SIR,

Comments on "Mass balance of glaciers other than the ice sheets" by Cogley and Adams

Cogley and Adams (1998; hereafter CA) make an attempt to estimate the contribution of glacier melt to global sea-level change. They do this by means of a statistical analysis of mass-balance observations and make some firm statements, most notably that "glaciers in general [made] little or no contribution to sea-level change during 1961–90". This is in conflict with the recent studies of Dyurgerov and Meier (1997; hereafter DM) and Zuo and Oerlemans (1997; hereafter ZO) who report values of about 8 mm for this period (Fig. 1).

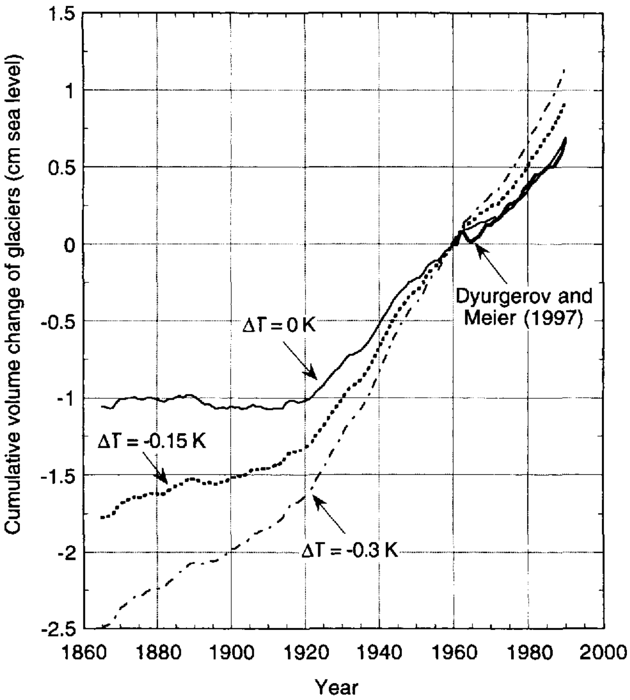

Fig. 1. Volume change of small glaciers and ice caps calculated by Zuo and Oerlemans (1997) for the period 1865–1990. The cumulative change is shown as sea-level equivalent with reference to the year 1960. Results are given for three different initial conditions (ΔT), expressed as the global mean temperature for which the average 1865–95 glacier volume would be in equilibrium minus the actual global mean temperature for 1865–95. The solid line shows the estimate of Dyurgerov and Meier (1997).

In the debate about anthropogenically induced climate change, sea-level rise is one of the hot issues. Glacier melt is generally assumed to make a significant contribution to sea-level rise and it is important to resolve the discrepancy between CA's conclusion and the earlier studies mentioned.

CA base their conclusion on a statistical analysis of the mean specific balance of glaciers for which observations exist. To arrive at a global number, they use the glacier size distribution of the inventory from the World Glacier Monitoring Service, Zürich. Although this certainly has its limitations, it may provide a reasonable first estimate of the size distribution.

Probably more serious is the bias in the distribution of mass-balance observations over glacier size classes. Considering figure 7 in CA, one can indeed see that the larger glaciers have a less negative balance in the period considered. A problem here is that many of the larger glaciers for which mass-balance observations exist are in Norway. They are located in the same geographic region, and most have a positive or only slightly negative balance. I have made a detailed study of the balance of these glaciers, including inspection of local meteorological records, and it is clear that excessive precipitation during the last 10–15 years is the most important factor here. Although CA's analysis includes some observations from larger glaciers in other parts of the world, the bias appears too large for the measured balance to be taken as representative. In my view the lack of regional differentiation and proper weighting of mass-balance observations in CA's analysis leads to too large error bars.

Both DM and ZO, although following quite different approaches, recognise the need for *regional differentiation.* DM make a careful compilation and extrapolation for mass-balance observations from the period 1961–90. ZO use sensitivities derived from calibrated mass-balance models as the basis for their calculations. By using observed temperature records they show that it is important to include regional and seasonal resolution. The estimates of ice wastage made in these studies are totally independent, but yield remarkably similar results for the period 1961–90 (Fig. 1). In fact, Figure 1 lends some credibility to the calculation for the entire period 1865–1990.

I also want to comment on CA's suggestion that larger glaciers are significantly less sensitive to temperature change. The temperature sensitivity of mass balance as a function of glacier size cannot be determined by comparing a *hemispheric mean* temperature signal with unevenly distributed mass-balance measurements. As noted above, many Norwegian glaciers now have a positive mass balance. Analysis shows that this is due to large amounts of precipitation, and that average temperature did not change very much. So including these mass-balance measurements in an estimate of temperature sensitivity of glaciers again creates a strong bias. I therefore cannot agree with CA's conclusion that larger glaciers are less sensitive to thermal forcing.

