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The Cryosphere Discussions

Interactive comment on "Improving estimation of glacier volume change: a GLIMS case study of Bering Glacier System, Alaska" *by* M. J. Beedle et al.

M. J. Beedle et al.

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Author's response to requested major changes to: "Improving estimation of glacier volume change: a GLIMS case study of Bering Glacier System, Alaska"

Matthew J. Beedle

February 28, 2008

The following document includes changes suggested by the editor, the three referees and a short comment. These suggested changes are written in quotes and a description of the subsequent revisions to the manuscript or substantiation of our position on the suggestion follows. Detailed suggestions regarding changes to grammar, word

choice and punctuation were all changed as per the reviewers suggestions, but are not listed here.

1. Editor's requested changes:

"Please give due consideration to [A. Arendt's] idea which is to select a fixed glacier surface and test the net mass balance sensitivity to a variety of glacier outline definitions, in favor of your more-complex approach."

MJB: This was a very important recommendation and a significant effort was made in revising the study in this regard. The revised manuscript uses one glacier surface (a single DEM) and uses this to test four glacier outline definitions. One of the four glacier outlines was supplied by Anthony Arendt and the other three were created to reflect various ways of defining the Bering Glacier System complex. This revision was the most substantive change and slightly altered the results of the study, but the general implications and conclusions remain the same.

"It is necessary early in the paper to define terms such as outline and hypsometry."

MJB: We have made a considerable effort to define the terms outline and hypsometry when they are first used and to reduce the ambiguity of their use within the text. The previous version of the manuscript used these two terms in confusing and sometimes inappropriate locations. We feel that this confusion has been remedied in the revised manuscript.

"[Make] the paper more readable and more concise . . . please take care to shorten the paper considerably"

MJB: Combining the data and methods sections has eliminated the redundancy in the initial manuscript. This combination and further revisions has reduced the total length of the manuscript by 7 pages or nearly 2,000 words.

"Combine tables 3, 4, and 5 into one table . . . [and] refer to the tables more."

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MJB: Tables 3, 4 and 5 were combined into one table and use of numbers in the text was reduced.

2. Referee (A. Arendt) requested changes:

"Choose a mass balance parameterization and define a fixed glacier surface, and test the net mass balance sensitivity to a variety of glacier out- line definitions. Here the authors choose a more complex approach . . . "

MJB: This requested change was carried out for the revised manuscript. See reply to the first requested change from the editor above.

"Make the distinction between the terms "outline" and "hypsometry" more clear. An outline is a minimum amount of information and is sufficient for many studies that extrapolate net mass balances as a function of total area. Describe how the surface area distribution is crucial when measurements or models provide vertical changes as a function of elevation. Bring out the point that in this paper you are investigating the more complex issue of hypsometry rather than just the area outlines."

MJB: See comments above regarding use of the terms "outline" and "hypsometry".

"Be clear on the dates and sources of your hypsometries, in particular the under- lying DEMs used to obtain the surface elevations."

MJB: This problem was remedied with the use of a common DEM to create hypsometries. The previously published hypsometries used in the previous version of the manuscript (TCD), which lacked a great deal of important meta data are not used in the revised manuscript.

"Be explicit as to why the modeling is introduced as the only method for generating the vertical component of mass change. Justify why, for example, differencing of DEMs or surface elevation profiles were not used instead."

MJB: The comment that "the modeling is introduced as the only method for generating

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the vertical component of mass change" is confusing. We do not introduce modeling as the only method, it is just the only method we employ. Many modeling studies rely heavily on accurate mapping of glacier outlines, shape, hypsometry etc. In the introduction of the revised manuscript we briefly mention both differencing of DEMs and modeling as methods used to broaden our understanding of glacier change beyond the small subset of glaciers with detailed, annual field studies. Many previous studies (e.g., Arendt et al., 2002; Muskett et al., 2003; Arendt et al., 2006; and Larsen et al., 2007) have used differencing of DEMs and/or laser altimetry to assess glacier surface height change in southern and southeast Alaska. As we show, there are many facets of measuring or modeling glacier change that are not fully understood or accounted for. Using models in our study serves to shed new light on some of these pitfalls. We do not wish to suggest that modeling is superior to differencing of DEMs or laser altimetry.

"Highlight the dynamic nature of surface area and hypsometry. Your model sim- ulations cover a 54 year period but make no mention of the feedback effects of changing hypsometry on the predicted balance."

MJB: This is a very good point. A discussion of the potential errors resulting from the use of a static glacier surface has been included in the revised manuscript.

"p.170, line 12: here and throughout, the term "outline" is used when really you are testing different hypsometries. This is because not only the outline but the DEMs to construct the elevation distribution are different for the different tests."

