Reply

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Prof. Woodall's point has bothered me too, and has been the subject of complaint by more than one student. I believe that the virtue of the Brier-Allen score can be defended, however, on the following grounds. Let us take essentially the situation mentioned in the second paragraph of Prof. Woodall's letter and require in addition that the forecasts be reliable in the sense of the term as used in the article. We have evidence that this is a reasonable requirement. We then consider the class of cases in which the estimated probability (and the relative frequency of occurrence) is 0.05. Our problem is whether to use 0.00 or 0.10 as the forecast probability. As Prof. Woodall points out the former scores 1.00 and the latter 0.81 on the occasion when disaster strikes. But what of the other, benign, occasions? Then the former scores 0.00 and the latter 0.01. The difference here is only one-nineteenth as large but the point is that it occurs nineteen times as often so that the long-run average score is the same for either strategy. Either suffers slightly and equally from bias forced by the requirement that the forecast be expressed in a whole number of chances in ten.

Partly as a result of our consideration of this problem we have recently relaxed this requirement in our daily forecasting at MIT and are permitting use of forecast probabilities to the nearest hundredth. I am using just the nearest 5% since I doubt personally whether we can order cases meaningfully into the resulting 101 prob-

ability categories. (It would take practically forever to answer that question on empirical grounds.)

The use of the magnitude of $(f_i - O_i)$ as a score would lead to biased forecasting because the score would be minimized in this case by forecasting 1.00 whenever the estimated (and presumably reliable) forecast probability is greater than 0.50 and by forecasting 0.00 whenever it is less. Squaring probably is instinctive with statisticians, but in this case the instinct seems to serve meteorologists well.