There are other arguments against a strong dependence of mass-balance sensitivity on glacier size. In recent years a number of glacio-meteorological experiments have been carried out on *small and large* glaciers to obtain a better understanding of the relation between glacier mass balance and meteorological conditions. I mention experiments in central West Greenland (two summers; three meteorological stations along a 60 km transect across the entire melt zone; e.g. Oerlemans and Vugts, 1993), on the Pasterze, Austria (one summer; five stations distributed on a glacier about 10 km long; e.g. Greuell and others, 1994, 1997), and on Vatnajökull, Iceland (8000 km^2; 12 meteorological stations covering the entire ice cap; Oerlemans and others, 1999). Together with the operation of automatic weather stations for longer periods of time, these have provided a wealth of data on the melt process without a clear bias towards small or large, dry or wet. The process of analyzing data and subsequent improvement of mass-balance models is still in full swing. A lot of work has already been done, however, and one can say that differences in climate sensitivity for glaciers of different size, inferred by CA, are not supported. The modelling and observational studies make clear that it is mainly the precipitation regime, not the size, that determines the sensitivity: wet glaciers are sensitive, dry glaciers not so sensitive.

Historic glacier fluctuations provide another argument against the idea that larger glaciers are much less sensitive

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to thermal forcing. One cannot say that larger glaciers showed little retreat compared to small ones. With proper correction for geometric factors, or better, explicit simulation with calibrated ice-flow models, it requires a certain mass-balance sensitivity to simulate the post-Little Ice Age retreat from observed meteorological records. Sensitivities thus found are in good agreement with the results from the meteorological fieldwork and mass-balance modelling referred to above. For further background the reader is referred to Oerlemans and others (1998) and references therein.

Altogether, I do not agree with the main conclusion of CA that glaciers have contributed little to sea-level rise in the light of an alleged dependence of mass-balance sensitivity on glacier size. On the contrary, the good agreement between the independent estimates of DM and ZO makes it very likely that glaciers made a significant contribution.

Institute for Marine and Atmospheric Research, Utrecht University, Princetonplein 5, Utrecht 3584 CC, The Netherlands

J. OERLEMANS

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SIR,

Reply to the comments of J. Oerlemans on "Mass balance of glaciers other than the ice sheets" by Cogley and Adams

Our statements (Cogley and Adams, 1998; CA), challenged by Oerlemans, were not "firm" but tentative. We were careful to qualify our main conclusion at several places. Given the extreme under-sampling of mass balance, it would not be surprising if our conclusion were shown to be in error, but we do not think that Oerlemans has done so.

Oerlemans notes that many of the larger measured glaciers are in Norway. If, following CA, we define "larger" as "larger than 16 km^2", then the nine larger glaciers from Norway, occupying the 16–32 and 32–64 km^2 size classes, contribute 68 of the 440 annual balance measurements made on larger glaciers. If they and other Norwegian glaciers are excluded, we find that the Norwegian glaciers do indeed shift our results in the direction argued by Oerlemans. However, these glaciers are not near to the peak of the frequency distribution of observed sizes (fig. 7b of CA), and CA's size-corrected estimate of the small-glacier contribution to sea-level rise, 0.058 mm a^{-1}, is revised only to 0.066 mm a^{-1} when they are excluded.

More seriously, we see no reason why Norwegian glaciers should be given special attention. The fact that many of the larger Norwegian glaciers have positive mass balance is not really relevant. What would be relevant would be a demonstration that Norwegian glaciers are so globally atypical as to make the available sample unrepresentative of the world's small glaciers. We do not think that this can be done. However, we agree entirely with Oerlemans on the need for regional differentiation. Indeed we took some trouble to evaluate spatial bias, and our paper contains an estimate of its magnitude: about −60 mm a^{-1}, or +0.10 mm a^{-1} of equivalent sea-level rise.

Our results are not in conflict with those of Dyurgerov and Meier (1997; DM). The cumulative data of DM shown in Oerlemans' figure 1 are for practical purposes identical with those shown as annual averages in figure 5a of CA. It follows that the latter are not in conflict with the modelling results of Zuo and Oerlemans (1997; ZO). In fact, Oerlemans' figure 1 reveals that the best agreement between ZO and DM (and hence CA) is for $\Delta T \simeq 0$ K. A reasonable interpretation of this agreement is that (i) ZO's model suggests that small glaciers were in equilibrium during 1865–95, while (ii) the measurements of DM and CA suggest that, when biases are allowed for, small glaciers were close to equilibrium during 1961–90. To assimilate this latter claim, the reader should mentally differentiate the curves in Oerlemans' figure 1, and should note that ZO's model *assumes* a (calibrated) dependence of balance on temperature, while the DM and CA data are complementary in that they *demonstrate* such a dependence.

CA's conclusions, restated succinctly, are as follows. Firstly, a naive calculation yields a moderately negative estimate of global average mass balance. Secondly, this estimate must be revised upwards because at least three biases distort the result: (a) neglect of internal accumulation, (b) the spatially uneven distribution of the measured glaciers, and (c) the size bias identified in CA's figure 7. Oerlemans does not address bias a, the significance of which is emphasized by results reported recently by Bazhev and others (1998) and Rabus and Echelmeyer (1998); he may not have understood that we had already addressed bias b; and we show above that his comments on bias c do not affect our conclusion. Thirdly, it is not practical to correct all of these biases at once, because they are not additive and are probably correlated. The extent of overlap needs to be determined carefully, which will require a substantial effort. Fourthly, CA's analysis, when taken *as a whole*, entails the conclusion that small glaciers were probably close to equilibrium during 1961–90.

Our empirical demonstration of the size bias warrants a practical response in the medium-term disposition of measurement effort, but its physical causes also deserve study. In this regard we accept Oerlemans' argument that sensitivity to precipitation should be examined as well as sensitivity to temperature. His earlier work (e.g. Oerlemans and Fortuin,

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