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.

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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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1992) certainly shows that precipitation plays a role, as might be expected. However the published micrometeorological works cited by Oerlemans do not discuss precipitation and so do not bear on the question, which remains open. There is no global precipitation dataset of a quality comparable to that available for temperature, and large-scale analysis is therefore not straightforward.

Oerlemans claims that "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". Why not? Standard temperature climatologies are based on measurements which, though far more numerous, are no less unevenly distributed than the mass-balance measurements. Improving the glaciological estimates deserves high priority, but dismissing what is currently available would not be a good first step. The mass-balance measurements, as assembled by CA and DM, constitute the best observational estimates glaciology has to offer for comparison with large-scale measures of climatic change.

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SIR,

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

Cogley and Adams (1998) provided a statistical analysis of mass-balance data from 251 glaciers for 1961–90. To my surprise, the mass-balance data from Stubacher Sonnblickkees (SSK), Austrian Alps, were excluded because the methods used to obtain them were deemed to be "cartographical or statistical estimates".

I believe the omission of these data is unwarranted, considering some of the data that were used, and suggest that the SSK data should be included in any subsequent analysis for the following reasons.

1. The methodologies used for mass-balance measurement are seldom explained (as I have done for SSK), so the accuracy of the data used often cannot be properly assessed.
2. Similarly, there are several datasets used by Cogley and Adams for which the quality of measurement cannot be adequately estimated due to lack of supporting information.
3. On SSK a very good relationship has been established between the accumulation-area ratio (AAR) and the mass balance, based on 17 previous years of "direct" measurement. If the AAR can be "measured" every year through an accurate survey of the "Ausaperung" (accumulation–ablation patterns) it suggests the accuracy is certainly adequate for the determination of the mass balance.
4. The Ausaperung method is certainly as accurate as "direct" mass-balance measurements on SSK since several statistical analyses have shown that the results, from a glacier with an extremely complicated topography, are very accurate. The "semi-direct" method here is as accurate, or inaccurate, as the "direct" measurements.
5. Of the many possible sources of error, one seems to have been overlooked by Cogley and Adams. This is the length of the balance year for which data are compared, which can make a big difference, sometimes much more than ± 200 mm a^{-1}. On SSK the natural system is used.
6. In the 1960s, much effort was made to achieve time- and labour-saving methods to ensure continuation of long-term mass-balance series. This is still important and will be more so in the future. Do Cogley and Adams mean to imply that the series from SSK, and those from many other glaciers, are intrinsically inaccurate and therefore useless?
7. The previous point will need to be addressed as more modellers and statisticians take advantage of the long-term data series collected by fewer and fewer field workers.
8. If the efforts being made to sustain long-term series by applying less direct measurement techniques mean that the resulting data are discounted, then more such series will be interrupted or discontinued. We will all be the losers if this happens.

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