

***Interactive comment on “Close range photogrammetric methods applied to the study of the fronts of Johnsons and Hurd Glaciers (Livingston Island, Antarctica) from 1957 to 2013” by Ricardo Rodríguez et al.***

**Anonymous Referee #1**

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This manuscript describes a long-term data set of glacial front position changes of Johnsons and Hurd Glaciers (Livingston Island). Front positions were obtained from satellite imagery, aerial photographs, GNSS surveys and a close range photogrammetric survey in 2013.

While South Shetland Islands are one of the most easily accessible sites in Antarctica, surprisingly the glacier inventories are not updated and commonly report the ice front positions from 1957. Therefore, each credible contribution that updates those inventories and provides a detailed record of glacial extend change is highly appreciated.

C1

However, in my opinion this is not the case of this study due to unsatisfying quality of both the manuscript and the presented data. Regretfully, my recommendation is to reject the manuscript.

First of all, the title “Close range photogrammetric methods applied to the study of the fronts of Johnsons and Hurd Glaciers (Livingston Island, Antarctica) from 1957 to 2013” is wrong and misleading: only a minor part of the results is obtained with close range photogrammetry. Most data comes from either aerial (1957, 1990) and satellite (2007, 2010) optical imagery or ground based GNSS surveys (2006-2012). Only a single front position obtained with close range photogrammetry is reported, as stated in the title (Johnsons Glacier 2013). Therefore, either the title or the manuscript itself needs to be changed in order to eliminate this discrepancy.

Overall, the quality of the submitted manuscript is in my opinion unacceptable for publication, basically it needs to be rewritten from a scratch. The structure of the article is inappropriate: a large portion of the text is describing the acquisition of just one front position of a single glacier (Johnsons 2013). Majority of the figures (1,3,4,7, 9-12) have embedded text in Spanish, whereas the Journal only accepts submissions in English. Large parts of the text are totally incomprehensible, it seems like it was translated from Spanish using an automatic translator. This alone would be a sufficient reason to reject the manuscript in the present form. In this case, however, I also have a major concern about the quality of the presented data:

Fig. 9 shows clearly that the ice front position of Johnsons in 1957 was erroneously delineated. Within the delimited area of Johnsons Glacier one can see icebergs and growlers floating in the sea next to the glacier termini. This is a profound error that undermines any potential trust a reader would have in the values reported in this manuscript. At this moment, there is no way to verify ice front positions in other years as unfortunately no source data is shown in figures apart from 1957 aerial photo. This could be solved by including corresponding, similar figures for other imagery used (1990, 2007, 2010) in order to ensure that such error was not repeated elsewhere.

C2

Another concern is that a similar, more detailed work of Molina et al. (2007) is surprisingly not cited. This comprehensive study of the volume changes of Johnsons and Hurd Glaciers reports similar long-term changes for a slightly shorter period of time (1957-2000).

I have also serious doubts about the reported ice front positions of Sally Rocks lobe (Fig. 10). Authors claim that the ice front advanced "47 m (C segment) in 2000-2006 and another 36 m (D segment) from 2006 to 2009". This means front advance rates up to 12m/y, which is simply unrealistic for this glacier, given that the typical maximum ice velocity of Hurd Glacier are 4 m/y and the ice front is frozen to the ground (Molina et al., 2007). Latter work of Navarro et al. (2013) shows that over this period mass balance was generally slightly negative, I do not see a good explanation for such high advance of the ice front, apart from an error or a misinterpretation, e.g. due to a presence of snow in the forefield of the glacier.

Specific comments:

P1 I.15-20: I do not understand this part, please rewrite it

P1 I.20-23: Indicate that the non-metric camera was only used in 2013

P1 I.23-25: Major part of the work was not based on close-range photogrammetry as claimed in this sentence and in the title

P1 I.28-35: Incomprehensible, please re-write

P2 I.1-2: Satellite, not aerial imagery

P2 Fig. 1: Names of ice domes are given in Spanish, integrate them within the map, now they are outside and are hard to read.

P2 I.11-26 Please re-write this section

P3 Fig. 2: Eliminate this figure, I do not see point of explaining with a figure the concept of radial distortion

C3

P3-P4 Photogrammetry is a well established method, no need to describe it in such detail. Just introduce it briefly and provide references.

