**The Cryosphere
Discussions**

Interactive comment on "Glacier changes on Sierra Velluda massif, Chile (37° S): mountain glaciers of an intensively-used mid-latitude landscape" *by* A. Fernández et al.

A. Fernández et al.

alfernandez@udec.cl

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The paper primarily supplies details of satellite remote sensing of a system of 13 glaciers in central Chile over 1960-2007. It appears to this reviewer (who is not experienced in remote sensing) to be strong on that subject but to be weak on glaciological interpretation; there should be a table, for instance, giving basic geometric properties of the glaciers: area, altitude range, aspect.

The paper's principal result (Fig 6) shows that altitude change is negative and is nearly independent of altitude. The authors seem to be unaware that the overwhelming pattern worldwide is that altitude loss decreases from the terminus to the higher parts.

The paper is moderately well prepared but would benefit from assistance with the English. Some papers cited in the text do not appear in the list of references, and some in the list are not cited.

Authors' comment: All of these general comments were changed from the response to the Editor's revision. Indeed, the locations referred by the referee match with the first submission instead of the discussion paper currently online. We strove to addressed them accurately. We agree in including more basic glaciological data in the manuscript. Regarding thickness changes, we will include more comment based in our findings, but we would like to recall that we found these altitude changes after a thorough analysis of uncertainties in the topographic and GPS data. Finally, all references were included in the discussion paper and the English has been improved in the discussion paper. However, it will be improved even more.

Comments in the following are indicated by page and line (page,line)

(5,19) The range of the summations in Eqn (1) are not defined nor are the units. The form of the relation seems peculiar, for I would expect something more like under the assumption that the individual and are mutually uncorrelated. An excellent reference is Bevington (1969) Data reduction and error analysis for the physical sciences New York, McGraw-Hill.

Authors' comment: This comment was done for the Editor in the first submission. We used the formula employed by Ye et al (2006). In the Discussion paper the writing was changed to make it clear.

(6,1) Eqn (2) also seems peculiar. Unlike Eqn (1), here there is no square root on the second term. Comments at (5,19) apply here.

Authors' comment: This comment was done for the Editor in the first submission. We used the formula employed by Ye et al (2006). In the Discussion paper the writing was changed to make it clear.

(6,10) faces and tandems need to be defined.

Authors' comment: This sentence no longer exists in the discussion paper, corrected from the Editor's review.

(7,11) Does "one GPS" mean one GPS station with accurately know position ?

Authors' comment: This sentence no longer exists in the discussion paper, corrected from the Editor's review.

(7,15) Is sector here the same thing as area at (7,14) ?

Authors' comment: Location in the discussion paper: (8.20). Yes, we will change for 'area' to make it clearer.

(7,17) Change "n" to n and also at (7,26)

Authors' comment: Location in the discussion paper: (8.24 and 9.5). It will be changed.

(7,30) The caption of Fig. 2 needs to define the coordinates in a and c.

Authors' comment: This comment was done for the Editor in the first submission and was unchanged. We wrote explicitly in the figure caption that we do not claim any unit of reference because the spatial distribution is in standardized principal components. This procedure shows the spatial variability and it is analyzed in that context. Thus, we will not include units.

(8,5) Why the Gaussian distribution is rejected because $p > 0.1$, whereas at (8,9) the normal distribution is rejected because $p < 0.01$ should be explained.

Authors' comment: Location in the discussion paper (9.18-9.23). We changed it in the first submission. The discussion paper clearly states that Gaussian is modeling adequately with $p>0.1$.

(9,5) Does this refer to the one GPS mentioned at (7,11) ?

Authors' comment: Since this sentence is related to obs (7.11) which no longer exists

in the manuscript. We do not see the need to change it.

(9,22) A reference for the Durbin-Watson test would be useful.

Authors' comment: A reference will be included: Durbin, J., and Watson, G. S. (1950) "Testing for Serial Correlation in Least Squares Regression, I." Biometrika 37, 409–428.

(11,1) I am unable to read Spanish.

Authors' comment: These paragraphs no longer exist in the discussion paper, corrected from the Editor's review.

(13,1) It would be helpful to give the aspects in Table 4. To make space for them, latitudes and longitudes could be given in degrees to two decimal places, as in most glacier archives, instead of in degrees, minutes, seconds.

Authors' comment: We will modify the table accordingly.

(13,20) Stating percentage changes to the nearest tenth of a percent, here and elsewhere, is not warranted by the accuracy of the numbers divided.

