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The Cryosphere
Discussions

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

Anonymous Referee #2

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The authors provide data on the thinning rate and volume loss (per unit area of transect) of the world's largest tropical ice cap, the Quelccaya Ice Cap (QIC). They provide support from the literature for the values observed thinning rates and modeled mass balance changes. These data are a significant contribution to our current knowledge about tropical glacier evolution over the past few decades, in a region where such measurements are limited. Additionally, the authors use the continuity relationship to evaluate various [climatological and dynamical] causes for the observed thinning at the QIC. Their conclusions about causes for observed thinning (mainly increased ablation rates in the ablation zone and a possible combination of density changes and changes in accumulation in the accumulation zone) are supported by their methods, data, and applications of previous research. The authors also outline areas where additional research could strength the results of their study and the field's understanding of the QIC

responses to climate change, namely whether ice velocities have changed since the measurements in the mid-1980s. The paper is well thought out and beneficial to the field, but I have some broad and specific comments on the content and presentation:

I suggest re-formatting on the Introduction. The Introduction provides a detailed and chronological list of past work at the ice cap. The driving problem and specific questions of the research, however, are not provided in the Introduction.

For the application of the continuity equation to enlighten the causes of the QIC thinning, there were instances where I was not clear on the assumptions being made. Below, in the Specific Comments section, I enumerate these occurrences.

Additionally, much of the results hinge on whether mass fluxes from the ablation zone to the accumulation zone have changed, which the authors outline as an area for further research. Are there end-member cases (on ice velocity) that that the authors could explore to quantify how dependent their conclusions are on whether mass fluxes have changed? By adding these end-member scenarios, their conclusions on the causes of observed thinning would be stronger.

The authors provide a valuable data set on ice thinning and mass loss at the QIC and that will aid future research of low-latitude glaciers. The writing and figures are clear and logical, and the paper is well supported by the literature. If the authors address the broad comments (above) and specific comments (below), then I would recommend this manuscript for publication.

Specific Comments:

pg. 1, line 2: should you cite a reference on the QIC being the largest tropical ice mass?

Figure 1/Figure 2: Can these figures be combined? I found the quadrilateral network in Figure 1 seems to be important in Figure 2 but slightly distracting in Figure 1 as a stand-alone figure.

pg., 2, line 16: Is there a citation for the increasing rate of volume loss?

pg. 5, lines 1 & 2: I'm not sure what this sentence about how 'typically such geodetic mass balances are based' is trying to say. Are your methods A-typical? By ending the introduction this way, I'm left wondering why this hasn't been done before, rather then excited that this paper will fill in the gap. This specific comment ties back to the first general comment.

pg. 5, line 7: Has the EDM data from 1983 not been published before?

pg. 7, lines 29 & 30: What are the implications of using a step-wise density profile? From Figure 7, I see that the density profile does affect the modeled results on thinning rate. Has a sensitivity analysis been conducted on the assumptions about the density profile?

pg. 8, lines 1 & 2: The last two listed possible causes for glacier surface lowering seem to contradict one another. If there is an increased flux out of the accumulation zone, must there also be an increased (instead of decreased) flux into the ablation zone; correct? Also, with tropical glaciers, which have large accumulation area ratios, a decreased flux into the ablation zone should mean that more mass stays in the accumulation zone, which encompasses a greater area, and thus increasing the total height of the glacier. Am I missing something?

pg. 8, line 19: Is it appropriate to assume negligible basal motion? The ice core records indicate that the QIC is not a cold-based glacier. Thus basal sliding is permissible. I am not sure if previous work has attempted to constrain the sliding versus deformational velocity of the ice cap. How substantial does basal sliding need to be before this assumption is invalid?

pg. 9, line 14: What information does assuming a steady-state height in 1983-1984 provide? This seems to be a key point of your analysis of the thinning rate measurements. Also, how reasonable is this assumption? The Qori Kalis outlet glacier has retreated

throughout the direct observational and photographic record. And from Figure 2, in the region of the survey, the ice retreated even between 1980 and 1984. These observed retreats would lead me to believe that the ice cap was not in equilibrium in 1983 – 1984 with the climate. Of course, assumptions must be made when using models, but what are the reasons for these assumptions and what are possible implications?

Figure 5: Is it a coincidence of the data that the emergence velocity is zero at $\sim$5400 m in 1983-1984 (blue curve)? Or did you prescribe the curve with that value, since that is roughly the ELA from snow line measurements in the late 1970s? Also, what does a lowering of 75 m in the ELA accounting for the observed thinning mean? I don't see this point discussed in the text.

Figure 6: Is it appropriate to think of the red dashed line as the specific balance profile today? And is the difference between the two curves is the mass balance anomaly per elevation? How does the 50 m increase in the ELA compare with values in the literature?

pg. 10, lines 13 & 14: This point should also be made in the introduction and the abstract.

pg. 12, line 11: How do you determine the perturbation in the ablation rate? Are you assuming that in Figure 6, the perturbation in the ablation rate is the difference between the green solid and red dashed curve? At $\sim$5400 m a.s.l., that difference looks to be $\sim$1 m w.e. per a, but at $\sim$5300 m a.s.l. (near the ice margin) that difference looks to be closer to 2 m w.e. per a, which is order unity and reasonable for Equation 6. But in the text or the figure captions a bit more information about what the curves mean would be helpful.

pg. 13, lines 6 - 9: These assumptions seem inconsistent with the ice core record and influence of ENSO. If I plot the 2003 South Dome ice core record (plus recent snow pits) (https://www.ncdc.noaa.gov/cdo/f?p=519:1:::::P1_study_id:14174) from thermal year 1976 through thermal year 2009 and plot a linear regression, I find a net summit accumulation rate of change of +0.001 m w.e. per year) (r-squared value of 0.001 and p-value of 0.81). If I regress from thermal year 1984 through thermal year 2009, the net summit accumulation rate of change is + 0.002 m w.e. per a (r-squared value is 0.001 and p-value is 0.84). If I were to just draw a line from the thermal year 1983 to the thermal year 2009, then I see a net summit accumulation rate decrease of -0.018 m w.e. per a, but this method would not capture a trends in the accumulation. During this period, the 1-sigma value of the net annual summit accumulation is $\sim$0.4 m w.e. Even if the starting and ending summit mass accumulation values (that I draw the line between) were several-year averages, connecting dots would only be valid if those averages had similar statistics on ENSO events.

pg. 14, line 26: Is ‘effective ablation rate’ a perturbation in the ablation rate compared to 1983-1984?

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