good reason to use it.

P. 1136, Line 24: "According to" rather than "After"; the latter makes it sound (on a quick reading) as if the catchment development happened after 2010.

P. 1136, Line 25: Similar to the Lluta

P. 1137, Line 11: localization, and flow

P. 1137, Line 12: landslide has. . . suggested for the

P. 1137, Line 14: system, we

P. 1137, Line 17: no relationship . . . and a giant landslide

P. 1137, Line 18: previously documented

P. 1137, Line 25: margin; other

P. 1138, Line 9: events, as

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P. 1138, Line 12: valley, as

P. 1138, Line 14: accumulated (be consistent with the verb tense)

P. 1138, Line 18: at 101.9 ka (delete “ago” and the uncertainty on the age should be added)

P. 1139, Line 2: I suggest avoiding the word “perfect”; instead: “TCNs are well-suited for dating the exposure of surfaces older than. . .”

P. 1139, Line 13: What is a “regressive erosion event”?

P. 1139, Line 19: Chile, most

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Interactive comment on Earth Surf. Dynam. Discuss., 2, 1129, 2014.

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