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Interactive comment on “Pluri-decadal (1955–2014) evolution of glacier–rock glacier transitional landforms in the central Andes of Chile (30–33° S)” by S. Monnier and C. Kinnard

Anonymous Referee #2

Received and published: 29 April 2016

General comments

This is an interesting study on glacier – rock glacier relationship in arid mountain context. The scientific significance is quite good and it addresses relevant scientific questions. However, the overall importance of the study should be better highlighted. As presented in the introduction and in the discussion, the study focuses mainly on the study sites and lacks a broader view. The research question is not enough developed. Why is it important to make this study ? Also, the state of the art should be more developed. Some recent references are missing (eg. Dusik et al. 2014, ESPL; Bosson and Lambiel 2016, Frontiers in Earth Science).

Regarding the methods, a large part of the study relies on geomorphological charac-

teristics of the investigated landforms. Whereas the descriptions are generally clear and well presented, geomorphological mapping can be highly subjective. Then you must explain how the boundaries between rock glacier parts and debris-covered parts were defined. Which criteria were used to distinguish rock glacier morphologies from debris-covered ones ? Besides, I have a special concern with the horizontal velocities shown on the figures, for four reasons. 1) The vectors are so small – especially for site 3 – that the velocities are almost impossible to read. 2) The vectors appear quite noisy, with many different directions ; or they are so numerous that the information is lost. 3) A systematic error seems to be present for site one, with many vectors in the Southwest direction. 4) The velocities seem homogeneous at the landform scale, which means that no movement differences would exist between the upper and lower parts, or debris-covered glacier and rock glacier parts. This seems unrealistic. In the results section, please group the chapters \hat{A} Rock glacier morphology \hat{A} and \hat{A} Thermokarst area \hat{A} . This would avoid having chapters with only 1 line, like in page 12. You should also display all aerial images, for the reader to make his own analysis. We feel sometimes uncomfortable because it is not always possible to verify your interpretations.

My major concern is probably the discussion chapter, for several reasons. This chapter must be clearly improved, both the content and the structure. The conclusions appear quite speculative in some places; they are not always supported by data. The interpretation/discussion must be clearly improved, for the reader to be convinced. The processes involved in debris-covered derived rock glaciers must be better discussed. And this must be done without separating the three sites. In the current state, it is more an interpretation than a discussion chapter. You must better discuss your interpretations. For instance you mention compression and bulging in chapter 4.2.4. How do you reach these conclusions? This is unclear. Another example : You use the permafrost probabilities given by your modelling for the interpretation of spatial differences on case one. This seems simplistic : a model is not the truth, especially with permafrost spatial models. The reality is often much more complex at the site scale. Honestly I do not see



any clear difference between Navarro and Las Tetas regarding glacier – rock glacier interactions. How can you conclude that there was only a colliding between the glacier and the rock glacier in Las Tetas, and no superimposition by the glacier, as it is the case in Navarro ? What about the origin of both rock glaciers ? Are they glacier-derived rock glaciers, or talus rock glaciers overridden by a glacier ? You should discuss this. However, you must be very cautious with the interpretation on rock glacier origin, since you have no geophysical data to show the internal structure. The references are rather few. You must much more discuss your findings regarding previous studies.

Specific comments

p.1, I.18. Æn monitored Åž is not suitable here. Prefer Æn investigated Åž, Æn assessed Åž,...

p.2, I.9-18. Please add references.

p.2, I.11. The debris cover can be continuous.

p.2, I.14. Æn RG morphology is coherent, stable, ...Åž. What is a coherent and stable morphology ? Please be more accurate.

Fig. 1 : Explain in the text the significance of the drainage network. In addition, it appears violet instead of blue on the map.

p.4, I.25 and Fig.3. It is unclear how the limit between the rock glacier parts and debris-covered parts were drawn. On Fig. 3 you should draw the limits of the Western and Eastern units.