P4 Fig. 3: Please translate the figure to English. Rotate the coordinate system, in this paper you consider a vertical ice wall and this figure seems to be prepared for aerial photogrammetry. Also, this and other figures are directly copied from the cited unpublished PhD thesis (Rodriguez, 2014). I guess this explains inclusion of labels in Spanish.

P5 Fig. 4: What are the black triangles? Caption states that these are ground control points at the glacier front, however they are also labelled as B1000, B2000 and B3000 bases used for theodolite measurements. I am a bit puzzled... Where are the ground control points located? Note that a numerical scale 1:6000 does not have much sense in electronic form of the article. Is this scale reported for the supposed printed size of the figure?

P5 I.18-20 What is the accuracy of theodolite measurements?

P6 I.9: Camera has 10 MP, not the lens

P6 I.10-22: This is a very important section for the paper, nonetheless it is totally incomprehensible. Please re-write it.

P6 I.19-24: I like the idea of calibrating lens on such big object, of a comparable size to the studied glacier front. This allows proper lens calibration with focus set to infinity, exactly as in the subsequent field work application on Johnsons Glacier.

P6 I.36: Couldn't you simply calculate the control point position from the architectural plans of the building? Wouldn't that give you precision higher than 53 mm?

P7 I.1: How precisely was the focal length determined? Why didn't you use three different prime lenses?

P7 I.8-13: Include a table with calibration coefficients. What was their estimated error?

C4

P8 I.5-6: This estimation is important, however the language quality makes this sentence unclear, please correct it. How was the error estimated?

P8 Fig. 6: Labels a and b in the panels and caption are missing

P8 Fig. 7: What is the name of the software?

P9 Fig. 8: Labels a and b in the panels and caption are missing. Scale bar would make this figure much more clear.

P9 I.4: How was the error estimated? Was it compared with ground control points? If so, how many? A proper assessment of the method performance is missing, for example I would expect a comparison of the calculated front position with a one obtained with a different method (high-resolution satellite imagery?)

P9 I.10: What was the rationale for repeating the photogrammetry on 1957 imagery? The ice front positions in 1957 have been already published and in fact are part of various glacial inventories. Randolph (Pfeffer et al., 2014) and Antarctic periphery (Bliss et al., 2013) inventories surprisingly were not cited. What are the differences between your results and the ones available from the inventories? Why don't you compare your results with those of Molina et al. (2007) who used the same imagery?

P9 I.20-21: I do not follow this sentence, why was this data excluded from your analysis? What do you mean by "upper front line"?

Tab.1-6: How were the mean square errors calculated? Why is the error of CNDP-ESP\_SIMRAD\_FRONT\_JOHNSON\_1957.shp two times lower than other glacier limits in 1957?

P12 I.3-9: Please correct the 1957 ice front position and change accordingly the corresponding description of ice front position changes. What was the reason for 1957-1990 advance? Why is the correct 1957 position in the region of sector E so close to the ones observed in 2007-2013? Can it be explained by the bathymetry of the embayment?

## C5

P12 Fig. 9: As stated above, the 1957 ice front position is wrong. It covers area of open water filled with icebergs (grey area near profile E). Correct front position is close to the ones for years 2007-2013. There is no way to verify ice front positions in 1990-2013 since no source data is shown. Panels should be labelled. When was the image in the smaller panel taken?

P12, I.14-17 and P13 Fig. 10: What are the black spots on the supposed glacial surface? Were they tephra layers as reported by Ximenis et al. (2000)? If so, would you associate them with any particular eruption on Deception Island? What does it say about mass balance rates of the glacier? Was the forefield snow free? If not, how was the snow surface distinguished from glacier surface? I have a major concern about the data quality: 47 meter advance in 6 years for such a slowly flowing glacier is very much, how do you explain it? The advance rate is even higher in 2006-2009 (12m/y), that sounds a bit unrealistic. Navarro et al. (2013) report negative mass balance for Hurd Glacier over this period, the snout is frozen to the bed, the maximum ice flow velocities are lower than 5m/y (Molina et al., 2007), how is it possible that the glacier front advanced 12m/y?

P13, I.14-17 and P14 Fig. 12: How do you explain the difference between 1957 and 1990 in the area to the sides of the snout? Was the forefield in 1990 snow free? Again, it is hard to verify a quality of the results without a figure with a corresponding aerial image.

P15, I.2-24: If the reported ice front positions are wrong, the discussion section has to be rewritten. Especially given that there is a serious doubt about data quality, mostly concerning position of Johnsons Glacier and Sally Rocks lobe fronts.

## References:

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## C6

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