

Interactive comment on “The equilibrium flow and mass balance of the Taku Glacier, Alaska 1950–2006” by M. S. Pelto et al.

Anonymous Referee #1

Received and published: 6 June 2008

General Comments:

1. This is mainly a data paper with some interpretation. It is good to finally see some results of the decades long JIRP research program at Taku Glacier. Unfortunately, the quality of the mass balance data suffers from a variety of factors. The first is that mass balance measurements are concentrated almost entirely in what the authors term the lower neve zone (~ 925 m to 1350 m). No measurements are made in the ablation zone (< 925 m) and few are made at elevations above 1400 m. Mass balance must then be extrapolated from the measurements in the “lower neve” both up and down glacier. They do this by using mass balance measurements made decades ago and by using the annual position of the ELA. The second problem is the measurements are made in July and then extrapolated to the end of the melt season. The third problem is

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quality control on mass balance measurements, which are generally made by students under supervision by a senior researcher. Despite these problems, this is the only 50 year long annual mass balance record available for any glacier in southeast Alaska and for that fact alone, the paper deserves to be published.

2. The paper reads more like a report than a journal article but perhaps that is inevitable in a data paper. I indicated below in “specific comments” where some editing could be done to tighten up the writing.

3. Our group has collect summer, winter and annual ablation data for the terminus region up to 8 km above the terminus over a three year period, 2002-2005. We offered to share this data with the lead author and our offer still stands. We have measured annual ablations ranging from 12 to 14 m w.e. at the terminus during these 3 years.

4. One serious deficiency in this paper is the lack of recognition of previous work. They do cite Nolan et al (1995) and Post and Motyka (1995) but seem unaware of more recent papers relevant to Taku Glacier. These include:

Larsen, CF, RJ Motyka, AA Arendt, KA Echelmeyer, and PE Geissler, 2007. Glacier changes in southeast Alaska and northern British Columbia and contribution to sea level rise. *J. Geophys.Res., Earth Surface*. 112, F01007, doi:10.1029/2006JF000586.

Motyka R. J., M. Truffer, E. M. Kuriger, A. K. Bucki, 2006., Rapid erosion of soft sediments by tidewater glacier advance: Taku Glacier, Alaska, USA, *Geophys. Res. Lett.*, 33, L24504, doi:10.1029/2006GL028467.

Kuriger, EM, M Truffer, RJ Motyka, and AK Bucki. 2006. Episodic reactivation of large scale push moraines in front of the advancing Taku Glacier, Alaska. *J. Geoph. Res.*, 111(F1), doi:10.1029/2005JF000385.

Motyka, R.J. and K.A. Echelmeyer, (2003) Taku Glacier on the move again: Active deformation of proglacial sediments. *J. Glaciol.*, 10(164), 50-59.

Arendt. A. A., K.A. Echelmeyer, W.D. Harrison, C.S. Lingle, and V.B. Valentine (2002)

Rapid wastage of Alaskan glaciers and their contribution to rising sea level, *Science*, 297(5580), 382-386.

I admit I am biased since I am author or co-author on almost all of these papers. However, the lack of citing Motyka and Echelmeyer (2003) seems particularly egregious since this paper contains results of laser profiling of Taku Glacier, which in most part agrees with the long term mass balance results of this paper! The Larsen et al (2007) paper also helps verify the long-term thickening of Taku Glacier during the last half of the 20th century. Motyka et al (2006) cautions about using surface elevation measurements as indicators of mass balance at Taku because of the high rates of erosion in the terminus region. All these papers have some bearing on Pelto et al's paper and I think that they should at least make some attempt to interpret their results in light of these previous findings.

5. Also vexing is not citing:

Motyka, R.J., and J.E. Beget, 1996: Taku Glacier, southeast Alaska, U.S.A.: Late Holocene history of a tidewater glacier. *Arctic and Alpine Research*, v. 28, no. 1, pp. 42-51.

The tidewater cycle is cited as the reason for Taku's high AAR in this paper as well as in Post and Motyka, 1995. In fact, although the authors recognize that Taku has an anomalously high AAR, they do not provide any discussion as to why.

6. There also appears to some confusion about citing data on the rate of the glacier's advance as coming from Post and Motyka (1995) when in fact this data was published in:

Motyka, R. J., and Post, Austin, 1995: Taku Glacier: influence of sedimentation, accumulation to total area ratio, and channel geometry on the advance of a fiord-type glacier. In Engstrom, D. R. (ed.), *Proceedings of the Third Glacier Bay Science Symposium, September 14-18, 1993*, National Park Service, Anchorage, pp 38-45.

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7. The authors report that they have monitored the elevation of the ELA on an annual basis. It would be great addition to their paper if they would include either a graph or at least a range of values for these observations. They quote 925 m as the ELA but do not state what this value is based on.

8. The second part of the paper discusses the “equilibrium balance of flow”. The authors should at least note that this was also done in Nolan et al, 1995 and we reached essentially identical results.

9. In summary, I would like to see this paper published but only after the above mentioned deficiencies are addressed.

Specific Comments:

Title: Should switch Mass balance and equilibrium flow

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Abstract:

L 7: Why not just say annual velocities?

L 14 Centerline velocity transects to terminus?

L 18: “describes’??

L 19: ELA at 925 m? How determined?

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L 5-6: Confusing the references: it should be Motyka and Post (1995)

L 10: Conjectured by whom? Reference?

L 10: Why does this glacier have such a high AAR?

L 14-15: Isn't this conclusion premature, anticipating the results of this paper?

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L 19: Confused by the Miller & Pelto 1999 reference. The title implies the reference refers to Lemon Creek Glacier, not Taku.

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SWE vs w.e. SWE defined. W.e. is not..

Snow pits mostly below 1400 m?? But this misses zone 3, the higher accumulation zone!

L 14: Does someone come out at end of summer to measure stakes? Or how is it done?

L 24 - It would be great to either have graph or table of annual “ELA” values. Or at least state what the variation is.

L 28 - 29: Not only ablation zone but also above 1400 m?

Also, we offered our ablation data to Pelto

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1st para: review this again and make sure it is correct. Implicitly assumes densification same from year to year?

L 11: “of” to “or”

L 20 – 21: GPS uncertainties? Particularly for kinematic profiles.

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L 14 – 15: Mixing results with methods

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L 8 – 11: These lines read more like a report than a journal paper. Discussion on what should be done in future programs doesn't belong here; maybe in the summary and conclusions?

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L 16 – 18: A comparison to results published in Nolan et al, 1995 is in order here. (the same result within error limits).

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L 13: superscript

P 287

L 3 – 5: These results should be compared to Nolan et al 1995, Table 2: the results are the same.

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L 27: Citation should be Nolan, *Motyka*, Echelmeyer, and Trabant.

Interactive comment on The Cryosphere Discuss., 2, 275, 2008.

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