Fig. 4 : Indicate the location of Las Tetas on Fig. 3. Indicate by arrows or signs the location of the central depression and of the thermokarst on the photos.

p.5, I.10-13. These lines are a bit fuzzy. The model used is only indicative and, in my opinion, cannot be used to explain in a so simple way the morphological differences encountered on the different parts of the landform. In addition, I observe that thermokarst

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is also present were permafrost probability is higher than 0.95.

p.5, l.28-32 : In my opinion this is not really a rock glacier morphology, because the landform is very thin, especially the northern lobe and because the surface ridges are not classical compression ridges of block-ice mixture, but rather ridges due to subsurface ice flow.

p.5, l.29-30 : À Small morainic... À : I do not understand what you are talking on.

p. 6 l. 1 : remove À ice À l. 22 : 100 m high seems to be exaggerated l. 27 and subsequents : À aerial photos À is more appropriated than À air photo À

p.8, l.1. Prefer “altitudinal changes” to “vertical displacement”. Valid for the rest of the text.

p.8, l.14. There is an issue with this interpretation of vertical changes. If a vertical change occurs locally, this means that an additional process to downslope movement occurs. Indeed, if only downslope movement occurs (for instance without ice melt, ice aggradation or compression), then the altitude at the same location will not change.

p.8, l.19. You must explain how this boundary was defined. Which criteria were used to distinguish rock glacier morphologies from debris-covered ones ? This is important since a large part of the study relies on this distinction.

p.9, l.3. As far as I know, it is not necessary that the two images must be taken from the same position. With remote sensed images it is even impossible.

p.9, l.15-20 and Tab. 3. It is unclear for me how the search window was defined : what is the relationship between columns 4 and 5 in Tab. 3 ? In addition, the maximum expected displacement can be well above the front displacement.

p.10, chap. 4.1.1. Without viewing the aerial images, it is impossible to see how you reached your conclusions about geomorphological changes. You give a change value of 75%, but how can you make a spatial quantification of the changes, knowing that dif-

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ferences between rock glacier and debris-covered glacier morphologies are somewhat not well defined and probably quite subjective.

p.10, I.20. What is this \dot{A} current morphology \dot{A} ?

p.10, chap. 4.1.3 and Fig. 6&7. As already mentioned it is very difficult to read the velocities. In addition, the directions appear to be very noisy, and a systematic error seems to occur, as indicated by the general south-west direction.

p.10, I.27. What are exactly the displacement rates for rock glaciers ? The values given by Barsch and Haeberli may be true for a period running up to the early 2000's, but now several studies have shown that the velocities have strongly increased in the recent years, and that velocities above 1 m/y is very often the norm.

p.11, I.17. I do not see the heaving in the fronts and the margins.

p.11, I.18. See my comments concerning the Method (interpretation of vertical movements).

p.11, I.27-30. This evolution is very difficult to see on the images. Could you make a zoom on the frontal part ? In addition, try to better show the rock glacier morphology (in Fig. 4_3 we do not see the front).

p.12. chap. 4.2.3. Obviously there are no velocity differences between the debris-covered glacier part and the rock glacier part. Is this realistic ? Again, are you sure that there is no problem with the method ? I.17-18. I do not see the two flow lobes in the upper part.

p.12. chap. 4.2.4. Your conclusions regarding compression, bulging, expansion and ice melt are too direct and lack any demonstrations. How can you conclude to these processes ? This must be better explained and discussed. For instance at the front, the vertical gains may also be attributed to the rock glacier advance and not by compression : in 2000 the rock glacier front was (?, you can verify) located upside the current position, so the blue colours here would just indicate the downslope progression of the

front.

p.13, chap. 4.3.3. The vectors are unreadable

p.15, l.29-32. I don't understand this sentence.

Interactive comment on Earth Surf. Dynam. Discuss., doi:10.5194/esurf-2016-16, 2016.

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