MJB: See comments above regarding use of the terms "outline" and "hypsometry". In the revised manuscript only one DEM is used to construct hypsometries, so the different hypsometries are completely due to differences in glacier outline. We feel that this has significantly clarified the revised manuscript.

"p.172: I suggest a more complete introduction here, before beginning to describe the BGS (Section 1.1). The material in Section 1.2 should come first, in which you detail what methods will be used to investigate the research questions."

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MJB: Sections 1.1 and 1.2 have been switched and minor revisions have been made for transitioning between these sections. We address the suggestion of "a more complete introduction" in the context of the more specific suggestions (e.g. re. Dyurgerov and Cogley work on benchmark glaciers, above).

"p.175, Section 2.1, Hypsometries: this section is incomplete because it only describes the glacier's surface area and elevation ranges. The distribution of area with elevation (hypsometr y) depends both on the glacier outline and the map or DEM used to describe the surface at a given time. Therefore it is impor tant that this section include information on the date at which the surface elevation distribution was obtained, as well as the source providing the elevations for each hypsometr y. The AH elevations come from 1972 USGS topographic maps, the T1H and T2H elevation dates/sources are not known, and the BH elevations come from a composite ASTER DEM (described later on page 179), with an unknown date (but sometime after 2000). This range of dates needs to be highlighted because it means that comparisons later in the paper are investigating the effects of different outlines as well as different area distributions on the calculated mass balance. Can you say something about how that will affect your comparisons?"

MJB: These suggestions are no long applicable as these issues have been avoided by the use of a common glacier surface to create four glacier hypsometries. See comments above.

"p.176, Section 2.2, Mass Balance Models: I suggest putting a complete description of the models here, rather than splitting between a "Data" and "Methods" section. The current layout is not consistent with your headings. For example, Section 3.4.1 in the Methods section describes data used to drive the PTAA model, which should appear under "Data" Reorganized data and methods sections."

MJB: This was a very good suggestion. We combined the data and methods sections, and worked to clarify this discussion, which reduces redundancy, confusion and

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manuscript length.

"p.176, line 20: Why generate the ELA from the PTAA model? This should at least be confirmed by examining the location of the ELA on your Landsat images."

MJB: Daily transient snow lines (TSL) as modeled by PTAA were compared with observed TSL from the two Landsat scenes used in this study. PTAA estimates of TSL may be too high by between 150 and 300 m. A discussion of this is included in the revised manuscript in the discussion section.

"p.178, Debris cover : I thought it was a GLIMS standard to outline debris cover separately from clean ice. Then the user can choose an outline based on their particular needs. Is this correct?"

MJB: This is incorrect. Outlines for "glacier" and "debris-cover" are stored separately in the GLIMS database, but "glacier" includes (or should include) everything that is glaciologically considered to be glacier - including the debris-cover. So the debris-cover polygons completely overlap the glacier polygon.

"p.179, line 3: What specific aspects of the image indicate mature versus other types of karst?"

MJB: We feel this is discussed sufficiently on previous page especially with reference to Fig. 5.

"p.179, line 10-12: "Previous work..." What is the reason for mentioning automated techniques? Did you test these on BGS?"

MJB: This was added to the text to set the present study in the context of previous work that has been conducted on the accuracy of glacier outlines, and to explain a rationale for using a manual outline here.

"p.181, PTAA model: If I understand this model correctly, nearby weather station data are used to drive a regression-based mass balance model for a specific glacier hyp-

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sometry. Which hypsometry did you use (date/outline/DEM)? Discuss how changing surface geometry during 54 years would affect the model results."

MJB: The model uses the Surging Bering Glacier System hypsometry discussed in the manuscript. We note this and address the effect of an unchanging glacier surface in the revised manuscript.

"p.181, line 7: Your use of the term "mass balance gradient" is confusing. The model produces an estimate of the balance as a function of elevation. The derivative of this function is the mass balance gradient. Figure 8, right chart, is a plot of balance versus elevation, not the balance gradient. If you wish to use this terminology, be sure to define it clearly."

MJB: Unfortunately such terminology is used differently in many publications (e.g., Mayo et al., 1972; Ostrem and Brugman, 1991), and you are correct that we should define this terminology clearly. The plot (now figure 10) you refer to is of average rates of change of mass-balance for the period 1950-2004 for every 50 m elevation bin. We have tried to clarify the use of this terminology in the text by including a brief definition when the 'mass-balance gradient' is first used in the manuscript. "The rates at which annual ablation and accumulation change with altitude are termed the ablation gradient and the accumulation gradient, respectively. Taken together, they define the mass balance gradient." (Benn and Evans, 1998, p. 77)

"p.181, line 14-16: This sentence does not make sense to me. Be specific with your use of the term "mass balance". Hypsometry determines a glacier's net mass balance, but mass balance at a point depends only on the climate. One could easily say mass balance (climate) exerts a control on the hypsometry."

