The Golden Fleece may not so much be awarded as pulled over our eyes. Diligent pursuit of minor derelictions gives the impression of protecting the public purse. However, if it becomes a substitute for dealing with major malfeasance, the results may be neither positive nor neutral, but doubly negative, as accomplishment is rebuked and malefactors rewarded.

Evidently what is needed for wholehearted government support is monstrous failure, accomplished "by the numbers" for bookkeeping purposes. No matter what the cost overruns or management ineffectivenesses may be, the Lockheed Aircraft Corporation, the Penn Central railroad, and the Social Security system, among others, receive massive infusions of government money with the only expressed desire that they try to use it more effectively. In Washington, gross failures seem to be either swept under the rug or alleviated by more money. In the meantime, projects that are infinitely less costly, and often quite successful, are made targets of ridicule and censure by these guardians of the public trust.

My hat is off to S. Dillon Ripley, who can build a National Air and Space Museum without having the contents ruined by a leaky roof such as the one at Kennedy Center. What Washington needs is more Ripleys and others like him who can get the job done, and fewer Proxmires. It is about time that bureaucratic spleen be spent on Washington-sponsored failures rather than on its sometimes eccentric successes. It is difficult to imagine that Buchwald's law is reversible, but one can always hope.

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Forecasting Climatic Fluctuations: The Winter of 1976–77

The highly abnormal winter of 1976–77, characterized by severe cold over the central and eastern part of the nation and drought over the West, has brought into prominence the subject of short-period climatic fluctuations and raised the question of whether such events might be foreshadowed. Evidence suggests that there was some consensus among people who have worked on seasonal forecasting problems for many years, so that the dramatic change of temperature pattern relative to the mild conditions observed in the East during the past five winters was foreseen.

Around 1 December 1976, I was furnished with copies of three independently made forecasts for the forthcoming winter (December, January, and February) by the National Weather Service; by Robert R. Dickson, a visiting scientist at the Scripps Institution of Oceanography on leave from Lowestoft Laboratories, England; and by Joseph Chase of the Woods Hole Oceanographic Institution. These predictions were compared with forecasts made by me at Scripps. The methods used in preparing these forecasts, while differing with respect to the weights given to certain parameters, include some common factors, particularly atmospheric behavior in antecedent months along with the month-to-month behavior of sea-surface temperature patterns over the North Pacific. A more detailed account of the methodology was given in an earlier report of a 5-year experiment (*1*) and more recently at a 1976 American Geophysical Union symposium held in San Francisco (*2*).

These forecasts have been verified by comparing them in three equally likely classes: below, near, and above normal temperatures for 99 equally spaced points over the contiguous United States. The ranges of temperature which define these categories (terciles) were determined from a 30-year climatic record.

There was reasonably good agreement among all four forecasters. For example, my forecast agreed with the National Weather Service's predictions at 50 of the 99 points, with Dickson's at 84 points, and with Chase's at 82 points. Chance agreement would be 33 points.

As for the temperature predictions, the National Weather Service's forecast was correct at 44 of the 99 points, Dickson's at 63, Chase's at 63, and mine at 59. The corresponding large errors of two classes (that is, when above normal was forecast and below was observed, or vice versa), were four in the National Weather Service's predictions, six in Dickson's, two in Chase's, and four in mine.

All forecasts correctly anticipated the switch to cold weather in the East relative to the past five warm winters. The major region of disagreement was at the boundaries between the warm West and the cold East. Agreement between all forecasts and observed temperatures occurred at 27 points of the 99 points. Chance agreement among the forecasters with observed temperatures would be at about 1 point [actually $(1/3)^4$]. The major regions of agreement occurred in the northwest and southeast quadrants of the United States.

The scores for temperature prediction cited above are in general higher than those obtained from the past record of a dozen experimental seasonal forecasts I have made, where the number of correct points averaged 42 and the number of two-class errors averaged 13. These past forecasts were significantly more accurate than climatological probability alone (33 correct and 26 two-class errors). The point I would like to stress is that, *at times*, premonitory "signals" may be sufficiently loud that moderately successful forecasts can be made by different individuals using objective methods as a base (*3*).

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References

1. J. Namias, *Mon. Weather Rev.* **93**, 449 (1964).
2. ———, in Geophysics Study Committee, *Geophysical Predictions* (National Academy of Sciences–National Research Council, Washington, D.C., in press).
3. Sponsored by the NSF Office for the International Decade of Ocean Exploration and the NSF Office for Climate Dynamics.

Low-Level Radiation: Predicting the Effects

In my letter to *Science* of 29 October 1976 (p. 478), I questioned predictions of cancer incidence based on the linear theory used by the American Physical Society in their reactor safety study (*1*), by the Environmental Protection Agency (*2*), and by others. Despite the arguments given in the letters from von Hippel (29 Oct. 1976, p. 479), Morgan (28 Jan., p. 344), and Brown (28 Jan., p. 348), no meaningful evidence of the validity of the linear theory is presented.

Von Hippel and Morgan cite an article by Modan *et al.* (*3*), on thyroid cancers produced in Israeli immigrants exposed to a thyroid dose of 6.5 rads during x-ray therapy for ringworm. Neither author notes that the 6.5-rad thyroid dose was the result of a cumulative scalp exposure of 350 to 400 rads. Evans (*4*), on the basis of personal communication with Modan, points out that the confounding effect of the concomitant irradiation of the pituitary, as well as the large statistical uncertainties, make use of the thyroid data questionable.

Morgan refers to an article by Stewart and Kneale (*5*) that is discussed in the BIER report (*6*, p. 165). That report points out that the linear relationship inferred by Stewart and Kneale is inconsistent with effects on 1250 children exposed to atomic bomb radiation in Japan.