

Interactive comment on “Reconstructing the glacier contribution to sea-level rise back to 1850” by J. Oerlemans et al.

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General Comments

The title of this paper is self-explanatory. The authors rely on a collection, extending back as far as the 16th century, of time series of changes in glacier length. They calibrate these series against a dataset of annual measurements of glacier mass balance made since 1960. Smoothing is required in both time and space to reduce irregularities in the raw single-glacier information. They convert normalized length to normalized mass balance with a minimal but plausible model having a single estimated parameter. Tuning means that the model agrees with the mass-balance observations in reproducing a rise in sea level of about 1.3–1.5 cm between 1960 and 2000. The reconstruction before 1960, which is not very sensitive to the fitted parameter, suggests a minimum in

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sea level at about 1850, some 4-5 cm below that of 1960. The implied rates of sea-level rise due to glacier mass balance, in half-century steps from 1850, are about 0.25, 0.50 and 0.40 mm a⁻¹; although the authors do not stress it, a moderate fall of sea level is implied for the 100-150 years before 1850.

The paper reports in a professional way on work which has been done competently. It advances our understanding by synthesizing two existing datasets and suggesting a quantitative estimate of the glacier contribution to sea-level rise over a period before the period of direct mass-balance measurements. On the other hand, it provokes several questions about the appropriateness of the procedures and the accuracy of the results. These questions, however, might be seen as a strength because they point the way towards further work. I recommend publication in *The Cryosphere* after consideration of the more detailed and technical comments offered below.

Detailed Comments

P82 line 23 Meren Glacier no longer exists (Kincaid, J.L., and A.G. Klein, 2004, Retreat of the Irian Jaya glaciers from 2000 to 2002 as measured from IKONOS satellite images, *Eastern Snow Conference Proceedings*, **61**, 147-157), and in Table 1 both Irian Jaya and Africa are given weights of zero. Perhaps, then, the remarks in this paragraph are not very important, but they do suggest some concerns about the way in which dL/dt might vary with glacier size. If the “law” governing this rate is absolute (under given forcing dL/dt tends to a constant for all glaciers in m a⁻¹) you get very different behaviour near the end of a shrinking glacier's lifetime from what you get if the law is relative (normalized dL/dt tends to a constant in a⁻¹); and a relative law will yield different late-stage behaviour depending on whether you normalize by the previous year's L or by a reference L chosen when the glacier was much bigger. I think that the authors have not yet considered a possible size dependence of dL/dt , and the distribution of glacier sizes in the length dataset, in sufficient detail.

P83 para 2 The 14 regions contain only about half of the ice outside the ice sheets.

(The 63% deductible from Table 1 includes the Subantarctic, but excludes all of the Antarctic islands.) On the whole the authors do an adequate job of acknowledging this unavoidable weakness, but they might wish to point out that the glaciers in the data voids are quite different from those in the data-rich regions (probably longer, and certainly colder, on average).

P84 para 1 Larger glaciers have length fluctuations of greater amplitude because they are flatter: this presumption sounds plausible on first reading, but on reflection I am quite sceptical. Surely they should be *less* sensitive to climatic change? They integrate the climatic signal over a greater horizontal extent and, perhaps more importantly, over a greater vertical extent (ranging further above and below the changing equilibrium line). I do not know why the larger glaciers tend to show larger fluctuations. But then I do not know what kind of law governs the dependence of dL/dt on glacier size (see comment on P82 I23 above).

A related concern is that the authors assume that measurements of L are not problematic. Perhaps L is underestimated in measurements on longer glaciers? The simplest estimate of L is obtained by finding the point on the glacier boundary most distant from the terminus (assuming that the latter is readily identifiable), but this will be consistently shorter than the most complex estimate (an average or median of many flowline lengths measured with a horizontal resolution comparable to the ice thickness). Presumably the authors' collection of glacier lengths is heterogeneous in this respect, probably with a bias towards lengths which are too short, increasingly so for longer glaciers with more scope for winding and bending. (For White Glacier in the Canadian Arctic the simple estimate is 15 km and the complex estimate is 21-22 km.)

How many calving glaciers are there in the length dataset, and what if any special treatment were they given?

P86 para 3 Correlation of S_{DM} and V_{14}^* : it would be safer to differentiate first, i.e. to correlate annual values dS_{DM}/dt with dV_{14}^*/dt .

The assumption that length and mass balance are in phase, discussed by the authors in the preceding paragraph, deserves more attention in future studies. The evidence for the approximate validity of the assumption is quite strong, but according to conventional wisdom (length changes should lag the mass-balance forcing by decades or more) it ought not to be. As far as I know, nobody has made a serious attempt to resolve this paradox.

P87 para 6 It is true that the global representativeness of the mass-balance compilations is less than adequate, but it does not follow that this is the “most critical aspect”. In fact the spatial coverage in the calibration period, 1961-2000, is better for mass balance than for length changes. And from equation 4 the formal error in the estimate of V_{14}^* is linearly proportional to the error in L_{14}^* , with a multiplier containing the parameter η , which could lie between 1.4 and 2.5.

The authors should try to say something about the error in the estimated date, 1850, of the sea-level minimum, because this may be the most valuable outcome of their work. The amplitude of the total change since that minimum will remain in doubt for some time to come. It may be easier for other workers to show that the date of the minimum is, or is not, consistent with other palaeoclimatic indicators. To judge by Figure 7 the dating error might be of the order of ± 20 years, but the reader needs to know something about sensitivity to modelling assumptions, etc.

Table 1 Say explicitly how the regional weights were obtained. They seem to be slightly greater than $(\text{Area} + \text{Addition}) / (\text{Total Area} + \text{Total Addition})$. Give the denominator (619840) explicitly in the caption.

Technical Comments

General The authors speak consistently of volume when they mean (or should mean) mass. The reader has to assume that they have not made the elementary mistake of failing to correct for the different densities of ice and water.

Abstract

P78 line 4 Perhaps “continuous” would be a better adjective than “systematic”.

1 Introduction

P80 line 3 Perhaps say “the only source of observational information”.

P80 line 11 “longer”, not “larger”.

2 Data

P81 line 8 “Kamchatka” (only one t).

P81 line 10 “where”, not “were”.

3 Stacked length records for regions

P82 line 23 “outlier”, not “outlyer”.

4 The global signal

P83 line 6 “weighing” should be “weighting” here and at several places later in the text and the caption of Table 1.

P83 line 7 “relative effect”.

P83 line 24 “known”, not “know”.

P84 line 11 “the behaviour of glaciers”.

5 Towards a proxy for glacier volume

P84 line 24 “glaciers”.

P86 line 10 “more quickly”; “quicker” is an adjective, not an adverb.

P86 line 15 “detailed studies support”.

7 Discussion

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P87 line 12 “Several tests . . . the results”.

References

P88 line 8 Do not capitalize “glaciers” (Arendt et al.)

P89 line 3 Leysinger Vieli’s initials are “G.J-M.C.”.

P89 line 15 Do not capitalize “global sea level rise” (Rignot et al.).

Table 1

Region 4 Does “west Arctic islands” mean “Russian Arctic islands”?

Figure Captions

P92 “The number . . . is given” (last line).

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TCD

1, S25–S30, 2007

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