MJB: This sentence is paraphrased from Tangborn (1999) where "the reason why these regressions generate mass balance results that appear physically real" is discussed. Yes, mass balance at a point depends only on the climate, but the elevation at the point significantly impacts climate. We have reworded this sentence to try and reduce

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confusion.

"p.182, Section 3.5: Remove this section, and just reference these results in the discussion when comparing to other measurements. I do not see how it is possible to compare an SLE value with a mass balance gradient?" MJB: This section has been removed in the revised manuscript.

"p 188, line 9-15: List the dates of surges occurring within the period of your measurements. The 1993-95 surge occurs between the 1972 AH and the $>$ 2000 BH and could explain differences."

MJB: Dates of recent surging (after 1950) have been added and are discussed in the revised manuscript.

"p.189, line 12: "average" bn , in what sense? Following standard mass balance terminology (see Mayo et al, J.Glac. 1972), bn means area-averaged net balance. By "average bn " do you mean time-averaged?"

MJB: Yes, we mean time-averaged. We have added reference to the temporal period (1950-2004) to try and eliminate this confusion.

"p.193-194: Implications Section: Your models are calibrated to climate data from 1950- 2004. You use this to obtain a time-averaged balance versus elevation curve. You then apply that rate to the 1972-2000 period to compare with laser altimetry. This is not a very robust comparison. It would be much better to calibrate the model to 1972-2000 data. One compelling reason is that 2004 had record high summer temperatures across Alaska and could be biasing your model results toward negative values. Also, you integrate the PTAA "balance gradient" against the BH hypsometry, representing a glacier surface that is considerably lower than the AH 1972 elevations. Thus BH samples a more negative climate and your comparison is not entirely valid. It should be reasonably easy to quantify the effects of changing surface geometry on your estimates, while being clear that your models do not account for the feedbacks between

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climate and glacier dynamics."

MJB: This was another very good observation. We have run the PTAA models for the period 1972-2000 with the 1972 elevations and these results are discussed in the implications section of the revised manuscript.

"Your models do not produce estimates that are precise to 3 or 4 significant figures. Recent results using GRACE agree closely with 1995-2000 estimates derived from aircraft laser altimetry. Be clear that your results suggest the "early period" laser altimetry assessment underestimated Alaska glacier mass losses."

MJB: Significant figures have been changed. We have made note that our results (1972-2000) largely differ during the 'early period' and are actually quite similar during the period 1995-2000. See 'Implications' section.

"If the authors wish to publish a new mass balance value for this glacier, one that would potentially be included in future mass balance inventories, then I would like to see a more rigorous treatment of the modeling effort. At a minimum, it would be necessar y to at least mention the potential effects of changes in glacier hypsometry, and surge dynamics, on the model results. In addition to the comparisons with the laser altimetry measurements on this glacier, the model results should be validated against measurements by Muskett and others (GRL, 2003, Vol 30), who have calculated geodetic balances for BGS by differencing a series of DEMs."

MJB: Very valid point. Our primary purpose is to illustrate the importance of using accurate glacier outlines and hypsometries. We include comparisons with previous results (Arendt et al., 2002) as it would be incomplete not to do so, and the differences and similarities afford a valuable discussion of the shortcomings of mapping, modeling and measurements - shortcomings that we argue need to be addressed. We have significantly reworded the implications and conclusions sections to illustrate the shortcomings in various methods, but have tried to suggest that our modeled results should not be a replacement of previous measurements. Our aim is to point out pitfalls in cur-

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rent mapping, measurements and modeling of large, complex glaciers so that we can move towards a more accurate understanding of how these glaciers are changing.

Muskett et al. (2003) address surface height change on Malaspina Glacier on portions of the upper Bering Glacier System. As only a small portion of BGS is addressed in this study we do not feel it is appropriate to compare the geodetic balances - such a comparison would be comparing very different glacier areas. Muskett et al. (2003) use the same PTAA model to derive accumulation to correct for time of imaging and thus these figures (Muskett et al. Fig. 2) are very similar/identical to our PTAA generated mass-balance.

3. Referee (Anonymous) requested changes:

"Equal prominence should be given in the paper to what Bering Glacier has been doing, compared with what is given to the analysis methodology."

MJB: I believe this is meant to say: " . . . compared with what is given in the analysis methodology."? I'm assuming the referee is referring to SBGS surge dynamics and dramatic retreat. If this is the case we have discussed some of this in the revised manuscript.

"The term "template method" (182,2) was not used in the cited reference (Dyurgerov, 1996), so the authors ought to abandon it. A more readily intelligible term would be the bn (AAR) method. Moreover, writing bn /AAR (182,9 and 193,9-12) is very misleading because that means the ratio."

