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Interactive comment on "Thinning of the Quelccaya Ice Cap over the last thirty years" *by* C. D. Chadwell et al.

Anonymous Referee #3

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General comments :

In this paper, the authors attempted to assess the mass balance of the Quelcaya Ice Cap (QIC) over the last 30 years. From a reduced dataset, they calculated the volume change between 1983 and 2013 by differencing the surface elevations suggesting an average annual mass balance rate of -0.5 m w.e a-1 over this period. Based on calculations of emergence velocities, they claimed that the thinning of the ice cap is due to an increase in melting and sublimation of 1-2 m w.e a-1.

However, the novelty of this paper and the relevance of the results can be questioned:

1) The volume change of QIC has been obtained previously between 1963-1978 and 1978-1991 from photogrammetry (Thompson et al., 2006). In the present paper, the elevation changes are obtained from 46 sites (mainly in the accumulation zone close

to the summit and 5 sites below 5500 m) between 1983 and 2013 on one longitudinal profile only. The added value of this new value can be largely questioned given the poor dataset shown in this study.

2) The new result of this paper could be relative to the cause of this thinning. From emergence velocities calculations, the authors deduced an increase in melting and sublimation of 1-2 m w.e a-1. However, these results are very fragile for the following reasons. First, the emergence velocities determinations and mass balance changes are supported by very few measurements and very large assumptions. 9 stakes measurements are available to assess the emergence velocities (line 2, p. 9) in 1983/1984 in accumulation zone and 3 stakes in ablation zone only. Moreover, in ablation zone, the measurements of ice flow velocities (for emergence velocities calculations) have been done over a duration of one month and extrapolated to an annual rate. Second, the density is unknown in the accumulation zone. The mass balance calculated in accumulation zone from continuity equation depends strongly on density assessment of the firn. The density used for the calculations of emergence velocities contains large uncertainties (Fig. 7). The inferred mass balances are strongly affected by these uncertainties. In this paper, the calculation of uncertainties has not been clearly explained. Third, the emergence velocities (and the inferred mass balances) are also strongly affected by the uncertainties on the slope. In this paper, the authors do not provide any information about the measurements of the slope, the distance on which the slope has been measured. It seems that the uncertainty on the slope has not been taken into account despite the strong impact on the results.

3) This paper suffers from large assumptions and large uncertainties: the calculations of uncertainties are not explained properly. In addition, I believe that all the uncertainties have not been taken into account. From this paper, it is not possible to assess properly the impact of the large assumptions on the uncertainties. What is the impact on slope uncertainties ? The authors did not explain properly the uncertainty calculations of inferred mass balance (bs from Equation 5) taking into account the uncertainty

on ws, us, slope, thickness change and density, and the duration of measurements (one month in ablation zone). The error bars are reported in Figure 6 without any explanations. In addition, regarding the error bars, are the calculated mass balance changes significant? It seems that the uncertainties are the same order of magnitude of the estimated values.

4) This paper suffers from confusion and vagueness: for instance, the results shown in Figure 5 and Figure 6 come from calculations which are not explained properly. It is confusing. From these Figures and from the manuscript, I understood that the emergence velocities calculated in 1983/1984 (blue line) shown in Figure 5 have been calculated from direct measurements w-utgα. The mass balance of 1983-1984 shown in Figure 6 (green line) come from these emergence velocities and are similar to emergence velocities shown in Figure 5 (blue line) corrected with density (If it is the case, I do not understand why the zero mass balance is above 5400 m (Fig. 6) while the zero emergence velocity is below 5400 m (Fig. 5)). I understood that the mass balance of 1983-2013 (Fig. 6, red line) have been obtained from the difference between dH1983-2013 and the emergence velocities 1983-1984. The explanations given in section 4.1 and 4.2 are very short and very confusing. The authors should provide clearly the meaning (equation) of each curve.

5) Finally, the mass balance change of 1983-2013 are calculated from emergence velocities obtained in 1983-1984 and the elevation changes obtained between 1983 and 2013. In this way, the authors assume that the emergence velocities are constant over the period 1983-2013 which would mean that the flow lines did not change over this period of 30 years. This assumption cannot be supported by the available data. In numerous studies for which the glaciers have been decreased over the last decades, this assumption is not valid. Unfortunately, the main new result of the present paper is based on this assumption. In section 4.2 (line 7, p. 10), the authors acknowledge that the mass balance and emergence velocities could change together between 1983 and 2013 but this assertion is almost ignored in Discussion, and totally ignored in

Conclusions and Abstract.

6) The discussion about the relationship between rate of margin retreat and ablation (lines 4 – 31 page 12) does not provide significant support to the conclusions. It remains very hypothetical and qualitative.

7) A large part of the discussion relative to accumulation zone (section 5.2, lines 14-30, page 13) should be moved in Data section. In addition, it is not clear how these data have been taken into account in the mass balance calculations inferred from emergence velocities measurements (Fig 6). In this study, it seems that the change of density with depth is not taken into account.

In this study, the authors attempted to use old (1983-1984) and recent data (2013 and 2015) to infer surface mass balance change over the period 1983-2013. It is obvious that these measurements have not been carried out for the purpose of the present study. Consequently, the dataset is very poor and many measurements are missing. I believe that the conclusions reported in this study cannot be supported by the available measurements given the very large uncertainties which affect the data. The main conclusion of this paper is that the results can be only solved by measuring new surface velocities across the ice cap as acknowledged in Discussion and Abstract (line 15, p. 14 and line 14, p. 1). Indeed I believe that the expected results about mass balance and the cause of mass balance changes need further measurements. Unfortunately, I do not believe that the results shown in this study are sufficient for publication in The Cryosphere.