Authors' comment: These percentages were corrected from the Editor's review; they do not exist in the discussion paper.

(13,23) Giving the entire code RC108376/1 instead of just 76/1 would make for easier reading.

Authors' comment: These codes were corrected from the Editor's review; they were included in the discussion paper.

(13,31) Instead of repeating in the text much of the material in Tables 4-6, it would be better to mention the possible causes, such as changes in precipitation and temperature.

Authors' comment: This section was rewritten after the Editor's review and much of the

interpretation is located in the Discussion and Conclusion section.

(14,5) Here and in many other places appears the term "map algebra," which ought to be defined at its first appearance or perhaps a more widely understood term could be used instead.

Authors' comment: We will include a reference to Longley, P. 2001. Geographic information systems and science. Chichester; New York: Wiley.

(14,14) Although -0.51 ± 0.67 m/yr, over 40 years, might not be significantly different from zero in a statistical sense, it is highly significant glaciologically and climatologically, which should be acknowledged.

Authors' comment: We have discussed the implications of this findings in the section Discussion and Conclusions.

(14,19) Is inside the range meant ?

Authors' comment: This section was rewritten after the Editor's review. We tried to make it clear that we did not use confidence limit to reduce the data analyzed. We wrote explicitly that the overall change is not significant, but there are significant trends.

(14,22) Fig 6b shows an extremely weak altitudinal variation of the change.

Authors' comment: Effectively the altitudinal variation of the change seems weak in the figure, but is statistically significant as we stated in the text.

(14,28) Saying 12 out of 13 would be more informative than 92%, and Table 6 does not give confidence intervals of the rates.

Authors' comment: Location in the discussion paper (16.6): It will be changed.

(16,17) These response times are extraordinarily short.

Authors' comment: Following the comments of the Editor in the first submission, the discussion paper has "reaction time" instead of "response time". So the reaction times

are short, which coincide with the climatic variability documented for that zone.

(24,2) This table is hard to understand. Why are there both rows and columns for MSS, ETM+, ASTER but only a row for TM ? The TM and ETM+ rows are identical. It would be easier to read were the values not repeated in two places, such as the 124.1 and 0.02 for MSS and ASTER.

Authors' comment: We will change the table accordingly.

(25,1) Quantities in Table 3 are inadequately defined. Are trends m of error per km of altitude ? See comment (9,22). Rows 2 and 3 have different values but have the same label.

Authors' comment: They are km of altitude. We will make it more explicit in the table.

(26,1) The inventory system in which the glacier numbers are used should be identified. Perhaps RC refers to Rivera, Casassa.

Authors' comment: This was clarified in the discussion paper following the Editor's review.

(27,2) Because |0.01| is positive, it would be less confusing for RC108370/5, for instance, to give -0.003 km2/yr

Authors' comment: Since we want to state that the change is less than -0.01, if we write the negative number, there will be a misunderstanding, since <-0.01 formally implies numbers like -0.02, -0.021 and so on, meaning more retreat, which is not true. With absolute numbers, our intention is to make it clear how small the rate of change is.

(28,1) That glacier RC108376/2 shortened (-353 m) and thinned (7.25 m) but gained area (0.14 km2) is worthy of comment.

Authors' comment: This was clarified in the discussion paper following the Editor's review.

(32,1) The glacier should be identified. It would be interesting to see the 1828 to 2003 lengths plotted as in Fig. 5

Authors' comment: We will include a graph showing the change from 1828 to 2003 in fig 4.

(33,2) Fig. 5 would be easier to interpret if the letters were also shown in Tables 4-6. The variables in the figure are length and area relative to 1960 for a-k, relative to 1974 for l,m. It should be said that the scale for the relative length (red) is given at the left in m and for the relative area (blue) at the right in km2.

Authors' comment: We will include numbers and we will improve the figure caption to make the scales clearer.

(34,2) Period of the changes shown in Fig 6 should be stated. Its horizontal scale in b would be much clearer were its tick marks labeled only at 2100, 2200, ..., 3600. Units are needed on values in the legend, which seems to have its signs reversed. Somehow locations of the 13 glaciers represented in Tables 4-6 and in Fig 5 should be shown, possibly in Fig 6a but perhaps by creating a new panel Fig 6c.

Authors' comment: We will include the periods, legends and we will modify fig .1 to show glaciers' locations

(35,1) "relative altitude distribution" apparently is the fractional area within 100-m altitude intervals.

Authors' comment: Yes, it is. We will improve the fig. caption to explain that scale.