MJB: Correct, the term 'template method' was not used in Dyurgerov, 1996. The method was further described and termed 'template method' in Khalsa et al., 2004, which is included as a reference. We feel that the term 'template method' is appropriate and while not descriptive (such as 'bn (AAR) method'), we feel this term is less likely to be confused with other acronyms and glacier measurements. We have changed bn/AAR - yes this was misleading.

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"The paper is long and in places repetitive. Some sections would be more readable if they cited Tables 3-5 (which should be combined to promote perspicuity) instead of repeating so many numerical values in the text."

MJB: Good suggestions. Tables 3-5 have been combined and the paper has been shortened significantly.

"How the bn (AAR) method was applied to the hypsometry (182-9) needs to be explained. If a bn (z) was obtained and integrated over the hypsometry, it should be shown. The idea in Dyurgerov (1996) is that bn (t) could be found from AAR(t), but here the authors presumably used a 1950-2004 mean AAR to get a mean bn (z). Lack of an explicit description of this, one of the three models considered, is a major omission."

MJB: A more specific explanation of the template method has been included in the revised manuscript. In short, we did use a 1950-2004 mean AAR to get mean mass-balance, based upon the mass-balance and AAR relationship at Gulkana Glacier.

"Although it is not very clear on the point, the paper seems to imply (193,3-6) that the map-view shape of a glacier determines its area-altitude distribution. This is not true, unless an additional assumption is made concerning the slope of the glacier surface. Specifying a region on a map does not determine the topography within it."

MJB: Yes, the slope of the glacier surface will have an impact on the measurement of the surface area. Hypsometries were done using the "mapped" area of a glacier, or the surface area projected to a horizontal surface. The section of the manuscript referred to (p. 193, lines 3-6) is discussing the shape of a glacier, or the distribution of area with elevation, i.e. does the brunt of the glacier area sit at higher elevations or lower elevations. This discussion does not discuss slope angle and the influences of slope.

"(183,9) It should be said where the 5173 km2 value was published, perhaps by Molnia and Post (1995) as (186,15) says."

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MJB: Reference to the US Board on Geographic Names and Molnia and Post (1995) has been made at first use of the 5,173 km2 area.

(191,2) It should be clarified whether the discrepancy refers to the entire bn (z) profile (which is published in USGS reports for many individual years) or to its integral over the glacier hypsometry.

MJB: This is a good point. The discrepancy refers to cumulative bn. (Fig. 15 in Tangborn, 1999). This has been clarified in the revised manuscript.

(192,9) Not only is outlining the region of debris cover necessary to determining ablation rates, but so is knowing its thickness.

MJB: We are fully aware of the importance of thickness, and we discuss debris-cover thickness at length.

(199) Should Quintino Glacier also be in first section of Table 1? The section "Portion of Bering ..." appears to be incomplete.

MJB: A good point. Table 1 presents a portion of the confusing component parts that make up Bering Glacier System. It is our opinion that the bulk of this confusion lies in the multitude of differently named features in the lower elevations of BGS. Thus, some glacier names (e.g., Quintino Sella and Columbus Glaciers) are left off of this table for the sake of brevity.

4. Referee (R. Williams) requested changes:

"The "already-published paper" was written by 8 authors; as a result, the manuscript reads like one written by a committee. It is ver y difficult to keep track of all of the acronyms and abbreviations per taining to the various studies of the Bering Glacier System (BGS), the Surging Bering Glacier System (SBGS), different approaches to hypsometr y, and what components actually comprise the BGS and SBGS, not to mention the accumulation area, ablation area, and ELA under each analysis."

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MJB: Significant effort was made to clarify and simplify the acronyms and abbreviations used in the revised manuscript.

"In addition to the hard-to-read text, it is obvious that the paper was written rather hurriedly and was not technically reviewed before submission to The Cr yosphere Discussions. For example, some citation dates in the text are different from those in the References. I could not find citations to two papers listed in the References. Before "republication" in The Cr yoshere journal, the authors need to make corrections to their ms. I leave it up to the editor to determine if the authors should endeavor to make the text more readable and lucid. However, the problems identified in my technical review need to be corrected before publication in The Cr yosphere."

MJB: Citation errors have been remedied. The revised manuscript is significantly shorter and concerted efforts have been made to improve lucidity and readability. All suggested changes provided in the 'technical review' have been made.

5. Short comment from M. Pelto:

MJB: We would like to thank Mauri Pelto for helpful comments and suggestions. Many of these were mentioned by the three referees and are addressed above. We have included demarcation on a figure and additional discussion of the PTAA modeled 1,500 m average ELA as per Mauri Pelto's suggestion.

Interactive comment on The Cryosphere Discuss., 1, 169, 2007.