Specific comments

Numerous specific comments should be needed to improve the clarity of the manuscript. Some specific comments have been mentioned here, although it is not necessary at this stage.

P.1, line 8 : the authors should avoid the term ‘ mass balance’ given the value has been obtained from elevation changes on one longitudinal cross section only.

P. 1, l. 10 : provide the uncertainty on the increase of melting

p. 1, l. 18 : m of water ?

p. 2, l. 8 : ‘ returned frozen to the laboratory ‘ : this kind of detail is not useful and can be deleted. Other details not directly related to the purpose of the present study can be removed from the manuscript.

p. 3, l. 2 : what do the authors mean by ‘ highly correlated’ ? is there a statistical relationship ?

p. 4, l. 7 : which flow model ?

p. 4, l. 9 : any reference for these previous measurements ?

p. 5, l. 1 : avoid the term ‘ geodetic mass balance ‘

p. 6, l. 6-7 : specify the instruments (EDM and theodolite).

p. 6, l. 7 : usually, the accuracy of EDM is given by a constant plus a value which depends on the measured length

p. 6, l. 12 : confusing. In my mind, the reciprocal vertical angles measurements should provide the orthometric height difference given that the observations performed with theodolite are relative to the geoid.

p. 6, l. 16 : the uncertainty relative to N is not mentioned. It is probably high. What are the spatial fluctuations of N in the studied area ? It should strongly affect the accuracy of the elevation changes obtained from classical topography in 1983 and from GPS in 2013.

p. 6, l. 7-28 ; the authors mention the accuracy of the elevation measurements only. They do not mention anything about the horizontal angles and the accuracy of horizontal coordinates. However, the accuracy of horizontal coordinates are crucial given that

the elevation changes accuracy depend also on the XY accuracy.

p. 6, l. 30 : specify the instrument and the method (differential ?). Specify also the duration of the GPS measurements.

p. 7, l. 3 : how is 0.36 m obtained ? what is the uncertainty on N ? How does N change over the ice cap ? It is probably badly known.

p. 7, l. 6 : mention clearly QSP1 and QSP2. Add QSP2 in Fig. 2

p. 7, l. 7-8 : how can the authors obtain an uncertainty of 0.01 and 0.03 m given that the uncertainty of global coordinates is 10 cm (p. 6, l. 31) ? The assessment of uncertainties is not clear.

p. 7, l. 11 : the authors should describe the surface mass balance measurements in this Section

p.7, l. 11 : the authors did not describe the terrestrial measurements of 1978 in this section (instruments, location of measurements, accuracy. . .)

p. 7, l. 13-19 : the authors should explain how the uncertainties have been obtained.

p. 7, l. 27 : ‘ mass loss’ ?

p. 7, l. 29-33 : I do not believe that the authors may obtain a ‘ geodetic mass balance ‘ from this very limited dataset.

p.7, l. 29 : why 440 kg/m^2 ?

p. 8, l.7 : ‘ assumed two dimensional flow, i.e no tranverse flow ‘ : it is not necessary to assume no transverse flow. See Equation 8.65 In Cuffey and Parson (2010). Here, the ice flow velocity has been measured in the direction of the ice flow. The horizontal divergence of the ice flow does not change anything.

p. 8, l. 27 : the uncertainties related to the short duration of measurements (one month) are not explained in the manuscript.

p. 8, l.27 : the uncertainty related to the local slope is not mentioned in the manuscript. In addition, the authors did not describe the method to measure the local slope. Which distance is taken into account to measure the slope ?

p. 9, l. 8 : the calculation of the uncertainty (0.28 m) is not explained

p. 9, l. 13 : I understood that the authors calculated the emergence velocities to infer hypothetical steady state surface mass balances in 1983-1984. However the sentence is confusing.

p. 9, l. 1-14 : the section 4.1 is too short. It should provide more explanations and should provide the calculations of the uncertainties (other parts of the manuscript could be strongly reduced)

p. 10, l. 3-9 : the section 4.2 is very very short. It should provide more explanations and should provide the calculations of the uncertainties. It should also provide clear explanations about the results obtained in Figure 5 and Figure 6.

p. 10, l.6-9 : which data could support that the emergence velocities did not change between 1983 and 2013 ? This assumption is not discussed in the manuscript.

p. 10, 11, and 12 : Discussion : a large part of the discussion is not very helpful. The calculation of ablation from the relationship between ablation and margin retreat is too crude to support the previous results given the uncertainties and the approximations related to this relationship.

p. 13 : Discussion A large part of Section 5.2 should be moved to Data section (accumulation and density measurements).

Figure 3 : the authors should explain the meaning of the lines.

Figure 4 : the two vectors of the ice flow velocities between 5600 and 5550 m are turned upward relatively to the slope although these sites are in the accumulation zone.

Figures 5 and 6 : red dash line of Fig 5: it is not clear how this curve has been obtained.

I understood that it is the difference between the emergence velocity1983-1984 and dH1983-2013 but I am not completely sure given it is not explained clearly. I would suggest to mention clearly the calculations relative to the red and blue lines (and red and green lines in Figure 6) in the manuscript.

Figure 7 : the authors should describe clearly in the manuscript how the red points and lines have been obtained

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